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**Proprietorship of knowledge: The politics of social science
research in the Third World**

Crocker, Joanna, Ph.D.

University of Hawaii, 1989

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**PROPRIETORSHIP OF KNOWLEDGE:
THE POLITICS OF SOCIAL SCIENCE RESEARCH IN THE THIRD WORLD**

**A DISSERTATION SUBMITTED TO THE GRADUATE DIVISION OF
THE UNIVERSITY OF HAWAII IN PARTIAL FULFILLMENT
OF THE REQUIREMENTS FOR THE DEGREE OF**

DOCTOR OF PHILOSOPHY

IN POLITICAL SCIENCE

December 1989

By

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In the body of this dissertation I state that it is not enough to acknowledge "the many kindnesses of the people of village x," if one wishes to escape making people objects of research. Nevertheless, I do intend that acknowledgement be made here. Indeed, throughout the years 1978 to 1979, my many interactions with Melanesian friends and colleagues built the experiences that made this work possible. I wish especially to thank Ruth Lechte, Diane Goodwillie, Kuria Hughes, Salome Samou, and the many women involved in the various South Pacific YWCAs. I also thank the founders of the Vanuatu Plantation Training Center, Charles and Lesava Rogers, Simon Tapl, S. Gemma, and the first graduating class, for bringing to light many of my assumptions about development and training and taking the time to discuss them with me.

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I am also highly appreciative of the support and encouragement from my colleagues in the Participatory Development Project of the Resource Systems Institute of the East-West Center. In particular, Dick Morse, Kersten Johnson, Kathleen Wilson, Tonton Contreras, Margie Robinson, and Syed Rahim allowed me to try out in our weekly forums during 1988 and 1989 much that is expressed in this dissertation.

I also found the support from my academic committee to be exceptional. Much participatory research material is critical of usual academic research practice, yet when faced with tensions raised by my work in this field, each committee member used such situations as opportunities for further exploration.

I also owe much to my husband, Rodwic Fukino, who read the dissertation critically from his point of view as a native Hawai'ian and fellow writer. My debates and discussions with Rodwic during the many months of writing made me a different writer and the dissertation a different work, and I hope a better one as well.

ABSTRACT

This dissertation examines the relations between local or Third World knowledge and usual social science research. Discussions are anchored by case studies from Appalachia in the United States and the Melanesian South Pacific. Discussions are also focused by examination of what is termed "participatory research," which has been proposed as an alternative to and improvement upon traditional research practice. Participatory research is also critiqued as continuing the problems created by traditional social science research. The ensuing essential question is then: Are there strategies available for sustaining a critique and altering some existing structures and relations of dominance in social science research in the Third World?

However, this dissertation is not primarily about Third World or Melanesian people, nor is it about participatory research. Rather, the cases cited are concrete examples to make the rest of the discussion intelligible. Participatory research is then the topic by which the discussion is focused. Discussion of participatory research makes it possible to become more precise in examining both the limitations and possibilities of social science research in general and its relation to local knowledge.

The methods of analysis and approach to writing are intimately linked to the issues addressed in the dissertation. Although the dissertation is not a participatory research project in itself, important elements of participatory research are employed. The actual issues analyzed, boundary crossing, restitution, and interaction with those cited in case studies, and an on-going cycle of reflection on the process of producing the work are the elements of participatory research engaged.

Both the central topics and the process of examining the topics lead to a synthesis and conclusion that there is an emerging alternative research paradigm. The practice of the new paradigm is characterized by researchers who not only respect local

knowledge by refusing to speak for others but also act to create spaces in which local voices may be heard.

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PREFACE

In February 1980, I completed a management paper summarizing my impressions during a one year internship as acting executive director (general secretary in Solomon usage) of the Solomon Islands Young Women's Christian Association (YWCA). This paper completed requirements for the Master's in International Administration awarded by the School for International Training in Brattleboro, Vermont. The paper was subsequently filed in the University of Michigan library in the Women in Development section.

Several years later, Claire Slatter of the University of the South Pacific, Suva, Fiji, catalogued all papers written by outsiders on South Pacific women's issues. In the course of this bibliographic project, she discovered that very few papers had ever been copied and sent to the women, their supporting organizations, libraries, or governments of the South Pacific. Mutual friends in the YWCA contacted me to ask why I had not sent my paper to them.

I was stunned. It had never occurred to me that anyone in the Solomon Islands would have a use for such a paper; after all, it seemed that no one else on our board had a college education, and the level at which I wrote for my American institution put the material beyond their reach. Several of the women then on the board did not speak English. Furthermore, I had seen no reason to waste their time with material that was probably useless to them.

Of course, I immediately sent the paper to those who had requested it. I also began a process of reflection which culminated in the decision to write this dissertation as it now appears. Not only the choice of issues, but the manner in which I write about them are a result of nearly a decade of reflection and practice on the ethics of research and development in the Third World.

One of the above choices is represented by the style in which this dissertation is written. Most dissertations are produced for an audience comprising the graduate student's academic committee. In fact, rarely does anyone outside the disciplinary subfield of the particular department easily understand the dissertation. In this case, from the commencement of my writing I hoped to be able to summarize and translate any materials I produced into forms accessible to people in Appalachia or the South Pacific who appear in the cases I cite. During the writing process, I met and briefly worked with Elizabeth Cox, currently director at the East Sepik Province University in Papua New Guinea. Out of our discussions came my decision to write in a way that would be intelligible to Cox's Papua New Guinea students or that Cox could readily summarize for her students. I then re-edited the entire work in order to attempt to bridge the gap between an extremely sophisticated graduate department and Cox's undergraduate university students whose native language is not English.

The style choice is reflected in efforts to render theoretically sophisticated arguments in straightforward language, to explain certain theoretical terms when first presented--foregoing the convenient shorthand of terms that are available to both my committee and me, and to add some explanation on the first citation of well-known authors.

While my style choice does change the aesthetic of the dissertation and may make tedious reading for some sophisticated readers, I am convinced that the heart of the issue is, at the very least, to make acknowledgement where the people discussed in a research document do not normally have access or control over the research process.

In part, the purpose of this approach is restitution. By restitution, I mean that material I have gleaned from my experience with Pacific Islanders and others cited at length in casework is returned to them. Not only do I oblige myself to return a finished copy to the people I cite, but I have presented preliminary copies in order that they

might interact with me. I thus hope to disconnect from at least some of the usual research process which tends to reproduce relations of dominance.

Even where an individual researcher's intent is "good," all of us who work out there--with people who do not normally have a say in the research process--automatically tend to make our research subjects objects. There are already many Pacific islanders who can describe their own cultures beautifully. Rather than describing and interpreting the situation of people in the South Pacific, poor communities, or elsewhere in order to better work for change in their communities, I focus on the intersections or clashes between their local interests and usual research practice.

However, I do not advocate somehow abolishing all outsider originated descriptions and interpretations of what is going on in the Pacific or elsewhere. What I do seek is the recognition from my own colleagues that no matter what we do, when we write about other cultures, we are imposing interpretations on them. This is problematic when the others we write about have neither a say in the process, nor a chance to refute or correct the distorted images of them which we present to the outside world.

This dissertation is not primarily about Third World or Melanesian people, nor is it about participatory research. Rather, the cases cited from the Melanesian South Pacific and elsewhere are concrete examples to make the rest of the discussion intelligible. Participatory research is then the topic by which the discussion is focused. Discussion of participatory research makes it possible to become more precise in examining both the limitations and possibilities of social science research in general and its relation to local knowledge.

Broadly, local knowledge is here meant as the knowledge people have about themselves, their environment, and relations to others. This knowledge does not

depend on scientists and experts, but it is part of the everyday lives of people. The word "local" is not intended as a diminution of the worth of the people who express a particular local culture and knowledge. Rather, local knowledge is intended to distinguish the specialized knowledge people have about themselves and their group and environment. Although every group, including powerful elites, has knowledge peculiar to itself, my use of local knowledge implies that this is the knowledge that helps small or traditional groups to survive upon the land or in a particular area or segment of society. Whereas the knowledge peculiar to a powerful elite might be of use to all people controlled or manipulated by that elite, local knowledge is usually valued only by those whose survival it ensures. Although local knowledge is often concealed from elites, concealment is often for the purposes of preventing manipulation and ensuring bare physical survival.

The main focus is then an area not usually considered researchable--research practice itself in the so-called Third World. Until recently, with the exception of ethics discussions, moral philosophy, cultural anthropology, and occasionally sociology, research practice in the social sciences has not usually been considered as a field of investigation. While there have been almost unlimited texts printed about improving research methodology and new tools--usually quantitative--for social scientists, the nature of research relations often goes unquestioned. Particularly in the United States, social science investigations have been assumed to progress through the rigorous application of the scientific method. Knowledge gained through correctly applied social science research has generally been considered neutral and value free; power relations are not considered inherent within research practice. It is precisely this phenomena of the near invisibility of the play of power relations in research practice that renders research a highly political topic. How have the prevailing scientific, university, and development discourses come to portray research practice as without power relations?

Although some excellent texts treating the above question have already been published, usual research practice continues nearly unchallenged. The resulting questions about the persistence of usual research are still worth asking.

In that mainstream research, in the manner of other commodities, is produced, marketed, and consumed, there is a political-economy of research. It is here that I link the designations "Third World" and "First World." Rather than use the term Third World in the demeaning sense that the World Bank or United Nations do, I attempt to turn it around. Here, Third World indicates those groups of people in situations where they do not participate or are blocked from participating in decisions about research that is produced about them. Third Worlders are excluded and blocked from participation by various relations, including research relations, which tend to perpetuate themselves.

Specifically, this dissertation will examine the relations between local or Third World knowledge and usual social science research. Discussions will be further focused by examination of what is termed "participatory research," which has been proposed as an alternative to and improvement upon traditional research practice. Participatory research has also been critiqued as continuing the problems created by traditional social science research.

The methods of analysis and approach to writing are intimately linked to the issues addressed in the dissertation. In the final synthesis chapter, a strategy of boundary crossing, of not staying within one field, is advocated, while the strategy is also employed throughout the analysis and writing process. In other words, the process of doing the project is purposefully connected to the objectives and physical product of the writing as well as disciplines other than political science. The boundary crossings are from local knowledge to erudite and scientific knowledge, from local to global, from personal to collective to institutional, and from personal reflection to scientific analysis.

There are three main features in the dissertation.

1. A broad reliance on a political-economy approach as described above and expanded in chapter III.

2. A broad reliance on discursive analysis. Here discourse is understood as essentially the way of talking about something which in turn includes physical, actual practices and especially talking or writing about things. This is important in examining the gaps between First and Third World and what is said and what is, because mainstream social science presumes language is used to become clearer about what is and helps reveal fundamental causes and underlying truth. It is predominantly Western, scientific culture which assumes that there are single, underlying causes, and it is this language which makes it difficult to see or work from cultures which operate differently. In discursive analysis, rather than asking why, I attempt to ask: How did this come to be?

3. Although this dissertation is not a participatory research project in itself, important elements of participatory research are employed. The actual issues analyzed, boundary crossing, restitution and interaction with those cited in case studies, and an on-going cycle of reflection on the process of producing the work are the elements of participatory practice engaged.

The dissertation chapters are organized as follows:

In Chapter I, the problem is contextualized in the symptomatic closing down of research access in Melanesia. Apparent causes--reactions to neocolonialism and cultural imperialism--are related to the current debates in the social sciences as to what is correct scientific practice. Participatory research is introduced as both a critique of mainstream practices and a bridge between erudite First World university research and popular, local, Third World knowledge.

Chapter II surveys a range of origins and definitions of participatory research and what this indicates about its limitations. The chapter concludes that in many cases participatory research has very little to do with local control of the research process itself; in these cases, the research process is still interventionist.

Chapter III presents a range of theories that recent participatory research facilitators have used to critique past social science practice and legitimize their own. Because of exposure to these theories my own analysis is influenced and filtered; so I choose to register these influences as well as start to outline what this means for the role of the outsider in participatory research.

Chapter IV presents real world applications of participatory research that have been successful, particularly in terms of increasing local control and analysis. The cases presented are the Appalachian Land Use Task Force Study, the Farm [Plantation] Management Training Program of Vanuatu, and the Vanuatu Culture Centre Fieldworker Training Project. These cases all led to transfer of consciousness, greater organization, and changes in policy at the state or national level. The cases all involved outsiders and suggest possibilities and limitations for the outsider's role in participatory research.

Chapter V examines the institutional limits of participatory research both in terms of how sponsoring institutions influence research and briefly in terms of what opportunities are available to do more participatory research or make it operational in institutional settings.

Chapter VI extends and anchors the analysis begun in Chapter V by examining the limitations and possibilities for participatory research within Melanesian nation-states.

Chapter VII summarizes and synthesizes and suggests tactics for both First and Third World recognition of local knowledge. It is important to note that the chapter

does not seek to outline a grand or radical strategy for overhauling all research programs. What is sought is a conscientization (consciousness raising) of those doing research in the Third World. Finally, Chapter VII makes an argument that a new, overall paradigm of scientific knowledge has already begun to emerge and outlines that paradigm and what that means for participatory research.

Where my intent is to conscientize myself and my colleagues, I not only look at local reactions to research practices, but endeavor to look at how we, the researchers, in reproducing ourselves and maintaining our institutions, perpetuate undesirable research relations.

These relations are often complex. They do not consist merely of a research elite's physical and coercive power over those who are researched. Thus, I aspire to treat knowledge and power not as something that some people have more or less of, but as lived relations.

In reflecting on this process as I do it, knowledge is not only a tool for institutionalizing unequal relations for the benefit of the dominant or for the oppressed to gain power over the dominant; knowledge is viewed as involving the experience of lived relations which may themselves be mined, suppressed, invalidated, depoliticized, or liberated and liberating.

CHAPTER I

INTRODUCTION:

RESEARCH ACCESS, ACADEMIC INQUIRY, AND RESEARCH RELATIONS

[O]ppression is the greatest calamity of humanity. It diverts and pollutes the best energies of man--of oppressed and oppressor alike. For if colonization destroys the colonized, it also rots the colonizer.

(Albert Memmi, The Colonizer and the Colonized.)¹

I

In 1988, the French colonial administration of New Caledonia partially destroyed the collection of documents and records assembled by the *Institute culturel scientifique et technique kanak* (Kanak Institute of Culture, Science, and Technology). These are the closing words in the order for destruction:

...and then go burn them at the dump and be especially sure that no one sees you and that you don't leave the place until everything is completely burned.²

According to the April 1988 edition of the Kanak newspaper, Ewenenando, 7,500 documents and recordings were destroyed, and ethnocide was being committed. Melanesian separatists and anti-colonialists (who prefer to be called "Kanak") saw the destruction of the research records of their culture as clear-cut ethnocide. Where genocide is the destruction of the genetic heritage of a people through the destruction of their physical bodies, ethnocide is the destruction of cultural heritage. Although the people may survive physically, it will only be as a darker skinned variation of the culturally homogenized European whole.

What is significant here is that the main avenue for carrying out the ethnocide was through the destruction of research records--clearly a case of book-burning. "Knowledge is power" goes the old adage and for good reason.³ An aspect of power is a group's ability to constitute itself as a group; to tell others "we are us, not you," or "thus are we related to you as fellow humans and demand our rights."

The Kanak research collection contributed to the knowledge base that served to assert and legitimate Kanak identity and rights' claims. The Chirac administration's destruction of most of the collection was a brutally direct effort to erase the Kanak knowledge and power base in order to keep Kanaky (the Kanak designation for New Caledonia) as New Caledonia--a part of France that provides important minerals for industry.

One may easily contend that there are few things more universally detested than the imposition--or the denial as in the Kanaky case--of identity by those who are dominant upon those who are not. Resistance or at least recognition of the imposition, however, may not be concomitant with taking up responsibilities and initiating action. Why do objectionable practices continue despite the growing clamor from the researched? The purpose of this dissertation is to use participatory research as a lens in order to critically examine social science research practice in Melanesia and in general in relation to social "scientific" practice including a background of bureaucratic institutions, transnational corporations, newly forming classes, the researcher herself, and their roles in the global political economy of research.

Ensuing questions and discussions of possible alternative practices are pursued in subsequent chapters. The purpose of this first chapter is to situate the dissertation by briefly outlining and linking research access, research relations, and the persistence of positivism in academic inquiry in order to problematize certain relations and practices. The first step is to problematize lack of respect for local knowledge and exploitation of local people for other knowledge bases.

In recent decades, many have equated powerful and valuable information with that generated by applied science and technology, leaving tribal, primal, local, and other knowledges as somehow "soft" and less valuable yet also dangerous. This is the paradoxical condition in which the colonizer, the dominant, or mainstream's identity is

more and more constituted by what it is not; identity is dependent on the continued existence of the other, sometimes more clearly so when the other is demeaned. The paradox becomes tragic as the dominant member of the bipolar pair seeks to cement over the places where differences exist in fear of the very difference which defines his existence. Often, as the cementing becomes more pervasive the repressed other springs up--sometimes explosively.

For example, the "soft" knowledge of women's issues and women's own efforts to reconstitute their identities has led to the powerful uplifting of women's voices in the last few decades in the West. "Saving" and revalidating of indigenous knowledges by their own people as, for example, in the Hawaiian renaissance and Hawaiian rights movements, along with others during the last decade has contributed to the formation of an international organization of indigenous peoples recognized by the United Nations since 1986.⁴ Australian aboriginal peoples have increasingly spoken out against the monopoly on the images propounded about them to the world by anthropologists and other social scientists.⁵ Indigenous Australians have demanded the right to determine what research is carried out in their communities by which researchers and for those purposes the aboriginals see as valid. In a recent document discussing research on relevant education for aboriginal communities, the writers announce, "We control the process ... we decide the questions to research ... we decide the way that the information from our research should be recorded."

The cementing over of otherness and resistance to it has occurred in more subtle and complex forms with recent changes in research access in the Melanesian Pacific. In the Melanesian Pacific, social science research access was frozen in the Solomon Islands and Vanuatu in 1985 and 1986 respectively. Research access has been increasingly controlled and limited in both Papua New Guinea and Fiji. In New Caledonia and Fiji, access both by outsiders to the rural Melanesians and by them to

outsiders has been limited in various other ways by the repressive aspects of the regimes now in power.

These research access changes work on several levels. On one level, rural and isolated peoples are protected from the disruption that can occur from research exploitation. On another level, local people are in a zoo-like situation where they are less likely to receive stimulation and make analyses and subsequent demands for participation in national government policy making. In the course of the research access changes, some government officials involved have been genuinely concerned by severe cultural disruption outsiders have caused. Other officials have hoped to prevent or dilute separatist demands from tribal groups, when these demands serve to further weaken the fragile and relatively new national governments. Yet again other officials, keen to increase their project portfolios which in turn justify on-going disbursements for their department overheads, seek ever more overseas aid projects and joint venture arrangements. Many of these projects are related to cash cropping and minerals extraction and involve "hard" and social science research predicated on assumptions of unlimited production and consumption.

Meanwhile, in the social science departments of universities and research institutions of the technological world--Europe, the United States, Canada, Japan, and others--debates increase over what constitutes a valid field of inquiry. These debates have also come to include issues surrounding Third World research and development.

The Third World intended in the title of this dissertation encompasses all those people(s) who tend to become the objects of research carried out by those either originating or educated in the technological First World. Peoples objectified by social science research live in the slums of East Los Angeles, hollows of Appalachia, Arctic reaches of Canada, remote islands of the Pacific, and in the low income echelons of First World communities. Broadly, this dissertation considers all those who do not

normally have control over the information generated and disseminated about themselves as a Third World of researched people. Rarely do these people generate information about themselves in forms that are recognized and validated by the outside. At the risk of attaching a monolithic label, research Third World is used to designate people who experience certain specific relations in the course of research projects. This designation builds on the usual geo-political and ideological meanings attached to the designation "Third World."

This dissertation also considers the researchers' First World to encompass those who do the information generating and disseminating within or formed by organizations related to international and global technologies. This includes trans-national corporations, universities, the World Bank, regional and development banks, other multi-lateral bodies including the United Nations, private and non-governmental agencies (PVOs and NGOs), and even churches.

Despite the growth and proliferation of organizations representing indigenous rights, limiting or freezing of research access by Third World countries, as well as numerous and substantial attacks on traditional research and development practices in the social sciences, relatively little effect as yet has been made on current mainstream research awareness let alone practice. It is for this reason that I have chosen to write at a broad level. Further work extending the limits of the problem should address a number of issues such as intellectual property laws and the New World Information Order.

An example of the overall situation in the university sponsoring this dissertation is represented by Earl Babbie's The Practice of Social Research. The book is typical of the fare in introductory methods courses in social science departments throughout the United States.⁶ Babbie's text contains no passages even in the recent fourth edition evidencing consciousness of the above sorts of issues. In fact, Babbie baldly states:

[S]ocial science is not only relevant to the kinds of major problems I've just listed, but it holds the answers to them ...Ultimately, only social science can save us from over-population.[Emphasis added].⁷

In other words, only experts from universities can save the world.

These same social science departments generally do have at least one course dealing with whatever theoretical trends are going on in their fields. Thus, students emerge from university departments aware that intense debates are going on, but unable to engage fully in those debates through the approaches they take in research. That is, a student may be able to write about a contentious topic, but be unable or unequipped to approach it with methods much outside the traditional research approaches and thus unwittingly recreate that which she critiques.

Another recent indication of the lack of awareness about social science research as a problem in itself is evidenced by the summer 1988 catalogue for the University of Michigan at Ann Arbor's Summer Institute on Survey Research Techniques.⁸ Not a single course description includes a suggestion that the preceding research issues exist.

When consciousness exists that research practice in itself is contentious, that consciousness is limited to the level of the outsider's lack of access or her own--individualized--presence. David Challinor, assistant director of one research branch of the Smithsonian Institute has said, "Our main concern is to keep research access open across borders."⁹ While Challinor's concern is understandable in the natural sciences where one might be pursuing a vanishing migratory species, there is little consideration that people at the local level have a stake. There might be concern that a wildlife refuge might displace bush people or peasants, but not much else. There is an implicit assumption that outside experts really do know best when it comes to research. This assumption becomes much more problematic when the social sciences are involved.

Most of the attention is focused on the researcher's interaction with those granting research access and those to become researched objects rather than the

reasons for the hosts' questions and most especially not on self-reflection about the nature of the researcher's institutional sponsorship. All responsibility is then vested in the individual researcher's interactions with host country governments and citizens.

It is difficult for most researchers to reflect on the very process of research that leads to their own book or dissertation in turn leading to their validation as a scholar. It is difficult to question the process that forms us, particularly when what is applied is supposedly the scientific process and hence neutral and serving the truth and where personal reflection is labelled "personalizing" and considered bad practice. This to some degree accounts for why the questioning of research practice does not occur so much at the First World institutional level, but at other grassroot levels.

Perhaps another reason that questioning of the research process has tended to come more from the grassroots' level has to do with the increasingly evident failures of modernization. Technological development has not erased poverty, and in fact absolute numbers of poor--those below the margin of adequate daily subsistence needs--is increasing. Development and "trickle down" have not occurred. In the Third World, the poverty and marginalization, the barriers and distances between classes are becoming greater, hence research and development failure is clearer. Whatever the case, village people among others are quick to note that although researchers promise that surveys are for their own good, usually nothing changes in the villagers' and others' daily struggles for existence. These people see very real needs, extraordinary sums spent on what seems like meaningless research to them, and no improvement in their lives.

Again, the first step is to problematize lack of respect for local knowledge and exploitation of local people for other knowledge bases.

II

During 1979 while working in the Solomon Islands, I visited Santa Ysabel Island for preliminary discussions with villagers interested in organizing women's centers. In the course of the visit, a village elder told me:

Some strange white men came to our village. They stayed maybe twenty minutes, and then went away. I'm not sure what they came for, but the last time I went to Honiara [the capital] my kinsman who works in a central government office showed me a report this thick (gesturing). Now what I want to know is what did they find to write about our village in such a short time? What do they really do with that stuff?⁹

More recently, Marjorie Crocombe, then director of the University of the South Pacific branch system, advised the East-West Center Telecommunication Conference (East-West Center, January 24, 1987) that research by outsiders in the South Pacific is highly problematic. In a subsequent interview, Crocombe cited one futurist at the conference. When told by islanders that his satellite communications research proposal was ten years too late and wouldn't yield locally useable information, the futurist was more concerned with securing his own research access into the country than giving any thought to his research relations.¹⁰

Research relations have been under fire from top to bottom throughout island Melanesia. What follows is a summary map indicative of research issues and current access status in each of the region's countries. An actual geographic map may be found in Appendix A. Additional analysis of this subject is included in chapter six.

The most recent letters on research access received from the various Melanesian governments may be found in Appendix B. Indeed, original, primary source material on the exact and current status of research access is difficult to come by. When I commenced the dissertation, I wrote letters to each of the countries surveyed requesting this information and more than six months later not a single reply had been received. A subsequent round of letters did generate a few brief responses. Most of my information

was culled from interviews with former research council members, long term researchers, university administrators from the various countries, and the like. Furthermore, a detailed survey of every issue of Vanuatu's newspaper and the Government Gazette during the period in which Vanuatu's research access was frozen yielded a single piece dealing with any aspect of the issue. The piece was a letter to the newspaper The Vanuatu Weekly from people on the island of Tanna asking that the government not allow in any more documentary filmmakers, researchers, journalists, and other outsiders who "get in the way."¹¹

Papua New Guinea

As early as 1973, two years before independence, The Economics Students Association of the University of Papua New Guinea published an editorial in their newsletter condemning the mining of research information by outsiders, the outsiders' overall cultural insensitivity, and the lack of regulation requiring researchers to provide duplicate copies of their publications as well as summaries in simple English.¹²

As of this writing, research access is permitted provided one goes through the relevant provincial government, relevant national ministry, the Institute of Papua New Guinea Studies in Boroko, and of course the Migration and Citizenship Division of the Department of Foreign Affairs. Prior to these contacts one must first acquire affiliation with an appropriate institution. Institutions offering affiliation are the Institute of Papua New Guinea Studies, the Department of Anthropology and Sociology at the University of Papua New Guinea, the Education Research Unit at the University of Papua New Guinea, the National Museum and Art Gallery in Boroko, and the Institute of Medical Research in Goroka.¹³ Research permit fees have run at approximately US \$200 during the early to mid 1980s.

Unfortunately, the presence of a complicated entry and access process does not automatically serve to educate those applying. Nor does the process protect rural

people from researchers on a quick in and out on a tourist visa or increase dialogue between outsiders and the people to be researched. As one Papua New Guinean colleague remarked, "The good ones never get to come, because they try to do all this stuff and no one answers their letters, meanwhile the bad ones come anyway and no one finds out that they pulled a quickie until they have already left."

The journal Research in Melanesia documents a debate between William E. Mitchell of the University of Vermont and Joseph Sukwianomb on behalf of the Wape group in Papua New Guinea.¹⁴ Mitchell in a rebuttal to a critique by Sukwianomb of his book The Bamboo Fire: An Anthropologist in New Guinea condemns Sukwianomb for his poor use of the English language and thus trivializes Sukwianomb's critique. Sukwianomb in turn replies:

It is of course easy to draw someone with an argument over language usage, especially the language of imperialism. Is he implying that I should not have ventured into reading, understanding and, most importantly, questioning what a foreigner like Mitchell himself writes about the people of Papua New Guinea?

The issue yet again is the image propounded and the participation or lack thereof of those propounded about in its manufacture.

Solomon Islands

From 1984-1985 on, social science research in the Solomon Islands was frozen by their government. The triggering incident was the publication of Roger Keesing's book co-authored with Peter Coriss, Lightning Meets the West Wind.¹⁵ Publication of the book was associated with compensation and separatist demands from the Kwaio clan on the outer island of Malaita. A number of concerns surfaced during the controversy, but general consensus has it that the greatest concern of provincial and central government officials was the potential of outside questioning to mobilize isolated groups such as those in the archipelago's outer islands. The political alliances that have turned hundreds of clan groups into a nation state are only eleven years old, and

those who run the national government and businesses would have much to lose if the central government were destabilized.

Following the months in 1983-1984 when Keesing was requested to leave the Solomons and research was officially banned, an East-West Center researcher, among others, pursued a survey of small business practices and needs.¹⁶ This survey was attached to an overall effort of the Central Planning Office in order to promote economic development and perhaps not coincidentally in the hopes of receiving overseas aid grants. Despite official word that all social science research was banned, research labelled as an economic survey and designed to lure in foreign investment was allowed where research into sensitive local issues--even though the local people requested that research--was blocked. The key point here is that local knowledge can become a research commodity involving many players with complex motives seeking to control that commodity. These efforts at control in turn shape research as a practice. The significance of this sort of practice will be discussed further in the analyses in Chapters Five and Six.

Vanuatu

From 1985 through 1989, officially, all social science research in Vanuatu was frozen. On occasion, some researchers with previous experience in the country have gone through, thanks to official oversight, as "consultants." In January 1988, however, even this access was tightened.¹⁷ Natural science, including research in the areas of crop production, cattle and crop diseases, soil composition, vulcanism, and the like, have however continued. The former research council which included the Prime Minister's first secretary explained that these areas of natural science research are "whiteman" things anyway and don't really interfere in the local culture and customs and are thus acceptable. Furthermore, the consensus is that in all things cultural it

will be better to wait until a Ni-Vanuatu (native of Vanuatu) is capable of undertaking this research.

Previous interviews of research council members lead me to suggest that at least for the individuals on the council the concern with disruption of local culture was and is genuine. Where natural science is concerned, the view is fairly pragmatic. There are just a few Ni-Vanuatu students who are receiving solid math and science background, and they won't graduate for years; in the meantime, meteorologists and volcanologists are welcome in a country prone to cyclones and dotted with live volcanoes.

Additional and substantial reasons provided for the research freeze are prevention of research mining, cultural imperialism, outright exploitation, and the like. Research mining occurs when the outsider exploits the tribe or village for information that contributes to her dissertation, book, or subsequent tenure while contributing nothing to those people. As indicated previously, few tribal or rural groups have had the opportunity to participate in any way in the manufacture of the image propounded about them. In many cases, researchers would stay in village homes, eat their food, hitch rides on their vehicles, and never even leave a copy of the documents they produced, let alone render them in the local language in a way that villagers could use to increase their understanding or action on local issues.

Incidents triggering research closure in Vanuatu may have been those prior to the publication of Jeremy McClancy's To Kill a Bird with Two Stones.¹⁸ During 1979-1982, McClancy, a British doctoral student, commenced his anthropological research on the island of Malekula in a highly traditional area. Shortly after he started his questioning process fights broke out among tribal people. This was considered serious enough that McClancy was requested to discontinue work on the island of Malekula, but he was permitted to stay in Vanuatu. McClancy removed himself to the southern island of Tanna but with much the same results as in Malekula. Finally, McClancy was

requested to stay in the capital city and produce a book in simple English that would give Ni-Vanuatu access to their own history. The subsequent book has been much appreciated by Ni-Vanuatu, but unfortunately for those seeking easy research access, McClancy's calamities came to the attention of the Prime Minister and others.

Removal of the colonial administration, the joint condominium of British and French, was achieved by forging a new national identity as Ni-Vanuatu rather than people of numerous clans on scattered and isolated islands. Vanuatu, like the Solomons, could not afford separatist or tribal movements while achieving and then maintaining some degree of independence. Researchers at the grassroots even with the best of intentions can inadvertently threaten the precarious balance by simply posing questions, and this is where in part the well intentioned and highly competent McClancy and Keesing erred.

The blocking of research access due to political motivations unsupportive of local groups connects to my concern over exploitation of local groups through research access. Where policy makers wish to solve the problem of research exploitation of local and indigenous peoples, they will find themselves caught in tugs of war. The tug of war is over who gets to control the resource--local knowledge. This is part of what is signified by the words propriatorship of knowledge in the title of this dissertation.

New Caledonia

New Caledonia, officially an overseas territory of France, is legally subject to the same research restrictions as any area in metropolitan France. During the 1986-1988 disturbances of the Chirac administration, it has been difficult for outsiders to do grassroots and/or rural research. Notably, one piece of "research" that was popularized during the period was an archeological report which contended that prior to the "black" Melanesians New Caledonia had been occupied by a lighter skinned race, and thus the

Melanesians--many of whom wish independence from the French--are just as much newcomers as the French Metropoles.¹⁸

The Metropoles are a sizeable group of recent newcomers (as of this writing any French citizen who has been there even a few months can vote in New Caledonia) who together with the white "Calédoche" minority, mixed race people, and some Polynesians and Indo-Chinese tilt the balance in referendums away from what was a Melanesian majority until the last decade.

For all practical purposes, access by non-French citizens to rural people has been sensitive if not dangerous. In some cases, the roads into the rural areas have been blockaded by the Calédoche (white) anti-independence groups. Explosive pressures gave way in April 1988 to the kidnapping of French gendarmes by pro-independence Melanesians or Kanaks (the French had refused any discussion or negotiation at all for over two years) and subsequent bombing and storming of the Kanak stronghold by the French navy under highly suspicious circumstances.¹⁹ This was the situation and context of the research collection destruction cited at the opening of this chapter.

The pushes and pulls over the independence question underlie all aspects of life, and of course research, in New Caledonia. For example, anti-independence French see Australian calls and support for Kanak liberation as an ideological disguise for a desire for new markets.

Even prior to the Chirac government, dissemination of political research was tightly controlled. In 1984, Monash University (Australia) political science doctoral candidate Myriam Dornoy (then a French citizen, now a permanent resident of Vanuatu and director of the Vanuatu branch of the University of the South Pacific) published the first in-depth English account of Politics in New Caledonia. When publication was announced, French government officials "requested" that Dornoy's father, a military

officer, have Dornoy withdraw her book--which is not particularly radical by American standards--from publication.²⁰

Although there do not appear to be many official restrictions on research in New Caledonia, what is limited in the colonial situation is interaction of Kanak activists with outside researchers when that researcher supports a particularly pro-Kanak image or appears to undermine the colonial government. While New Caledonia is still a colony, superficially the research access situation appears much different than the other Melanesian states. The issues involved when outside researchers work with minority and tribal groups or local people in general remain the same.

Fiji

In Fiji, in recent years, applications for research access have been processed through the Ministry of Home Affairs. Currently, one would approach a sponsor of any sort and then make concurrent applications to the Ministry of Home Affairs and the Permanent Secretary of Education and the Ministry of Education's Research Committee. My most recent interviews with Western researchers going to Fiji reveal that applications tend to languish for as long as six months.²¹ Several researchers have said that due to time limitations, they had their sponsoring agency file on their behalf and just went ahead. The basic research access situation for a First Worlder has been controlled but not prohibitively so.

Since May 1987, however, in depth activities have been hampered by two military coups which although not particularly violent have included up to this writing curfews, harrassment of dissidents, and a large exodus of technical and professional citizens. The result of the coups--staged by a Melanesian army colonel--has been to create greater divisions among a population split between Melanesians and Indians. Obviously research on the coups and related subjects, including the Christian church's role in supporting the curfews, would be blocked. On the other hand, natural science

researchers and economists interviewed have experienced no difficulties in obtaining research access. On the whole, the research access situation in Fiji is similar to that of Papua New Guinea.

The preceding map of research access is complex even in brief. Although it is clear research relations are contested, there are several points of view represented:

1. Local people such as the representative of a women's group who feel they have been exploited by researchers and/or inattentive central government officials..

2. Researchers ranging from those who respect local knowledge, but until too late do not recognize their own political impact, to those who feel the people are necessary but inconvenient objects in their research tasks.

3. So-called Third World governments run by native and expatriate people with various alliances to rural majorities and capitalist and trans-national elites.

4. International institutions and multi-lateral organizations sponsoring research and development.

These points of view are rarely neatly uniform and monolithic. Alliances, allegiances, and understandings cross all over. Few outside researchers totally disrespect local people, but their underlying attitudes and practices reveal a number of contradictions. These will be expanded in other chapters and particularly chapter six.

III

Relevant aspects of the current critiques, debates, and dead-ends in the halls of academe are mapped next. In the center of this map is the question: what constitutes a valid field of inquiry?

Central to many of the critiques including that of participatory research is that any area of inquiry that does not fit the methods of scientific investigation and that does not lead progressively to technological products is not a fit area of inquiry. The ways of knowing outside of those scientifically applicable have become seen as unworthy.

irrelevant, and not even real. The consequences for local, non-technological knowledge is disastrous, but first this map sketches the situation in academe.

No country has produced more social scientists who are proud to be called logical positivists than the United States, so it is not surprising that scientists in the United States have also become major contenders in the debates and general questioning about what constitutes scientific inquiry.

Here, positivism is the name of the doctrine that the sense perceptions are the only valid basis of human knowledge and precise thought. Positivism assumes that there is a correspondence between what we perceive and the real reality out there. Positivism is characterized by the type of quantitative social science so popular with Americans which in turn is characterized by surveys and supported by mathematical computations and expressions.

Thomas Kuhn is widely acknowledged as a pathbreaking writer in the history and philosophy of science. His 1962 work on the nature of paradigm shifts in scientific revolutions has opened spaces for questioning and debate.²² For the most part, Kuhn discussed the natural rather than the social sciences. Nevertheless, Kuhn's work has had an important effect on social scientists. Social scientists have often sought greater legitimation for their work by attempting to pattern their methods after the methods of natural or so-called "hard" sciences. Consequently, in both cases where social scientists have accepted the natural science model, or they have rejected it, Kuhn's work has been recognized as raising important questions. Debate on these questions has continued to increase in the years since Kuhn's publication.

Central to understanding Kuhn's importance is the concept of a paradigm shift. Kuhn's writing provides a number of definitions for the concept. For purposes of this dissertation, a paradigm is understood to be a way of thinking that is greater than theoretical understanding generated from a single theory. A paradigm shift then is a

fundamental change in the thinking of a number of people. The shift would include underlying assumptions about how the world works and color all subsequent models.²³

Kuhn is important to my work for launching basic questions about the way that scientific practices come to be considered valid and how those practices change over time. Kuhn describes the increasing accumulation of anomalous experience as rising to a tide that overwhelms old theories. This is starting to happen in respect to strictly positivistic social science which has held sway in the United States during the twentieth century.

One of the more recent and complete deconstructions of American social science has been published by philosopher of social science Peter Manicas in A History and Philosophy of the Social Sciences.²⁴ Manicas points out that nearly every practice of mainstream natural science that has been used in social science has come under fire and/or been abandoned by many during the last decade.²⁵ Although Manicas places himself squarely in the middle of the social sciences, "My aim ...is to defend social science as a theoretical science which, like physical science analogs, seeks to understand the world," he is also clear about what he is not willing to accept:

Given the extraordinary limits on experimentation in the social sciences, it is no wonder, then, that regression techniques, path analyses, and so on are so attractive. Given all these assumptions, one can be a real scientist without having a theory and without ever doing a real experiment. All we need is data--and plenty of it!²⁶

Richard J. Bernstein in his The Restructuring of Social and Political Theory critiques the mainstream represented by sociologist Robert King Merton and his theories of the middle level.²⁷ Bernstein, like Manicas, demonstrates central difficulties with positivism:

[T]hey interpreted the history of thought as one where disciplines floundered until reformulated into the canons of scientific discourse. In its boldest phase, positivism claimed that anything which cannot be so reformulated was to be judged meaningless, or to be seen as raising pseudo-problems incapable of rational solution.²⁸

That American social sciences are undergoing paradigmatic and theoretical shifts is apparent across disciplines. These shifts are most apparent where scientific knowledge and local knowledge intersect or clash. The clash is even more pronounced where the type of scientific knowledge at stake has been produced by the positivist paradigm. Positivist type mathematical models, multi-variate regressions, and the like are the least accessible to local people. On the other hand, local knowledge about complex environmental relationships, when couched in the language of metaphor, myth, and feeling, is least accessible to the positivists.

Although positivism still prevails in the majority of American university social science departments, there are many powerful critiques and a few alternative practices emerging. In his opening essay "Partial Truths" for Writing Culture, ethnographer James Clifford discusses changes in his discipline.²⁹ The power and knowledge relationships inherent in portrayal--in "writing about peoples" are the essence of the shifts:

Ethnographic work has indeed been enmeshed in a world of enduring and changing power inequalities, and it continues to be implicated. It enacts power relations. But its function within these relations is complex, often ambivalent, potentially counter-hegemonic.

Anthropology no longer speaks with automatic authority for others defined as unable to speak for themselves ("primitive," "pre-literate," "without history").

Princeton economist Albert Hirschman writes in Getting Ahead Collectively about grassroots initiated projects in Latin America, and as he writes he strays from conventional neo-classical economic wisdom.³⁰ Hirschman moves far from discussions of export oriented industrialization when he discusses the importance of cooperative efforts and collectivity at the grassroots:

But it seems safe to assert that, with such a network, social relations become more caring and less private. Now we know that certain types of regimes depend for their stability and untrammelled authority on the thorough privitization of their citizens' lives ...in authoritarian

regimes the masses are demobilized to the greatest possible extent. Demobilization means privatization: everyone is to be concerned exclusively with his own welfare and that of his immediate family.³¹

Further reading in Hirschman reveals that marginalized citizens can and have organized themselves in various collective forms to overcome oppressive problems. In Hirschman is also the view that local knowledge and analysis has its own wisdom which is often superior to that of the outside.

Hirschman's work is important for advocates of letting local people and knowledge be, because of his divergence from the majority of economists. For the most part, American neo-classical economists consider themselves closest to the natural sciences due perhaps in great part to their reliance on mathematical models. The more economists and other social scientists work to build sophisticated mathematical models, the less they are likely to be interested in local knowledge produced by local people who have no access to the language of mathematics and modelling.

Contrasting Hirschman's view with those still current in realist and balance of power approaches to international relations and comparative politics helps to reveal current debates. Many "realist" policy analysts still subscribe to Henry Kissinger's approach which he summed up during his Harvard student days with his favorite quote from Goethe:

If I had to choose between justice and disorder, on the one hand, and injustice and order, on the other, I would always choose the latter.³²

Certainly, Kissinger style realism is inimical to local collective analysis and respect for local wisdom. Local analysis at its democratic best can lead to messiness and disorder as a group reflects, debates, and acts on its perceived alternatives.

Many texts in sociology or political science surveying current theories are exploding with the evidence that there are multiple and almost totally contradictory theories in their fields. Sample the 1987 Idioms of Inquiry, edited by Terence Ball; essay topics range from Jurgen Habermas, to post-structuralism, to realism. About the

only things some of the essays have in common is that they occur in the same publication.³³ Despite the growing diversity, positivism persists. The tenacity of positivism is revealed by the vast amount of space devoted to articles critiquing it; many scholars find that before they can discuss alternatives, they are obliged to deal with the monolithic past of the social sciences. Although certain discussions are emerging in academe such as those of Clifford and Hirschman cited above that respect local knowledge, the great majority--centered around positivist and realist paradigms--do not.

IV

What is really of interest given the preceding maps and their intersections is how little aware the First World researchers are of the objections to and reasons for Third World critique of First World research practice. Yet many of the preceding concerns were raised in alternative forms in the concluding "Resolutions and Recommendations" of the Young Nations Conference held at the University of New South Wales, Australia in August, 1976:

Resolutions and recommendations urged that all [foreign] social scientists working in the Pacific ...study their own social and cultural systems, power structures, and elites rather than those of South Pacific countries alone...The products of such research would be provided to people living in the Pacific...³⁴

In particular, the third resolution called for the following areas of research to be given support:

- (a) The involvement of Christian Missions in the Pacific, including Church investment in foreign-directed business enterprises operating in the Pacific region.**
- (b) The motivations and aspirations of universities and their links with the power structure.**
- (c) Research into policies of multinational firms whose decisions affect island nations, including activities and impact of these foreign-based business organizations in the Pacific.**

To the degree possible, this dissertation was initially planned with respect for the resolutions, particularly 3B. A copy of the resolutions may be found in Appendix C. Although I still greatly respect the resolutions, I found that the degree to which I was able to address them was quite modest. However, I reemphasize my commitment and the fact that concern for the resolutions underlay most all of this work. The situations limiting my work will be treated as they come up in the context of the dissertation.

The Young Nations Conference documents and appeals remain important over a decade later for the following reasons: 1) This was the first and so far only major conference where Pacific Islands research was the main and exclusive concern, and 2) where native Pacific Islanders played a major role in discussing research done about themselves and their people. It also bears mentioning that 1976 was the year after Papua New Guinea's independence and two years before the Solomon Islands' celebration of independence; ideals and aspirations of emerging, young scholars and leaders were at a high. In fact, one of the resolutions calls for outside researchers to respect the aspirations of Pacific peoples.

Many of the conference resolutions are in conjunction with ideals and practices of what is popularly known as participatory research (PR) or alternatively participatory action research (PAR), or even action research (AR). Throughout this document, the term participatory research will be used generically. Likewise, participatory research ideals and practice are adopted, as described in the preface of this dissertation, to the limited extent possible while also serving as a focus for discussions of possibilities and limitations.

Budd Hall, one of the co-founders of the Participatory Research Network in the early 1970's, describes the definition of participatory research that emerged from the international forum on participatory research in Ljubljana, Yugoslavia in 1980:

Participatory research is a three-pronged activity: a method of social investigation, an educational act and a means of taking action.³⁵

The majority of participatory researchers to date would agree with Colombian Orlando Fals Borda who has described the relation of popular knowledge formed through participatory research and traditional research:

[T]here exists a scientific apparatus built to defend the interests of the bourgeoisie, and this is the dominant apparatus today at local and general levels of the so called Western nations ...But this popular wisdom, folklore, or knowledge also has its own rationality and its own structure of causality; that is to say, it may be demonstrated that it bears merit and scientific validity *per se*. This knowledge quite naturally remains outside of the formal scientific structure built by the intellectual minority of the dominant system because it means a breach of its rules, and hence the subversive potential which is seen in popular knowledge.³⁶

Thus this dissertation places research practice itself as the problematique while attempting to employ to the degree possible participatory research practices which coincide with many of the Young Nations Conference resolutions cited.³⁷ Attempting to employ aspects of participatory research in this way also serves simultaneously as critique and counter-discourse. By counter-discourse one refers to other ways of talking about and approaching a generally accepted practice. In other words, the process of looking at an old practice in a new way in itself may become a tactic for changing that practice. Here, my main intent is to change my own discourse and raise the consciousnesses of my research colleagues. I do not aim to work first on Pacific islanders, but rather more modestly hope to alter my own sphere in hopes that spaces will be made for Pacific island colleagues to explore.

The second level of this project is to use the difficulties encountered in the effort to employ new tactics--including the attempt to work on and with participatory research--in the traditional university research structure as data. Difficulties encountered can become part of a self-conscious, self-reflective, and interactive process. Again, the project is not about Melanesia or participatory research as much as it is

about politicizing research practice in the Third World and searching for alternative practice.

Another level of the problem is the production of knowledge itself. Somewhere at the level of trivializing the other's knowledge, we make our knowledge more secure, our identity supposedly more secure. How do we know what we know? In short, conducting this investigation and analysis in the spirit of participatory research while simultaneously examining participatory research, potentially allows me to do several useful things either within this project or the longer term.

Firstly, in practicing participatory research, the collection of local knowledge and analysis of existing problems and structures often generates critique and then action. This in turn may lead to other practices.

Secondly, applying participatory research to a limited extent as I submit my documents for review by those cited may allow for both critical self-reflection and collective analysis and interaction both between Pacific island colleagues and me and university colleagues as well as among and within each group.

Thirdly, and again to a very modest extent, observations come out of both my own practice in action research in the Third World and from listening to the histories of other participatory researchers and local people as well as inviting them to interact with me while I produce this document. While this seems deceptively simple and not unusual, the intent is important.

Fourthly, a long term participatory research approach could lead to some degree of bottom-up/top down synthesis while addressing the institutional paradox inherent in the efforts to institutionalize participation.

In respect to this dissertation, even at the outset, discussion of the topic itself had the unexpected effect of causing several casual reviewers to change their own research approach and/or start discussion within their own groups and institutions.³⁸

Limited actual participatory practice has also produced effects. Initial discussion of restitution with some Pacific Islanders, has in turn resulted in very clear statements about what kind of research they don't want; that is, once I introduced the issues, many Pacific island colleagues were very expressive about past experiences with exploitive researchers. It is also expected that some of the results of restitution--of exchanging this document with the Pacific people cited--will result in reactions and discussions over years not just with me but within their groups.

Another unexpected consequence of engaging in participatory research practice relates to the fourth feature stated above. In attempting to work on bottom-up/top-down synthesis, within a big institution while also writing and organising around participation issues, I became far less apologetic about my old role as an outsider in Third World research. In the great majority of American research institutions, those who advocate participatory research when it is defined as centering around local control find themselves part of their institution's Third World--that is to say marginalized. Nevertheless, no institution's power structure is perfectly uniform or without its own discontinuities; there are a range of tactics and options available to do creative work. In short, while I found many people uncomfortable with participatory research when I insisted that control should be turned over to local people, no one could really say they were against participation, and this opened up possibilities. Again, this will be expanded in later chapters.

V

Despite the many publications since the mid 1970's critiquing research and development in the Third World, little has been done to create any alternative paradigms. Where it has, the beginnings are fledgling. In short, critiquing and pushing at the boundaries of inquiry about research practice are still necessary.

One notes the underlying assumptions still extant in the "Statement of Principles" for International Scholarly Exchange produced at the East-West Center Conference on Research Access, Equity, and Collaboration in December 1985 (see appendix D).³⁹ The underlying assumption throughout the conference was that all worthwhile information "exchange" is at the university or multi-lateral agency level and that the knowers--"scholars" are always First Worlders or First World educated seeking access to information in the Third World. The opening statement of the conference document embodies the problem:

The generation of knowledge benefits all societies by increasing capacities for choice, enriching diversity of cultures, strengthening human values, and preserving the integrity of our environment. Scholars contribute to their societies by the systematic quest for knowledge and discovery.

One hopes for this statement to become true, but for the most part the people engaged, enriched, and strengthened by research--perhaps this very document--are hardly ever village level people. Those enriched are indeed the scholars, those with the university and research institution sponsorships. The village or local people on the other hand, never see the document or any sort of summary that will make sense in their context or widen their choices. Quoting the Conference document again, "maximum possible access to information and research settings should be provided to all scholars within the limits of appropriate constraints," one is led to ask: but what about researched people--peasants, tribals, or slum dwellers? These are the people who are supposed to benefit from all the subsequent developments.

There are real limits and dangers in this dissertation project. One may slip into focusing on "participatory" research and development technique and improving one's role as a facilitator. Since the focus is on achieving something with other people, one may revert to doing it to them, to having them participate in our plan. They get no significant share in the decision-making process, no real control. Even in the case of

participatory research we may only provide palliatives which Denis Goulet neatly defines as preventing "fundamental change by lulling people into accepting minor improvements instead of demanding adequate solutions to problems. As time passes by, palliatives worsen the condition they set out to cure."⁴⁰

The more we examine the paradoxes of using language to understand and to make clear, we come to see how dangerous looking for commonalty, for a single definition of success (in research) is. Language is so much a product and producer of culture that when examining other cultures, even those geographically within our own, at some junctures it is perhaps better to avoid interpretation as much as possible and rather strive to let otherness speak for itself. That is why this project is also launched from the perspective of otherness and of facilitating the fine line between the creation of spaces for the other to speak rather than speaking for them. In other words, the task is to find ways to validate the practice of helping people get their own stories out.

Sometimes, "helping" is best done by simply getting out of the way. The following comment resulting from extensive dialogues between Marxists and Native Americans is indicative of some of the difficulties:

As the Marxists criticized the Native Americans for a fatal underestimation of imperialist ease at corrupting and undermining traditional societies, the Native Americans responded with criticism of Marxism as being part of the corrupting tradition itself. The Marxists saw no hope for the Native Americans: traditional societies were doomed long ago. The Native Americans saw no hope for Marxism: any participation in Western Society, including internal criticism, only contributes to the suicide of humankind.⁴¹

In short, the Marxists saw the Native Americans as oppressed, while they also asserted that the Native American way of life was doomed and not worth fighting for. By imposing their views, the Marxists also oppressed the Native Americans.

Given the above world, I see my project as using participatory research to both critique old research practices and suggest alternative but very basic and often tentative

practices. These alternative practices often have to do with helping us get out of the way. In short, this is a call to study our own roles and institutions and how our institutions reproduce themselves. Where this is examined, more tactics will be suggested.

At least one of my colleagues who initially reviewed the proposal for this work asked, "Given the nature of modernity, capitalism, racism, and the like, what hope is there really of getting people with power to listen to marginalized peoples' local histories?" Another comment received was "Isn't participatory research just another no hope end-run?"

Conceptualizing social change only in terms of reform and revolution via hegemony over a central state power or single central concern is limiting. Given a world system, but one full of contradictions, fluxes, and intersections, there are non-violent things to be done--even tactics. As to reformism, Appalachian educator and activist, John Gaventa has said:

It is not reformist if asking this question--doing this thing [in this case participatory research] permits and leads to asking more questions.⁴²

Part of the strategy will be to know the limits of participatory research and the marginal spaces around those limits. If good space is where we are right now, how do we recognize it? How do we best move through it? Can anyone with more resources ever really talk with their binary others, i.e. men with women, First Worlder with Third, Marxists with Native Americans?

Research access closure in Melanesia and the Third World is one of the indicators of what is going on. It does not tell the whole story. In any case, just fixing the access problem for those educated in First World institutions would tend to downplay the necessary criticism being focused on research mining and research imperialism.

Social science research practice in the Third World faces increasing challenges. It is not enough superficially to alter some current research approaches to be more cross-culturally sensitive or achieve some participation of marginalized peoples in preexisting research with preestablished objectives. At this level, researchers in the First World are still "doing it to them" or engaged in what I from hereon term "the interventionist imperative."⁴³

The ensuing essential question is then: Are there strategies or tactics available for sustaining a critique and altering some existing structures and relations of dominance in Third World social science research? These strategies would be in both the First and Third World. In turn, the first tactic is to render social research political where it has not been seen as political in order to open up discussion and negotiation.

NOTES FOR CHAPTER I

- ¹Albert Memmi, The Colonizer and the Colonized, introduction Jean-Paul Sartre (Boston: Beacon Press, 1965), p. xvii.
- ²Ewenando. Le premier Journal de Kanaky, April 1988. This newsletter-sized monthly newspaper has been in publication since 1985 in Noumea, New Caledonia and to date has been the main source of news about Kanak activities.
- ³Although this adage is in the public domain, the original formulation is attributed to Francis Bacon.
- ⁴Hayden Burgess, seminar "What is a Kanaka from Wai'anai Doing at the UN in Switzerland?" sponsored by the Working Group: Creativity for Social Change, East-West Center, Honolulu, Hawaii, 7 October 1988.
- ⁵Ralph Beals, "Background Information on Problems of Anthropological Research and Ethics," in American Anthropological Association Fellow Newsletter 8 (January 1967): 12-13 on host country criticisms. See also Raymattja Marika, Dayngawa Ngurruwutthun and Leon White, "Always Together, Yaka Gana: Participatory Research at Yirrkala as part of the development of a Yolngu education," paper (School of Education, Deakin University, Geelong, Victoria, Australia, 1989).
- ⁶Earl Babbie, The Practice of Social Research 4th ed. (Belmont California: Wadsworth Publishing Co., 1986).
- ⁷*Ibid.*, pp. xviii-xix.
- ⁸University of Michigan, "The 41st Annual Summer Institute on Survey Research Techniques, 1988," catalogue (Ann Arbor, Michigan: University of Michigan, 1988).
- ⁹David Challinor, assistant director of research, Smithsonian Institute. Interviewed 19 February 1987 at the East-West Center, Honolulu, Hawaii.
- ¹⁰Marjorie Crocombe, then director of the University of the South Pacific branch unit. Informally interviewed January 1987 at the East-West Center, Honolulu, Hawaii.
- ¹¹This piece is cited and analysed at length by Monty Lindstrom in his book Power and Knowledge on Tanna forthcoming 1989. Lindstrom was a research fellow of the Institute of Culture and Communication at the East-West Center, Honolulu, Hawaii in 1989.
- ¹²The Economics Students Association of the University of Papua New Guinea, "Academic Exploitation: A Contagious Disease," May 11, 1973.
- ¹³Colin Filer, ed. Research in Melanesia, University of Papua New Guinea, Educational Research Unit, 8 (June 1984).
- ¹⁴Research in Melanesia (June 1984).

- ¹⁵Roger M. Keesing and Peter Corris, Lightning Meets the West Wind (Melbourne: Oxford University Press, 1980)
- ¹⁶James Rizer, then acting deputy director of the Pacific Islands Development Program, interviewed 2 March 1987, at the East-West Center, Honolulu, Hawaii.
- ¹⁷Kirk Huffman, director of the the Vanuatu Culture Centre and a member of the Vanuatu Research Council, interviewed 4 May 1988, at the East-West Center, Honolulu, Hawaii.
- ¹⁸For background information see Linley Chapman, "Recent events in New Caledonia: Impressions Created by Selective Reporting," paper (University of Hawaii, History Department, Spring 1985). See also Alan Ward, "New Caledonia: The Immediate Prospects" Discussion Paper No. 4 (Canberra, Australia: Parliament of the Commonwealth of Australia, Legislative Research Service, 1983).
- ¹⁹For the Caledoche interpretation of recent events in New Caledonia, see the daily newspaper Les Nouvelles Calédoniennes starting with issue 4448, 14 March 1986 through May 1988.
- ²⁰Myriam Dornoy, Politics in New Caledonia (Sydney: Sydney University Press, 1984). And interview with Myriam Dornoy then lands officer in the Department of Rural Lands, Port Vila, Vanuatu, May 1984.
- ²¹Interview May 1989 with Jay Slivkoff, Resource Systems Institute, East-West Center, Honolulu, Hawaii.
- ²²Thomas S.Kuhn, The Structure of Scientific Revolutions, 2nd ed. enlarged (Chicago: University of Chicago Press, 1970.)
- ²³Elisabeth Schussler Fiorenza, In Memory of Her (New York: Crossroad, 1986), p. xii. Schussler Fiorenza describes the wider import of a paradigm well: "A paradigm defines the type of problems to be researched, interpretations to be given and interpretative systems to be constructed. Thus a scientific paradigm determines all aspects of scientific research: observations, theories and interpretative models, research traditions and exemplars, as well as the philosophical-theoretical assumptions about the nature of the world and its total world view. All data and recorded observations are theory laden, no bare uninterpreted data and sources exist. Equally there are no criteria and research models that are not dependent on the scientific paradigm in which they were developed."
- ²⁴Peter T. Manicas, A History and Philosophy of the Social Sciences (Oxford, UK: Basil Blackwell, 1987).
- ²⁵*Ibid.*, p. 243.
- ²⁶*Ibid.*, p. 266 and p. 293.

²⁷Richard J. Bernstein, The Restructuring of Social and Political Theory (University of Pennsylvania Press, 1978) and Robert King Merton, Social Theory and Social Structure (New York: The Free Press, 1957).

²⁸Bernstein, see Chapter I in The Restructuring of Social and Political Theory.

²⁹James Clifford and George E. Marcus, eds., Writing Culture: The Poetics and Politics of Ethnography (Berkeley: University of California Press, 1986), pp. 9-10.

³⁰Albert O. Hirschman, Getting Ahead Collectively: Grassroots Experiences in Latin America (New York: Pergamon Press, 1984).

³¹*Ibid.*, p. 97.

³²John G. Stoessinger, Henry Kissinger. The Anguish of Power (New York: W. W. Norton & Co. Inc., 1976), p. 14.

³³Terence Ball ed., Idioms of Inquiry: Critique and Renewal in Political Science (New York: State University of New York Press, 1987).

³⁴Grant McCall and Alexander Mama'., eds., Paradise Postponed (Rushcutters Bay, New South Wales, Australia: Pergamon Press, 1978), pp. 256-260.

³⁵Folke Dubell, ed., Research for the People, Research by the People (Linköping, Sweden: Linköping University, 1981), p. 6. Selected Papers from the International Forum on Participatory Research in Ljubljana, Yugoslavia, 1980. (In my opinion the best and most concise collection of essays on issues central to participatory research.)

³⁶Orlando Fals Borda, "Science and the Common People," in Folke Dubell, p. 16.

³⁷Here, the term "problematique" is employed as a code; what is being discussed is one of the central problems, and it implies that what is being treated as a problem is not usually thus treated; it is not usually considered problematic.

³⁸Two of these early reviewers were Diane Goodwillie of the Ofis Blong Ol Meri in Nadi, Fiji and Dr. Annie Walter of ORSTOM (L'Institut Francais de Recherche Scientifique) then in Vanuatu. I am most appreciative of their comments.

³⁹East-West Center, "Conference on Research Access, Equity, and Collaboration," December 1985, Honolulu, Hawaii.

⁴⁰Denis Goulet, The Cruel Choice: A New Concept in the Theory of Development (New York: Atheneum, 1971), see especially chapter 13 "Despair's Dilemma: Ethics and Revolution."

⁴¹Phil Heiple, "Reds vs Redskins" in Marxism and Native Americans, ed. Ward Churchill (Boston: South End Press, 1983), p. 178.

⁴²John Gaventa, interview 3 December 1987, on institutionalization of participatory research at the Department of Sociology, University of Tennessee, Knoxville.

43For an excellent discussion of the imperative to intervene that is implicit in all development, see Antonio Contreras, third chapter of doctoral dissertation in progress, University of Hawaii at Manoa, political science department, 1989.

CHAPTER II

SURVEY: THE VARIOUS ORIGINS AND DEFINITIONS OF PARTICIPATORY RESEARCH

If all these things are going to die anyway, why do I have to ... learn their names? -Because you're human and humans have this deep desire to classify, to apply labels to everything ... that way we lay claim to what we name. We assume an ownership that can be misleading and dangerous ... My street, my lake, my planet ... my label forever. A label you give to a place or thing may not even last out your life ...
(Frank Herbert, Chapterhouse Dune)¹

I

My past experience in rural, Third World research and development has led to my advocacy of what has been alternatively labelled participatory research, participatory action research, or action research. In some cases distinctions have been made between these terms. In general, throughout this dissertation, the term participatory research will be used generically and assumed to include the variety of practices referred to by the other terms.

At the outset, I was most concerned with more and better community development. My efforts to improve development practice and later theory subsequently led me to realize that the same type of imperialistic practices inherent in First World approaches to Third World development are also inherent in the research practices of First Worlders and those educated in the First World.² As I came to ask "is there any good development?" I also came to think that failed development efforts were rooted in the initiators' lack of local knowledge and respect for local culture.

My later work has shown that providing more information to those social scientists engaged in mainstream research and development practices is not enough. Problems of respect for local culture cannot be changed while outsiders believe that local culture is at best quaint and at worst dangerously superstitious.

The other side of the coin in this view is that progress, development, modernity, and technology are the objectives. This leaves local people as empty, waiting for their vaccination of improvement which will be provided by outsiders. In the field of

education, Paulo Freire likened this situation to "banking deposit education" in which the teacher comes and deposits information into the heads of the ignorant pupils.³

It should be noted here that although Brazilian educator Freire has written no work of his own on participatory research, he has had an enormous, direct influence on both myself and many of those cited throughout this chapter. Central themes of Freire's work are the inherent dignity of any marginalized people and their potential to create their own analyses and solutions to their situations. In Freire's view the task of an educator is to raise consciousness; this is not making students aware of what information they haven't and the teacher has. To raise consciousness is to throw off limits--often internalized, stereotyped, negative self-images--on critical analytic thinking. This work cannot really be done by the teacher; it is the students' rediscovery and revalidation of their own everyday knowledge that is important. In Freire's best known work, Pedagogy of the Oppressed he speaks out:

Consequently, no one can say a true word alone--nor can [sic] say it for another, in a prescriptive act which robs others of their words ...Those who have been denied their right to speak their word must first reclaim this right and prevent the continuation of this dehumanizing aggression.⁴

The field of social science research and its off-spring, participatory research, contain and exemplify the complex web of relations surrounding the problematic of local knowledge and mainstream knowledge and power. Keeping Freire in mind, participatory research is then in part about creating situations where the word is spoken by rather than for the other.

When I discovered the label "participatory action research," I was delighted; the term well described the best aspects of pedagogical and training processes in which I had been engaging. By "worked well," I mean that participatory research seemed to allow space for practices that are cross-culturally sensitive and go beyond the simplistic

objectives of increasing immediate production to objectives of long term individual and collective analysis and local control. The label however was followed by a plethora of weak and inadequate definitions; either there were no discussions of origins or they were attributed to quite different sources.

As I then attempted to develop a more substantial definition of participatory research and its practice, I came to pose yet more questions. Is participatory research a practice that will engender more real, useable knowledge and serve to alleviate the problems of those people labelled poor and marginalized in development literature, or is it as now defined a code for inspiring hope while continuing the same preemptive and coopting practices? From hereon, the use of the word "cooptation" denotes, "the processes whereby individuals are assimilated and committed to the institutions and values of the dominant socioeconomic group."⁵

What then are the limits of participatory research? The purpose of this chapter, while written by a participatory research advocate, is not so much advocacy, but examination of some of the limitations of participatory research engendered by the ways in which we as scholars, academics, and researchers discuss research in general and participatory research in particular. It is hoped that understanding the limits of participatory research practice will provide the basis for more effective practice. The means employed here for getting at the limits is achieved through an outline of the literature defining and describing participatory research.

II

The bulk of printed material that discusses participatory research is produced by various development and/or education related organizations ranging from the World Bank to small non-profit groups such as the Participatory Research Group in Toronto and by social scientists located in universities. While for the most part participatory research literature is produced in the technological West, there is a body of

participatory research writing which has emerged from long term work in nations such as India, the Philippines, and several Latin American countries. For example, the Indian practitioners' network was founded by Rajesh Tandon in the early 1970's.⁶ Another major founder, contributor, and practitioner of participatory research is Orlando Fals Borda, a Columbian.⁷ Additionally, many of the participatory research documents produced in the West are based upon long term Third World experiences.

Current correspondence with the organizers of a Participatory Research Conference to be held in Calgary, Canada in mid-July 1989 indicates a world-wide explosion of interest in the field occurring during the past several years.⁸ Conference planners expect over 500 delegates. However, it is not clear that all who claim to practice participatory research are operating from the same base paradigm.

The majority of participatory research writers and practitioners find traditional research practices, especially when applied in the Third World, to be inherently unbalanced. The imbalance is most often attributed to an over-reliance on outside experts and ignorance of the opinions, local knowledge, and aspirations of the people in the communities being studied.

Generally, participatory research has been proposed as an alternative practice where any outside research facilitator involves the people in the community to be studied in the research process. Involvement has ranged from (a) bringing as much of the community as possible together to discuss the research topic to (b) eventual community control where the community controls all information generated by the research effort. In these last circumstances, one could describe the participatory research process as one where citizens invite an outside researcher to work with them on collecting, analyzing, and acting on information to solve community problems. Other cases, labelled "participatory research" merely involved the community in development efforts based on outside intervention with a new name.

The objectives of participatory research are variously stated as improved research project efficiency, development, and/or social justice. The differing statements of objectives lead to quite different outcomes, and again, are based on different assumptions and paradigms.

Budd Hall, currently director of the International Council on Adult Education and one of the founders of the Participatory Research Network, provides a description of participatory research process characteristic of the Network:

The problem originates in the community or workplace itself.

The ultimate goal of the research is fundamental structural transformation ...

Participatory research involves the people in the workplace or the community in the control of the entire process of research.

[The] focus of participatory research is on work with a wide range of exploited or oppressed groups: immigrants, labour, indigenous peoples, women.

Central to participatory research is its roles of strengthening the awareness in people of their own abilities and resources and its support to mobilizing or organizing.

The term "researcher" can refer to both the community or workplace persons involved as well as those with specialized training.

Although those with specialized knowledge/training often come from outside the situation, they are committed participants and learners in a process that leads to militancy rather than detachment.⁹

The following is a Solomon Islands micro-example of the type of interaction and process that Hall describes.¹⁰

In the course of a locally organized workshop to discuss development issues, the workshop leader (a person from that island) learned that the village had been approached by an international timber corporation for logging rights to the forested areas on their island. This facilitator, having already witnessed the havoc wreaked by logging on other islands, shelved his own strong views. He knew the villagers had not seen other islands.

Instead, on his suggestion, nearly the entire village took a "walk-about" through the forested area all the while naming the different plants and creatures in the forest that they used, i.e. medicinal plants, food plants, housing materials, and so on. On return to the village and after lengthy discussion, people decided that monies from the lease of their land would not offset the loss of the perpetual use of the forest. For a time the timber company was forestalled.

The facilitator helped create a collective information gathering effort in order for local people to make an important decision bearing on their future. It was more powerful than a mysterious cost-benefit analysis made by a foreigner or foreign educated local person.

This brief narrative contains most of Hall's characteristics with the exception of any militancy or efforts at structural change. Obviously, the narrative of this one day event is not sufficient to show whether the experience was powerful enough to lead to further community analysis and action. It is, however, possible that an experience of this sort could lead villagers to question all subsequent visiting experts and in turn lead to further analysis of the systems in their islands.

These last two characteristics from Hall's list--structural change and militancy--initially appear somewhat problematic. After all, what if a local group goes through participatory research and analysis only to decide not to take any action? That in itself is an action; the important aspect is that the local people decide to make the decisions instead of outsiders. When local people take over the decision making process that in itself can be or lead to structural change. When this happens it is often perceived by administrators and government officials as militant or revolutionary.¹¹

Further examination of Hall's and Fals Borda's work among others from the original core of the Participatory Research Network is instructive. Both Hall's and Fals Borda's work reveal that respect for the "word of the other" precludes pre-judgement of

outcomes in good participatory research. Any outside facilitator or technical person called in cannot push people to make structural change. Structural change is not a foregone conclusion. In fact, Fals Borda makes his view quite clear as he describes an earlier attempt at "critical recovery of history" with oppressed Columbian peasants:

Being by then quite impatient with such a situation, active researchers and our political allies (following the Leninist theory of the revolutionary cadres) began to inject our own definition of proletarian science within the context of reality and in the base groups. It was like searching for a ghost and when not finding it to feel the need to create one. The result was a "science for the common people", not a genuine science of the people.¹²

This comment suggests that Fals Borda was expecting to be able to draw forth from citizens a particular description of unjust labor relations and perhaps a call to revolutionary activities. When citizens' description of reality did not match Borda's he realized that it was his imposition of meaning rather than the citizens which was at fault.

Before going any further with the work of Participatory Research Network members, it is useful to consider the origins of both the term "participatory research" and the Network.

III

Hall attributes the first participatory research explicitly labelled as such to work in Tanzania. During 1971, Maria-Liisa Swantz and Hall were working with local groups under the sponsorship of the Tanzanian Bureau of Resource Allocation and Land Use Planning. During this time, Paulo Freire visited Tanzania, and along with others subsequently transported the term and concepts to colleagues throughout the Americas, Europe, and other regions of Africa.

Through Hall's subsequent return to Toronto, Canada and the International Council on Adult Education, participatory research and Freire's ideas on education and development were dispersed world-wide starting in 1974. Also during this time, Hall

and a few others founded the Participatory Research Group in Toronto. In 1976 a session on research at the Dar-es-Salaam Conference on Adult Education also broadened the dissemination of participatory research. Until 1988, the Participatory Research Group served as a world-wide clearinghouse for participatory research documents and networking. In January 1988, at a conference in Nicaragua, Highlander Research and Education Center in New Market, Tennessee was chosen as the 1989 to present clearing-house.

There has been a vigorous percolation of concepts through this network. Many of the key practitioners have been strongly influenced by Freire--both his writings and personally. In interviews, Freire himself has indicated that he in turn over the years has been deeply impressed by the work of the liberation theologians, and especially by the work of his friend Leonardo Boff as well as many of the Latin American dependencia (dependency) theorists, and others.¹³ The power of this network and the important concepts circulating through it should not be underestimated.

Many of those participatory researchers working in Francophone Africa have also been influenced by animation-rural and participatory development espoused by Roland Colin. In essential features, animation-rural is a French originated approach to community development that is dependent upon local collective reflection and analysis. In Colin's case (Senegal), powerful elements of local control were considered key:

to attain the objectives of integrated rural development it is essential that the disadvantaged rural populations participate in decision making at all stages of the development process.¹⁴

As mentioned previously, the current (1988, 1989) clearinghouse for the Network is at Highlander Research and Education Center near Knoxville, Tennessee. The founder of Highlander, Myles Horton, visited Denmark in order to understand the Danish Folk School movement and its relations to political and cultural identity. Horton vast experience also includes union organizing in Appalachia during the 1920's

and 1930's. Horton was also responsible for co-facilitating a tremendous amount of adult literacy training among Afro-Americans during the Civil Rights movement. Passing the final exam in those literacy courses was achieved by successfully registering to vote. In the early 1980's Highlander's work--and by association Horton's--was recognized by a Nobel Peace Prize nomination.

Eventually Horton and Freire met, bringing these rich strands of human experience together.¹⁵ Other Highlander staff, notably John Gaventa, Juliet Merrifield, and Helen Lewis, among many others had already been engaged with people throughout Appalachia in participatory research work; in recent years this has spread world-wide. Some of this work will be cited in the case studies in Chapter IV.

Other early core members of the Network include Orlando Fals Borda-(Columbia), Yusuf Kassam and Kemal Mustafa-(Tanzania), Francisco Vio Grossi-(Venezuela), Jan de Vries and Gerrit Huizer-(Netherlands), Vijay Kanhare-(India), Ton de Wit and Vera Gianotten-(Netherlands and Peru), Morten Levin, and Rajesh Tandon-(India).¹⁶ By the 1980's the list of names had expanded greatly.

IV

The term participatory research itself does have quite a few antecedents. Action research has been in usage since at least 1946 when American Kurt Lewin wrote "Action research and minority problems."¹⁷ The usage at that time was in educational and community work contexts. It appears that action research was for the most part dormant until the 1970's when it reappeared in two different contexts in England. The first context is that of education. In The Action Research Reader, Stephen Kemmis and Robin McTaggart cite the Ford Teaching Project of 1973. Other diverse sources place action research in the Tavistock Institute (U.K.) where it was a technique of humanistic research in corporate, factory, and worker/management studies.¹⁸

Kemmis and McTaggart in their own essays hold to the following definition:

Educational action research is a term used to describe a family of activities ... These activities have in common the identification of strategies of planned action which are implemented, and then systematically submitted to observation, reflection and change. Participants in the action being considered are integrally involved in all of these activities.

Furthermore, they see the aims of action research as follows:

- 1 the improvement of a practice,
- 2 the improvement (or professional development) of the understanding of the practice by its practitioners, and
- 3 the improvement of the situation in which the practice takes place.¹⁹

Participant observation has been increasingly in use among anthropologists and ethnographers since the 1960's at least.²⁰ Although there were earlier works, from 1929 on, that classified themselves as participant observation, the term gained renown through William Foote Whyte's Street Corner Society.²¹

In anthropological usage, participant observation has meant that the researcher actually participates in activities being studied in order better to interpret them, but this can mean many things. Works on participant observation surveyed included researchers who were quite aware of their own subjectivity and limited participation and its consequences on people studied. Other researchers were more concerned with "standardized participant observation of various objects in a field by several observers on the basis of a uniform observation schedule."²²

Some participatory observation in cases like Whyte's have lead increasingly to an awareness that it is not appropriate to treat researched populations, who bear the consequences of that research, as objects. Participatory observation has not necessarily lead to prevention of research mining or facilitation of local awareness and control. Participatory observation is then just a step in the process of participatory research--a necessary but not sufficient condition.

Participatory action research has come into usage in the 1980's in a so-called Third World development context. Several of my colleagues (in different places) claim to

have coined the term.²³ Apparently, adding "action" to "participatory research" was a logical step for those researchers wishing to stress that action is a necessary component of research.

Several writers have provided quite different histories and definitions of the terms. It is not that the writers are wrong or disagree, but simply that the label has been adopted by a wide range of people. Each of these people operates within a particular discipline with its own history and practitioners. Many of these earlier practitioners were engaged in similar or intersecting approaches and practices in different contexts such as education, union organizing, regaining native rights, environmental battles, and so on. It was only later that the practices were named, hence the confusion.

For example, Lyngrid Rawlings, writing for the Participatory Research Network, lists Bishop Nikolai F. S. Grundtvig of the Danish Folk Schools, Tao Shing-Chih of an early twentieth century Chinese educational movement, Dr. M. M. Coady of the Anitgonish Movement in Canada, Myles Horton of the Highlander Research and Education Center, Paulo Freire working in Brazilian adult literacy, Julius K. Nyerere, who as president of Tanzania worked for education for self-reliance, and Budd Hall currently director of the International Council on Adult Education.²⁴ Rawlings sees participatory research as having evolved through these persons' work. Other discussions of the origins of participatory research or action research omit anyone on the above list and insert others.

Typically, Freire is included on both Rawlings's list of participatory researchers and in Kemmis and McTaggart's list of action researchers. Yet, in his one essay specifically on research, "Creating alternative research methods: Learning to do it by doing it," Freire uses neither label.²⁵

One essay in circulation draws distinctions between participatory and action research. David Brown and Rajesh Tandon describe factory based action research as essentially concerned with individual analysis and underpinned by an ideology of efficiency and growth. Participatory research on the other hand is concerned with questions of societal dominance and is based on the assumption that equity/self-reliance/oppression problems are central.²⁶

Brown and Tandon explain the differences between action and participatory research by citing a hypothetical factory research situation in which accidents are caused by stress. They see action research leading a joint research committee to an agreement that certain management practices do indeed lead to stresses that result in workers' accidents and changing immediate practices. After all, the reasoning would go, everyone benefits from the greater production when there are fewer accidents. A participatory research approach on the other hand, would tend to ally itself with worker interests, going after larger changes in management structure. The way Brown and Tandon draw their distinction suggests that action research would lead to management walk-throughs on the shop floors and better relations. Participatory research, however, might lead workers to try taking over part of management and or ownership--something that might not work. Brown and Tandon contend that:

Participatory research on the ... problem would probably challenge management exploitation of workers - even if the workers themselves did not question management legitimacy - and thus alienate all the parties.²⁷

A survey of alternative research descriptions does show factory based alternative research more frequently labelled "action" and so-called Third World research labelled as "participatory." However, the terms are often reversed; some research in factories has been labelled "participatory research," and some research in so-called underdeveloped countries has been labelled "action research." Furthermore, it appears that the majority of the research facilitators who form the core of the Participatory

Research Network would not push "structural change" unless it came from local people's own definition and analyses of problems. Therefore, the distinctions between the two labels and the label "participatory action research" are no longer particularly useful.

V

Further scrutiny of the various definitions of participatory research demonstrates additional attributes. These attributes become manifest in reaction to usual research dichotomies. Several important dichotomies that are accepted in mainstream research are challenged by participatory research. These dichotomies are overlapping and include the relations between 1) subject and object, 2) means and ends, 3) reflection and action, 4) research neutrality and political power, 5) experts and the researched people, and 6) development and research itself. In the type of participatory research engaged in by the Network, the preceding boundaries are often blurred.

Mainstream researchers and development specialists may see a much greater separation between research and development than participatory research practitioners. Research is supposed to be conducted in as an objective manner as possible, i.e. using the scientific method. Development is supposed to provide either directly or indirectly those things that people in the Third World don't have that people in the First World do, again, usually technological products. If a development specialist calls on research to inform a decision, the norms call for an objective and informed decision, and it is assumed that only experts can produce the necessary information. The above dichotomies are key, because they characterize assumptions that separate non-expert citizens from their own knowledge bases; these separations make citizens into objects and can have serious consequences in the citizens' daily lives.

Returning to the traditional research methods text cited previously (Babbie), all the previous dichotomies are abundantly evident. Ethics are seen as the responsibility and personal domain of the individual researcher and politics as a public arena; the social scientist must neatly separate herself from the object of study. Babbie proclaims:

Science, as a collective enterprise, achieves the equivalent of objectivity through intersubjectivity. That is, different scientists, having different subjective views, can and should arrive at the same results when they employ accepted research techniques. Essentially, that will happen to the extent that each is able to set personal values and views aside for the duration of the research.²⁸

The primary way the above and the rest of the scientific method play out in relation to Third World research is to make researched peoples into objects instead of subjects. Not only do they have little say in the research that is conducted on them, but that research, because of disciplinary boundaries is usually mono-dimensional. The image that is then produced may be wrong because of lack of information at two levels; the outside researcher may lack not only local knowledge, but her scientifically gained knowledge may be incomplete as well. The process of doing the research is rarely related to any local objectives for changing local conditions. The research is rarely "contaminated" by action. If the final research documents are given to anyone in-country in order to effect change, it is rarely to the local people who hosted that research. Any changes are expected to be effected at higher, policy making levels of government.

The social sciences often refer to the methods of "hard" science for their authority. Ironically, the hard science of physics has long since abandoned the notion that the observer can be isolated from that which he observes. Quantum physics, for example, shows that there is no observer that does not effect the observed.²⁹ Physicist Fritjof Capra has suggested that these changes in physics are indicative of an important paradigm shift. Capra suggests that 1) every structure is a manifestation of an underlying process, 2) that there is a shift from objective to systemic science, 3) the

observer plays a role and should be explicit about that role, and 4) the concept of knowledge as a building has changed to that of knowledge as a network. These are useful conceptualization for the participatory researcher who wishes to cease turning researched people into objects.

VI

During the initial stages of this project, I expected to find that the definitions of participatory research provided by larger institutions would be quite different from those provided by smaller and local institutions.

I expected the main differences would become manifest in language, to the extent that local control was an explicit value and goal or not. I expected that larger organizations would be less capable of participatory research, because their own steep hierarchies would preempt local control. Smaller organizations were expected to have fewer strata of personnel, thus more direct awareness of researched people and communities as people rather than sources of research data. In review, the distinctions between different types of organizations did tend to follow the above, but not always, and the distinctions are blurry.

Below is a sample of the range of participatory research definitions. Review of the definitions reveals that the objectives of organizations doing participatory research has a relationship to whether they define their efforts in term of local control, but even this is not always clear.

A 1980 publication of the United Nations Research Institute for Social Development (UNRISD) entitled "Popular Participation Programme: Debaters' Comments" defines participation around control of resources:

For the purpose of defining an adequate area of research in a way that does not permit the evasion of the central issue of power, the definition proposed for this inquiry is: "the organized efforts to increase control over resources and

regulative institutions in given social situations, on the part of groups and movements of those hitherto excluded from such control."³⁰

Despite this seemingly strong definition, the subsequent UNRISD program generated several case studies of Third World participatory research which did not markedly differ from usual community development project descriptions.³¹

The Canadian based International Development Research Centre (from hereon IDRC) outlines three main attributes of participatory research.³²

1. Shared ownership wherein the "researched ... become themselves the researchers."
2. Mutual learning, in which research and training are complementary.
3. Action based, where the group examining information also must deal with it and related issues in the course of their lives.

The IDRC working group indicates in their paper important seeming contradictions and limitations of participatory research. These seeming contradictions may also be indicative of the type of institutional sponsorship. First of all, the working group notes that it is "impossible to construct a generalizable methodological model of participatory research."³³ The other limitation is described:

Most IDRC-supported participatory research projects which were reviewed appear to have been participatory in the intermediate or consultative sense ... [These people] have probably engaged in very little discussion and decision-making beyond their own immediate personal and family needs. When such people are engaged in a participatory research project, their participation may simply be a painstaking process of initiation, of tentative steps at the lower end of the participation continuum.³⁴

This passage continues by suggesting that choices must be made by the researcher about whom to participate with. Sometimes the researcher may not be able to work with the poorest of the poor. Further on the document notes, "increasing recognition and moves to institutionalize participatory research carry with it risks of its being used as a means of self-serving legitimation."³⁵

This has already occurred quite subtly in the IDRC text where the language is that of the IDRC choosing whom to develop. Implied is the initiation of all work by the IDRC experts. Also implied is that things must be done, that some degree of efficiency is necessary. The appendix of the document lists IDRC participatory research projects which range from grain storage to credit to family-school relations.³⁶

Even the World Bank now has something to say about what they term "community participation." Samuel Paul of the World Bank Public Sector Management Division explains:

CP refers to an active process whereby beneficiaries influence the direction and execution of development projects rather than merely receive a share of project benefits.³⁷

Paul is quite frank in his discussion of the range of definitions noting, "Some would regard participation as an end in itself, whereas others see it as a means to achieve other goals".³⁸ Paul's point is the central issue. When an organization such as the World Bank intervenes in local affairs, it cannot possibly be towards local objectives if those objectives differ from national and/or Bank objectives. Labelling development efforts and research participatory then becomes a justification for doing what was planned all along.

The larger organizations sponsoring participatory research projects such as the World Bank, UNRISD, UNFAO (United Nations Food and Agriculture Organization) and IDRC generally define the overall objectives of their work in terms of increased project efficiency and greater development. Almost all of these projects are tied into specific technical fields such as agriculture, energy, or forestry. Even where specific participatory research efforts attempt to define themselves differently, the overall practices and mandates of the institution tend to reimpose themselves.

Turning again to Third World based participatory researchers, one finds indications of a flip side to the imposition problem. Among the Participatory Research

Network, Ljubljana conference papers, there is one debate that recurs. The debate is whether participatory research must be defined in terms of historical materialism (Marxism) or North American pragmatism.

Where dialectical materialism has been generally thought of as the philosophy of Marxism, historical materialism has been considered the science of Marxism. Texts discussing historical materialism have often argued for the scientificity of Marxism based on natural science.

Adoption of either approach (pragmatist or Marxist) presupposes the entire problem to be researched as specifically class based or not. Either approach imposes a prefabricated framework of reality on the research subjects and the world. Perhaps local people would eventually tell the researcher, "It is none of the above." There was, however, an agreement among researchers collected at Ljubljana that use of historical materialist interpretations must be non-dogmatic.³⁹

From yet another perspective, Third World researcher Arturo Escobar sees participatory action research as entirely new and hopeful:

Central to PAR philosophy is the question of popular power, i.e. the investigation of the mechanisms necessary to develop popular counter-power for social transformation and their relation to the production of knowledge. Thus it seeks to generate popular power, [emphasis mine] not only "to develop." PAR projects combine techniques of adult education, social science research and political activism ... and the use of novel means of diffusing knowledge (all knowledge being considered the property of the community).⁴⁰

Escobar appears to have a bipolar conceptualization of power when he implies that participatory research must seek to generate "popular power." If Escobar understands the state to have power and the people lack it, his statement could be read to suggest that the task of a participatory researcher is to mobilize people. Again, under some circumstances this could be an imposition.

Keeping in mind the range of participatory research definitions, imposition of possible solutions and dilution of local understanding comes in many forms. If

grassroots people involved in a project define their objectives as an improved water supply rather than energy a large, sponsoring energy agency with all its resources can usually do nothing. An agency constituted to do one thing--energy--can't do just water, there are too many inter-disciplinary and technological boundaries. But equally problematic, what if the local people after good solid participatory analysis persist in defining their problem only in terms of a material lack such as a mill or a pump? They may be well aware that their regional or central government is oppressive and equally aware of the risks and costs of doing anything about it. They may decide that all they want is a pump. How then would someone like Escobar measure popular power? Either approach is then a denial of the local people's voice and an imposition.

In summary, texts from different agencies and institutions have some differences in approach to participatory research, but these differences were not as great as expected. However looking at actual practices and the context of the sponsoring and funding bodies, there is a gap. What seems important then is not as much what is said, but who says it and in what context.

There are individuals in sponsoring organizations who can--at least for a time--subvert the interventionist intent of their organization's objectives. Such individuals can also act as buffers preventing interference from higher level personnel in the organization who may be more interested in survival of the organization than local control.

Often, where the participatory research facilitator has been an outsider but from a smaller agency he or she has been conscious of the inherent inequalities in the situation and made efforts to work within the local definition of the problem. Often there is an important difference between efforts by smaller groups or communities and projects sponsored by large institutions. The facilitator usually sees herself as answerable to the local community whereas in the cases sponsored by larger

institutions although the facilitator may feel responsibility toward the community she feels and is answerable to the larger organization. In either case, where such a facilitator feels primary responsibility to the local community tremendous stress is created for the facilitator. Facilitator burn-out often leads to at least some cooptation of the work. This is not unlike the phenomena of social workers who are at first client-oriented, but as increasing administrative pressures come to bear, become more interested in doing the paperwork that leads to their job security.

Common factors in burn-out are pressures to achieve visible results within a certain time period. These pressures are in conflict with participatory research conceptualized as dialoguing over time and patiently waiting for consensus to emerge.

The difference between institutions is that, not always but often, the smaller agencies impose fewer levels of bureaucratic red tape. Decision-makers are more accessible, and thus the outside sponsored facilitator can bend rules to the needs of local people.

In terms of participatory research, where just the local community is involved, information produced is theirs. Where a larger organization has been the sponsor or funding source, the information tends to become the property of those with the resources to disseminate it--usually the larger organization.

Generally, the Participatory Research Network members represent smaller institutions and the people in them tend to see themselves primarily as facilitators rather than scholars. Most of these individuals are still involved with grassroots groups on a day to day basis. Considering the monumental commitments in personal time that these people make, it is interesting that on a whole they tend to write themselves out of the picture; research efforts are usually described as those of the local people. An example that springs to mind is John Gaventa's work with the Appalachian Land Use Task Force; Gaventa is first to give credit to the Task Force and play down his own role.

On the other hand, in documents of cases where the sponsoring institution has been larger, that institution is usually described as the primary actor.

Agencies organize themselves around control of certain kinds of terrain with their managers defining their careers based on their expertness in particular fields. Intervention is the *raison d'être* of the research and development organization as well as bureaucracy in general and their expert managers/intervenors. It is necessary to do something, usually with physical artefacts whether they are dams or documents, to legitimize continued existence and funding. Again, to paraphrase a colleague, this phenomena is hereon termed "the interventionist imperative."⁴¹

Where it is finally becoming evident that both modernization type development and positivist research are problematic, imagine the enthusiasm with which interventionist agencies receive the news that there is a technique available which purports to solve the problems of both research and development--without the guilt. As long as organizations' and agencies' existences are based on their capacity to intervene, and science is the legitimizer, the space is permanently opened for coopting whatever local populations define as the problem and necessary solution.

Mexican activist and writer Gustavo Estava tells us:

In the peasant world, for a long time now, "development" has been recognized as a threat. Most peasants are aware that it has undermined their subsistence based on centuries-old diversified crops.⁴²

By naming these people peripheral, marginalized, poor, rural, or slum dwellers, people are objectified as a field of intervention. This field of intervention is further objectified as needing development--from A to Z. Almost universally, posing lack of development and underdeveloped people as the problem simultaneously implies that development via technological inputs and greater production is the solution. This in turn implies those people must be changed.

The difficulty with these often well-intended characterizations is that they ignore the voices of the people who are the research objects. Participatory research practice supposedly opens up a space where the people's voice or voices may be heard, but by continuing the old naming practices the space is once again closed. Do the people who become research objects see themselves as peripheralized, marginalized, or poor? Perhaps the case may be made simply to get the intervening organizations out of the way.

What the participatory research advocates call for is a recognition that without local people's participation in the informing process there will be many gaps. At one end there may be a lack of information as to local conditions. At another end there may be a complete lack of awareness that it is local people who are expected to employ the new technologies often without preparation.

Where the interventionist imperative is operative, success in the above projects is still technologically determined; how many windmills, biogas digesters, or efficient stoves were built? What is not considered is the strengthening of individual and collective analysis skills and strengthening of local organizations for decision making and long term planning and organization.

The authors of participatory research projects and papers contribute to the cooptation of participation by what they do not write, including the history of the institutions which sponsor them and the history of their own processes. In order to win the support of their organizations their definitions and objectives are stated in watered-down language. The project writers tame their writing, calling for increased project efficiency while hoping a greater participation nevertheless manages to emerge.

What I have found to be characteristic of the above and other PVO and grassroots approaches to participatory research is the relation of the outsider and/or the research facilitator to the community and the designation of all information as community

property. All who are dependent upon a basic terminology of research and development re-peripheralize and re-marginalize researched people even as we seek to de-marginalize them. Even advocates and practitioners of participatory research tend to reimpose the conditions they wish to change.

These are slippery and complex issues which circle around and loop back on themselves. For example, even participatory research conceptualized as empowerment is limiting and constraining. Such a statement can be used to justify the activities of those who have towards those who are not perceived as having and do not have control of the activities. This is where power is located. Power sustains itself by its very ability to remain invisible or unquestioned. The tremendous power of social science research practice to maintain itself and preclude participatory research because it is "unscientific," "descriptive," or even worse "intuitive," is located in the ideological spaces where the nature of sponsoring institutions is invisible. All attention is focused on the role of the individual facilitator and the grassroots rather than the procedures of the sponsoring institution.

Power is also located in relations where participatory researchers think they are obliged to get local people to make structural changes. Likewise, as long as "participation" in participatory research definitions is left invisible, in that it is left as an assumed positive value that is achievable and has been already achieved, we are left with a thoroughly reified concept. To reify is to discuss, in an unconscious manner, that which is abstract as if it were concrete. Again, participation can then become "motivating them to participate in our plan to do it--development--to them."

Thus textual and discursive analysis become important. Those texts which discuss participatory research not only make visible and disseminate a hopeful practice, but may by virtue of their mode of textual production, make invisible and reproduce some of the imperialistic research practices PAR is supposed to supplant.

A Philippine advocate puts the dilemma clearly:

Like the much-abused word "development," the terms "people's participation" and "community organization" today no longer inspire easy confidence and trust. Now more than ever, their meanings reside in the context of their actual use and indeed, in the identity of their users. Community organization, especially, has been effectively de-contextualized from its original moorings, and as a consequence, has reemerged as nothing more than an ideologically neutral technique that can be placed in the service of any master.⁴³

Roland Colin points out:

It has almost become obligatory to present "participation" as one of the essential conditions for the success of development projects and programs.⁴⁴

As long as participation in research and development is understood as indigenous people receiving infusions of technology from outside rather than becoming able to do their own analysis to solve problems, there is a problem. As long as development is understood as something that technologically advanced countries have as a result of something intrinsically and inherently better in their culture or structure, there is a problem.

As much as an institute or organization advocates some vague "peoples' participation" but those barefoot "People" never enter the carpeted halls of the institute much less participate in management decisions to allocate resources, there is a problem.

Finally, how much can individual facilitators do when trapped in the position of acting in a fully participatory manner respecting all local initiatives, when they are sponsored by patriarchal, hierarchical organizations who do not, for example, value participation of their own employees in their own allotment processes?

Gustavo Esteva, director of the *Comite Promotor de Investigaciones para el Desarrollo Popular*, Mexico, claims:

Popular participation is generally perceived as a non-manipulative style of mobilization of the people. It has been proposed as a democratic or revolutionary tool for those "hitherto excluded" from development and economic and political power to successfully overcome that exclusion.

I will try to show that these contentions are but an illusion. As a social construct, "popular participation" is now being built as a myth, to renew the appearance of colonizing metaphors which are now in agony.⁴⁵

Estava tells us to beware of [the myth of] participation!

The final question is: given the available definitions of the variety of approaches to grassroots research known as participatory research is the label so wholly coopted as to be useless if not damaging to those who endeavor to engage in forms of research that let the local people be the proprietors of knowledge?

I would like to think that participatory research is still worth doing, but self-awareness, changing language practice, and remodeling definitions are not enough alone. If participatory research is still worth doing, then the level at which further examination is required is also the institutional level.

NOTES FOR CHAPTER II

- ¹Frank Herbert, Chapterhouse Dune (New York: Ace Books, 1987), p. 17.
- ²For a good discussion of imperialism including cultural imperialism see Herb Addo, Imperialism: The Permanent State of Capitalism (Tokyo: The United Nations University, 1986).
- ³Paulo Freire, Pedagogy of the Oppressed, trans. Myra Bergman Ramos (New York: Continuum, 1970).
- ⁴*Ibid.*, pp. 76-77.
- ⁵Martin Carnoy, The State and Political Theory (Princeton, New Jersey: Princeton University Press, 1984), p. 211.
- ⁶For an example of his work see Rajesh Tandon and David Brown, "Ideology and Political Economy in Inquiry: Action Research and Participatory Research," The Journal of Applied Behavioral Science 19 (1983): 277-294.
- ⁷See Orlando Fals Borda in Folke Dubell, ed. Research for the People, Research by the People (Linköping, Sweden: Linköping University, 1981) and "Participatory Research and Rural Social Change," Journal of Rural Cooperation 10 (1982).
- ⁸International Centre, The University of Calgary, "Participatory Research Conference: A Celebration of People's Knowledge," announcement for conference held July 12-15, 1989.
- ⁹Budd Hall, Participatory Research/Popular Knowledge and Power (Toronto: Participatory Research Group, 1984), p. 23.
- ¹⁰John Roughan, "Village Organization: Transformation and Development in the Solomon Islands," (Ph.D. dissertation in political science, University of Hawaii at Manoa, Honolulu, 1986).
- ¹¹It is necessary to add that although Hall's description remains valuable, it was part of one of his early 1970's writings. Based on an interview with Hall in 1987, he would probably be far more precise about the necessity for structural change and how it is accomplished.
- ¹²Orlando Fals Borda, "Science and the Common People," in Folke Dubell, ed., p. 22.
- ¹³For an excellent recent summation of dependency theories see "The Dependent State," in Martin Carnoy, The State and Political Theory. An important aspect of dependency theory for this work is that it has been one of the first theories to gain international attention that has also been developed by Third Worlders.
- ¹⁴Roland Colin, Participer au Développement (Paris: UNESCO, 1985). In the original: "Pour atteindre les objectifs du développement rural intégré, il est essentiel que les populations rurales défavorisées participent aux prises de décision à tous les stades du processus de développement."

- ¹⁵Freire and Horton are currently (1987-1989) writing a joint work attempting to crystallize their experiences.
- ¹⁶These persons were all in attendance and provided papers for the 1980 Yugoslavia conference. The conference book is cited throughout under Folke Dubell, ed.
- ¹⁷Kurt Lewin cited at length in the introduction to The Action Research Reader, ed. Stephen Kemmis and Robin McTaggart (Victoria, Australia: Deakin University, 1988).
- ¹⁸Tavistock is cited by William Foote Whyte and Kathleen King Whyte in Learning from the Field: A Guide from Experience (Beverly Hills: Sage, 1984). Foote Whyte attributes Eric Trist, one of the founders of Tavistock, with a major and positive impact on applied social research. Typical of the works that came out of Tavistock in the 1960s is: R. E. Emery and Einar Thorsrud in cooperation with Eric Trist, Form and Content in Industrial Democracy: Some Experiences from Norway and Other European Countries (London: Tavistock, 1969).
- ¹⁹Shirley Grundy and Stephen Kemmis, "Educational action research in Australia: The state of the art (an overview)" in The Action Research Reader, ed. Stephen Kemmis and Robin McTaggart, 3rd ed. substantially revised (Deakin University, Victoria, Australia: Deakin University Press, 1988). p. 322.
- ²⁰Participant Observation is well discussed in terms of theory and method in Jurgen Friedrichs and Harmut Ludtke, Participant Observation: Theory and Practice (Westmead, England: Saxon House, 1975) . In Glenn Jacobs ed. The Participant Observer (New York: George Braziller, 1970) a selection of case studies where participant observation was used is presented.
- ²¹William Foote Whyte, Street Corner Society (Chicago: University of Chicago Press, 1961).
- ²²Friedrichs and Ludtke, Participant Observation, p. ix.
- ²³See Richard Morse, et. al., PAR Handbook for Rural Energy Planning Studies (Honolulu: East-West Center, Resource Systems Institute, 1985). Colleagues involved in describing the project and PAR were Deepak Bajracharya, Kersten Johnson, and Victoria Rummenap.
- ²⁴Lyngrid Rawlings, "Participatory Research - A Vehicle for People Empowerment," paper (Toronto: Participatory Research Group, March 1983).
- ²⁵Paulo Freire, "Creating alternative research methods: Learning to do it by doing it," in The Action Research Reader, pp. 269-274.
- ²⁶L. David Brown and Rajesh Tandon, "Ideology and Political Economy in Inquiry: Action Research and Participatory Research," The Journal of Applied Behavioral Science, 19 (1983): 277-294.
- ²⁷Ibid., p. 289.

- ²⁸Earl Babbie, *op. cit.*, pp. 462-463.
- ²⁹Fritjof Capra, The Tao of Physics (New York: Bantam Books, 1980). Also lecture notes from lecture at the Campus Center of the University of Hawaii, Honolulu, 2 September 1987.
- ³⁰Andrew Pearse and Matthias Stiefel, "Debaters' Comments on Inquiry into Participation: A Research Approach" (Geneva: United Nations Research Institute for Social Development, 1980), pp. 24-25.
- ³¹UNRISD case studies included: Fernando Rello, "State and Peasantry in Mexico: A Case Study of Rural Credit in La Laguna," and Andrew Turton, "Production, Power and Participation in Rural Thailand: Experiences of Poor Farmers' Groups," (Geneva: UNRISD, 1987).
- ³²Anne Whyte and working group, "Participatory Research in IDRC," a working group paper (Ottawa, Canada: the Social Science Division, International Development Research Centre, September 1988), pp. 9-13.
- ³³*Ibid.*, p. 15.
- ³⁴*Ibid.*, p. 19.
- ³⁵*Ibid.*, p. 30.
- ³⁶*Ibid.*, pp. 40-41.
- ³⁷Samuel Paul, "Community Participation in Development Projects: The World Bank Experience," World Bank Discussion Papers (Washington, D. C.: World Bank, 1987), p. v.
- ³⁸*Ibid.*, p. 2.
- ³⁹Folke Dubell, ed., Research for the People, p. 7.
- ⁴⁰Arturo Escobar, "Discourse and Power in Development: Michel Foucault and the Relevance of his Work to the Third World," Alternatives 10 (winter 1985): 391.
- ⁴¹Antonio Contreras, third chapter of doctoral dissertation in progress deconstructing development discourse, political science department, University of Hawaii at Manoa, Honolulu, 1989. Throughout my dissertation writing phase, Contreras and I were both involved in a weekly seminar and discussion series of the Participatory Research Project of the Resource Systems Institute of the East-West Center. We had many debates and co-facilitated sessions around the themes of development, intervention, etc. Two of Contreras key statements crucial to the work at hand have been: "development for liberation is an oxymoron, and that attempts to make development an instrument of liberation are contradictions and are very problematic. In the final analysis and at the bottom-line, it will be shown that development, in all its faces and despite attempts to revise its tactics and strategies, will always be contingent on the exercise of power and control." And "The articulation of different models of development, aside from being a

purely pedagogic enterprise, is a manifestation of the exercise of politics in the realm of the production of knowledge." pp. 3, 12 respectively of work in progress.

⁴²Gustavo Esteva, "Regenerating People's Space," Alternatives (1987): 77.

⁴³Karina Constantino-David, "Community Organization and People's Participation: The Philippine Experience," Kasarinlan: Philippine Quarterly of Third World Studies 1 (4th quarter 1985): 5.

⁴⁴Roland Colin, Participer au Développement, p. 67. The original French is: "Il est presque devenu de rigueur de présenter la "participation" comme l'une des conditions essentielles de la réussite des projets et programmes de développement."

⁴⁵Gustavo Esteva, "Beware of Participation," Development: Seeds of Change (1985): 77.

CHAPTER III

THEORY: ITS RELATION TO PARTICIPATORY RESEARCH PRACTICE

What sets worlds in motion is the interplay of differences, their attractions and repulsions. Life is plurality, death is uniformity. By suppressing differences and peculiarities, by eliminating different civilizations and cultures, progress weakens life and favors death. The ideal of a single civilization for every-one, implicit in the cult of progress and technique, impoverishes and mutilates us. Every view of the world that becomes extinct, every culture that disappears, diminishes a possibility of life.

(Octavio Paz in Jamake Highwater, The Primal Mind).¹

I

The first, substantial activity in this project has been the attempt to clarify both my interpretive position and those of other participatory researchers. Through what filters is the information gathered about social science research practice in the Third World interpreted? Traditionally, researchers have labelled this activity as "stating one's assumptions." As a consequence of examining power and knowledge relations in research practice in general and participatory research in particular, the possibility of achieving a truth--complete and finished--about one's assumptions is questioned. There are assumptions we consciously make on a small scale in order to make tidy and intelligible our explanations of how society or the portion of it we examine works. Then there are assumptions we carry--usually less consciously--about what constitutes a legitimate field of inquiry and the processes for interpreting it. For this reason, this inquiry is approached by (1) surveying or flagging the theories that participatory researchers have cited and used as authority referents for their participatory practice, thus (2) flagging my own theoretical filters and subsequent methodological approaches.

As discussed in the previous chapters, increasingly, researchers and particularly participatory researchers are questioning the traditional approaches to social science research. All decisions to use a particular research approach are inherently political decisions. Such decisions weight some areas as valuable and negotiable areas for

inquiry while leaving others insignificant if not invisible. Those fields of inquiry that may be easily quantified have often been considered more valuable, more scientific than local knowledge, which is often derided as superstition, because it does not fit methods of scientific inquiry. What then is the approach of the participatory researcher in the midst of social science debates about what constitutes a legitimate field of inquiry and ethical considerations about her own particular interactions with marginalized groups?

The participatory researcher might suggest that closed and/or traditional methods should be avoided; the people's voices should be heard and rendered as unfiltered by the writer's views as possible. However, it is theoretically and practically impossible to render other's voices unfiltered, and doubly so in a university dissertation. The dissertation writing process does not easily lend itself to a grassroots information gathering endeavor and local control and dissemination of the knowledge produced. The dissertation production process is historically and inherently a solitary endeavor at the level of individual physical output.

On another level, the dissertation is as much produced by the culture, university, and department in which the candidate/researcher is enrolled or employed. Either way the dissertation is not produced by local, grassroots, or Third World people who are its objects of interest. Even a dissertation about participatory research is not itself participatory. A dissertation effort could be participatory in its approach, especially in field work and interviews, and later restitution and dissemination of results, but the requirements of single "original" authorship and exigencies of university life will work against this.

On a third level, the researcher may not be allowed the luxury of saying, "there is no theoretical approach, no methodology, because I don't want to be bound to any one approach that will determine my outcomes," or alternatively, "I don't want to be bound by an approach that is not the people's." Although some participatory

researchers have used this no-theory approach, it is a license to be unaware of the circumstances of their own knowledge production. Consciously or unconsciously, theoretical orientation does influence methodology.

Some participatory researchers have deliberately steered away from various theoretical orientations, because they realize all orientations are potential impositions. Other participatory researchers fall into a more traditional trap when they are consciously informed by a single theory as in the case of Orlando Fals Borda previously cited.

No participatory research is totally free and unbound. The research facilitator's knowledge production and motivations for acting emerge from reactions to prior socialization into ways of doing science and ethical approaches to being a citizen or member in a particular community. Participatory researchers should not presume their implied ethic of creating spaces where the other may be heard will be self-evident. Such assumptions lead to cooptation and the reimposition of the ethics and values of the dominant culture. But having an explicit ethic seems to be an even more obvious imposition. What then is the researcher to think and do if both having a theoretical structure and not having a theoretical structure are full of pitfalls? What is to be done if interpretation is an inherently violent act--in that it always imposes meanings on others--and we all are engaged in continuous interpretation?

A more fruitful way of getting at the preceding questions is to ask What are the intersections between speaking for the other and making spaces where the other may be heard? Keeping this question in mind, those theories that have served as authority referents for participatory researchers, including this writer, are examined.

The production of this dissertation is an action and doubly so where it proposes tactics. The reintegration of Logos, Eros, and Mythos is presented as part of the general grounds for action and filters the rest of my work, especially where specific critiques are

launched from class analysis, post-structuralist approaches to discourse, and any other potentially dangerous methodological tools of critique.

We may use post-structuralism, class analysis, branches of critical theory, or a specifically Gramscian approach in order to critique those structures and their resultant practices where they make it difficult to hear others in marginalized or disadvantaged groups. All of these approaches are potentially cooptive in that they may impose a vision of the Truth--the goals to be sought and the means to attain them--that is neither the vision of the local group nor in their long term interest. Yet what is to guide the participatory researcher, particularly when institutionalization of participation is sought?

Analysis is launched from this general and open concept of reintegration that demands acting to let otherness be, and it is impossible to do so while remaining exclusively in the realm of logos--the rational. When I explained my analysis dilemma to a native Hawaiian he said, "It's only you white people who think there has to be one idea to explain everything."

The introduction of this chapter asks, "What then is the approach of the participatory researcher?" This brief survey of models of critique and subsequent legitimation for some participatory research practices suggests an approach. The approach, if drawn, might look like the two intertwined spirals of a double helix. What is suggested is open ended.

One spiral of the double helix would include all those practices that critique the enduring orders that marginalize and separate local knowledge and prevent the recognition of interrelatedness.

The first half of the helix' second spiral would include ethical and political practices and consciousness raising that outsider researchers would engage in order for local people to do their own participatory research. Training and documentation

specific to the situation and by request of the local people would also be included. Here the role of the participatory researcher is not to act on the local people. Rather the aim is to raise the consciousness of her own colleagues and prevent impersonal institutional structures from preempting and coopting local people's work. From this perspective, the outside participatory researcher acts to get her own strata out of the way of local people--to let otherness be.

The other half of the second spiral would include all those practices that are enabling local groups to maintain or regain their uniqueness and autonomy. These local practices are often precise and specific to the local context.

The production of this dissertation has led to an approach that is suspicious of all single theories proposing a solution. This approach begins to sound like a genealogical analysis, but it differs from some forms of post-structuralism. Where choices are made and actions are taken, if only on a tentative, temporary basis, some visions of global and long term views are inevitably assumed.

II

A number of participatory researchers, generally those whose practice derives from Marxist critiques, cite the work of Antonio Gramsci.²

Antonio Gramsci, an Italian Marxist writing from prison during the years 1926-1937, coined the term "organic intellectual."³ Gramsci, observing the failure of the First International and the results of the Russian revolution, attempted to address the education of the masses, particularly the workers, towards building hegemony in civil society as requisite to achieving control of the state. The organic intellectuals developing within the working class would facilitate the workers' education.

The organic intellectual was a person from the working class who facilitated the articulation not only of their immediate needs but just as importantly facilitated deep reflection and analysis. Note however that Gramsci considered someone born into the

working class who became, for instance, a priest to be representing the interests of the bourgeoisie and no longer organic to the working class.

The usual Marxist definition of hegemony, especially as developed by Gramsci, includes more than domination by force. The Gramscian concept of hegemony encompasses a working class strategy of alliances, compromises, and intellectual and moral leadership as the social basis for building a workers' state.⁴

The Gramscian concept of deep organic analysis is potentially important to those participatory researchers who wonder why they should bother with theory or even shun theory. I recall many discussions with local people and facilitators who said they had no interest in "theory." Further probing invariably revealed that it was academics and lengthy, incomprehensible documents that signalled theory to the local people. Lengthy discussions in local language of long term causes and effects were welcome as long as they were in a safe and appropriate setting.

What Gramsci does not resolve is the dilemma of someone born into the bourgeois class and actively engaged on behalf and with working class people. Jerome Karabel in his "Revolutionary Contradictions: Antonio Gramsci and the Problem of Intellectuals" cites some aspects of the above dilemma.⁵ The dilemma should be of particular concern to participatory research facilitators who often are outsiders and/or educated far beyond the present means of the villagers and grassroots people with whom they work. By virtue of taking a leadership role even insiders are automatically in a position open to cooptation.

On first reading, it seems the research facilitator, like the organic intellectual, is engaged in organizing a group towards self-emancipation. If, as in Karabel's summation, the task of the organic intellectual is to draw out and articulate the understanding that is already starting to be manifested in class actions, this is an absolutely necessary endeavor. Without it the grassroots will lack critical

understanding and even after initial successes adopt the language of the dominant strata and be overcome. In other words, a repressed class needs to understand the nature of the structure that represses it, otherwise the repressed will resist only what appears to be immediate enemies. The question is how someone from the class that does the repressing can ever get completely outside of the structure and help provide conditions for liberation of the repressed.

The dilemma of the organic intellectual, as well as participatory researchers, is dangerous to the grassroots in the following way: at times of crisis the leadership tends to lose emotional solidarity with the grassroots; they tend to desert during crisis situations. In many cases, it is not even crises that precipitate desertion; oftentimes, the leadership's--even when they intend to act only as facilitators--cooptation by their class ideology is quite prevalent though invisible. It is often difficult for the type of enthusiastic person who did not originate from the grassroots yet who becomes a research facilitator to see that the best interests of her class are not the necessarily best interests of the grassroots. Examples abound in Melanesia and elsewhere. Sincere and enthusiastic outsiders and volunteers engage community members for a short time in new production activities. As soon as the volunteer leaves, all project activity ceases, because local people did not see increased production as necessary and/or the project required too many new responsibilities, unaffordable components, or even risks. It is easy to say after the fact that the leadership involved was not organic enough, but this oversimplifies the situation.

Another difficulty for the organic intellectual and facilitator role not addressed by Gramsci or Karabel and other recent writers is advocacy. Part of the facilitator's problem is that if she is working with a less advantaged group, the role of the facilitator is often in part that of an advocate. The facilitator may become obliged to act as an interpreter of local needs to intermediate level bureaucracies or act as a go-between. Of

course, the facilitator should be stimulating the group's self-analysis and action, but in the final analysis she cannot pretend to neutrality or she ends up participating with the wrong people such as the local bureaucracy. Gradually, such a facilitator may find herself acting to get local people to conform to bureaucratic requirements to get loans or needed changes, which can lead to by-passing the group, its analyses, and initiatives.

The facilitation in what came to be the Kentucky Fair Tax Coalition is an example (cited fully in the fourth chapter) of recognition of this dilemma by facilitators and their refusal to allow cooptation.⁶ Whenever the powerful Appalachian Regional Commission attempted to appeal to the expertise of the facilitators and asked them to meet separately from their community groups, the facilitators declined until and unless all members of the group were included.

There is no precise resolution for the organic intellectual and the facilitator's dilemma. In many cases, at the outset, the facilitator serves to legitimate the group's first activities. But in this middle person's approach to those with the resources lie all the seeds of cooptation. Ultimately, all facilitators can do is (1) be conscious that they are open to cooptation, and (2) return to the realization that "the ultimate indignity is speaking for the other" and that those "others" are often the weakest.⁷ Furthermore, there is a very fine line between facilitating or creating spaces where the other is heard and speaking for the other, especially when one slips into advocacy.

The facilitator can draw two substantial theoretical and practical understandings for future work from Gramsci. The first involves the delicacy of the role of the organic intellectual in the difficult task of achieving an alternative and partial hegemony in an age that sees the increasing commodification and interdependence of everything. Secondly, there is in Gramsci's effort a firm belief that the working classes can create their own hegemony without an elite vanguard. Another way of reflecting on the above is to recognize that ultimately calls for letting otherness be cannot survive the

building of an alternate hegemony. That is, any complete hegemony would exclude and impose on some other people. A partial hegemony perhaps over the definition of participatory research could be useful up to a point, but only where that definition were based upon letting otherness be.

A great many participatory researchers base their personal grounds for action on a very general Marxist critique and analysis of class structures; those that refer to Gramsci are somewhat more aware of the difficulties of their theoretical orientation, and among these may be included Vera Gianotten and Ton de Wit. Gianotten and de Wit critically link participatory research, class analysis, popular education, rural transformation, and the role of traditional and organic intellectuals. In their opening statement, Gianotten and de Wit link theory to practice:

Participatory research strongly upholds the view that conventional research methods and techniques take the target group as the object of the research and not as the principal subject, and that, furthermore, there is an undesirable separation between theory and practice, between the social research and the concrete action.⁸

Gianotten and de Wit see what they term "popular education" as part of the participatory research process. One of the objectives of this process involves the disruption of the monopoly over knowledge. But disruption is not enough. More importantly they see popular education as a class task in which one critically recovers history in order to transform society. Specifically:

[I]n the context of the peasant economy as a function of the (traditional) communal organization, the option of intercommunal organization allows the peasantry to relate itself to the larger society as a social class. The role of education and research points to this political-economic option, in which both are converted into organic popular instances.⁹

Eric Olin Wright put it most succinctly:

The purpose of studying class structure is to be able to understand the constraints on and possibilities of transformation.¹⁰

There is no doubt that a Marxist critique is enabling in the attempts to identify and dismantle the monster of only one--the modern, technocratic, scientific, and capitalist--way of being. However, as much as Marxist analysis and critique is powerful and not to be abandoned, the participatory researcher must be aware that this orientation, like any other orientation, will leave her vulnerable to imposition of an interpretation on the very people she hopes will be liberated; this again is precisely the predicament of Fals Borda referred to earlier.

Of participatory researchers surveyed for this project, those who rely on some form of Marxist critique usually do so in a very open-ended way. Likewise, in my own double helix approach, I would place Marxism, critical theory and the like as useful only in the first part of the helix. This sort of theoretical background is more useful for critique and a constant checking, rather than for generating impractical calls for some kind of unattainable revolution.

III

The work of post-structuralist Michel Foucault has attracted participatory researchers such as Arturo Escobar who see participatory research as a means of dismantling dominant development discourses and building a strategy of resistance.¹¹ Escobar notes that "PAR projects have already formed networks that have led to the emergence of important nonparty regional movements."¹²

From hereon "discourse" is used to signify the specific practices of talking about things in specific fields. This implies more than just what is talked about; how things come to be talked about in specific ways is important. The word "discourse" is useful, because it refers to a specific set of practices without the pejorative connotations of the word "ideology," and because it refers to practice as well as thought processes. Post-structuralism in turn is itself a discourse that is an approach to discourse emerging from the French intellectual experience of the late 1960's. Rather than positing a

central truth and model, the central task of post-structuralist writers has been to problematize discourses which do posit a central truth.

Since the last 2,000 years of Western and particularly Christian thought are characterized by central truths, this provides the post-structuralists a rich field upon which to work. More specifically, much of post-structuralist writing problematizes modernity. The discourse of modernity has been variously characterized as assuming that progress is the objective of humankind. It is assumed that aside from peculiar setbacks here and there, when rationality, science, and technology are applied, the history of humankind will be a march towards ever greater progress and development.

Implicit within post-structuralist approaches is the assumption that modern societies socially reproduce themselves through enforcing relations of power and control including relations of power over knowledge. In short, Western and technological society is able to continue as it is through discourses that leave out other kinds of knowledge. In much of post-structuralist writing, bureaucracies are particularly suspect in reenforcing usual ways of doing business. The bureaucracies control information about the individual citizen, and by categorizing the citizen as more or less able, worthy, or moral, control the information that flows to the citizen. Depending upon how the citizen is categorized, information coming from the citizen is recognized or not.

Foucault is a powerful referent for the participatory researcher in his approach to what he calls "subjugated knowledges," the location of his genealogical analyses.

Foucault understands subjugated knowledges as primarily:

[A] whole set of knowledges that have been disqualified as inadequate to their task or insufficiently elaborated: naive knowledges, located low down on the hierarchy, beneath the required level of cognition or scientificity.¹³

Foucault sees moves over the last generation wherein marginalized erudite knowledges and marginalized popular knowledges have been joined in a rediscovery of

local struggles. He terms the process of joining erudite and popular knowledge and subsequent research "genealogical"--the asking not why, but how did this come to be?

For Foucault, the problem of modernity is that people are made objects. He has described three main ways in which this happens. People are made objects by modes of inquiry, i.e. the sciences. People are separated from others by dividing practices such as the naming practices--rich and poor, sick and healthy, sane and insane. People make themselves into objects by internalizing the ideology and language of the controlling strata. For example, local people may come to think they are too uneducated to challenge the experts.¹⁴

According to Foucault the above practices of power will generate anti-authority struggles. These anti-authority struggles will have certain characteristics which are important to examine. The characteristics included:

1. Struggles are transversal; they are not limited to one place or necessarily strata.
2. The aim of the struggle is power effects, i.e. getting control over whatever it is that is perceived as causing the problems. Often, local opposition is aimed at the privileges of knowledge, i.e. expertness, but also secrecy and mystification.
3. The struggles are immediate, because a) people look for the immediate enemy, and b) they expect no long term project or overall structural solution.
4. The struggles question the status of the individual in two ways; a) the individual asserts his right to be different or ok as he is, and b) the individual attacks that which severs his links to his own community.

Foucault would then focus on the how the controlling institutions define themselves. Foucault then contends:

[T]hese genealogies, that are the combined product of an erudite knowledge and a popular knowledge, were not possible and could even not have been attempted except on one condition, namely that the tyranny of globalising

discourses with their hierarchy and all their privileges of a theoretical avant-garde was eliminated.¹⁵

Chapter four documents some cases of participatory researchers with university backgrounds who worked together with various local groups. It is this sort of interaction that I see as a product of erudite and popular knowledges. Another excellent example of the cooperation of erudite and popular knowledges is the work of environmental anthropologist, Juliet Merrifield, with small communities in Appalachia. Merrifield worked with high-school educated local people to document and learn the properties of toxic chemicals dumped, on the sly, into their local landfill. When state health department experts visited telling people that everything was within "safe" limits, local people were able to cite at length side-effects of the dumped wastes.

Foucault not only seems to provide the method for solving the dilemma of the organic intellectual, but also the method for creating spaces where the other may be heard. Foucault does indeed have an "experimental historico-critical" project which is unraveling how knowledge has been produced and how our world has been constituted. In short, within Foucault's genealogical approach is the possibility of being otherwise. Strictly speaking, however, Foucault's approach is clear only where it pertains to critique. One does have to dismantle the old discourse in order to take new action.

What Escobar does not come to grips with in the article cited above is what one does once the dominant discourse is dismantled. Embedded in Escobar's article is the assumption that people need to take over power and engage in revolutionary actions.

Put another way, a strictly genealogical approach, results in the continual revelations of will to power and knowledge; as reconstruction or alternative construction starts, will to power and knowledge likewise recommences. In other words, any action is an imposition of the actor's will on others and/or the environment, but does this mean that the actor should not intervene? If it does, one seems to have a limited scope

of action. As Jurgen Habermas, perhaps Foucault's most admiring critic, puts it, a genealogical approach leads to endless case studies:

The genealogist explains this to-and-fro movement with the help of countless events and a single hypothesis--the only thing that lasts is power, which appears with ever new masks in the change of anonymous processes of overpowering ...¹⁶

Foucault evidences awareness of the above difficulty in his essay "What is Enlightenment?":

Still, the following objection would no doubt be entirely legitimate: if we limit ourselves to this type of always partial and local inquiry or test, do we not run the risk of letting ourselves be determined by more general structures of which we may well not be conscious, and over which we may have no control?

To this, two responses. It is true that we have to give up hope of ever acceding to a point of view that could give us access to any complete and definitive knowledge of what may constitute our historical limits. And from this point of view the theoretical and practical experience that we have of our limits and of the possibility of moving beyond them is always limited and determined; thus we are always in the position of beginning again. [Emphasis added].¹⁷

The essential difficulty then for the participatory researcher is at the levels of agency and institutionalization. What if a grassroots organization attempts to go beyond the experience of an isolated village or one favela (barrio)? It seems that any attempt to talk about society in the long run or replicate a liberating or empowering experience tends to will to knowledge and power. As Terry Eagleton, one of Foucault's sharper critics, contends:

One advantage of the dogma that we are the prisoners of our own discourse, unable to advance reasonably certain truth-claims because such claims are merely relative to our language, is that it allows you to drive a coach and horses through everybody else's beliefs while not saddling you with the inconvenience of having to adopt any yourself.¹⁸

Eagleton also raises the problematic aspect of language. Post-structuralist writers have done great service in calling into question Western conceptual frameworks based on a single, unifying, underlying truth. The post-structuralists have also done

service in calling into question modernity's assumption that better use of language is capable of getting one closer to reality and truth.

This in turn has led many post-structuralist writers to resist usual metaphors and assumptions about language. Their resistance in turn manifests in their own writings which are often extremely difficult to understand for those readers uninitiated to post-structuralist critique and debate.

Where a participatory researcher tries to link local and erudite knowledge this is problematic; what good then is post-structuralism for local people? In this respect, post-structuralists cannot escape their antecedents; they are correct also in that they cannot escape language. In trying to escape the dominance of language, they have created one that is even more elitist. They have not undermined the process that a popular writer describes: scientists discover a working rain god, but they must make it into a scientific entity, so they name it a "transcendental precipitation inducer," in order to make it their speciality, not the populace's.¹⁹

Foucault did indeed hold some definite opinions on world structure; this is apparent in his lines, "They naturally enter as allies of the proletariat, because power is exercised the way it is in order to maintain capitalist exploitation."²⁰ Foucault could not escape from his own background as an European intellectual, but he did propose to disrupt that background. As William Connolly notes:

[I]t seems possible that Foucault believing that the forces of order are always well represented and armed in the bureaucracies and academies, has decided to focus exclusively on the genealogical project to provide a counterpoint to the forces of order and unity.²¹

Foucault's death in 1984 precludes us from seeing him fully address the dilemmas raised here. Hence, however much Foucault's work attracts through its tremendous power as a tool of critique, Eagleton, Connolly, and Habermas' points remain unresolved.

For the researcher from a university who genuinely wishes to engage in ethical and participatory work, Foucault's genealogical approach can be frustrating. There is a constant tension between not imposing meaning on local people and not having the security of a guide to action; one must be willing to patiently stand by as actions emerge out of very specific local conditions.

Some scholars have assumed that applying Foucault's genealogy leaves one to decode documents and precludes local action. This is incorrect. Foucault himself engaged in modest and painstaking efforts with various marginalized groups.²²

This (genealogical approach) is complex and it circles back on itself, but there are important cautionary elements that could be useful to participatory researchers and inform my own approach. Although, Foucault has been abused to varying degrees, and I think Escobar cited above is one case, he does provide important insights about power.²³

While still using the language of tactics and strategies--battle language, we try to get beyond the usual oversimplified discussions of power. If we think of power as electricity or mana or some thing, then we end up describing those in the Third World as those without power. Usually, it is believed they are without power, because someone else has the means or resources to coerce them. Implicit throughout Foucault's works is a theory that power is that which relates one person or a group to another. These relations are not just of force, but of desire. Although Foucault never described it as such, his work also implies that power is a series or patterns of interactions.

What this means for the participatory researcher is that it is not appropriate to talk about empowering other people. Just because an outsider is able to incite a community or group to take action--to mobilize--does not mean that the outsider or leader or facilitator has empowered people. The message we derive from Foucault is:

People empower themselves; intellectuals, researchers, and facilitators may help this process but only through modest actions which make spaces for others to be heard.

IV

We turn then to critical theory, in particular the work of Jurgen Habermas. Recently, participatory researchers who have grappled with theory have increasingly come to refer to critical theory and Habermas.

Among these are Donald Comstock and Russell Fox. In their essay "Participatory Research as Critical Theory: The North Bonneville, USA Experience," They define critical theory as follows:

Critical theory is both a development from and a critique of historical materialism. It preserves the method Marx used in analyzing both religion and 19th century political economy while applying that method to the structures of domination that characterize the 20th century. Critical theory is an historically applied logic of analysis derived from a nonpositivist epistemology.²⁴

Implied in the above statement seems to be an escape from mainstream, American, quantitative, patriarchal social science. The balance of the paper which discusses a highly successful community action supports this. Unfortunately, Comstock and Fox do not fully develop the approach in their paper, and they neither provide adequate discussion of its potential limitations or the role of research facilitators like themselves.²⁵

The Comstock and Fox paper is extremely valuable in providing evidence that university based scholars can also act to help communities in information collection and analysis that leads to important action. In this case it was the survival of a town that was at stake. The townspeople had already identified their problem, when they turned to the two college teachers for assistance.

Here I would suggest that the reason the Comstock and Fox paper does not provide a really clear-cut link between critical theory and the precise role of the

facilitator is not a fault of their analysis but rather an intuitive and practical understanding of what is really at stake. This gap between theory and guides to action occur in other cases cited below and is expanded later.

Another case, in which a writer tries to apply critical theory and Habermas to participatory research, is briefly suggested by Stephen K. White who cites John Gaventa and the Appalachian Land Use Task Force experience. White sees the Appalachian experience as a case for the type of work that might arise from application of Jurgen Habermas' theory of communicative action.²⁶

An Australian group of action researchers and educators has been working for many years with aboriginal groups to help enable them to develop their own system and curriculum of education. The university researchers' text, Becoming Critical: Knowing through Action Research, discusses critical theory and Habermas at length.

The Australians find Habermas useful as a tool of critique:

The purpose of critique, then, is to provide a form of therapeutic self-knowledge which will liberate individuals from the irrational compulsions of their individual history through a process of critical self-reflection.²⁷

Carr and Kemmis describe the above as an "ethical theory of self-realization."

Yet, the Australians seem to find Habermas inadequate. Interestingly, they make the same complaint about Habermas that many do about Foucault; it is:

...a problem which is the source of considerable frustration to those who look in vain to Habermas's work for the praxis of critical theory: its use in real social action.²⁸

The lack of a guide to praxis may not be a fault. Reviewing Habermas's own work is in order.

Habermas in his 1987 Philosophical Discourse of Modernity launches his theory of communicative action from an extensive critique of Foucault. The essence of Habermas's critique is that Foucault uses reason--rationality to escape the norms and limitations imposed by the modernity project whose essence is dominance by

rationality. Habermas's communicative action project issues from his implied need to work from some universal principle within the world of reason.²⁹

Habermas's project, like Foucault's, is centered in the problematic of discourse, but for Habermas the achievement of something--of understanding--is paramount. Ideal communicative action occurs when consensus is achieved in understanding speech actions; this achievement Habermas also labels "competency."³⁰

Considering facilitation of participatory research filtered by Habermas's project leads to immediate difficulties. Who determines that consensus has been achieved? What are the measures of competency? What happens if the goal of one of the speakers is domination rather than consensus? Before ideal communicative action can occur, barriers must be removed. If the ultimate goal of participatory research is to create spaces where the other may be heard, this desire for speech, for interaction, seems to aim in the same direction. Yet, when the two or more parties interacting are endowed with vastly unequal resources, liberation and empowerment of the speech as well as any other practices of the less well endowed party is unlikely. Space and competence are only goals here.

Without further development, applying Habermas is likely to lead to Foucault's complaint that:

In fact we know from experience that the claim to escape from the system of contemporary reality so as to produce the overall programs of another society, of another way of thinking, another culture, another vision of the world, has led only to the return of the most dangerous traditions.³¹

In short and almost inevitably, today's liberating idea is often tomorrow's prison.

It has been suggested that Habermas sees the modernity project as nearly inevitable, especially where it homogenizes and subsumes local peoples and their knowledges. On the other hand, Habermas implies that if we are interested in the

victims of modernity rather than just describing the institutions of subjugation, there may be some political action that could come out of it all.

Returning to my double helix, Foucault and Habermas have both provided referents for the first strand that is critique. Foucault especially could be a referent for the half of the second strand that is based upon constant checking and raising the consciousness of fellow researchers when he asserts:

...the Greek sage, the Jewish prophet, and the Roman legislator are always the models who haunt those who today make their profession from speaking and writing. I dream of the intellectual destroyer of evidences and universalities.³²

But it is Habermas who points toward the second half of the second strand, when he suggests that subverting institutional practices is not enough. Habermas sees politics as a positive and rational enterprise for doing something about the injustices of modernity.

Finally, neither Habermas nor Foucault is at fault for their lack of guides to praxis. Neither philosopher as a writer could provide a guide to praxis. Even though Foucault may have engaged in specific actions with groups that have been the Third World of Europe, those engagements are specific to those people and those conditions. It is simply that one cannot speak about those with whom one has not acted without enacting undesirable power relationships. That would be to base action on theory without the theory coming out of action. That would be speaking for the other.

V

Part of consciousness raising--or one might say expanding--for university based participatory researchers involves stepping outside of usual scientific rationality. One must, at least temporarily, step outside of logos into Eros and Mythos. That is knowing through feelings and stories and myths must become

possible or reintegrated back into being. Both writers and activist writers in the eco-spiritual and liberation theology movements have important insights, based upon deep actions in their communities, to offer in this regard.

I am not suggesting here that university professors need to go out and become shamans in order to understand, for example, native American medicine men. What I am proposing is accepting the possibility that there are other ways of knowing and that they contribute to local people's identity and survival.

In recent years, participatory researchers and liberation theologians have been mutually influential. The work of Brazilian Leonardo Boff applies.³³ Boff devotes the first chapter of his St. Francis to the Logos and Eros question:

Everything points to the fact that we are arriving at the end of this long process, not at the end of reason--that would be absurd--but at the end of its total rule.³⁴

The factor that Boff allows to connect logos and Eros is his concept of Christian love and its relation to power. Boff explains:

Others contrast love and power, linking them in an inverse relationship: the more the power, the less the love, and vice versa. This opposition also takes place on a superficial and psychological level. Love is understood as a subjective emotion and power as compulsion and domination. On an ontological level, power is power to be, a condition for love itself. Love, ontologically, is the power of giving, of surrender, or the capacity to accept the other as other. Love and power are not mutually reducible, but maintain dialectical relationships between each other ... the one must be articulated with the other in order to accurately describe reality [emphasis added].³⁵

One does not need to be a Christian in order to work from Boff's understanding of power and love as the attractive forces between logos and Eros. Boff's concept of power is not unlike that of those engaged in developing the Gaia hypothesis and/or eco-spiritual movements.

The Gaia hypothesis, first developed by James Lovelock, then a scientist working on the Martian atmosphere, suggests in his words, "that the entire range of living

matter on Earth, from whales to viruses, can be regarded as a single entity, capable of manipulating its environment to suit its needs."³⁶ For those who espouse the Gaia hypothesis, every species is vital in its variety and interdependence. It is more than just biological diversity that is celebrated; human, cultural diversity and the diversity of ideas and beliefs are seen in the same light of reintegration.

Boff goes on to further connect his reintegrative concepts:

It is important to underscore that Pathos (feeling) is not in opposition to Logos (rational comprehension). Feeling is also a form of knowledge, but more comprehensive and enveloping than reason. It embraces reason within itself, releasing it in all directions.³⁷

Boff's understanding holds special significance for participatory research facilitators who attempt to validate local knowledges and refuse to dismiss them as mere superstitions.

Likewise, Dominique Barbé, a French priest and liberation theologian living in a Brazilian favela (slum), writes that the greatest problem stemming from Marxist analysis is the lack of treatment of the state and the lack of direction as to the political means of organizing and directing a new society. Barbé pronounces:

Marx limited himself much too strictly when he said, or let others conclude, that a communitarian society would come about through the dictatorship of the proletariat--as if the seizure of power by the oppressed class were of itself an event sufficient to release a talented social inventiveness leading to a human industrial society.³⁸

After further consideration, Barbé concludes:

In our opinion, one cannot in fact speak of a science of history. It would be more correct to say that there can be found rational aspects and scientific elements among the historical data.³⁹

Barbé elaborates why relying on a science of history is problematic:

If whatever is real is rational, [paraphrasing Hegel] we must submit ourselves without complaint to the proprietors of knowledge: either to the science of the party, the enlightened vanguard of the proletariat, which knows where things are going; or to the orders of the capitalist technocrats, who know what they are talking about when they manage industrial society ...If they believe that they truly know, it is normal for them to conclude that they have a right to be obeyed

under all circumstances. We condemn a philosophical error, not transient errors in behavior. Philosophy is not a neutral exercise. It has consequences; sometimes these are tragic. It is necessary to oppose Hegel, Marx, and bourgeois-industrial scientism to the extent that they reduce the real to the rational.⁴⁰

Thus it is we find participatory researchers and liberation theologians using a variety of scientific and rationality based critiques and then moving beyond and through them in their practice and reconstruction. This is equally applicable to both those whose critiques are informed by Marxism and those informed by post-structuralism.

To Boff's and Barbe's inclusive and reintegrative understanding, one could also make a place for myth and Mythos. It is not mere chance that has allowed "California gurus" and religious fundamentalism to explode in popularity through their various attractive powers. Hans Peter Duerr, Germany's answer to Carlos Casteneda, in his best seller Dreamtime suggests that modern technocratic society has paved over all the places for being otherwise and that sooner or later the repressed otherness flies out the cracks, boundaries, and locations of contradictions. Duerr is quite clear in his final chapter as to what the problem and the task are:

No wonder that in such a society, understanding what does not belong to it most often takes on the character of appropriation by subjugation or pronounced rejection. Our Western anthropologists rarely applaud such 'cultural annihilation' openly any more. Instead, these scholars often engage in a more subtle incorporation of material that otherwise might endanger their basic assumptions.⁴¹

Duerr's project is based on a powerful understanding of otherness:

The professor does not realize that "not being able to be the other person" is not a hindrance to, but rather the prerequisite for understanding the strange.⁴²

Duerr's description of his project indicates that he is prepared to let otherness be, to let Mythos exist without giving up logos. His statement is concise and passionate:

What this does not mean, however, is that we should endlessly move our fenceposts further and further into the wilderness and ceaselessly clear, work, and categorize what is 'out there.' It means instead that we ourselves should turn wild so as not to surrender to our own wildness, but rather to acquire a consciousness of ourselves as tamed, as cultural beings.⁴³

VI

There is indeed a general ground upon which advocacy and strategy in this dissertation rest; these grounds are a seemingly contradictory call for letting otherness be based upon our knowledge of our coexistence in Gaia--the earth--and our knowledge of our own existence as creatures acting in logos, Eros, and Mythos. In short, this is a call for letting individuality and autonomy exist based on a knowledge of the interrelatedness of everything. That call for the simultaneous recognition of autonomy and interrelatedness is eloquently expressed by Octavio Paz in the introductory epigraph to this chapter. The call is echoed in the Gaian writing of ecologist William Clark:

We are now destroying crucial planetary resources of species, forests, and fisheries without knowing the full consequences of our actions ... Perhaps our most turbulent transition will be the establishment of equity among ourselves--the only basis on which the transition to sustainable management of our natural environment can succeed.⁴⁴

Increasingly, participatory researchers in the so-called developed world find themselves engaged with local people in local and regional environmental battles. Several participatory research efforts undertaken by the Highlander Center have been made in the context of a marginalized people challenging the harmful legacies of industrial society.⁴⁵

Morris Berman calls for an integration of mimesis and analysis based on an approach to power that draws from centeredness rather than "the ability to make others do what you want them to against their will."⁴⁶

Berman devotes a great deal of attention to his seeming utopianism and its inherent dangers in his final chapter:

As Reich realized, industrial democracy is dry tinder for fascism and the irrational precisely because it is so sterile, so Eros denying, and because it has been with us now for centuries.⁴⁷

All the while realizing his approach could lead to the very injustices he hopes to avoid, Berman cannot avoid acting, and he announces his grounds for action:

In short, it is my guess that preservation of this planet may be the best guideline for all our politics, the best context for all our encounters with Mind or Being. The health of the planet, if it can be successfully defended against the continuing momentum of industrial socialism and capitalism, may thus be the ultimate safety valve in the emergence of a new consciousness.⁴⁸

The above eco-spiritual and liberation theology bases for recognizing other ways of knowing, while extremely important in my own assumptions, do have serious potential limitations for the participatory researcher.

While not denying the tremendous effects liberation theology has had for groups in the Philippines and Latin America, there is no avoiding the evangelical requirements of the Catholic Church and Christian God. Liberation theology can not liberate non-Christian indigenous peoples. In some situations even liberation theology might not serve to let otherness be.

Environmental, eco-spiritual arguments also have their essentialist, possibly totalizing aspect: earth first. What happens then to some Third World producers or those poor Brazilian farmers clearing the Amazon rain forest?

The brief survey of theory in this chapter supplies some referents and bases for the activities of participatory researchers--both insiders and outsiders--but it focuses more on the outside participatory research facilitators. The reason for this focus is that I as a university based researcher cannot speak for others, local or Third World people.

There are specific activities in which the outsider participatory researchers can engage. One strand of these activities is cycles of critique and reflection. Part of a second strand is consciousness raising of self and colleagues. Specific training, documentation, and other activities might be undertaken on request of researched

groups, but this second half of the second strand can only be undertaken in conjunction with local people, otherwise theory will dictate action and others are spoken for or manipulated.

The next chapter turns to local actions in which outside participatory and action researchers engaged. The role of the participatory researcher is developed from the cases presented

NOTES FOR CHAPTER III

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- ²Quintin Hoare and Geoffrey Nowell Smith, eds. Antonio Gramsci: Selections from the Prison Notebooks (New York: International Publishers, 1983).
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- ⁴Tom Bottomore, Laurence Harris, V. G. Kiernar, and Ralph Miliband, eds. Dictionary of Marxist Thought (Cambridge, Massachusetts: Harvard University Press, 1987), p. 201, notes that "hegemonism" has also been used by Mao Tse-Tung to indicate domination of one country over another.
- ⁵Jerome Karabel, "Revolutionary Contradictions: Antonio Gramsci and the Problem of Intellectuals," Politics and Society, 1976.
- ⁶John Gaventa, in Research for the People, Research by the People ed. Folke Dubell (Linkoping, Sweden: Linkoping University, 1981).
- ⁷Michel Foucault with Gilles Deleuze, "Intellectuals and Power," in Language Counter-memory and Practice: Selected essays and interviews, ed. Donald F. Bouchard (Ithaca, New York: Cornell University Press, 1977).
- ⁸Vera Gianotten and Ton de Wit, "Participatory research and popular education in a context of peasant economy" paper (The Hague: Amersfoort, December 1982), p. 1.
- ⁹*Ibid.*, p. 7.
- ¹⁰Eric Olin Wright, Class Structure and Income Determination (New York: Academic Press, 1979), p. 365.
- ¹¹Arturo Escobar, "Discourse and Power in Development: Michel Foucault and the Relevance of his Work to the Third World," Alternatives 10 (winter 1984-1985): 377-399.
- ¹²*Ibid.*, p. 391.
- ¹³Michel Foucault, Power/Knowledge (New York: Pantheon Books, 1980), p. 82.
- ¹⁴Michel Foucault, "The Subject and Power," afterword in Hubert L. Dreyfus and Paul Rabinow, Michel Foucault: Beyond Structuralism and Hermeneutics (Chicago: The University of Chicago Press, 1982). This is Foucault's most explicit outline of what he actually proposes to study.
- ¹⁵Michel Foucault, Power/Knowledge, p. 83.
- ¹⁶Jurgen Habermas, The Philosophical Discourse of Modernity: Twelve Lectures, trans. Frederick Lawrence (Cambridge, Massachusetts: The MIT Press, 1987), p. 253.

- 17 Michel Foucault, The Foucault Reader, ed. Paul Rabinow (New York: Pantheon Books, 1984), pp. 46-47.
- 18 Terry Eagleton, Literary Theory, an Introduction (Minneapolis: University of Minnesota Press, 1983), p. 144.
- 19 Douglas Adams in The Hitchhiker's Trilogy (New York: Harmony Books, 1983).
- 20 Michel Foucault with Gilles Deleuze, "Intellectuals and Power," in Language Counter-memory and Practice, ed. Donald F. Bouchard (Ithaca, New York: Cornell University Press, 1977), p. 216.
- 21 William E. Connolly, "The Politics of Discourse," ed. Michael J. Shapiro, Language and Politics (New York: New York University Press, 1984), p. 144
- 22 See Jean-Marie Charpentier, coordinator, Michel Foucault: une histoire de la verité (Paris: Syros, 1985), p. 98. Published by Foucault's friends and colleagues a few months after his death. In this book Seweryn Blumsztajn, leader of Solidarnosc in France, recalls: "This is the same man, who with an exceptional devotion, consecrated entire hours helping us with the most bureaucratic and repetitive tasks. One could always count on him." [translation my own], p. 98.
- 23 An interview with one researcher on Melanesia revealed that his work drew on Foucault. The researcher claimed to be relieved. Because he was using Foucault no one on the island he studied would be able to understand him, and he would not have to deal with local people's interpretations of his intent and work. I really doubt that this is what Foucault meant when he talked about erudite and popular knowledge coming together. This case seems like a case of the erudite mining the popular and local.
- 24 Donald E. Comstock and Russell Fox, "Participatory Research as Critical Theory: The North Bonneville, USA Experience," paper (Olympia, Washington: Evergreen State College, December 1981).
- 25 *Ibid.*, through interviews with Russell Fox, Milwaukee, Wisconsin, November 1987, and correspondence with Donald Comstock November 1987, I understand that they became so involved in everyday work with their grassroots groups that they have not to date pushed their theoretical horizons any further.
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- 27 Wilfred Carr and Stephen Kemmis, Becoming Critical: Knowing Through Action Research (Victoria, Australia: Deakin University Press, 1986), p. 138.
- 28 *Ibid.*, p. 151.
- 29 Habermas, The Philosophical Discourse of Modernity.
- 30 *Ibid.*, pp. 295-301.

- 31 Foucault, The Foucault Reader, p. 46.
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- 33 Paulo Freire, interviewed 4 December 1987 at Highlander Center, New Market, Tennessee. Friend to many in the Participatory Research Network, Freire has said that he counts Leonardo Boff as one of his most respected friends.
- 34 Leonardo Boff, St. Francis (New York: Crossroads, 1985), p. 8.
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- 36 James Lovelock, "Elements," in Gaia: An Atlas of Planet Management, gen. ed. Norman Myers (Garden City, New York: Anchor Books, 1984), p. 100.
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- 38 Dominique Barbé, Grace and Power: Base Communities and nonviolence in Brazil (Maryknoll, New York: Orbis Books, 1987), p. 79.
- 39 *Ibid.*, p. 86.
- 40 *Ibid.*, p. 86.
- 41 Hans Peter Duerr, Dreamtime: Concerning the Boundary between Wilderness and Civilization, trans. Felicitas Goodman (New York: Basil Blackwell, Inc., 1985), pp. 125-126.
- 42 *Ibid.*, p. 132.
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- 44 William Clark, "Management," in Gaia, p. 252.
- 45 John Gaventa, interviewed December 1987 at Highlander Research and Education Center and March 30, 1989 at the East-West Center, Honolulu. See also: Juliet Merrifield, "Putting the Scientists in their Place: Participatory Research in Environmental and Occupational Health," Highlander Center Working Paper (New Market, Tennessee: Highlander Research and Education Center, January 1989).
- 46 Berman, The Reenchantment of the World, p. 292.
- 47 *Ibid.*, p. 295.
- 48 *Ibid.*

CHAPTER IV

CASE STUDIES: SUCCESSFUL PARTICIPATORY RESEARCH AND THE ROLE OF THE FACILITATOR

Our society is not a collection of separate people and customs and traditions. No, it is a complex system, with all the pieces as dependent upon each other as the animals and vegetation of the savanna. If you burn the grass, you will not only kill the impala who feeds upon it, but the predator who feeds upon the impala, and the ticks and flies who live upon the predator, and the vultures and maribou storks who feed upon his remains when he dies. You cannot destroy the part without destroying the whole.

(Mike Resnicks, "Kirinyaga")¹

I

In order to think through the closing question of the second chapter: Is the label and practice of "participatory research" so wholly coopted as to reimpose the very practices it hopes to supplant, we turn to some real world applications of participatory research. The difficulties discussed previously will be enlarged upon in Chapter V in the context of institutionalizing and sustaining participatory efforts. The purpose then of this chapter is to introduce one case in which participatory research is deemed by Participatory Research Network members to have been well practised; the case is that of the Appalachian Land Reform Study documented by John Gaventa.² In addition, two Third World, Melanesian cases, thus far undocumented in participatory research materials and libraries are included. One case covers major aspects of entrepreneur and small farmer training in Vanuatu as well as the results of long term self-organization.³ The second Melanesian case involves participatory research and cultural identity in Vanuatu. Although not labelled as participatory research by the facilitator and local originators, the effort was always clearly considered to be under local control by the originators--the National Council of Chiefs and the facilitator and eventually the grassroots people involved.⁴

The common factor in the cases assembled and analyzed here is that from the outset facilitators and initiators were committed to participatory practice, whether they described it as "participatory development," "action-research," "participatory research," or more directly "local control." This commitment to participatory practice is clearly present in locally prepared documents supporting the start-up and continuation of these various projects. In all these cases, the primary outside facilitators had received their education as social scientists, one from Oxford, one from the Sorbonne, one from Cambridge, and one from Columbia University, yet their entries into local affairs were at points where local people had already started to define their problems and call for outside assistance. There was a definite invitational element when the research facilitators first engaged in these projects with local people; local people had a choice as well as input as to whether or not they engaged in these projects. In all of these cases, there was an iterative system of local information collection, analysis, and on-going bottom-up change in the work.

Before continuing, it is important to note my usage of the words "researcher" and "facilitator." In these action based cases I have used "facilitator" interchangeably with "participatory researcher." In the better attempts at participatory research, the researcher does not just gather information to take it away for processing, but rather engages and helps facilitate a group analysis, decision making, and possibly action process. Furthermore, in participatory research, the researcher is the entire community or group.

Again, this chapter is not intended only to present recent Third World and specifically Melanesian histories. There are increasing numbers of Pacific Islanders who can make a good critique and analysis of development and research efforts. Neither is this chapter intended to present a perfect model of participatory research projects that can be used as a template everywhere. The purpose is to introduce several situations

were at the outset local people wanted participation and local control and managed to retain, regain, or increase that control. The more modest task here is to begin to explore where participatory research flourishes and where it meets its limits.

There have already been numerous case studies of participatory research projects. For example, in the late 1970's the United Nations Research Institute for Social Development supported a dialogue, debate, and then commissioned a series of participatory research investigations.⁵ The majority of these analyses do not go far enough. Analysis of many of the participatory research cases described to date reveals (1) the intent to engage in participatory research, and (2) initiation of participatory activities. What most descriptions do not give us is enough information about what happened in the long term and/or difficulties in sustaining and institutionalizing participatory activities or even retaining local control.

A major part of the difficulty with many so-called participatory research case studies to date is that they confuse ethical development practices with participatory research. Such case studies, often quite thoroughly, describe the careful inclusion of local people in an already existing development plan. While this is not necessarily all bad--and there is a continuum of practices inbetween--it is different than locally originated, locally controlled efforts where information gathering and analysis is key.

The reasons for the above confusion are several. Many participatory researchers avoid separating action and reflection, thus activities that appear developmental are often part of their work. There are cases where facilitators, whether insiders or outsiders, apply principles of local control in an action project, but forget or skip iterative processes of local information gathering, analysis, and decision-making. In reality, where both action and reflection are important parts of local group problem solving, research and development-like components often overlap and make it difficult

to define a project as mostly participatory research or mostly ethical community development.

An additional difficulty is that many efforts at community change in the Third World have occurred which although not labelled "participatory research" have in every way used the approaches described. The intent in these cases is to initiate a change effort which includes information gathering and analysis and gaining of local control. Over the years this researcher has encountered many facilitators and communities who had not labelled their work as participatory research, but when encountering the name said, "Oh, yes, this really seems to be what we are already doing." The Vanuatu Culture Centre's work which led to many local cultural festivals is one of these types of cases. It is included, because over a number of years the work has led to an increased pride in local culture and identity as well as increased self-organization towards larger activities--a sort of re-enfranchisement of people who were not formerly citizens in their own land. The people involved in the Vanuatu culture project regained proprietorship over their own knowledge base and used it to construct their cultural and political identity.

Another possible problem in some of the cases where the label "participatory research" was applied retroactively is that the initiators might not have been as clear about participatory values and objectives as they should have been. Clear-cut conceptualizations of participatory practice emerged as people engaged in a struggle or attempted self-organization.

Participatory research is sometimes seen by critics as only useful (and perhaps dangerous) in raising social and political awareness. Mainstream research in the Third World context is often only directed towards those problems that are assumed solvable by increasing economic production via technology. The cases presented run across both these artificial boundaries between economic and social well-being.

The questions that are raised are: to what did this participatory research lead? What was the purpose of engaging in participatory research? Most importantly, did it lead to any long term consciousness raising that would help retain local control?

Thus, the reasons for re-presenting this particular set of cases are: (1) the deep familiarity the author has with their locale--in the Melanesian cases--and hence ability or at least potential to act in participatory manner in reviewing and making restitution of this very writing. When I commenced this dissertation, it was my hope that even some correspondence or other communication with local facilitators could lead to local discussions and consciousness raising about research practices. Specifically, I hoped to at least offer some local groups some language to use or legitimating presence in challenging exploitative research practices. Obviously, this interaction would serve to make my work more immediate if not more honest.

(2) I know of no documentation to date on specifically participatory research or action research work in the Melanesian Pacific, although people have been engaging in these practices. Simply documenting a few cases may again open up discussion in the area of research ethics in ways that have not been broached before.

(3) Reconnection to global research issues across hemispheres was one reason for including the Appalachian case. The kinds of relationships I find problematic do not just exist in the geo-political Third World; they exist in so-called developed countries as well. These relationships do occur where and when people live in exclusionary relationships oftentimes characterized by poverty. Furthermore, the Appalachian case is well known among participatory researchers and serves as a guide to many. It is important to represent this case, because it makes clear what the ideal practice of participatory research can be and just how much resistance can be generated by opponents of participatory research practice.

Again, most participatory research project case studies available through United Nations and other literature seem to highlight the successful, e.g. material aspects of projects or at best pedagogical techniques of the facilitators and trainers. Although these usual case studies go beyond just counting numbers of buildings completed or children vaccinated, they often get stuck at the level of saying a group was organized. Few case studies to date analyze limitations from above and below. In short, structural and political issues are avoided.

II

Details of the first case to be considered are found in a paper entitled "Land Ownership in Appalachia, USA: A Citizens' Research Project" by John Gaventa, one of the project's facilitators.⁶ Another primary source is the book prepared by the citizens themselves, Who Owns Appalachia?⁷ Interviews were held with the facilitator, support staff, and participants during the first week of December 1987. Most fortuitously also present at Highlander Center during the week was Paulo Freire, the Brazilian educator.⁸

Regional Background

Appalachia comprises a mountainous region including western New York state and parts of Pennsylvania, West Virginia, Kentucky, Tennessee, Virginia, Alabama, and Mississippi. Through much of the twentieth century the region has been characterized by the kind of relations with the rest of the United States that are seen in the Third World. Removal of a formerly self-provisioning peasantry from lands and engagement in the extraction and/or production of raw materials (in Appalachia most often it was coal) at low wages created profits for capitalists in distant cities.⁹

It seemed for a while in the 1970's that the battles fought by union organizers had been mostly won. By the 1980's, however, many of the transnational corporations moved overseas in search of lower costs and cheaper labor, leaving workers throughout

the region stranded and unable to tap into corporate resources such as land. The transnational corporations did leave a considerable legacy--the consequences of drastic erosion, floods, and environmental pollution.¹⁰

John Gaventa's role as a facilitator in this case was through the Highlander Research and Education Center. The Center has acted since the 1930's as a place where activists could come together and where local people could come to work out--analyze--their own problems and solutions. Among other achievements Highlander is now seen as a major contributor to the civil rights movement. During the 1950's Highlander helped support a large, grassroots literacy program which registered Blacks to vote. Throughout all its activities Highlander founders and current staff continue to see the dignity of ordinary people and their abilities to learn from each other and run their own lives as paramount.

Many participatory research practitioners consider the Appalachia Land Use Survey to be the epitome for participatory research efforts and, even subsequent to my own visit and interviews in Appalachia, the case has acquired increasing citation.¹¹ The chronology of the original project follows.

Case Summary

In 1978 a citizens' group in Mingo County, West Virginia formed a six-state coalition which became the Appalachian Alliance. The Alliance's primary purpose was to do something about taxation, particularly the relationship between absentee and corporate land ownership and taxes. During the previous decade of its existence the multi-million dollar federal and state agency, the Appalachian Regional Commission (henceforward the ARC) had been confronted by local people on these issues to no avail. This reoccurred with the Alliance; the Alliance requested a land use survey, and the ARC refused to do such a survey. The Alliance then formed a Task Force joined by scholars from the Appalachian Studies Conference.

In August of 1978 the ARC finally decided to do a study but labelled it "settlement patterns." At this point the citizens' Task Force decided to go ahead and do their own study. From the outset both the purpose and process to be used in the Alliance Task Force study were clear and shared with all involved. The essential points of the study were:

1. The study was to be a model for citizens to do their own research without reliance on outsiders dependent upon government agencies for their pay.
2. Not only would the study be done by citizen researchers, but their training would continue from start to finish of the study.
3. Conducting the study was intended to help network citizen's groups and use their joint collection of information for social action.
4. The results of the study would be used to mobilize a larger constituency.

What happened next was a calculated risk on the part of the citizens; the Task Force submitted their proposal for funding to the ARC. There was an immediate meeting in which the ARC tried to coopt the Task Force. As John Gaventa, a member of the Task Force, put it, this was a "classic confrontation between those who control the production of information, and those who need it for social change."

The Task Force came prepared with a strategy. These were no wild eyed idealists. They fully expected that the ARC would argue that research is neutral anyway and requires experts thus appealing to the egos of those Task Force members who were academically trained. Prior to ARC meetings the Task Force had agreed to a sort of "collective bargaining" procedure where no moves could be considered let alone made without the entire group. Despite clever manipulation by the ARC, the Task Force also held firm in the decision not to share their knowledge products. The ARC had in effect proposed that the Task Force do the footwork of gathering the information, but that the ARC would decide how to analyze it and how to disseminate it. The Task

Force decided that if they couldn't control these activities, they would withhold the information they gathered. Thus, the Task Force engaged in a direct confrontation against the research process itself. Gaventa notes, "The collective withholding of knowledge, especially from those so used to controlling it, provided great conflict."

The ARC eventually did fund the Task Force proposal. Precisely because the ARC had argued that research is neutral and used that to legitimate themselves, their organization's reputation was open to tarnishing by the Task Force. In short, the ARC would have looked absurd if they had not funded such a mass based request as that of the citizens' Task Force.

The Task Force had tried to put together a cooptation proof plan. Their plan included six states and eighty counties. Each state had a representative on the Task Force. Information was to be collected from tax rolls on all land owners with over 250 acres and all corporate or absentee land owners with over 20 acres. In 20 counties, there would be a detailed study of the impact of land ownership/use patterns in relation to such diverse areas as education, jobs, etc. This was kept fairly open for each group to decide for themselves while keeping some of the base data to be collected on the same basis in all counties.

By January 1979 the proposal was accepted by the ARC, but there were conditions which as it turned out were more onerous than was at first supposed. The conditions included:

1. The funds were not to be released until a planning phase was satisfactorily completed.
2. Funds would not be released in any state until the governor's office gave approval.
3. The ARC called for more "structured scientific designs."

Since the first citizen training workshops were slated to begin in May 1979, and people had already been lined up and quit jobs this left Task Force organizers in straits. The demand for "more structured scientific designs" was particularly onerous, because the study had been designed to work with the grassroots organizations' definitions of the problems. Calling for exactly the same investigations of the same statistics everywhere, would have neglected and diverted energies from investigating important local relationships. Based on this the Task Force decided to threaten the ARC with a walk-out. This succeeded and the Task Force started to receive funds just in time to start the training workshop.

The workshop encompassed a three day intensive session with sixty people with follow-ups in various locations. Again, the case study approaches were left open for local groups to work around their local issues. The workshop also catalyzed networking.

Within a few months, the research products were prodigious: Page after page of property figures which would have been tedious, meaningless numbers for the outside expert became items of great intrigue for the citizen researcher. To them, the numbers and names represented power and powerholders they knew. The data quickly gave them insights into local community affairs. With such motivation, the citizen often took time to search out information that investigators who were simply in it for "the job" would not have pursued or would have reported as missing.¹²

As it turned out, the open ended research design was no less scientific than anything else that would have been done. Knowledge was shared, linkages were made, and action groups were formed. Anticipating that the ARC would balk at publishing the final results the Task Force had written the original proposal so that the ARC had only one month to accept and publish the results, and that if they did not the Task Force would then have complete rights to do so in whatever manner they saw fit.

Case Analysis

The immediate result of the Appalachian land use study was that all the grassroots organizations and individuals involved gained experience in participatory research, using information and analysis towards changing local problems, and networking. Some were able to engage in large, long term campaigns towards tax restructuring and policy change. Among these campaigns is the Kentucky Fair Tax Coalition.¹³

In an interview, a member of the Kentucky Fair Tax Coalition noted that even though a lot of the research was into what the power structure already knew about itself rather than people's local knowledge, the experience was nevertheless useful and powerful.

"In fact," Gaventa says in another participatory research case, "the community definitions ... contrasted so dramatically with those of the power holders, that participants were then able to analyze the next stage of why the 'official' bodies failed to reflect their own needs."¹⁴ It seems that much of the information the Task Force dug up was already known to the ARC, but what that information meant was interpreted and presented (if presented at all in the case of the ARC) very differently. "Why then," citizens ask, "has this agency, supposedly created to help us, acted in this way?" Further, even though many battles on taxation policy have not been won, obstacles and further analysis by citizens may be leading toward increasing mobilization.

During interviews and subsequent dialogues, Gaventa has been careful to point out the difficulties in institutionalizing every aspect of this work. In particular, Gaventa has stated that the Appalachian case was a unique and ideal opportunity, because the communities and citizens' groups came to Highlander with their problem already well-defined after years of community-based work.

Let us return to Foucault to try to press analysis of the Appalachian land study further. By seeing where Foucault's approach and what actually happened in Appalachia meshed or not, we may also find some limits and possibilities in Foucault's theoretical work and our own.

Foucault generally contends that people are made objects of controlling practices, and in this case we include the kind of research and development in which the ARC engages. In the Appalachian case people were made objects in three ways. First, local people were made objects by the mode of inquiry of usual social science. Second, they were subject to what Foucault calls "dividing practices;" labelling them "poor and illiterate" separates them from those of "us" who have and know. Third, until engaging in participatory research and analysis, many people silenced themselves. Juliet Merrifield tells us of the typical reaction of a fisherman who was suffering from the effects of severe pollution in the local waterways. After a meeting with visiting experts the fisherman said, "I didn't say a word during the meeting ... [they] had fifteen lawyers, what's a man with a high school education going to say to a bunch of college professors?"¹⁵

When people decide to resist the above types of controlling practices, Foucault suggests that their anti-authority struggles can be characterized in several ways.

The first characteristic is that the struggle was transversal: it cut across 80 counties and six states. Second, the aim of the struggle was the power effects of taxation. Third, the struggle questioned the status of the individual in two ways. The individuals involved were asserting their right to be different from the elite in the ARC and ok as they were--highschool educated. The individuals involved were also attacking that which separated them from their community life, i.e. land tenure and taxation practices which push rural people off the land and into cities. Opposition to the effects

of power were linked to struggles against the privileges of knowledge while also linked to secrecy and mystifying representations.

Foucault also suggests that such struggles are immediate. This immediacy is characterized in turn by looking for the enemy in the immediate situation and not expecting any solution. This is where the Appalachian effort does not follow Foucault's characterizations. Through the participatory research process, people learned to look further than the local representatives of corporations for the enemy. There was a fair amount of group analysis and discussion of the world-economy, transnational corporations, and their links to the state. After the study was complete, the Task Force held a "Land Visions Workshop" at Highlander Center. The group asked themselves, "If we owned the land what would we do with it?" They visioned a different structure and subsequently issued the "Appalachian Land Ownership Manifesto."

Furthermore, although hoping for immediate local solutions, people did organize themselves for the long haul into such organizations as the Kentucky Fair Tax Coalition. In West Virginia, a State Supreme Court decision overthrew the taxation system that had left rural schools severely underfunded.

The authoritative source throughout was the citizens' study. Many of the citizens came to realize that only major restructuring of transnational organizations and situations would really cure the roots of their problem. Although it is obvious that any significant restructuring is a long term process at best, citizens did have the confidence to model other research efforts along the Task Force lines. One of these important projects was the Southeast Women's Employment Coalition in which rural and poor Southern women engaged in their own research, analysis, and action.

III

The next two cases to be considered both continue to take place in the Republic of Vanuatu. The first case is that of the Vanuatu Plantation Training Center and its offshoot, the Plantation Support Association. The second case is that of Vanuatu Culture Centre and its program to support the revitalization of traditional and local culture.

The regional background provided below is relevant to both cases.

Background

Vanuatu became independent from a joint condominium of England and France in July 1980. The new Republic modeled partly on the Westminster system needed overseas financial aid to run nearly 100% of its administration and ironically any projects aimed at economic self-reliance and less aid. Despite service to the ideal of "Melanesian socialism" the new, Melanesian leaders did envision a private sector. Furthermore there was a desperate need for people with management skills and/or able to evaluate proposals of outsiders. There were only eight Melanesian (from here-on Ni-Vanuatu meaning indigenous to Vanuatu) university graduates at independence. The few Ni-Vanuatu with management skills were usually lured away from public service by the private sector.

At independence, that part of the economy not based on aid was basically raw materials exports. Copra (dried coconut) comprised 80% of these exports, with tuna fish, cattle, and small amounts of coffee, cocoa, and timber comprising the rest. In addition, there was a small tourist industry mostly based in the capital city of Port Vila.

During the previous decade many plantation owners, for the most part French, had left due to declining world commodity prices. Most of the rest departed at independence, because in the new Constitution all land returned to custom owners-- that is the traditional owners. Although restitution was made, it was a lengthy process.

Generally it became more profitable to move elsewhere, particularly for French colonists who received resettlement sums from their metropolitan government.

Case Summary

Meanwhile, several communities had already indicated their desire to run collectively these abandoned, decaying, and often stripped plantations. Government agriculture officers knew they had to encourage diversification or their dependence on one crop--copra--for export would leave them vulnerable. The plantation sector was seen as important during the transition to any new forms of agricultural organization and diversification, but was not seen as replacing or usurping individual subsistence gardening. No post-independence agricultural documents which I reviewed called directly for the removal of subsistence farming.

At independence there already existed some communities such as one on the island of Pentecost where during the mid 1970's villagers cut approximately \$70,000 U.S. worth of copra, purchased the plantation, but couldn't make a success of it. The local people kept firing the managers, thinking that when large profits were not achieved that the manager must have had his hand in the cash box. Eventually, people realized that they needed training and information. At a pre-independence meeting held with agriculture officials, agents, and some of these community representatives it was made clear that Ni-Vanuatu wanted to run plantations as groups, wanted training, and would need some outside financing to do so. It was then planned that any outside training of trainers would be done with the assistance of Jean Delion (an action research facilitator from New Caledonia who had already worked in Vanuatu) and Charles Rogers (a former government extension agent and Vanuatu citizen), both of whom were explicitly committed to local control and experiential learning and training from the outset.

Government at that time was looking to diversify an aid basket previously completely French and British and through their Planning Office engaged a small, private United States agency as donor for initial sums to construct a training center and portions of first years' operating costs.¹⁶ Over several years, production by the students was expected increasingly to cover operating costs. It eventuated that a later association formed by the first graduating group of students also contributed to the center's costs by sponsoring later students.

In preparation for the set-up of the center, Rogers and others associated with the initial planning visited and studied other Pacific agricultural training projects. Most Pacific Island agriculture training is through government supported extension work which is typified by the problems with cooperatives. To the uninformed, cooperative organization appears to be a wise approach given the collective and consensus aspects of Melanesian culture. To the organizers of PTC many cooperatives and the structures within which they are formed are disappointing. Often, the cooperative approach has been institutionalized within government bureaucracies. The result is local cooperative managers keeping records without understanding why. The local manager waits for the government inspector to pass by and correct their records, but no dialogue as to what long term planning should be done is engaged. Meanwhile, cooperative members in the community often become disillusioned when the small returns on their shares fail to make them "rich like expatriate or capital city store owners."

In the Papua New Guinea case, Melanesian managers are seen on large coffee and tea plantations, but are usually dependent upon the weekly visit of an expatriate advisor for long range planning. Quite a few of these managers have received training in Australia, but often this training is inappropriate to the level of technology available and unaffordable on locally owned and operated plantations.¹⁷

The key to the PTC project and training activities was the respect given to local knowledge and emphasis on trainees' abilities to work things out for themselves. Over and over again, Delion and others in the program said, "Don't give the people a prefabricated answer; be patient; help them create the situation where they can derive answers from their own experience." The visual metaphor that was often used was that of people on land in a bay trying to get out in boats through breaks in a dangerous reef. Rather than tell people which break you--the facilitator--think is the best, be the lighthouse and let them see as much of the reef as possible and work out their own decision. If the analysis process is engaged, the facilitator should be less and less necessary. In fact, the facilitator should endeavor to make him or herself obsolete as soon as possible.

Prior to the start of the first PTC courses, extensive preparation was undertaken. Local advisors visited each community and after thorough discussion of the qualifications necessary, communities selected manager couples and agreed to pay a portion of their training. This sponsorship became an essential element towards local control and accountability when the managers returned home and worked for the community.

Next, the local trainer/teachers participated in training of trainers with Delion. The main thrust of the training was towards building the trainers' trust in experiential learning and ability to do curriculum planning. This author participated in much of the training assisting Delion. We saw the training process as leading towards "thinking it through" or towards a future action research process. Again, when it came to management and planning skills, the goal was to get the trainers and eventually the future trainees (PTC students) to work things out from their own information gathering processes. However, you can't just introduce the action research process as "let's trust our students to do the right thing." To teach this process in itself requires a

participatory, experiential process. One trainer said "the first three days I was angry because Delion kept asking me what I thought, and I thought he should give me my training, my rules and information, but then I realized what he was doing; he expected me to be able to work it out for myself; he respected me."

After many rounds of discussion the following were the key elements undertaken which supported or lead towards eventual local control.

1. The Center itself was constructed on land provided by an already working plantation that supported a Catholic church secondary school.
2. The manager of the plantation, Charles Rogers, became the manager of the Center as well.
3. All techniques taught at the Center were those already tried and tested first on the working plantation.
4. All tools used were those that were obtainable or readily obtainable on the various islands.
5. All long term teacher/trainers were to be Ni-Vanuatu.
6. The courses were commenced running ten months each with time to return to one's home plantation and work out a long term plan with the community and committees and then return to the Center for working through the details. This was a key part of the information gathering, action and reflection cycle.
7. If at all possible, married couples were recruited with women participating in some courses (the ones that they themselves deemed useful), group planning, and having access to their own individual and community garden plots.

During the first course, the local trainers and manager gained great experience and were able to work out a few additional steps--seemingly minor at first, but which had an excellent impact later. The first was to hold a community, center-wide meeting every Friday afternoon. At this session, tasks and plans for the following week were

discussed and responsibilities taken up by different group members. During the first weeks, the trainees were dumb-founded; they kept waiting for the manager to give them the answers and be the boss. The manager was very clear about local control and responsibility and was able to engage people in helping to run their own program. This included the women, albeit sporadically (some were too shy or too tired to come, especially at first) and was a novel but eventually welcome innovation. A key component of the Friday meetings was presenting adequate information for the community to make decisions.

The second, at first seemingly minor innovation involved the final examinations. The examination was the formal submission of each manager's real life program and budget which they had worked out in action research cycles with their own communities and polished with the center director and trainers. The formal submission was to a volunteering plantation manager from one of the government centers, joint ventures manager, and/or even the veterinarian. Thus, even the final examination became an opportunity for on-going learning. It also engaged the attention of possible future resource people for the Center as well as the new managers' home plantations.

As mentioned previously, the first graduating group planned and formed a national association, The Plantation Support Association (PSA) which would raise fees from tonnage copra sold in order to provide advisory and extension services for locally managed plantations. These services were in addition to the very specific technical services already provided by government and were especially directed towards management, marketing, planning, budgeting, and community/committee relations.

PSA advisors did not pass by and tell local managers what to do. They worked hard to get managers to work things through on their own and in regular meetings with their committees. There are continuing strong linkages between the Center and the community held plantations represented by the Support Association with the Center's

curriculum constantly changing in response to the real life, everyday problems and feedback of the locally managed plantations.

The PSA support staff meets regularly with the PTC director and staff to reflect on problems farmers are having. The PSA staff keep records on the progress of each plantation and work on the basis of this information with their communities and with the PTC staff. None of these people have more than a high-school education. When this project was started senior expatriate advisors in government didn't think it could be done with local people. Even some local people complained about the inefficiencies of local support staff, but because support staff were directly answerable to the farm managers they served rather than central government officers, they became more and more knowledgeable and useful.

Case Analysis

As of this writing, Spring 1989, both the Center and Support Association are still in operation and are less and less dependent upon outside and government financial assistance.¹⁸ The elements that made this program unique are listed below:

1. The Ni-Vanuatu had already decided on main goals, objectives, and problem definition and their government was supportive as this effort did not duplicate any official efforts but rather was perceived as complementary.
2. Preliminary planning was in depth, included local people, and surveys of similar successful and unsuccessful work.
3. The key facilitators and trainers, Rogers and Delion were absolutely committed to local control, experiential learning and in the case of Delion and myself, we were explicit about action research.
4. The Ni-Vanuatu willingness to take "proprietorship" of the project by paying their trainees' fees.

5. Experiential learning and training feedback, really building on the trainees' and managers' own knowledge and self-confidence. Reflection and practice are not artificially separated.

6. Opportunities to break from traditional training practice were available: 1) because of the outside facilitators' long experience in Melanesian culture and aforementioned commitment to participation and action research, and 2) lack of new regulations, etc. on the part of the newly government (however, this was also a limitation). When the PTC and PSA started, government regulations concerning legal entities such as non-profit agencies, corporations, joint-ventures, and sole proprietorships were unclear. There were no filing or tax requirements in this regard, which was convenient. On the other hand, farm managers and their communities were uncomfortable, at first, signing or making agreements about how their communities would work together with the PSA, because there were no assurances that the government would not suddenly put onerous obligations on them.

7. In addition, Rogers had an extremely practical attitude about the trainees' own food during their course; while they weren't expected to start from nothing, they were expected to grow as much of their own food as possible. The status of packaged, imported food was demystified as much as possible. This last should not be underestimated in its contribution to recovering and revalidating local knowledge about food in a society where the majority still rely on subsistence agriculture and the status of packaged imported foods act to undermine that local knowledge.

The limitations of the efforts occurred in two major areas. The first was the lack of trained agricultural personnel, i.e. Ni-Vanuatu people with technical, management, and training skills. Often despite their best efforts the personnel fell short, but repeatedly this was offset by the all-around learning and confidence built by working

with local people. Local people often know the why's and wherefores of their fellows far better than any highly skilled outsider.

The second area was the limitations created by obligations to an outside donor. Most of the donor agency's funds for the programs were raised from the United States Agency for International Development (US AID).¹⁹ Whereas grants were made on a two year basis, many of the Center's tree crops planted in the first several years were not expected to bear or generate full self-sufficiency until five to ten years. As in many projects, as soon as outside funding was solicited, managers and facilitators found themselves drained of energy as they drew up documentation in the vocabulary suited to AID. Within a three year span there were several visits from both AID and the donor agency staff, all planned, without real concern or consultation with Center staff, at times extremely inconvenient to the on-going work of the Center and reduplicating one another. Evaluations tended to be limited to noting whether or not the Center's building construction was adequate and budget spent on time, rather than the quality and appropriateness of the training and degree of local analysis and self-sufficiency emerging.

Earlier the difference between ethical community development practice and participatory research was mentioned. This case does manifest aspects of both. If we characterize a positivist developmental effort as looking at problems only as technical issues capable of solution and forbidding local people to say "no" to some practices, we can see that the PTC effort went beyond that. PTC went beyond by refusing to hire outsider staff and sticking with bumbling but learning local staff. PTC went beyond by refusing Asia Development Bank aid offers, because they were too large and would have perverted the goals of self-sufficiency and local control. But was the PTC effort still the ethical inclusion of local people in an overall development effort? This is not so easy to sort out. Although the government certainly had developmental goals for production

and export that would guarantee its survival, the local people on many different islands had called for the project and training in order to run plantations collectively.

The research, or information gathering, analysis, and reflection aspects of the PTC are important, especially as they were carried out by the Ni-Vanuatu, locally educated staff and trainers. This is particularly important in the case of Vanuatu where just after independence I went around to various government departments and found I was still dealing with French, British, and Australian expatriates; the Ni-Vanuatu were only in the political posts--typing secretaries, or political secretaries of the ministries!

Again, specific action research--as I then named our practice--components occurred with the following situations:

1. The collection of information about failed and successful farm training projects was shared and reflected upon by the center director and staff.
2. This in turn was shared with local plantation communities in the initial stages where people also discussed and organized how they would determine who would be the managers and how they would be sponsored.
3. In fact, each new stage in the PTC and PSA involved staff touring around the communities through several cycles where people had time to discuss and reflect on what they wanted to do.
4. More traditional information gathering occurred when PTC trainees returned to their communities and together with community members made surveys of their situations, i.e. how many cattle of what kinds, how many coconut trees of what age, location and duration of water supplies, etc. The importance of this information gathering should not be underestimated in communities where many of the adults cannot count above twenty.
5. In the PSA sharing of such information started to include whole islands, market shares, and had and has the potential to influence inter-island shipping and

supplying in a large way. Although astute politicians later created a Vanuatu Agriculture Chamber of Commerce, the PSA in a very careful and quiet way had already started to act in that way on behalf of rural people on the outer islands. Sometimes these activities were very different than those proposed by central government officials. In the PSA a local knowledge base serves the identity and rights claims of rural farmers, sometimes against claims of central government officials and/or transnational corporations and organizations seeking to develop the land.

Here one might interject that increasing local people's capacity to work and contribute to the world-economy will most certainly act to homogenize them into the global culture of consumption. Local culture will sooner or later disappear. On the other hand, where people are adapting local skills and using traditional forms of consensus to make management work, they are retaining their culture. Where local people are running projects, farms, and plantations a vacuum is filled. Australian and other joint venture projects, large agriculture research stations, and the like are less likely to seek entrance to those areas of islands that already have something going.

In the long term, much of the work done through the Center could be negated by the world-economy. Large decreases in the price of copra and other commodities on the world market could possibly cause the plantations to fold. Since the individual families are for the most part self-sufficient, it might not be worthwhile for them to continue. However, in the past several years the world prices have indeed been low and the work continued.

Again, I return to Foucault and push the analysis. Local farmers were made objects of inquiry by both traditional research practices and development surveys done by outside experts. Local farmers were subject to dividing practices that made them poor, untrained, and unorganized. Until the times surrounding independence, most people by joining Christian churches, where missionaries had forbidden traditional

practices, had made themselves to some extent objects. As local knowledge, "kastom," was revalorized by a variety of activities including those of the Vanuatu Culture Centre, Ni-Vanuatu sought activities that countered colonial and post-colonial controlling practices. This is manifested in the PTC and PSA in many of the ways that Foucault suggests.

The PTC and PSA became increasingly transversal as people on very different islands agreed to work together (Vanuatu has 110 separate languages distributed among a population of 120,000). The PTC and PSA farmers and their communities were seeking the power effects of land tenure most clearly manifested by their capacity to produce on that land. Their efforts asserted their status as farmers, Ni-Vanuatu, rural people. By taking on plantation management as communities they sought to oppose the capitalist forces that would have made some of them much richer than others. By agreeing to keep their management and trainers local (within the PTC) they opposed privileges of knowledge. They did their own surveys.

Like the Appalachian case, the PTC efforts differ from what Foucault suggests in an important way. Foucault uses the language of traditional power when he writes of enemies in anti-authority struggles. Where the enemy has been the colonial white man, the current effort is more positive and seeks to build their own Ni-Vanuatu identity. Furthermore, there is a definite sense that PTC and PSA members are already engaged in their own solution.

IV

The final case is that of the Vanuatu Culture Centre's Fieldworker Training Project.²⁰

Additional Case Background

Prior to independence, there was extensive, nation-wide discussion of what comprised "good" and "bad" "custom." Custom or in the Pidgin of Vanuatu known as Bislama, "kastom," may be described as anything that makes up Vanuatu's culture. During the days of the missionaries "kastom" was often seen as traditional dancing, singing, story-telling, kava drinking, healing, and other magical practices. For the most part, the missionaries simply banned kastom in general.

In preparing for independence, now Prime Minister, Anglican priest Walter Lini and others toured the islands discussing the notion of politics and "good" kastom. There is no doubt that these discussions raised consciousness and pride in traditional culture and identity--that which was Ni-Vanuatu rather than European.

Even during the early 1980's, one might have talked with a village elder and heard that kastom was "bad" or perhaps one would have received silence. If one had asked about the history of the village one might have learned that the history started with the arrival of a missionary. On probing further, one might have heard that before the missionary was "taem blong daknis" or the time of darkness.²¹

This apparent cultural ethnocide caused by conversion to the Christian religion was never complete. Increasingly during the 1980's this discourse was replaced by one where the elders might reply "kastom hem i no ded, hem i slip no moa," or "custom is not dead, it is just sleeping." The fieldworker training efforts appear to have had an important relation to independence and subsequent activities where kastom appears fully awakened and recovered. Ni-Vanuatu have reclaimed their proprietorship of their traditional and local knowledges.

Case Summary

This project of renewing Vanuatu's many cultures does follow the main paths of participatory research. First, the people there had moved into post-colonial

independence proclaiming a return to "good custom." Thus, the project was embraced by the local populations. Second, the local populations were involved at all levels in the information gathering and that has probably included some decisions not to record sacred and tabu rites. All this has been achieved with no interference from disrespectful outsiders. Third, analysis of materials that lead to local cultural festivals and strengthening of identity in the face of enormous post-colonial pressures has been spontaneous and locally generated. Fourth, the facilitator and Culture Centre eventually worked and trained himself out of his job, and the efforts continued.

The Vanuatu Culture Centre fieldworker training effort was inaugurated by an Oxford and Cambridge trained ethnologist, Kirk Huffman, who had already spent many years working with traditional groups in various parts of the country when he became head of the national Culture Centre in 1980. From the beginning, he saw himself as facilitating the preservation, renewal, and sharing of culture for Melanesians rather than for outside scholars, European museums, or his own professional advancement.

The Culture Centre in the capital city of Port Vila has sported a large screen video room since 1982, and here every Saturday and upon request, videos of Vanuatu's various cultural events are screened. Often there are European tourists present along with Ni-Vanuatu. What is unusual is that even scruffy young boys from the most remote village feel comfortable attending the screenings where all the explanations are given at length in the lingua franca, Bislama, rather than English for the many tourists, although there is a brief English explanation.

This is unusual for many Pacific Island museums where so much is oriented towards tourists and scholars from thousands of miles away rather than the supposedly disinterested youth and supposedly simple villagers. The Vanuatu Culture Centre is perceived as being a special place for Man-pies, a Bislama term meaning local people and implying those rooted deeply in the land.

The Culture Centre's efforts have gone far beyond this. Realizing that there would never be enough anthropologists both adequately trained and accepted by the traditional groups they would like to work with, the director with Ni-Vanuatu staff decided to train chiefs and other well-respected community leaders to be their own fieldworkers.

This decision had many advantages. The Ni-Vanuatu fieldworkers knew their own culture best; they could decide which events would be the most important to record, and they knew how to do so without violating traditional mores and trust. They were on the spot and would not miss an event nor important details an outsider-no matter how well trained-might overlook. They would know how each event interconnected with others.

The director arranged training sessions for each new fieldworker and set up the fieldworkers with sturdy, serviceable cameras, tape recorders, and later in some cases video recorders. Some things were subsequently recorded that may only be shown with the permission of the village or group involved, but once permission is granted many groups have been quite proud to show others their kastom.

Summary Analysis

As of this writing, over twenty male fieldworkers have been trained, and a Ni-Vanuatu woman is now receiving education in Australia in order to organize training of women fieldworkers.

Most interesting to date is the organization of island and regional cultural festivals throughout the islands. Many of these festivals entirely exclude outsiders; tourism generated cash is not the incentive for the activity. In the last several years, it seems that a great deal of pride has come to be attached to the resurrection of nearly forgotten dances and songs. For example, in May of 1988 one elder was preparing for a festival that would require killing 500 pigs. In that region, this type of event had long

been discouraged by Christian missionaries, and this particular event was last performed in 1920. Another facet of the cultural festivals is pride in the return to traditional dress. Those dancers who appear in the partial Western dress promoted by the missionaries now find themselves chided for their "lack of custom."

Originally, little of this would have happened without the dedication of the Culture Centre director, but part of his commitment was to always return, always to direct efforts towards restitution of Ni-Vanuatu culture in such a way that Ni-Vanuatu would make all the decisions about what to do with the information. Evidence of the director's clear-cut vision was his insistence on taking only a local salary-not so easy in Port Vila which is still in many ways dominated by hundreds of expatriates working at enormous salaries for such institutions as the Asian Development Bank. The director was also strongly supported in much of this work by his wife Claudia Huffman, a Colombian. The point of taking a local salary was not to wear a hair shirt, but to make it that much easier for a Ni-Vanuatu to take over the post and set realistic precedents. Total localization of the post is currently set for August 1989.

What is of even more interest is that this project involved the three prongs of participatory research--education, research, and most importantly action on local social problems, but for Ni-Vanuatu the local social problem in these cases was erosion of custom, self-respect, and identity. These actions did not lead towards economically based production increases. They have already led to preservation of traditional knowledge as well as new skills in organizing this work and subsequent large, regional culture festivals.

Additionally, in years to come the efforts may strengthen local cultural and political identity to the extent that there is some sort of organized action that buffers complete articulation with the world-economy and neocolonial dependency. One of these may be manifested in the pressure by this group and the National Council of

Chiefs on the government which has resulted in the refusal to date to allow the licensing of a television station. This license could be potentially quite lucrative for the government and certainly with just one station a wonderful mouthpiece for government policies. Nevertheless, the chiefs and cultural workers felt that the introduction of television at this time would be too abrupt a change. These resistances may be temporary and undone by other influences, but they have happened.

Here again, Foucault's approach to studying local resistance to authority and privileged knowledge is useful. Prior to the work of the Culture Centre, traditional modes of anthropological inquiry had left the rural Ni-Vanuatu's local knowledge as quaint, superstitious, or just plain "uncivilized." Under the missionaries and colonizing officials Ni-Vanuatu were the poor, rural, uneducated, Black, or uncivilized. Colonized and Christianized Ni-Vanuatu oftentimes saw themselves as inferior and without the almost magical powers of the colonizers' technology.

Recording and celebrating their own culture, while an anti-authority struggle in many aspects, was also a celebration of renewed identity, of joy and carnival, for the Ni-Vanuatu.

As in the other cases, there were transversal elements particularly in the organization of culture festivals. People from isolated tribal groups who engaged in warfare just a decade ago have come together to organize regional festivals. The power effects are those of traditional culture--arts, music, dance, story-telling, kava drinking ceremonies, etc.--overcoming the colonizing effects of missionaries and central government officials bent on modernizing.

The culture festival activities have been especially powerful in linking young Ni-Vanuatu back to the knowledge and traditions of their elders. For some of the ceremonies, younger people have gone off into the bush for up to six months in order to learn secret songs and dances from the elders.

As in the Appalachian and PTC/PSA case, the struggle has gone beyond the immediate stage. I strongly suspect that additional meetings, interviews, or visits with the Culture Centre's local researchers would reveal beginnings of more overtly political activities among the village and island groups who organized culture festivals.

V

The preceding cases all represent important components of participatory research. The components in common include:

1. Local control of project or movement management.
2. Fade-out of any outside facilitators or their supersession.
3. Local definition of the problems to be addressed before the admission or invitation requesting the facilitators' assistance, and a good sense of group identity.
4. Local information gathering by the people to be effected by the effort on an on-going basis.
5. Most outside facilitation occurring at and directed towards the stage of information gathering, analysis, and planning and training.
6. Subsequent actions were taken--unpredicted by facilitators--organic to the local people that moved towards local organization for collective action.

The cases here assembled seemingly represent a strange smorgasbord of issues. In Appalachia, it was a post-industrial rural situation in which information gathered by citizen groups was used to generate public pressure on state and federal policy. In the Vanuatu Plantation Training Center and Support Association, post-colonial Ni-Vanuatu were able to engage outside sources to meet their local farm training and financing needs and continue to survive as smallholders in communities. The farmers also formed a support association that has been acting increasingly in political ways. In the Vanuatu Culture Centre, a post colonial recovery of traditional local knowledge and subsequent identity took place with the aid of photographic and video technology and

training. In both Vanuatu cases, there is evidence that both groups are using their respective organizations in ways that are becoming increasingly political. What all these cases have in common is participatory and action research phases from initiation on through.

In each case, the central problem was either already defined by an organized group of local people and/or the facilitator broached the idea based on numerous similar contacts and requests from local people over a period of many years.

In every case, the outside facilitators engaged had already been involved with the local people for a number of years. The facilitators were all involved on a temporary, albeit long basis, and in every case some aspect of the work continued among organized, local groups after the facilitator's departure. A measure of success in the three cases is people's ability to transfer awareness from the original activity to later activities, particularly new organizing.

In every case, the facilitator was committed to a specific agenda including local proprietorship of the effort, local collection of information for locally defined purposes, and experiential training and learning to do so.

In every case, the facilitator helped the local group minimize effects of outside funding (the ARC, French ACCT, U.S. AID, etc.) on the direction of work.

In each case, the facilitator's role decreased proportionally to local people taking charge. The facilitator helped people get on with what they had wanted to do anyway. To the extent that local people gained confidence to make decisions and take action that enhanced their well-being whether cultural, political, or economic that process might be termed empowering. However, it is important to be absolutely clear that no facilitator really empowers local people; people empower themselves. Usually, the facilitator is essentially getting things out of the way--making the space so that local people can

express themselves as they see fit. To the extent that the processes above occurred, participatory research is useful and successful.

If Foucault instructs us to be very cautious about shooting from the hip with grand strategies, he neglects to tell us what to do when organized, political agents come on the scene as they did in Appalachia and Vanuatu. This leads me to reintroduce Habermas to the analysis.

Habermas does not reject all technical knowledge, only the claim that it is the only legitimate type of knowledge. In order to get at other kinds of knowledge barriers to communication must be removed; alienating conditions must be recognized and undone. In the Appalachian case, Habermas applies. The Task Force members were able to cross professional boundaries among themselves and communicate. They were able to gain control of their local knowledge base and work on alienating conditions. To the extent that the State governments and unconverted mainstream researchers and their institutions can step back in and block communication, Foucault is right; these struggles are only immediate. Foucault, however, does not account for long term political organization.

Habermas on the other hand, does imply that there is a way out through some undefined political organization. Habermas seems to suggest that the purpose of critique is a sort of ethical theory of self-realization. This certainly seems to fit with the participatory research cases at hand. What has continually frustrated those participatory researchers who seek theoretical guides is Habermas's work on the praxis aspect of the issues; there isn't any. As Australian action researchers Wilfred Carr and Stephen Kemmis say, there is no guide to "explicate the criteria of rationality in terms of which emancipatory knowledge generated by a social science could be validated or rejected," and there is no guide for "its use in real social action."

In an analysis of participatory research cases, both Foucault and Habermas are useful in understanding the persistence of controlling institutions and the difficulties in achieving participation. Both philosophers could and have been accused of offering no anchors for praxis. In reflecting on this case and the others that follow, it seems they should not be expected to offer neatly packaged guides to action; they have not engaged in the actions and reflections that came out of very specific local conditions. If they were to offer guides to action, they would be reproducing exactly those practices that they both see as part of controlling intervening institutions, world capitalism, or the modernity project. This does not mean that local intellectuals should not engage in political action with local groups. Analyzing language in documents and staying aloof from one's own local conditions is not enough.

Before discussing the role of the outside or university sponsored facilitator further, it is important to separate ethical and political aspects of the role and technological and pedagogical aspects. There have been many excellent works on pedagogical and training techniques especially in the context of community development. Depending on the goals, even the most effective training techniques remain a manipulation. All of us who wish to work in participatory research situations need to constantly remind ourselves of the paradox of our role. An outsider's activities are interventions. The outsider is encouraging some activities while discouraging others. Neither the outsider nor the tools of analysis she introduces into a community are neutral.²²

I suggest that the role of the participatory researcher--whether an insider or an outsider--is first to decide with whom she is really participating. That is one needs to decide where the accountability lies when engaging with a community. This is extraordinarily difficult if one takes the paradox seriously.

Action researcher Jean Delion contends that Freire is "too political." Delion explained that Freire's way of analysis, which Delion sees as coming to understand structures of domination, can lead to blaming too many problems on the far away central government. Although the reality of domination may be ultimately correct in some cases, it can short-circuit worthwhile immediate efforts. In other cases, if a group is too successful in this sort of analysis (Freire's), they may find themselves in violent conflict with the national authorities.²³

On the other hand, as already explained, no matter how hard a facilitator tries there can be no absolutely neutral local analysis. This kind of decontextualized analysis tends to focus only on getting something done rather than the process by which it is done. Under these circumstances it is easy to get impatient and see problems as solvable by technological fixes. Of course, a really good participatory research process does not preclude the use of technology.

Viewing these cases from another angle leads to asking: if people successfully mobilized and a participatory researcher was involved, where was that researcher useful? Where local control became stronger in these cases, the participatory researcher may have contributed to consciousness raising, appropriate skills training that led to self and group confidence, knowledge about how to use institutional resources while avoiding cooptation, and documentation and subsequent legitimation of group efforts.

The Appalachian case is an exemplar of university professors working together with local people to understand how the system--in this case land taxation--worked. The tactics which they used, whether consciously or intuitively, may be derived from a close reading of the case.

As in the other cases, the research facilitator did not start to work with the groups until they had already formed an identity and an idea of what their problems were. There was an invitational element.

The facilitator helps local people to gain space in which to be heard using a variety of tactics. These include: 1) taking plenty of time for groups to reflect on any new information as well as taking time to consult among one another; 2) going together when dealing with authorities; 3) taking decisions together rather than relying on internal experts; 4) the facilitator may use specialized knowledge in skills training with the local group, but then the decisions about what to do with the skills are with the group; and 5) throughout, the facilitator increasingly must avoid speaking for the group.

The type of resistances encountered by the Appalachian Land Use Task Force highlight the need to study up.²⁴ By study up is meant acquiring practical knowledge about how institutions such as the ARC work. This would also include finding sympathetic individual administrators to work with while understanding the pressures that their organization places on them.

Finally, it is crucial that an outside facilitator always engage in local action with the intent to pass all training and facilitation skills to people within the community. The facilitator must work herself out of that role as soon as possible. In effect, the facilitator needs to have infinite patience with group process as the group works things through, but she must be somewhat impatient with herself.

Discussing intervention and participatory researchers Paulo Freire said he could not just stand by:

I agree, but I cannot do that, because I have the duty to intervene in reality. It is the position of Gramsci. He said the role of the intellectual is to intervene but not manipulate. There is a democratic way to intervene. In the night, when

people tell me to be silent I have the duty to speak. Intervention means creating circumstances in which people become more curious.²⁵

NOTES FOR CHAPTER IV

¹Mike Resnicks, "Kirinyaga," Fantasy & Science Fiction 75 (November 1988): 24.

²See both Folke Dubell, ed., Research for the People, Research by the People: Selected Papers from the International Forum on Participatory Research in Ljubljana, Yugoslavia (Linköping Sweden: Linköping University, 1981). See also John Gaventa and Helen Lewis, "Participatory Education and Grassroots Development: Current Experiences in Appalachia U.S.A." paper (New Market, Tennessee: Highlander Center, January 1989).

³The cases are:

1. The Vanuatu Plantation Training Center and Support Association--see International Human Assistance Programs, "Revitalization of Vanuatu Agriculture," a grant document filed with the United States Agency for International Development, Suva, Fiji in May 1985. In March 1985 IHAP closed all operations except for its Bangkok, Thailand office which continued to operate under the initial charter. By this time the Vanuatu Plantation Training Center and Support Association were both 100% locally controlled and nearly self-sufficient. Mention of the success of these projects was confirmed by a personal interview with Ms. Mary O'Sullivan of the Asian Development Bank who surveyed efforts in Vanuatu in 1987. This is also confirmed in the Asian Development Bank/Australian Development Assistance Bureau report, "Vocational Training and the Labour Market in Vanuatu," Joint Technical Assistance Team, 1987. Additional confirmation through personal correspondence and telephone calls, May 1985-current by the author with IHAP and other Vanuatu personnel. 1981-1985 documentation of the projects were all made by or with the collaboration of the author.

2. The Vanuatu Culture Centre Fieldworker Training, 1979-current and on-going. The acting Culture Centre director and facilitator is Kirk Huffman in collaboration with the National Council of Chiefs. Interviews both formal and informal occurred during the years 1981-1985 in Vanuatu. 1985-1989 information from visits to the East-West Center by Vanuatu participants and personal correspondence.

⁴Kirk Huffman, lecture for the Institute of Culture and Communication, East-West Center, Honolulu, Hawaii 4 May 1988.

⁵United Nations Research Institute for Social Development, "Inquiry into Participation: A Research Approach," ed. Selina Cohen, (Geneva: UNRISD Popular Participation Programme, October 1980).

⁶John Gaventa, "Land Ownership in Appalachia, USA: A Citizens' Research Project," in Research for the people, Research by the people, ed. Folke Dubell, (Linköping, Linköping, Sweden: Linköping University, 1981). More recently see John Gaventa and Helen Lewis, "Participatory Education and Grassroots Development: Current Experiences in Appalachia, USA," (New Market, Tennessee: Highlander Research and Education Center, January 1989).

⁷Appalachian Land Use Task Force, Who Owns Appalachia? (Kentucky: University Press of Kentucky, 1983). See also John Gaventa and Bill Horton, "A Citizen's Research Project in Appalachia," Convergence 14 (1981): 30-42.

⁸Not only were we able to discuss Highlander's work, but I was able to directly or in groups discuss Freire's views on research, "scientism" as he terms it (to be discussed in depth in chapter V), and intervention.

⁹See the novel Storming Heaven by Denise Giardina for a fictionalized but historically based account of miners' union organizing in Appalachia in the 1930's.

¹⁰Some important participatory research efforts taken to combat post industrial environmental degradation are documented by Juliet Merrifield, "Putting the Scientists in Their Place: Participatory Research in Environmental and Occupational Health" (New Market, Tennessee: Highlander Research and Education Center, 1989).

¹¹Citations of the Appalachian case include Stephen K. White on Habermas in "Toward a Critical Political Science," in Idioms of Inquiry, ed. Terence Ball, (New York: State University of New York Press, 1987), p. 126. Wherein White suggests that high level theory such as Habermas does not have to be divorced from the world of action: "there can be empirical tests for the proffered interpretations."

¹²John Gaventa, in Folke Dubell, ed., Research for the People, p. 127.

¹³Kentucky Fair Tax Coalition, interview with coalition members visiting the center for a facilitators' training workshop at Highlander Research and Education Center, New Market, Tennessee, December 1987.

¹⁴John Gaventa and Helen Lewis, "Participatory Education," p. 14.

¹⁵Juliet Merrifield, "Putting the Scientists in their Place," p. 18.

¹⁶The donor agency was IHAP (International Human Assistance Programs, Inc.) as previously outlined in end note 2.

¹⁷It is not unusual to encounter situations such as that where a Melanesian who returned from an Australian stock course was unable to round up cattle, because they only used helicopters in Australia and now he had none.

¹⁸Asian Development Bank/Australian Development Assistance Bureau, "Manpower Planning in Vanuatu," and interview with Mary O'Sullivan formerly a consultant on that report as well as personal correspondence.

¹⁹The United States Agency for International Development (US AID) first entered the South Pacific directly in 1978. By act of Congress it was decreed that there would be no bilateral aid; rather all aid was directed to US voluntary or private agencies already working in the Pacific. This opened an immense cornucopia to the agencies who often became too reliant on large Operating Program Grants (OPGs) from AID. During the mid 1980's in the Reagan years, bilateral aid started, but the small agencies had already become over dependent on aid.

²⁰Kirk Huffman and Claudia Huffman, Culture Centre, Port Vila, Vanuatu, from lectures, interviews, and correspondence, 1981-1989.

21Although I experienced "taem blong daknis" comments directly, this phenomena has also been noted publicly in lectures by East-West Center, Institute for Culture and Communications visiting anthropologist, Monty Lindstrom.

22Credit for this insight goes to colleague Margie Robinson, a fellow of the Participatory Development Project in the Resource Systems Institute of the East-West Center, Honolulu, Hawaii 1989.

23Jean Delion in an interview 24 July 1986 at the Resource Systems Institute, East-West Center, Honolulu, Hawaii. Delion speaks from an experience in Senegal with Roland Colin in which a very successful national community development organization was squashed by the national government. It is also important to recall that Freire's first big organizing efforts in Brazil in the 1960's resulted in his imprisonment and the execution of some of his colleagues.

24Gerrit Huizer first suggested this term. Huizer is a participatory researcher working in the Netherlands. His papers in this field include "The A-Social role of social scientists in underdeveloped countries: some ethical considerations."

25Paulo Freire speaking with Myles Horton and a combined graduate class of John Gaventa's sociology students and Jon Peter's education students at the University of Tennessee at Knoxville, 3 December 1987.

CHAPTER V

INSTITUTIONALIZATION OF PARTICIPATORY RESEARCH: OBJECTIVE OR LIMITATION?

The census has a scientific purpose. The census is a sociological investigation. But the aim of sociology is men's happiness.

(Count Lev N. Tolstoy in What Shall We Do Then, On the Moscow Census & Collected Articles).¹

I

This chapter discusses problems of and related to institutionalization of participatory research from several perspectives. Drawing on the cases cited in the previous chapter as well as other attempts at participatory research which have been preempted at various stages, this chapter addresses several issues and questions. Why is participatory research work such as that in the Appalachian case attempted infrequently, and what are the institutional limitations that prevent it from being more regularly practiced?

In order to address these questions, the First World institutional context of Third World research relations is reviewed:

1. How social science is constituted as a field

and how this relates to bureaucratic systems within the capitalist world-economy. This is expanded in relation to the Melanesian state and research in the next chapter.

2. How the above plays out within research institutions and how it effects

researchers and researched populations.

In the course of producing this dissertation, hundreds of participatory cases have been reviewed. The bulk of the documentation has been located among and through the various North American members of the Participatory Research Network.² Other sources provide scant if any documentation. Although World Bank officers have addressed participatory research issues, a well-known paper discussing participatory

research appears with a cover page disclaimer to the effect that the paper is not part of the series of World Bank policy papers.³ In the great majority of American colleges and universities, a random survey of both libraries and syllabi of social science research methods courses does not list or provide materials specifically on participatory research. Despite hundreds of seemingly successful participatory research actions, a major United Nations project aimed at defining the approach, and World Bank papers, participatory research as defined in previous chapters is not a mainstream approach; it is not well institutionalized outside of the Participatory Research Network⁴. In short, if participatory research is a good means of addressing social problems, why is it not more widely known, practiced, and taught?

II

In workshops and seminars where participatory research is presented as marginalized or local people gaining control, one often encounters the comment that participatory research would be difficult to undertake in an authoritarian state. It is also difficult to undertake in other settings. An example from the State of Hawaii is illustrative.⁵

One low-income native Hawaiian community found themselves frequently being used as a research resource. This led to the community asking, "Who benefits when we cooperate with researchers?" The community board formed a sub-committee to develop some criteria for research done on their own people. By posing the question as, "How do we as native Hawaiians benefit from research?" they developed three key criteria for deciding whether or not to endorse research. The criteria were:

1. There have to be clear benefits to the community.
2. If the study is about native Hawaiians then half or more of the researchers should be native Hawaiians.

3. The community should have a right of review and participants should be consulted about findings. This last item later caused the Hawaiians to be accused of censoring.

On hearing that federal and state funds would be available for improving the lot of Hawaiians, a group of Hawaiian women made preparations to apply for funds. The women, based on previous community discussions and already available statistics had a commitment to do this particular research. The women also decided that they wanted to involve the community as much as possible and clearly stated that their research approach would be participatory research.

In the course of preparing funding applications, a non-Hawaiian faculty member in the state university learned of the women's group's application. The university individual undertook to apply directly to the funding agency--without first consulting the Hawaiian objects of the study--to conduct the research and to administer the funds. Of course, those who conduct research write direct and indirect overhead costs into their research proposals. In this case, the funds involved were to be once again diverted away from the intended Hawaiian beneficiaries. Yet even more concern provoking was the reply of one university researcher when queried by funding officials as to whether he/she would be willing to work with any of the Hawaiian research facilitators. The university researcher said that not only were the Hawaiians academically unqualified, but that Hawaiians on the whole were too lazy to do a good job.

Usually the racism underlying cooptation of people's initiatives is less blatant than this Hawai'ian case. Often, that which is involved is not only racism for or against a specific race, but what Paulo Freire has called "scientism."⁶ The process by which social science, as presently constituted, invalidates local knowledge is briefly identified in the first chapter. It is to this that Freire refers. How does scientism operate in

relation to participatory research efforts? What is the interlocking complex of theories, assumptions, beliefs, and practices that leads to scientism?

Orlando Fals Borda has put it most succinctly:

Science is constructed by applying rules, methods and techniques subject to a certain type of rationality conventionally accepted by a minority community constituted of persons called scientists, who, being human, are subject to motivations, interests, beliefs and superstitions, emotion and interpretations. Consequently, scientific knowledge does not possess any absolute value because it varies according to the objective interests of the classes involved in the accumulation and the systematization of knowledge, that is, in its production.⁷

Fals Borda, himself a participatory researcher, applies two of the main Latin American participatory research critiques of mainstream research. The first element of the Fals Borda critique like that of Freire's "scientism" is that anything not expressed in scientific language as generally accepted becomes treated as unscientific, not rigorous, perhaps even backward, superstitious, or worst of all, simply invisible. The second, closely linked critique is that mainstream scientific practice further institutionalizes undesirable class relations between the bourgeois educated and the uneducated peasants and favela (slum) dwellers.

Throughout the proceeding discussions a working definition of applied social science's purpose (of which participatory research is an off-shoot) is adopted. This definition is alluded to in the opening epigraph from Tolstoy and by philosophers of the social sciences such as Peter Manicas:

[A]ppplied social science, whose ostensible task is to use knowledge to solve some of life's social and individual problems.⁸

In usual social science, definition of its global purpose is almost completely absent. This is not a fluke; it is an indication of a fundamental problem in the conceptualization of the fields of social science. A brief survey of some standard texts in social science further indicates where the conceptual problem lies.

Johan Galtung's 1967 text, Theory and Methods of Social Research, states:

The present work is an effort to stay in the middle of the road, discussing the properties of social data with a view to weaknesses and strengths of various approaches in data collection, data-processing, data analysis, and theory formation.⁹

The preface is followed by 534 pages discussing data matrices, curve shape analysis, covariation, and a page and a half (489-490) discussing the role of the scholar in relation to society.

Galtung provides no definition of applied social science, neither does he discuss the purpose of applied social science. On the basis of his text, one would conclude that the task of the social scientist is to accumulate data. It is at this point that one can see clearly the connection between traditional social research practice and the complaint of research mining in those Melanesian countries now trying to control research access. Although Galtung is no longer writing in this manner and in fact is an enemy of applied social science when it is used for violent purposes, his text is typical of social science production until recent years. Galtung's development is useful in tracing how applied social science has been conceptualized and why undesirable practices still persist. Aside from the fact that one's earlier writings are still available, the central problem is that one simply cannot remove himself from the structures which he critiques.

Many would comment here that social research is changing and of course, the current debates over what constitutes a valid field of inquiry have been briefly outlined in the first chapter. Nevertheless, it is essential to remember that the debates are between a dominant, majority approach, and a variety of other, by no means mainstream approaches. And, as we've seen in the second chapter, participatory research can be most conservative when on the brink of cooptation.

More directly, the problem is the place of science in social science. This notion of science in social science becomes more evident with an examination of university history at the end of the nineteenth and the beginning of the twentieth century.

A student at Cambridge or Harvard during the nineteenth century would have read Machiavelli or Thucydides. One might have discussed the goal of knowledge as that of understanding the factors that caused wars, instability of the state, and survival of the state, but one rarely did anything with this knowledge let alone measure factors that contributed to the conditions of wars. By the end of the nineteenth century however, the notion of the scientific study began to emerge.¹⁰ Lenin, discussing Frederick Taylor's principles of scientific management, asserted:

[L]ike all capitalist progress [Taylorism] is a combination of the refined brutality of bourgeois exploitation and a number of the greatest scientific achievements in the field of analysing mechanical motions during work, the elimination of superfluous and awkward motions, the elaboration of correct methods of work, the introduction of the best system of accounting and control, etc. The Soviet Republic must at all costs adopt all that is valuable in the achievements of science and technology in this field. The possibility of building socialism depends exactly upon our success in combining the Soviet power and the Soviet organization of administration with the up-to-date achievements of capitalism.¹¹

Several themes are introduced here: the notion of social science and how social science is done; the practice of science--technology--in capitalist and Marxist systems; and the administration of society supposedly using the knowledge imparted by applied social science. During this century, universities--particularly those which were state supported--increasingly came to produce administrators who could apply Taylor's principles. The universities also came to develop more tools for carrying out the above administration. Indeed, more and more the role of the university became seen as producing scientists.

One of the classic textbooks of sociology is Robert King Merton's Social Theory and Social Structure.¹² Merton, like the early Galtung, does not define applied social science nor its global purpose. In his introduction he states that his main task is the formulation of:

special theories adequate to limited ranges of social data, and through the evolution of a more general conceptual scheme adequate to consolidate groups

of special theories ...paradigms make for the codification of methods of qualitative analysis in a manner approximating the logical, if not the empirical rigor of quantitative analysis.¹³

Merton, like many others both before and after, is using natural science as an authority referent. The central question of these mainstream social science researchers becomes how best to accumulate data. The essential practice, at the worst, then becomes finding data that fits with already existing theories. In the best case, the result of this practice is that often the researcher does not look at other, larger, substantive questions.

For example, even if a researcher seeks the causes of poverty, her study will tend to look only to the local level for fundamental causes although the study itself will usually originate from a central institution rather than the local level itself. This is the reverse of the situation in the Appalachian participatory research case in which the study originated at the local level and located at least some of the deeper causes of Appalachian poverty in the larger structures of trans-national corporations and the federal government.

Merton's approach to applied social science is also possible because of assumptions about separation of subject and object. If all research is predicated on an ethic that subject and object should be separated, then it will never occur to the researchers (1) to involve local people in research that concerns them, nor (2) to view their [the researchers'] own sponsoring institutions and structures as elements of the problem.

Thomas Kuhn's The Structure of Scientific Revolutions is one of the best known American examinations of conceptualizations of scientific practice.¹⁴ In his preface, Kuhn explains that his task is to examine the way in which new scientific theories are adopted by men committed to older and incompatible theories. In the preface, Kuhn also mentions that when starting his examination he was struck by "the number and

extent of the overt disagreements between social scientists about the nature of legitimate scientific problems and methods."¹⁵ Although Kuhn has little to say about social science practice per se, his work has opened space for critique of social science practice and is much read by social scientists. In this context, two of Kuhn's most powerful contentions are:

[M]yths can be produced by the same sorts of methods and held for the same sorts of reasons that now lead to scientific knowledge.¹⁶

And:

Competition between segments of the scientific community is the only historical process that ever actually results in the rejection of one previously accepted theory or in the adoption of another.¹⁷

The obvious implications for practitioners of social science are that at least some of what is thought to be practice of scientific method is cultural construct and that the history of social science practice is written by the winners.

More recently, mathematician, Evelyn Fox Keller has demonstrated that traditional scientific practice has counterposed objectivity, reason, mind, and male to subjectivity, feeling, nature, and female.¹⁸ Fox Keller points out that the middle-class, white men who produce science "fail to see how such pressures can affect their results, the description of nature."¹⁹ Fox Keller continues to sharpen her critique:

It is through these day-to-day practices that the selection of preferred descriptions and the dismissal of less congenial ones take place; this is where the truly subversive force of ideology makes itself felt.²⁰

In Fox Keller's final chapter, she contends that what counts for scientific knowledge is dependent upon the framing of the terms of the argument and upon the kinds of questions one asks and the explanations that one finds satisfying which in turn depend upon one's relation to the objects of study.

Again, in the face of what is now beginning to be a barrage of critiques, why is it that the standard line of applied social science prevails? In terms of the second theme

introduced--the links between social science and capitalism--Fox Keller suggests "gender ideology as a crucial mediator between the birth of modern science and the economic and political transformations surrounding that birth."²¹ Fox Keller has part of the analysis--the feminine voice--well anchored in her analysis. If one of the main ways the male defines itself in Western society is as that which is not female, that which does not fit the rest of the definition of the Western, technocratic, middle-class, white male will tend to be associated with the characteristics of the female. Thus Third World peasants and their cultures and all the excluded others have often been characterized by the dominant order as soft, emotional, superstitious, and/or irrational.

While there are many differences between the various critiques of social science, authors as diverse as Fox Keller, Peter Manicas, and Martin Carnoy return to the epoch of the Royal Society and its emerging prescriptions for the form of the modern state.²² It is no coincidence that Bacon's Royal Society is bracketed by Hobbes and Locke. Briefly, it is not a grand leap from Bacon's world of truth as the material world to Adam Smith's virtue of the market place. Nor does it take a leap of the imagination to link capitalism, as a belief system with moral duty lodged in the value of work achieved now, to Frederick Taylor's principles of scientific management.

Modern liberal and pluralist conceptions of the state do have a great deal to do with the form that application of social science takes. Martin Carnoy details how pluralism and corporatism lead to particular interpretations of democracy, the state, and the relation between the state and civil society. Carnoy describes pluralism as the official ideology of capitalist democracies which assumes competing interest groups and define the public policy. Carnoy quoting Macpherson explains:

Participation is not a value in itself, nor even an instrumental value for the achievement of a higher, more socially conscious set of human beings. The purpose of democracy is to register the desires of a people as they are, not to contribute to what they might be or might wish to be. Democracy is

simply a market mechanism: the voters are the consumers; the politicians are the entrepreneurs.²³

This description goes at least part way in explaining why some university faculties of social and particularly political science never seem to get beyond counting the vote, rather than examining structural issues. If the pluralist state is conceived as being neutral and its decisions are assumed to define the common good, the brand of social science developed to understand that pluralist "reality" is enframed by its own understanding. If pluralism is the state ideology of capitalism, small wonder that the central objective of applied social science becomes how best to expand productivity.

This has direct consequences in the various Melanesian states. To date, most national planners and development directors are expatriate Europeans and Americans and/or those educated in the American/Western institutions. And it is important to note that it is through these channels--both those that form the planners and those that form the delivery mechanisms of planning--that most Third World research is directed. Particularly in the American case, planning and development approaches are predicated on certain basic assumptions about national and international development that are encultured during university training and subsequent research.

Talcott Parson's work in sociology is both representative and underlies most university background acquired for those working in fields from international relations to development.²⁴ Parsons contends that the major problem for both international and domestic systems is that of maintaining equilibrium in order to manage inner tensions; thus, the formulation of common values which cut across national boundaries is essential to order. Parsons finds that the importance attached to economic development would lend itself to becoming one of these common values.

The two most obvious problems with Parson's formulation are first, economic expansion is assumed to be of universal and unquestionable value, and second, that order and democracy are inherently linked. The gross result of this type of theorizing is

the applied social science of the type that would justify authoritarian social and political measures in order to increase order and thereby economic production.²⁵ The contradiction here is that one suspends some degree of democracy in order to do what might--later--lead to increased democracy. On a gross scale, leaders, planners, and researchers often assume that since their goal is increased production, democracy will eventually take care of itself (if they are interested in democracy at all), and the thought of consulting local level people is never entertained on more than a superficial basis.

A third theme also plays out in Third World development situations. The problematic in question is the role of applied social science in "administration." Sirianni and Fischer in their work Critical Studies in Organization and Bureaucracy link the domain of public administration with notions of scientific management, noting:

In all these theories [of organization], implicitly or explicitly, the function of the manager and the social scientist is to replace "irrationality" with formally rational actions. Moreover, underneath this assumption is the belief that better managers and better social scientists will keep less irrationality from creeping in.²⁶

As Fischer and Sirianni point out, as long as researchers and social scientists continue to focus on issues of technical and administrative efficiency, they are limited. "[R]esearchers have primarily obtained their data and orientation to organizational problems from the administrative frame of reference."²⁷

A compelling set of arguments as to how we are constituted as objects of administration and surveillance is made by Jacques Donzelot.²⁸ Donzelot contends that the family came to be constituted as an effective way of warding off perceived dangers to the liberal state. The reconstitution of the family saved the liberal state from pauperism and pacified the barbarian urban proletariat. Donzelot relates philanthropy and child psychiatry/psychology to intervention in the family:

Philanthropy in this case is not to be understood as a naively apolitical term signifying a private intervention in the sphere of so-called social problems, but must be considered as a deliberately depoliticizing strategy for establishing

public services and facilities at a sensitive point midway between private initiative and the state.²⁹

In asking how the politics of the family became the business of psychiatric agencies, Donzelot surmised that "insertion into the economic sphere in the name of morality" and "the economic administration of individuals in the name of morals" were key.³⁰

Donzelot also relates the production of goods and the production of the producers. Donzelot critiques the central question of mainstream social intervention and applied social science where that question is: how to maintain the moral order, wherein the moral order is implicitly capitalist relations of production and reproduction.

Donzelot's work deals with dividing practices between rich and poor that justify interventions into the lives of the poor. This seems applicable to the relations between First World states donating aid to poor Third World states.

In conclusion, it is not surprising that the central questions posed by mainstream, Western applied social science are not otherwise; specifically, mainstream researchers and their institutions do not question their own practices. Returning to Thomas Kuhn, the aim of normal science (and by implication social science and social research) is not major substantive novelties. Furthermore, "failure to achieve a solution discredits only the scientist and not the theory."³¹

And according to Fals Borda:

Thus, for example, the knowledge of a peasant healer is unacceptable to a medical doctor. And its unacceptability stems from the fact that it ignores and surpasses the medical doctor's institutional schemes whose abstract prescriptions play like chips in a large exploiting game aiming at an accumulation of capital and at enjoying the lucre from his profession.³²

And thus it is that a university administrator can get away with labelling Hawai'ian participatory researchers as "lazy" and "incapable" and participatory research as "not real research."

III

Knowing the arguments of the preceding section, it would seem that the critiques made by participatory researchers should have had more effect by now. Yet even participatory research has rarely achieved the full participation and proprietorship by local people in its various projects. A recent text on ethics, international research, and participation locates the problem as follows:

The essential argument is that social sciences diffuse the accepted images of humanity and society from the First to the Third World. They provide the normative definitions of social reality for the elites and policymakers of the latter. The effectiveness of the social sciences in this diffusion lies in the fact that the normative elements in the social sciences pass as scientific truths. Such normative elements are not dealt with as ideology ...³³

Again, although the above comment seems reasonable in the light of the previous description of social science, little has really changed. Even the authors of the text are currently participating in extremely conservative research practices.³⁴ The previous section has outlined how social science research has come to be constituted as a field and what subjects--those easily quantifiable--are deemed worthy of research, and who is held worthy to receive funding to conduct that research. This section aims to "study up" in the words of Gerrit Huizer, a participatory researcher.³⁵ The purpose of studying up is to see where and how oneself and one's own sponsoring institutions have contributed to and/or caused the problem being researched.

In even the most sincere research efforts to discover the causes and alleviate the vast and complex problems of the Third World and often rural poor, the problem is always constituted as precisely that--a problem of the poor. Rarely, if ever, has the field of examination been constituted as the researcher, her sponsoring institutions, or the practice of scientific research itself. This one-sided constitution of the problems of the poor (or any researched people, whether they are characterized as poor or not) is nearly the only one available to Western, capitalist as well as socialist, and other technocracy

supported researchers. Even participatory researchers rarely think about, let alone attempt, to restructure their own sponsoring institutions.

In the volumes of articles on participatory research surveyed for this project, the authors tend to disappear and their sponsoring institutions are invisible. That is, although the institutions may lend their name and publish the article there is no discussion of how the institution's inner workings have affected the lives of the researched people through their research practice.

At first mention, this proposed practice seems extraordinary. When shared with a colleague practicing ethnography, the colleague responded that most decent researchers are aware--even painfully aware--of their role, so what's the difference? Other colleagues felt that explicit recognition of one's role and institutions would be an unprofessional personalization of their "scientific" work. We hardly ever recognize these influences and roles for at least two reasons:

1. The language for discussing these matters is not available.
2. Researchers intuitively recognize that the personal costs of a confrontation with their own institutions would be too high.

How different, one asks, is this lack of action on the part of researchers from the so-called "apathy" of village people who do not participate in development plans or take up new technology? Often the villagers may lack the language to explain why to the well intentioned facilitator, or the facilitator may not know how to listen. But as Foucault has pointed out local people know quite well what is going on. They know the risks.

What is then asked in this section is not only how research is constituted as a field, but further how do researchers and their sponsoring institutions constitute and reproduce themselves? Later chapters will then ask how these various depictions of

reality effect the socioeconomic and political reality of the researched Third World subjects--often those people labelled poor.

Depending upon one's orientation, the phrase "peripheralized poor" can be read as meaning that the poor are outside ordinary life due to their own practices, or the poor are outside ordinary life due to others' practices towards them. Either way, labelling researched people "marginalized," or "poor" focuses on them and makes the practices of those in the research center invisible except at the level of technique.

To get at this another way, let us turn to the work of participatory anthropologist, Carol Colfer:

In the most general terms it seems that insofar as a low status person interacts regularly with people of higher status, low status people must understand and be able to operate in the system accepted by the high status people.³⁶

Expressed another way, low status people don't usually write and get paid for articles when they finally figure out how the high status people work and make decisions.

Colfer's article is worth summarizing, because it is one of the few that reflects "up"; it summarizes the process, invisible to mainstream participants, by which an intercultural workshop was dominated by white, middle-aged, male, Western researchers. Colfer uses her observations of this experience to illustrate her larger theme, interaction between rural people and those of higher status or from a wider sphere. Here it is equally applicable to the experiences of researched peoples in interaction with outside or outside educated researchers. Domination occurred as follows:

[T]hese men were able to dominate the proceedings so effectively because of 1) the higher status of the scientific paradigm they were representing (manipulations of sophisticated, mathematical models), 2) their higher status as representatives of 'America the Powerful,' and 3) their higher status as middle aged, white males.³⁷

Colfer also includes the facts that they had previously worked together, were native speakers of English, but most of all had the luxury of operating within one cognitive system. Thus, they had no need to recognize any alternative realities. Colfer notes that the only challenges that were issued were from the dominant group, and those challenges were merely technical, rather than epistemological, never questioning the shared, dominant scientific paradigm.

Colfer concludes:

The cohesiveness of the dominant view seemed effectively unassailable, though wrong, completely partial. From the perspective of a member of a muted group, the paradigm that dominated ignored important empirical factors; yet the confidence of the dominant group in the logic and consistency of their model of reality combined with their aforementioned sources of personal power, made effective communication or real dialogue impossible.³⁸

The dominant, scientific paradigm is reproduced throughout all stages of the research process. One may start with the links between proposal writing and individual or project research funding.

Much of participatory and other new paradigm research is dependent upon the researcher/facilitator's careful and delicate coordination with the researched group, often marginalized, and usually already suspicious of any bureaucratic finagling and delays. With this sort of background, what happens when a researcher requests funding for a project without the usual sorts of declared epistemology, not in a single discipline and without neatly deduced hypotheses, is more delays. This is in the more fortunate cases where the researcher is allowed to provide additional information. No amount of discussion of local knowledge and locally derived theories satisfies many panels who require proposals couched in the safe, familiar terms of usual social science. It is far easier to dismiss an unusual proposal as part of the lunatic fringe or radical 60's left-overs and go on to the usual "worthy" scientific proposals, than expend time trying to understand often complex approaches to local interaction and knowledge.

Stephen Fineman, an action researcher of the Centre for the Study of Organizational Change and Development in the U.K. describes the "Kafkaesque atmosphere of bureaucracy ... [where] open expressions of ambiguity, curiosity, and bald intellectual honesty are often quite contrary to the requirements of sponsors."³⁹

By slanting their funding proposals to meet the needs of reviewing committees, researchers avoid being labelled radical and may guarantee funding but will also find themselves in a number of binds. These binds continue even after the completion of the immediate project to affect local research populations long into the future.

The researcher's real intent is watered down or coopted. In a major participatory action research project coordinated by the East-West Center, funding could only be obtained by tying the project objectives to specific technical fields.⁴⁰ It is simply not enough to say that the local people will determine what is to be worked on. From the outset, the project was tied to getting people to produce or acquire objects that would have something to do with producing rural energy. The originators of the project had conceived of talking with villagers at length to find out what they wanted to change and helping them work through an analysis of how to go about it--all very open and participatory. The minute outside funding tied to energy technology was acquired the fundamentals were altered. The problem choice and definition were no longer local, they had already been determined as energy technology by funding sources thousands of miles away. Although local people and research facilitators still went through lengthy and often educational analyses of which energy related problem to solve and how to go about organizing themselves towards obtaining and maintaining particular technological artefacts, the outcome was still defined and controlled by outsiders. The project would not be considered "successful" unless a particular technology was adopted and visible, physical products built and/or installed. Certainly, a project is not considered successful if local people go through an analysis process--no matter how

carefully--and decide to take no action because the costs or even risks are too great for their best interests and survival. This was the case with one village contacted in the rural energy planning project: after collective analysis, the village decided none of their problems were energy related and opted out.⁴¹

Furthermore, the interactions and facilitation with local people may be changed from the outset. William Foote Whyte, based on his action research work with worker-owned companies, readings of cases from the United Kingdom's Tavistock Institute, and Max Elden's work with Scandinavian workers, discusses types of researchers.⁴² In addition, Foote Whyte describes the work of then doctoral student, Timothy Kennedy who while working with Alaskan Eskimos identified three types of change agent roles that outsiders can play: power broker, liberal advocate, and facilitator.⁴³

The facilitator role as conceptualized by Kennedy is essentially that of the participatory researcher described in previous chapters. The liberal advocate, according to Kennedy, tends to take over some degree of leadership of local people in confronting established powers. Not only does this maintain dependency relationships--local people still depend on the advocate, but the advocate closes out opportunity for education of resistant bureaucrats and politicians, and himself burns out. Kennedy describes the burn-out process:

They see themselves as self-sacrificing individuals, serving the common people. This self-conception may support them for a while, particularly if their organizing efforts produce some movement from the establishment, but sooner or later the inherent weaknesses of the role manifest themselves. The "common people" are quite content to follow the liberal advocate when he or she is successful, but take no initiative themselves. If the next project proposed by the liberal advocate fails, support for him or her crumbles. This leads the liberal advocate to become disillusioned and cynical, feeling something like, "After all I have done for them, is this all the reward I get?"⁴⁴

Very few cases reviewed for this project had ideal facilitation despite extremely good intentions and hard work. In the participatory rural energy project cited, a

facilitator in Bhutan who was also a researcher from the sponsoring institution experienced extreme burn-out, but not as a leader/advocate so much as a buffer.⁴⁵

The facilitator first had to deal with the village. In the one year allocated for the project there was barely time to gain a measure of confidence from village people. The facilitator had constantly to be on her guard to behave appropriately in a new culture and language. It was only as the project ended that the facilitator could become a co-learner with the local people. This is typical of most projects, and it should be noted that the facilitator in question actually worked and learned faster than most in equivalent situations. Few projects sponsored by any organizations, i.e. from the United Nations, Ford Foundation, aid agencies, universities, to non-governmental, church, and other agencies allocate more than three years if that much, for initial funding.⁴⁶

The participatory facilitator became a buffer at two levels. First, between the local people and central government officials, and secondly, between both the local people and government officials and her sponsoring and funding institutions. Another facilitator from the same project found himself acting as a buffer when a visiting central government official exclaimed:

These people are too poor to understand. They are like wild animals that run away the first time they hear an automobile horn.⁴⁷

This particular agency manager had been well-trained in First World social science research methods and proceeded to completely ignore the local group's judgement of the problem.

In one of the final workshops of the project, participatory-minded organizers brought together village people, grassroots facilitators, researchers, and government officials. The organizers had to be on their guard and quick-witted (organizing a

separate session for village people to get together on their own where they would not be muted by the experts) to ensure that villagers were actually heard.

Kennedy also points out another disadvantage of falling into the buffer role:

Advocates frequently act as a buffer between community members and decision-makers. In so doing they unwittingly protect officials from being directly accountable to constituents, thus making it easier for them to ignore the consequences of their actions.⁴⁸

The outcome and final document product may be changed. In the case of the participatory rural energy project, although original project documents clearly seemed to state that the objectives of the project were greater participation of local people and a first time opportunity for the sponsoring institution to actually engage in participatory action research, evaluators in one of the funding agencies saw the final objective as a standard, social science document. The final document handed in to the funding agency by the research facilitators entitled "Village Voices" contained hundreds of pages of analysis by villagers and facilitators. Shortly thereafter, a funding agency evaluator telexed that the document was inadequate and needed to be 1) shorter, 2) "more quantitative the way social science is supposed to be," and 3) didn't need so much of the villager's opinions. In this case, such requests did compromise the truth of the participatory project, because the main intent was to represent the participatory process and present the voices of the village people involved.

Throughout the process, from obtaining funds to monitoring and evaluation, the researcher's personal and professional identity may and usually does become threatened. Fineman notes that where increasing tightness of the existing professional boundaries leads to reformulating a proposal:

to try and make it more acceptable can destroy the very essence and spirit of that venture, as well as threaten the identity of the researcher.⁴⁹

Not to mention even more serious, subtle tampering with a project can have consequences that effect the physical and cultural survival of researched people.

This dissertation was produced under the sponsorship of a branch of an organization that normally funds a part of the field research of all Ph.D candidates whose academic committee announces a pass on the candidate's comprehensive examinations and proposal. This candidate/author met those requirements with adequate performance, yet was initially denied field research funds for research into institutional limitations of participatory research itself. Initial comments from the reviewing committee included several to the effect, "Obviously, this candidate is bright, but I do not see how this proposal meets the requirements of scientific research." The proposal was resubmitted with several pages suggesting how locally generated information could contribute to national level policy decisions and allocation of resources, and the proposal was accepted.

Burn-out may be a function of incorrect practice, but it may also be a function of trying to work opposite the grain of power well entrenched in the ideologies of research discourse. Martin Carnoy describes the problem:

Neoclassical economic theory, Parsonian social theory, and empiricist-pluralist political theory are so pervasive in U.S. intellectual circles, particularly in universities, that anyone who does not employ them as the basis for research is required to couch theoretical and empirical work in terms of a response to that dominant formulation.⁵⁰

The production of this dissertation accrued many of the same problems and was subject to the same pressures as any other "scholarly" work generated from field work. How different researchers deal with the anomalies produced by the very definition of research is illustrative.

John M. Johnson, a critic of usual social science field research, notes:

[W]e are ill served by maintaining the illusion that the field notes and other observational records kept by an observer during a field project approximate a mirror of reality. These chapters point out that the data collected during an investigation are affected by how the researcher defines the project, how others in the setting define the research, relations of trust, personal feelings, and so forth ...⁵¹

What Johnson describes essentially parallels comments by Manicas and Fox Keller that much of scientific research practice involves the reorganizing of data until they fit preexisting theory. But there is something else going on here that bears examining.

This kind of science is generated at least in part from the call that it is necessary to have testable hypotheses. In the case of Third World research populations an outside researcher arriving with this neatly preconceived hypothesis and intellectual baggage is not likely to come to mirror any reality other than that of narrowly defined scholarly production.

In the most recent set of instructions for preparation of dissertations issued by the university sponsoring this author, there is an entire appendix on "Recognition by Authors of Contributions to their Work by Students and Others."⁵² Although the university sponsors many hundreds of students who do graduate field work "on" Third World populations, it will surprise no reader that the relations discussed are rarely those of the First Worlder recognizing local Third Worlder input. An occasional acknowledgement of for example, "the many kindnesses of the tribes people of village x" does not go very far towards describing the actual relations of research.

Noting the anomalies between ideals and actual practices, many researchers have come to reflect on the nature of scientific paradigms themselves. This certainly involves reflection on very broad, substantive issues.

In the specific case of this dissertation, I had planned to return to Melanesia to discuss and possibly interact on other levels with local organizers of spin-offs from some of the original action research projects and others discussed in the fourth chapter. Since one does not know the outcome of participatory efforts, one tries not to limit the project by too quickly predicting outcomes. Nevertheless I had hoped to draw

some key Pacific Islanders into discussion of research practice and act as a co-learner with them.

As I continued to prepare the dissertation, I realized that (1) although participatory research engaged in may be discussed within a dissertation, (2) a dissertation under normal university and life circumstances can never be a participatory project in and of itself, and (3) participatory research is almost impossible to initiate properly from the top (outside researcher) down.

Nevertheless, I planned a trip to Fiji, New Caledonia, Vanuatu, and the Solomon Islands to examine the longer term effects of original action research projects and open dialogue about research process with group organizers, local scholars, and leaders. The trip was to have spanned May through September 1987. A few weeks before the trip was to begin, I received a lengthy telex and follow-up calls from grassroots organizers and friends suggesting I cancel the Vanuatu portion of the trip. My friends and colleagues explained that although my previous personal reputation had been good, my very presence, posing the questions I wished to pose would be seen as political agitation by delicately balanced political leaders and that in turn would endanger the work of the action research based groups we had previously worked so hard to build.

While reworking my itinerary, a series of ensuing events caused me to rethink my entire dissertation's *raison d'etre*. First, one of my key interviewee's in Fiji's government was detained by leaders of the May 1987 coup. A leader of that coup had previously been a key panel member of a Pacific oriented research review committee linked to this university. Secondly, actions of the Chirac administration in New Caledonia became extremely hostile towards Kanak so-called "separatists" and their advocates; it was physically dangerous to talk with the people I had known in New Caledonia, and my presence as an outsider might cause them to be perceived as yet more dangerous by the administration.

It was at this point that I more clearly realized that there were several sets of people with whom I could choose to participate. There were the village level and grassroots organization people and small to medium level farmers and entrepreneurs. There were also the administrative and political people in the capital city centers and universities. It seemed more and more that the administrators and politicians had far more in common with equivalent level people here (in the West) in universities than their own countryfolk in villages. It was at this moment that I decided to "study up."

There is one more element of field research and participatory research processes alike that requires discussion. In this overall discussion of what happens to participatory researchers who try to operate within the dominant systems, is the question: what happens to the researched people after the research facilitator departs? Even where a good analysis process has been engaged and confidence to work together as a problem solving group has been developed, there is no guarantee that once the facilitator is gone that the group will not lose momentum.

Ideally, if the participatory research process is truly owned and commanded by the local people, and those who are researched and those who do the research are the same, and that which is useful will continue to be used. The fourth chapter documents some of these cases.

Tony Williamson, a participatory researcher in Newfoundland now linked with the Participatory Research Network, documents many cases where analyses and collective action have continued long after the departure of any outsiders who had acted as initial catalysts.⁵³

Despite video-taped and scholarly documentary evidence that the participatory process works, Williamson encountered the administrator who pronounced, "Well, that's all very well, it may work in practice, but it will never work in theory."⁵⁴ At first glance, merely an amusing anecdote, the above bureaucratic comment is deeply

indicative of the degree to which the administrative, interventionist reading of the world is everywhere ingrained.

This ingraining works in two directions. Most researchers, administrators, university, research, and social service institution staffs operate in a milieu in which researched people are characterized as being without: They lack; possibly they have caused their own problems. On the other side is scientific expertise, supposedly neutral, progressive, dependent upon technologies which in turn are dependent upon strategies of production. It is little wonder that researchers do not question their capacity or even right to pass judgment, nor likewise do administrators question their right to intervene. It is assumed that the researched people do not know and those in the research centers do know.

IV

Given the preceding maps, we turn full on the institutionalizing question. Local people have come to define problems, organized themselves to collect and analyze information, and taken action to change their situations. In the cases under consideration--nearly ideal participatory research--outsiders have been called upon by these groups to facilitate or catalyze the effort or simply provide basic assistance. Upon the withdrawal of the outsiders, the effort continued; consciousness was transferred. Yet when considering institutionalizing participatory research, the structures and systems described in this chapter are everywhere extant. French participatory development practitioner, Roland Colin has termed this situation the "participation paradox."⁵⁵

The opening chapter of this dissertation asks if there are strategies researched people can develop to overcome the colonizing and imperialist tendencies of mainstream research practice. In the Melanesian examples described, closure of research access was one--if only partial and complex-- strategy. Obviously, if a regional, racial, or other

group organizes sufficiently they can gain a partial hegemony over the production of knowledge about themselves, at least within their own locale or nation. Yet until some new paradigms are embedded in our consciousness and practices, these measures of independence are limited and coopted.

Documents from a conference on International Research Equity and Collaboration hosted by the East-West Center in December 1985 demonstrate the problem.⁵⁶ Several (approximately one quarter) of the individual essays presented at the conference were quite critical and seemed to provide openings for going beyond current practice. The final document with resolutions prepared by the conferees was considerably weaker in its language. Resolutions were confined to general calls for collaboration with host country governments and university level researchers. Weakened resolutions may be attributed to all the elements previously described in this chapter as well as the kind of gestalt that was created by the conference hosts. Most people are simply not going to challenge their hosts in a strange, formidable setting. Most research institutions are doing very specific kinds of science, and questioning outside those types of intellectual frameworks is seen as unhelpful at best. This gives lie to the myth that science is neutral and objective.

One field worker from Southeast Asia after working on participation issues for a month in a large research institution finally wrote:

It's funny to see that you (university people) look a lot at participation at the village level and try to give us relevant advice for facilitating participation at this level. But many field workers, like me, have the impression that part of the institutional answers are at your level, in your research institutions. Is it possible, that more research is needed on ways to improve participation at your own level?⁵⁷

Another call for researchers to study their own institutions comes from the one major South Pacific conference to date that dealt with research issues. The conference participants comprised more practice-based Australian researchers and many younger

scholars from the then newly independent South Pacific nations. This was the Young Nations Conference held in New South Wales, Australia in 1978. Again, the conference resolution of interest here is Resolution No. 3:

That foreign researchers be encouraged to study their own social and cultural systems, power structures, and elites, rather than those of South Pacific countries alone [see appendix C for the complete resolutions].⁵⁸

These calls for studying one's own and/or sponsoring institution's relations to the researched project and people are proposed as a strategy of "studying up." Studying up is proposed as a strategy to disrupt established notions of what is proper as a topic of research, to contest what was previously uncontested, to render political what was considered as given and basic, and perhaps to open up opportunities for new practices.

As a corollary, a second strategic practice to studying up is proposed. This second practice is here labelled studying process. By studying process more is meant than merely documenting the scientific method by which an inquiry was conducted. As pointed out by John M. Johnson earlier in this chapter, real research rarely corresponds point for point with the methodology proposed. Not only should these gaps be appraised as part of the process, but also the research facilitator's various relations with local people, herself, and sponsoring institutions become part of the analysis process.

In usual social science research, where the good scientist is expected to maintain the maximum objectivity (distance) from local and researched populations, this proposal would make no sense. Here these relations are seen as inseparable from the rest of research practice and hence should be recognized.

In the case of this dissertation, this strategy is employed to the limited extent possible in efforts to make restitution, choice of language, and right of review. Although under most circumstances a dissertation is not a participatory research project--what local group ever asked a university student to come and write a dissertation for them--it

employs the two strategies in incorporation with others. Reflection on the process of using these strategies in on-going work and the writing at hand are aspects of the process strategy. It is expected that in reflecting on the processes that one simultaneously critiques and still to some extent uses, that previously unexpected gaps and crossings will become visible. Here the words of Paulo Freire come to mind, "Indeed, a reactionary social worker can't be interested in individuals' developing a critical view of their reality, that is, in their thinking about what they do while they actually do it."⁵⁹

The problems and strategies discussed in this chapter are those of the usually First World or First World educated research facilitator and their institutions. This chapter does address the problems of the researched local people if one considers the main problems to lie in the research institutions and researchers.

NOTES FOR CHAPTER V

¹Lev N. Tolstoy, "On the Moscow Census," in What Shall We Do Then. On the Moscow Census & Collected Articles, trans. Leo Wiener (New York: AMS Press, 1968), p. 343.

²The current 1988-1989 North American center for the Participatory Research Network is located at:

Highlander Research and Education Center
Route 3, Box 370
New Market, Tennessee 37820

The primary contact person is John Gaventa. During the last decade the North American center for the network was maintained by the Participatory Research Group and The International Council on Adult Education both of which are in Toronto, Canada.

³Samuel Paul, "Community Participation in Development Projects: The World Bank Experience," World Bank discussion papers series (Washington, D. C.: The World Bank, 1987).

⁴United Nations Research Institute for Social Development, Popular Participation Programme, debaters' comments on "Inquiry into Participation: A Research Approach," ed. Selina Cohen (Geneva: UNRISD, October 1980).

⁵Interviews with Ho'oipo de Cambra, American Friends Service Committee, Honolulu, Hawaii, November 1988 and with Stacey McConlogue, Waianae Health Center, March 1989. After my first draft, I reviewed my account of the Hawaiian research predicament with Ho'oipo de Cambra. She suggested that it is important to explain how the community had already given a great deal of thought to research relations when they had the difficulties with outside researchers. While I do have detailed notes on precisely what was entailed in the research project, we decided that it was not appropriate to be more specific at this time. The community is still awaiting notice on their grant/award.

⁶Paulo Freire, lecture at the University of Tennessee, 3 December 1987.

⁷Orlando Fals Borda, "Participatory Research and Rural Social Change," paper (Bogota, Colombia: Punta de Lanza Foundation), pp. 5-6.

⁸Peter Manicas, A History and Philosophy of the Social Sciences (New York: Basil Blackwell, 1987), p. 266.

⁹Johan Galtung, Theory and Methods of Social Research (Oslo, Norway: Universitetsforlaget, 1967), p. 1.

¹⁰I am indebted to my grandfather Richard H. Campbell (Harvard Class of 1915) for starting this discussion with me.

- ¹¹Lenin, "The Immediate Tasks of the Soviet Government," in A Dictionary of Marxist Thought, ed. Tom Bottomore (Cambridge, Mass: Harvard University Press, 1983), p. 269.
- ¹²Robert King Merton, Social Theory and Social Structure (Glencove, New York: The Free Press, 1962). This classic textbook was first printed in 1947 and revised in 1957.
- ¹³*Ibid.*, pp. 10-15.
- ¹⁴Thomas Kuhn, The Structure of Scientific Revolutions (Chicago: University of Chicago Press, 1962, 1970 2nd edition enlarged).
- ¹⁵*Ibid.*, p. viii.
- ¹⁶*Ibid.*, p. 2.
- ¹⁷*Ibid.*, p. 8.
- ¹⁸Evelyn Fox Keller, Reflections on Gender and Science (New Haven: Yale University Press, 1985).
- ¹⁹*Ibid.*, p. 6. Fox Keller here is not far from the Heideggerian notion of enframing. In his essay "On Technology" in The Question Concerning Technology and Other Essays, Trans. William Lovitt (New York: Harper & Row, 1962) Martin Heidegger speaks of the "enframing power of technology." What he seems to be moving towards is the notion here labelled "scientism." In Heidegger's rather more complex words: "Everywhere we remain unfree and chained to technology, whether we passionately affirm or deny it. But we are delivered over to it in the worst possible way when we regard it as something neutral; for this conception of it, to which today we particularly like to do homage, makes us utterly blind to the essence of technology." pp. 287-288.
- ²⁰*Ibid.*, p. 11.
- ²¹*Ibid.*, p. 44.
- ²³Martin Carnoy, The State and Political Theory (Princeton: Princeton University Press, 1984), p. 36.
- ²⁴Typically, Talcott Parsons is found in most basic social science texts including international relations. See for example, James E. Dougherty and Robert L. Pfaltzgraff, Jr. Contending Theories of International Relations: A Comprehensive Survey (New York: Harper & Row, Publishers, 1981), pp. 141-142.
- ²⁶Frank Fischer and Carmen Sirianni, eds. and introduction, Critical Studies in Organization and Bureaucracy (Philadelphia: Temple University Press, 1984), p. 9.
- ²⁷*Ibid.*, p. 10.
- ²⁸Jacques Donzelot, The Policing of Families, foreward Gilles Deleuze, trans. Robert Hurley (New York: Pantheon Books, 1979). Donzelot, a student of Michel Foucault, using a genealogical analysis, describes how state approaches to families, child

psychology and psychiatry, juvenile courts, and even desire itself are constituted as domains for surveillance and intervention.

²⁹Ibid., p. 55.

³⁰Ibid., p. 163.

³¹Kuhn, The Structure of Scientific Revolutions.

³²Fals Borda, "Science and the Common People," p. 16.

³³Michael P. Hamnett, Douglas J. Porter, Amarjit Singh, and Krishna Kumar, Ethics, Politics, and International Social Science Research: from Critique to Praxis (Honolulu, Hawaii: University of Hawaii Press, 1984), p. 81.

³⁴Interviews with Michael Hamnett, Social Science Research Institute, University of Hawaii at Manoa, Honolulu, July 1987.

³⁵Gerrit Huizer is a participatory researcher in the Netherlands and is attributed with having coined the phrase "study up."

³⁶Carol J. Pierce Colfer, "On Communication Among 'Unequals'," paper (Honolulu: Participatory Research Collection, Resource Systems Institute, East-West Center 1980). In this article Colfer manages to use mathematical formulas to illustrate and support her description which is, as she bluntly puts it, one of scientific imperialism.

³⁷Ibid., p. 20.

³⁸Ibid., p. 22.

³⁹Stephen Fineman, "Funding research: practice and politics," in Human Inquiry, eds. P. Reason and J. Rowan (New York: John Wiley & Sons, Ltd., 1981), p. 473.

⁴⁰For the background to this project see Richard Morse, ed. PAR Handbook for Rural Energy Planning Studies, handbook for facilitators' workshop held in Bangkok, Thailand, July-August 1985, sponsored by Chulalongkorn University Social Research Institute and East-West Center Resource Systems Institute, Honolulu, Hawaii. Also see "Rural Energy Planning Studies: Project Summary," memo produced by Resource Systems Institute, East-West-Center.

⁴¹Richard Morse, gen. ed. Village Voices (Honolulu: Resource Systems Institute of the East-West Center, and Bangkok: Chulalongkorn University, Social Research Institute, 1988).

⁴²William Foote Whyte with the collaboration of Kathleen King Whyte, "Types of Applied Social Research," in Learning from the Field: A Guide from Experience (Beverly Hills: Sage Publications, 1984).

⁴³Ibid., pp. 174-180.

⁴⁴Ibid., p. 174-175.

⁴⁵Personal interviews with Kersten Johnson, Resource Systems Institute, East-West Center, Honolulu, Hawaii. During 1985-1986 Johnson was a research facilitator for the Participatory Rural Energy Planning Project in Bhutan.

⁴⁶During one evaluation session held in Vanuatu by U.S. AID officials, local participants suggested that all farming related projects be funded on a five to ten year basis. There was no way their new coconuts were going to mature before five years nor cocoa before ten, let alone provide a real measure of self-sufficiency. Subsequently, those types of projects (supposedly leading to self-sustaining agriculture) throughout the region were funded not on the previous three year basis but a two year basis.

⁴⁷Reports to the Rural Energy Planning Project, East-West Center, Resource Systems Institute, Honolulu, Hawaii, 1985-1987.

⁴⁸Timothy Kennedy in William Foote Whyte, Learning from the Field, p. 176.

⁴⁹Fineman, Human Inquiry, p. 480.

⁵⁰Martin Carnoy, "Class and State in Recent American Political Theory," in The State and Political Theory (Princeton: Princeton University Press, 1984), p. 209. In this passage, Carnoy calls up Erik Olin Wright's experience as a graduate student: "As a graduate student in sociology I constantly confronted the hegemony of an empiricist, positivist epistemology in the social sciences. In virtually every debate over Marxist ideas, at some point I would be asked, 'prove it!' To the extent that Marxist categories could be crystallized into 'testable hypotheses,' non-Marxists were willing (sometimes) to take these ideas seriously; to the extent that debate raged simply at the level of theory, non-Marxists found it relatively easy to dismiss our challenge." See also Erik Olin Wright, Class, Crisis, and State (London: New Left Books, 1978) p. 9.

⁵¹John M. Johnson, Doing Field Research (New York: The Free Press, 1975), p. 186.

⁵²University of Hawaii, Graduate Division, "Instructions for the Preparation of Theses and Dissertations," (Honolulu, Hawaii: University of Hawaii at Manoa, Graduate Division, revised September 27, 1988), p. 34.

⁵³Tony Williamson is a key proponent and co-creator of the "Fogo Process" in which simple video equipment and basic training is provided to local groups in order to document situations and speak with one another or bureaucracies far away [many applications throughout the 6,000 miles expanse of Inuit groups in Canada]. Right of review and control over distribution remains in the hands of the local group.

⁵⁴Tony Williamson, personal correspondence. 1988.

⁵⁵Roland Colin, "Participer au développement," UNESCO, p. 129.

⁵⁶See Appendix D, East-West Center, Conference on International Research Access, Equity, and Collaboration. Honolulu, Hawaii, December 1985.

57 Jean Delion, "Objectives, Pedagogy, and Institutions of Participation in Rural Development," working paper (Honolulu: Resource Systems Institute, East-West Center, 1987), final page.

58 See Appendix C, Young Nations Conference, "Conference Resolutions."

59 Paulo Freire, The Politics of Education: Culture, Power and Liberation, introduction Henry A. Girous, trans. Donald Macedo (Massachusetts: Bergin & Garvey Publishers, Inc., 1985), p. 39.

CHAPTER VI

RESEARCH AND THE MELANESIAN STATE

"Yu gat nektal, yu gat numba."

(The first line of a Papua New Guinean popular song about experts and bureaucrats, meaning, "You wear a necktie, you are a big-shot.")¹

I

In order to deepen and expand the analyses begun in the previous chapters, this chapter examines the issues surrounding social science research practice and research access in the Melanesian state. The intent is to discover limitations on research and potential participatory research within Third World states. Most background cited is derived from studies in the Papua New Guinea, Solomon Islands, Vanuatu, and Fiji with the intent that the larger issues and conclusions obtained will be at least partly applicable to the other Melanesian situations--New Caledonia and Irian Jaya (West Papua)--as well as the Third World in general.

The chapter is based upon some elements of world-systems analysis, which seems to be the best available to explain intensifying internal social, economic, and political contradictions in part resulting from capitalist exchange relations.² In terms of Melanesia, Immanuel Wallerstein's work is useful in viewing how a number of relations with the outside world increasingly draw local people away from self-sufficiency on their traditional lands and into wage relations. The subsequent exchange relations have important effects on state policy and participation in regional bodies including research institutions.

My vantage point and concern here is based upon my own past and present participation in voluntary organizations and a Pacific university (the University of Hawaii) which contribute both directly and indirectly to the continuation of undesirable research relations. However, I am not calling for the total cessation of research activities by research First Worlders from the Pacific rim countries within the research

Third World of the Pacific basin. What I seek is understanding of the dangers of some research practice and the limits of potential participatory research practice. I have asked what one can do and where one can act given various institutional limits. I now ask: what are the limitations and possibilities for participatory research and for both research outsiders and insiders operating within the Melanesian State?

II

I return to the research access situation described in the first chapter as well as some notes on the current situation of university education within the Pacific basin. The purpose is to give a context to discussion of how the various states have tried to control research and local knowledge.

One of the first documents concerning research as a generalized field in the South Pacific was issued by the Seventh Pacific Science Congress held in New Zealand in 1949. The Congress' document "Division of Research" discussed the newly created Fulbright Act, the primary purpose of which was "the exchange of students."³ The document continues:

[O]ne of the primary purposes of the grants will be to enable New Zealand and American scholars to exchange ideas pertaining to their specialized fields. It will be possible for American scholars to obtain useful information and experience on scientific problems peculiar to New Zealand, and likewise, it will be possible for New Zealand scholars to become acquainted with current American ideas and techniques.⁴

Note that the purpose is not stated as the solution of pressing problems, but the gathering of information and acquisition of [scientific] technique, and that technique is first and foremost located with Americans. The document did not even consider the possibility that Pacific Islands other than Australia or New Zealand would ever have their own researchers.

The post-war Pacific was increasingly deluged with ideology of the above type-- ideology of the neutrality of science for the good of all.⁵ The ideology of the neutrality of

science is held and promulgated, for various reasons, by capitalists, Marxists, socialists, and in fact the vast majority of people engaged in formal research, government, transnational business, and philanthropic work. This group includes central planning office economists who in many of the Melanesian countries wield tremendous influence as to what research and assistance is required in their countries.

In the forty-four years since the war ended in the Pacific, the ideology of development and modernization via technology has been the dominant paradigm to the extent that any other representation of the world becomes either invisible or at best trivialized. Furthermore, failure of the developmental models based on these paradigms becomes invisible. In other words, if a project doesn't work either the local culture is blamed as not sufficiently malleable, or it is believed that not enough was done or spent. As a consequence, what tends to be recognized and critiqued is not the dominant ideology but those who have been its carriers to date, that is European colonials and other European descended individuals.

At the 1976 "Young Nations" conference at the University of New South Wales, Australia, indigenous South Pacific participants for the first time as a group called attention to both external and internal relations of research dominance.⁶ One participant noted that even the name of the conference, "young nations," was a value-laden construct of the center country organizers. [The resulting conference resolutions are cited in the first chapter and are appended in C. Again, in essence, these resolutions called for foreign researchers to also take responsibility for studying the power structures and institutions that permitted their own research both at home and in the Pacific.

Despite the sophisticated and thorough critiques as well as action proposals concerning research practices in the 1976 conference, most attempts to come to grips with obvious problems have resulted in blanket moratoriums that tend to limit research

access by social scientists, particularly anthropologists, while leaving wide open the space for corporate and transnational scientific research as well as so-called "government studies." Other than a few published documents little has happened to support the Young Nations' resolutions.

This is not to deny the blatant individual as well as more subtle systemic exploitation of island peoples by researchers over the years cited in previous chapters. One anthropologist I interviewed called my attention to a letter to the government from a group of ordinary villagers on the island of Tanna in Vanuatu. These villagers asked the government to please "block," as they put it, anymore anthropologists, documentary filmmakers, and other outsiders who bother people with useless stuff. There is no doubt that research-mining of this sort is unjust and renders its objects--indigenous people--in the same way that pieces of cell tissue may be cut up and put under a microscope. Furthermore, these objections don't even begin to touch upon questions of the validity and importance of local knowledge.

Nevertheless, liberating indigenous peoples from one form of exploitation by limiting research access may neglect or even disguise other forms of control. A case in point is that of Roger Keesing and the Kwaio people of Malaita, Solomon Islands also briefly cited in the first chapter.

During the early 1980's all research access in the Solomon Islands was closed. Closure centered around the activities of American anthropologist, Roger Keesing, now at Australia National University. Keesing had worked with the Kwaio peoples of Malaita for sixteen years and in 1980 published Lightning Meets with West Wind. The book meticulously documents a 1929 massacre of hundreds of Kwaio people--many of whom were women and children--by a constabulary force of other tribes supported by the British protectorate.

During his research process Keesing opened up his agenda in numerous group meetings with Kwaio; he explained what he could and could not deliver, and on publication he put all profits into a trust fund for the Kwaio. The Kwaio were just about unanimously in approval of Keesing's process and the conclusions of the book itself. Nevertheless, Keesing was denied further research access to Malaita. The first level reason was that the provincial official for Malaita in the early 1980's was a member of the tribe that had perpetrated most of the massacre, and even though most of the 1929 protagonists were dead, the particular official was afraid of compensation claims being organized.

Previously, Keesing had publicly notified the Kwaio after consultation with attorneys, that since the affair occurred before independence, compensation would be legally impossible. Nevertheless, Keesing's position vis a vis the Solomon Islands government remained the same--no entry to Malaita. What the government position throws into relief is the position of government bureaucrats in relation to the most marginal tribal peoples. In "Politics of Fieldwork: Solomon Islands 1962 - 1984" Keesing summarizes what Kwaio elders told him:

The white elite has been replaced with a black one, but the issues are the same: taxation without visible return, and relative autonomy to live, and settle disputes, according to custom.⁷

If there remains any question about what research occurs in the Solomon Islands, a March 1987 interview with the acting director of the Pacific Islands Development Program (PIDP) of the East-West Center makes it clear. The director of PIDP said that his organization did do research in the Solomon Islands. Since these were "government studies" and the Solomons' government had the right of review on any reports issued, continuing research was acceptable. The particular research in question in PIDP was research on problems of indigenous entrepreneurs. It was assumed that the study could lead to an aid project and/or foreign investment.

In 1985, research in Vanuatu was limited to six new researchers a year who after paying a \$200 research permit filing fee would also be required to pay a \$3000 non-refundable research fee--half to go to the national research council and half to the community under observation. By mid-1986, there had been so much confusion and complaint (only two researchers were willing and able to pay those fees) that the research council disbanded and a blanket research moratorium was enacted.

Again, as in the Solomons, this does not mean there is no outsider funded and facilitated research in Vanuatu. The French organization ORSTOM (Organisation de recherches scientifiques et techniques d'outres mer) which is funded by ACCT (Acte de cooperation et culture technique), the French equivalent of US AID, maintains approximately ten geologists, anthropologists, fisheries specialists and others in Vanuatu at any time. IRHO (Institute de recherches d'horticulture d'outres mer), a French horticulture research organisation also maintains a large research station and approximately six specialists on the island of Espiritu Santo. Australian and British scientists are likewise employed in agricultural research, much of which is now funded by the Asian Development Bank.

Noting the types of research that are still supported--indeed with fanfare--is instructive. The August 31, 1985 (during the heart of the research ban) edition of the major Vanuatu newspaper The Vanuatu Weekly describes the formation of a new research institution: L'Institute de Recherches du Café et du Cacao. The institute in Valetteruru, Santo was expected to specialize in soils research.

Two trends are immediately apparent in the above examples. The first is a continuation of research where first world researchers are tied to continuations of both bilateral and multi-lateral aid flows. The second trend is the continuation of agricultural and other scientific research, particularly those forms of agriculture aimed at promotion of cash crops and directly linked with international financial inputs.

These inputs include loans from the Development Bank of Vanuatu which received its funding from the Asia Development Bank. In turn, loans purchase foreign produced vehicles, barbed wire, fertilizer, pesticides, consultants, and even more schooling in techniques (read more status too) of corporate and foreign dependent agriculture. Almost always aid agreements require all physical inputs to be purchased from the donor country.

There is yet a third trend masked by the two described above. The third trend is a move away from individual outsider contact with remote rural groups. Again, there is no doubt that many anthropologists have been carriers of a dominant ideology and research-mined their subjects.⁸ Many on the other hand have served as advocates of the groups with whom they lived worked. Nevertheless, direct outsider contact with those groups most distant geographically and culturally from central governments seems to be decreasing.

An example in Vanuatu is that of the well-known Canadian anthropologist Margaret Rodman. After fifteen years of part-time residence in Vanuatu, Rodman published a book that thoroughly documented the concentration of land and power in the hands of rural elites. Implicit throughout the text is the creation of new rural elites by central government officials' blind adherence to developmentalism. Rodman has chosen to participate or advocate for what she sees as the marginalized rural group--the losers. During 1985-1986 when her book The Masters of Tradition was being finalized she did not have a research permit. Rodman then worked as a consultant on a fisheries evaluation sponsored by CIDA (Canadian International Development Assistance Agency). This is not in anyway to condemn Rodman's activity. It was well-known that she was finishing other work, and the people of the area in which she worked were highly supportive. What one questions is her access if she had not been tied to an evaluation that was seen as bringing more aid flows to the central government.

Review of a number of similar cases led me to conclude early on that research access, particularly in the Solomons and Vanuatu, would not remain frozen. My most recent round of letters (see Appendix B) with the various governments indicates that research access is no longer officially frozen in either country.

Another example of the above focus on research tied to cash crops and/or aid flows appears in the previously cited (first chapter) edition of Research in Melanesia.⁹ Of the externally contributed funds for university agricultural research in Papua New Guinea, the contribution for study of the most nutritious of indigenous vegetable crops--wing beans--was less than .1% of those funds provided for research of cocoa dieback disease and less than .2% of funds provided for insecticide research.

It seems then that only research which threatens the political or economic security of national elites is controlled. However, resting with this conclusion tends to oversimplify research relations and implies that the officials in the central governments wield a much more instrumental power than they actually do.

The above examples could also lead one to think then that really good usual research that leads to radical problem solving could justify the presence of a lot of high-powered outsiders.

This leads to some of the reasons I continue to stress the importance of focusing research issues through the participatory research lens. Returning to the Fulbright document which never even considered the possibility of Pacific islanders doing research, until the last ten or so years, there were next to no Melanesians who did what we have been labelling research. There are still just a handful of Melanesians who can evaluate foreigners' research proposals. There have been many, many research efforts which did not go deep enough, because the foreign researchers only stayed two or less years--only enough to skim the surface of local culture. There have been almost equally many cases where local informants played the game of trick the anthropologist with

made-up stories. Some of these tricks have actually been a sort of angry resistance to social scientists' encroachments on taboo or inappropriate areas. Research that is good in the usual sense is often not enough. Although individual researchers may work with exemplary intent, the sheer numbers of foreign researchers in comparison to the local researchers and the ability of the foreigners to get published and heard, overwhelms any collective self-portraits by the local researchers. In many cases, the Melanesian researchers are still the "counterparts"--the children of the patriarchy.

III

Clearly, there is a trend towards Melanesian private and public involvement with foreign funded research in areas of natural and social science that contribute to cash cropping and transnational agricultural interests. The question is through what groups of people and institutions do these involvements come to be? How has the state imposed these research controls? Again, what are the impacts?

In the last decade, the people who act as the main agents of these research decisions and changes are the politicians and public service officials in the central governments. The background of their own training and education is relevant.

During the last one hundred years, the few educated Pacific Islanders received their education almost entirely in theological colleges and seminaries in Australia, New Zealand, Great Britain, and the United States. It was only at the beginning of the various independence movements in the late 1960's that the University of Papua New Guinea (1966) and the University of the South Pacific and University of Guam (1968) were created, and that Pacific educated Pacific Islanders emerged.¹⁰ Even so, the percentage of Pacific Islanders receiving higher education remains quite small although it is beginning to grow more quickly.

Pacific Basin higher education institutions also include Cenderwasih University, Irian Jaya, founded 1964; Papua New Guinea University of Technology at Lae, 1969;

College of Micronesia at Kolonia, Federated States of Micronesia, 1970; American Samoa Community College, 1970; Atenisi Institute, Tonga, 1971; the Université Française de Pacifique, serving New Caledonia, Wallis and Futuna, and French Polynesia, 1987; as well as several lower level teacher's colleges and church sponsored training centers. None of these institutions were envisioned as necessary before the 1960's.

Before the formation of the above institutions, as well as currently, many Pacific islanders have attended First World institutions rimming the Pacific. These rim institutions include the universities in Australia, New Zealand, Hawaii and the West Coast of the United States. In general, these institutions need physical fields of study in order to train their graduate students, polish their faculty, and in some cases provide consultancies to keep their faculty. The peoples of the Pacific as objects of study have presented a vast resource. Ironically, most university educated Pacific islanders have been exposed to and internalized this approach to establishing oneself as a scholar.

Although it seems that national universities would tend to reflect the aspirations of Pacific islanders, the standard Western ideas about scholarship and research persist. Pacific island universities are still heavily staffed by expatriate personnel. Other ties are also quite important.

Bishop and Wigglesworth connect the University of the South Pacific with the influence of post-colonial powers:

The maintenance of western control over the South Pacific provides the impetus behind foreign policy in the region... No US aid was given to the South Pacific until 1977 and it has increased since with the 1980 expenditure, being double the previous year. No bilateral aid is given [this changed in 1985], but financial support is given to regional organizations, particularly the South Pacific Commission and the University of the South Pacific, and to American private and voluntary organizations, [sic] particularly the US Peace Corps.¹¹

Obviously, US AID representatives then have tremendous influence simply by funding certain large research projects or funding certain departments and subjects or not.

Amarshi, et. al. as well as many others argue that the small pool of university educated Pacific Islanders found themselves emerging along with opportunities in their newly independent governments, but that as they accepted those opportunities they formed new, post-colonial elites.¹² Many of the newly formed, educated Pacific elites received additional education or training in other Pacific countries or their former colonial masters. Equally, many of these people returned to their home countries and islands with very mixed feelings about northern scholarship coincident with strong needs to re-identify with their own cultures.

The new governing elites found and continue to find themselves sandwiched between abhorrence of the colonial systems and the Europeans who administered those systems and tacit support of the Western systems including university education that gave them their political and economic status. Such administrators and politicians find themselves engaged in anti-colonialism simultaneously with technological biases against their own rural people. An example provided in the previous chapter was that of the administrator who said "people were too poor to understand, and ran away like wild animals at the first sound of an automobile horn."

Any Melanesian development administrator involved with researchers is under the immense pressure of highly contradictory and inconsistent systems. Amarshi speaking of Papua New Guinean administrators says:

...greatest weakness lies in his inability to extract himself totally from the system he dislikes so much ...such contradictions are "understood and tolerated in a country such as ours."¹³

IV

Current Melanesian research access policy is a manifestation of the functioning of the post-colonial capitalist state. In order to comprehend what kind of state produced those policies and the influences coming to bear on what research is forbidden, permitted, or enabled, one should attempt comprehension of the social

formation shaping the state. Outside of neoclassical economics, little work has been done on the relation of political economy and the Melanesian state. All that is available for analysis is weak descriptions covering factionalism or dependency approaches both of which are inadequate in describing and explaining internal relations of production and their relations to external relations of exchange.¹⁴

Class analysis in Melanesia is close to nonexistent to date as well. Lack of any, let alone adequate class analysis leads to a mono-dimensional view of the Melanesian world, especially on the part of government planners, many of whom are still expatriate economists trained in the neoclassical tradition. Planners tend to categorize all relations as economic and grounded in relations of exchange (Adam Smith's market) rather than in production. It is often these planners and department heads who exert greatest influence on what aid funded projects are undertaken and what research is done.

My perspective and thesis about the Melanesian state is influenced by the views of political economist Osvaldo Sunkel. His main thesis is that the capitalist system is undergoing increasing change from an international to a transnational system. Because of this transnationalization process many societies are undergoing severe stresses that result in decay of traditional social structure while simultaneously producing counter-processes of reintegration. As a consequence of these processes distinct transnational communities are emerging within national societies.¹⁵

The Melanesian state is far more complex than appears at first glance.¹⁶ Least it appear that a case is made for the complete instrumental wielding of state power by the large state bureaucracy, note the comment of political-economist Fred Block:

[T]hose who manage the state apparatus--regardless of their own political ideology--are dependent on the maintenance of some reasonable level of economic activity. This is true for two reasons. First, the capacity of the state to finance itself through taxation or borrowing depends on the state of the economy. If economic activity is in decline, the state will have difficulty

maintaining its revenues at an adequate level. Second, public support for a regime will decline sharply if the regime presides over a serious drop in the level of economic activity.¹⁷

To review the traditional approach to analyses on the Pacific economy, note one of the few texts written by a Pacific islander trained as an economist (Australia) Te'o I. J. Fairbairn's Island Economies: Studies from the South Pacific. Fairbairn's text is universally typical of government analysis of rural problems and prescriptions. Definition of problems and problem areas limits almost all research and/or government studies to working within the paradigms of neoclassic economics and modernization. Since few researchers are able to function in Melanesia outside of government or foreign assistance projects, grants, and other ties, the body of printed public knowledge flows forth from a limited world view.

Because Fairbairn's text is so typical of what is constituted as public knowledge about Pacific economies, poverty, and social relations, I quote him extensively. Fairbairn below attributes rural poverty to unemployment and underemployment which he explains as follows:

A first possibility is the disparity between actual production, as determined by prevailing consumption patterns (and need for capital items) of village households, and what villagers are capable of producing if labour were fully employed in the conventional sense. Consumption levels are said to be constrained by the physical capacity to consume goods (taking into account goods required to meet ceremonial and prestige needs) and limited "felt wants" in relation to trade goods. The weak demand for trade goods can be explained by the limited exposure to the demonstration effect; a continuing preference for some subsistence goods and services; and a failure to appreciate the utility of many available durable and capital items. The quantum of goods, both subsistence and trade, demanded by a household at any given time is therefore limited, and this constrains the full employment of labour. The above explanation is most relevant in rural villages at the early stages of transition from a purely subsistence to a strongly monetized economy. Demand for trade goods is in the process of being built up, but in the meantime it remains at a level requiring a less than full application of available labour. More intensive utilization of labour awaits the demonstration effect to come into full play.¹⁸

Fairbairn's analysis of the problem leads to the following prescriptions:

Schools, health centres, piped water, access roads, and sporting and community facilities can enhance the attractiveness of rural life and induce villagers who might otherwise migrate, to remain and work in the rural sector.¹⁹

and thus:

Technical change to improve efficiency is the key to the full realization of the agricultural potential inherent in most Pacific countries.²⁰

In context, this indicates an Emperor's new clothes situation; after nearly three decades of effort, no one has been able to make these prescriptions work even in the relatively small populations of Melanesia. Migration to the urban areas is increasing and putting severe pressures on those areas. The gaps between formerly self-sufficient rural people and central government officials and those working in transnational businesses are increasing. Youth are ever more disaffected and law and order is increasingly an issue. Fairbairn's prescriptions are not working yet planners and policy makers keep prescribing them.

There is no other world view made publicly available., nor is there known any way or reason to identify other views. Certainly government planning posts are often tied to the continuance of aid funded projects and large project portfolios from the so-called developed nations. Governments need these projects to manage in order to justify the existence of their over-developed bureaucracies. In turn, developed nations use these aid projects to extend regional influence and ideologies of material consumption and production.

Hand in hand with neoclassical assumptions about Pacific economies go assumptions about classlessness in Melanesia which for the most part did not have the system of hereditary chiefs that Polynesia did. A typical proponent of this view is Canadian anthropologist Jean-Marc Philibert writing in the early 1980's:

The emergent political elite has not yet coalesced into a social class of its own. Indeed, social classes do not exist among Ni-Vanuatu. If the country has a proletariat it is still an infinitesimal one.²¹

Although Philibert's analysis may have been in some ways correct for pre-independence Ni-Vanuatu Melanesians, all of whom were colonized, he seems to have been unaware of the social processes in which he was living. Relating the discussion commenced in regards to Fairbairn's analysis, perhaps another part of the answer as to why this world view is so prevalent, lies in the strength of ideology.

Philibert like Fairbairn (and all of us) is a product of mainstream, developed country universities. His anthropologist's training surely has not been to use research as a tool for local social transformation at the behest of the local people. He sees his role in social science as the impartial and thorough observer.

Other social scientists may see problems in the Melanesian context as lack of modernization. Problems then become conceptualized as solvable by applied science. Policy change then comes from recommendations made at the national level, where national bodies act to promote and regulate the production oriented activities of local people, whether those local people are interested in production or not.

More recent work on related issues in Melanesia is beginning to emerge from different frames of analyses. An example is Margaret Rodman's Masters of Tradition: Consequences of Customary Land Tenure in Longana, Vanuatu which details:

[h]ow the flexibility of customary land tenure allows the system of land holding to change while appearing to remain the same. Out of older kinds of inequality are emerging new kinds of inequality in cash income and in control of land--land is being concentrated in a few hands while the subsequent social differentiation among the peasant copra producers is being obscured ...The way in which the penetration of capitalism has taken place in Vanuatu makes possible the persistence of an illusion that rich peasants are the same as traditional men of [achieved] rank and influence.²²

The main publications (other than a rare handful of masters and doctoral theses and dissertations) to date, discussing class formation and structure are few.

In view of Rodman's work, and particularly in the case of Vanuatu which was simultaneously a colonial condominium of both Britain and France, factionalism

appears to be an adequate description of the complex groups pulling on the strings of the state.²³ Further examination reveals that factionalism remains an entirely inadequate description for the fundamental reason that it depoliticizes the large differences in access to power and resources that some groups maintain and exert over others on an increasingly long term, systemic and systematic basis.

Dependency theory, on the other hand, does seem to describe the relations of post-colonial Melanesian states to their former colonial masters. Few would argue that dependency does not in part describe, for example, 1980 Vanuatu which at its independence was fully reliant on British and French aid for its recurrent budget. Dependency theory is inadequate in dealing with increasingly complex encroachments of transnational and multi-lateral businesses and organizations in consort with national governments.

Strictly speaking, the dependency approach sees the relations of the post-colonial state as a function of external forces. The main characteristic of a dependent state is surplus extraction such as export of raw minerals or a few main cash crops and dependence upon more expensive, imported manufactured goods to enable the extraction. Surplus extraction is then both a cause and a continuing condition of the relations between the "peripheral" states and "core" states. Further, these are a sophisticated continuation of colonial plundering.

The dependency approach focuses on bipolar relations with the outside, thus depoliticizing internal, asymmetrical relations of the state bureaucracy's maintenance at the expense, neglect, and exploitation of the ninety percent of the population who are engaged in some degree of subsistence agriculture.

Leaning on political economists Blomstrom and Hettne, an approach more appropriate to conditions in the southwest Pacific would also encompass some discussion of land tenure and dualism in which:

A large portion of the population finds itself outside the modern, dynamic sector (or in the terminology of the dependency school, it is "marginalized"), because it lacks the power to increase wages at the same rate as it increases its productivity. This dualism in which a modern sector, with a dynamic of its own, has been placed on top of a traditional sector, with a dynamic of its own, will continue to exist as long as there is peripheral capitalism, which precludes any development that will benefit the entire population.²⁴

In an application of dependency theory which generally lacks sophisticated class analysis, one may be locked into categorizations of indigenous, dependent, rural, and poor insiders being sold down the river by their own bourgeoisie (those who control the means of production) to external, capitalist exploiters. These categories in facile application may become quite literally black and white and thus mask the subtleties with which exploitation and subsequent dependence occur.

At any rate, functional dualism and other approaches to analysis have not yet been much applied to Melanesian states. There is, to say the least, not a lot of interest in identifying the mechanisms of surplus extraction. Perhaps this is in part due to the lack of the grim Latin American proportions of peasant and rural problems. In the Solomons, Vanuatu, Fiji and many parts of Papua New Guinea as long as there are no major natural disasters such as cyclones, most rural people still have enough land to exist in traditional patterns of subsistence agriculture, and in fact, there are many thousands of people still outside the cash economy.

However, those days are quickly coming to an end. As population increases (perhaps as much as by 3.8% in Vanuatu), as land is turned over in large chunks to joint ventures, as people are increasingly taxed, and as desires for manufactured goods are themselves manufactured, processes such as functional dualism will be increasingly apparent. It will become more obvious (counter Philibert) that there are classes--or at least class relations. This is already abundantly clear to the Papua New Guinea Highland women who wrote the popular song that introduces this chapter: "You wear a necktie, you're a big shot."

In the case of Vanuatu there is a great deal of rhetoric about Melanesian socialism. The 1980 constitution returned all land to the "custom" (traditional) owners. Theoretically, all land is held communally and can never be sold, only leased. Houses on top of land in the municipal areas may be sold, but again, the land is only leased. Communal ownership of land does not comprise any more than an attempt at socialism, and anyway, in Vanuatu, returning land to the custom owners was the most important part of the drive towards independence in rural areas. In the rural areas, it was a return to the traditional that was discussed, not socialism.

The owners of the means of production are for the most part expatriate capitalists. Although the mode of production appears not to be fully capitalist, because many rural people are still relatively self-provisioning, they are increasingly being drawn into cash cropping through minor taxes and capitalist ideologies that promote material desire. In any case, it is increasingly difficult to reconcile the notion of a Melanesian socialist state with Vanuatu's international off-shore banking center and no corporate taxes.

The above supports Wallerstein's central thesis that everywhere the world capitalist economy is inexorably drawing in resistant groups to its processes and relations. This process of drawing in is complex. Increasingly polarized class relations are evident, but it is hard to pin down actual classes, because there is yet mobility in and out of categories and the categories themselves have been changing during the first decade or so of independence.

There is a range of peasants from those still outside the cash economy to those beginning to feel the squeeze of functional dualism and a tiny handful beginning to be selected out as the carriers of transnational agribusiness, tourism and the like--rich peasants.

There is a small group of rural and urban micro-entrepreneurs who are mainly service oriented rather than producers (see Lamont 1984 and Crocker 1985).²⁵ The micro-entrepreneurs may be a truly miniscule, emergent national bourgeoisie, although since all business relies heavily on outside products and contracts "national" may be a misnomer.

There is a much larger international bourgeoisie increasingly engaged in the management of foreign owned businesses (Japanese and Australian hotels for example), state enterprises such as commodity boards, and managers of joint enterprises which are for the most part agricultural or basic commodities producers and supplier/distributors.

Finally the government and its agencies may be characterized as a state technobureaucracy. Although few salaries actually derive directly from aid flows, much of the public employees' work is based on the management of aid funded projects. Ever increasing aid flows and new projects mask the lack of real accomplishment on the part of service and extension agencies. It may be argued that this last group is then also part of the international bourgeoisie as well. This last group is also where the majority of technical institute and university graduates fit in. Again, until very recently, the bulk of education was received from expatriate teachers and/or overseas as well.

V

In order to be more precise about influences on research relations, documents issued by the various states during the heart of the research freeze were examined. The Vanuatu documents are particularly instructive.²⁶

Referring to any of the Development Plan reviews published in these countries, one finds statistics on amount of recurrent budget either directly supplied by former colonial powers or indirectly generated or related to aid projects.²⁷ Papua New Guinea is still tremendously reliant upon Australia, whereas Vanuatu diversified from complete

dependence on Britain and France to smaller inputs from Australia, New Zealand, Japan, Canada, and others through non-governmental agencies.²⁸

The Vanuatu statistics are 21.5 and 14.2 million US dollars worth of aid from France and Britain respectively in 1979 down to 1. and 4.8 million respectively in 1984. Planners often use these sorts of statistics to produce analyses demonstrating that dependency on the former colonial powers is decreasing and self-reliance is increasing with attendant calls for greater economic expansion. This sounds logical, but "greater economic expansion" usually means trying to attract more foreign investment and aid projects, all of which have many strings attached.

Examination of just one month's volume, during the heart of the research freeze, of the Vanuatu Weekly, Vanuatu's only newspaper, reveals a pattern. Issue No. 62 for the week of October 19, 1985 notes a 1.2 million ECU grant from the EEC under Lome I and II for heavy equipment for the public works department. All of this equipment is obviously supplied from overseas and will require importation of parts, fuel, and expatriate mechanics.

In the same issue of The Weekly, we see that the French government awarded 300,000 vatu (US \$2,800 approximately) to translate the new national, companies act and regulations into French. Presumably, French companies will now know how to proceed in starting up or establishing branches in Vanuatu.

Issue No. 64 notes a conditional loan from the European Investment Bank for 2 million ECUs to the Development Bank of Vanuatu for part financing of small to medium scale capital investment in industry. However, through December of 1985 no Ni-Vanuatu (born in Vanuatu of Melanesian parents) fully owned a business with more than twenty employees (see Lamont 1984 and Crocker 1985 a). Much of this finance either moves through joint ventures, or where Ni-Vanuatu do come to hold the

business, through expatriate consultants and/or decisions about release of funding are made by expatriate advisors or Development Bank employees.

Issue no. 66 notes seventy prospecting licenses issued, nearly all of which were taken by eight Australian multi-national mining firms.

Finally, issue no. 68 notes an EEC grant of 590 million vatu or approximately US \$5,700,000. The Vanuatu Weekly seems to be the weekly documentation of where multi-lateral agencies have promoted corporate capitalism.

A further reading of the 1986 issues of The Weekly provide documentation of the erosion of any pretensions to so-called "Melanesian socialism" that the state leaders proclaimed at independence. A national teachers' strike was put down by the central government. Likewise, after pressure from multi-lateral agencies and representatives from international joint ventures, particularly the Malekula island cocoa holding, all the teeth were removed from the new minimum wage law in regards to agricultural workers.

Similar relations exist throughout Melanesia. What they all indicate is not so much less dependency on former colonial powers (bilateral relationships) as an increasingly complex set of relationships with multi-lateral agencies and corporations. This is essential in understanding what research is enabled or not. Melanesian governments are still enormously dependent upon aid flows. Aid in turn comes from nations (mostly Western) wishing to influence various policies and foreign relations. These Western nations also have powerful interests in multi-national bodies, for example the United Nations and the various development banks.

Leaning on Vanuatu as the prime example, some of the ways in which multi-national and transnational influences have increased since independence have already been sketched.

Although direct British, French, and Australian aid is decreasing in Melanesia, it is no surprise that the Asia Development Bank and ESCAP have established regional

offices there since 1984. Again, in the case of Vanuatu where before independence in 1980 there were none of these direct linkages to multi-lateral organizations, by 1987 the linkages, loan obligations, large research stations, and expatriate personnel have proliferated in magnitudes of millions of dollars for a very small population of 120,000.

Many of the financial packages relate to the promotion of cash cropping and enterprise development. Although the packages are labelled for small holders or rural people, they often serve more to promote providers of services and imported agricultural products.

Outside of major town areas, plantations, and mines, the hand of transnational corporate activity does not appear so heavy. This is deceptive.

In a July 1983 interview of provincial planners in Enga Province in the Papua New Guinea Highlands, I was told that Burns Philp, an Australian multinational, had taken very specific actions that limited expansion of small holder agricultural production throughout the highlands.²⁹ Apparently, Burns Philp engineered it so that the local airline, Talair, would not fly produce down to Port Moresby, the capital, and other coastal town areas where vegetables do not grow so well as the cooler highlands (there are no roads from Port Moresby to the highlands).

Subsequently, Burns Philp continued to import massive amounts of fresh produce from their own corporate farms in Australia. Note that the highlands represent the majority of the Papua New Guinea population and other than wage labor in some areas on coffee and tea plantations, etc. the bulk of the productive activity is small holder, self-provisioning farming.

Although this sort of machination is only recently coming to be documented, Melanesians long since dubbed Burns Philp "Bloody Pirates." The previously cited, A Touch of Australian Enterprise does document Burns Philp activities in Vanuatu. It also should be noted in view of the Young Nations Conference call to research the role of

churches, that a great deal of BP's original Australian support came from Presbyterian churchmen.

This is the context that outside researchers encounter in Melanesia.

VI

With the increasing complexity of class and transnational relations in Melanesia, it is difficult to predict what will occur with social science research practice in general and participatory research in particular. One suspects that none of the research moratoriums will last much longer or that they will be circumvented or that they never really existed in certain sectors.

The above are my May 1987 conclusions in an outline of this dissertation. As mentioned earlier, concurrent with this writing, I received a letter from the National Planning Office of Vanuatu stating that research access is decided on a case by case basis by interested government departments and the Department of Foreign Affairs. Personal telephone calls revealed that there is no research council to read proposals, so that in effect, social research not tied to aid projects remains frozen unless the researcher already has Ni-Vanuatu government friends who are willing to act on her behalf. Many of the research institutions supplying agronomists, soil specialists, entomologists, and others also still have the odd anthropologist to employ. The influence of large funding institutions on small governments will probably erode or weaken most strictures.

There is increasing momentum particularly in the field of anthropology to engage in various ethical codes which themselves are increasingly politically conscious. Some of the more "radical" forms of participatory research are exemplary. In this context, to what extent indigenous grassroots organizations facilitated by participatory research and/or grassroots values might sustain long term social change is unexamined to date. Likewise, the extent to which participatory research was thus far attempted and subsequently preempted or coopted is unexplored.

At independence in 1980 Vanuatu only had eight college graduates. As college graduates in the social and other sciences clamor for employment, one suspects research avenues may again be opened up to some extent for nationals and perhaps closed for foreigners. Yet this does not mean transnational influences would decrease; rather, indigenous scholars and researchers would tend to become agents of transnational organizations and businesses.

All of the above is not to predict that the forces of transnational capital and modernity will steamroller over all of Melanesia. The more transnational and modernity forces operate, the more local people become aware that they are being exploited. For example, in 1964 an Australian administrator--engineering the take over of local land for the huge Bougainville (Papua New Guinea) mines--was able to get away with quite a lot: "In one sentence he told the landowners, 'you get nothing', climbed back into his helicopter and left."³⁰ In 1989 this behavior is no longer possible without meeting or causing resistance. Bougainville landowners have been engaged in armed resistance and sabotage against the mine managers and their national government who needs the revenues from the mine.

In the Solomon Islands, transnational logging companies and the central government are meeting resistance from landowners:

I don't want foreign logging companies here: we are unable to go and do the same in America, Australia, Britain or Japan, so why should they be able to do it to us? The way I see it, the foreigners are just here to rob us, cause problems for landowning groups, and run away when they are finished. It should be up to us to invite companies here if we want them, not for them to force themselves upon us. I wholeheartedly believe that our elected members are behind foreigners and overseas companies who come and operate in this country. They are selling us, and our land, children, chiefs, trees and everything. [Chief Billy Anitarau and Chief Sam Sikotasi speaking to a reporter from [Link](#)].³¹

Given the preceding complex of positions and relations, one cannot make perfectly accurate predictions about what research is enabled, permitted, or forbidden.

When I learned that research access in Vanuatu was no longer officially frozen, I discussed the situation with some fellow Melanesia researchers: Is it that research supporting aid projects is permitted? Is it that research in villages is blocked? Is it that only those outside researchers who have prior experience in the Solomons or Vanuatu can find a friendly official to pass their permit application? All of the above and its opposites are true, we concluded.

Depending upon senior officials' ties to rural life, home villages or islands and consequent views, one finds different concerns. Concerns overlap and confound one another; they range from genuine concern about exploitation of local people, to not endangering foreign ties, to promoting production, to keeping their own aid project portfolios going.

As we have seen the national politicians, policy makers, and planners do not have a free hand, but neither do the transnational organizations and corporations.

James Winkler, who writes for Pacific Islanders about transnational control, tells us:

The corporation does not have free reign, however. It is under siege, and its attempts to bring others into management is due to the pressure exerted from those who are intensely dissatisfied.

The controversies surrounding infant formula and the injectable contraceptive Depo Provera illustrate the anger and frustration women feel toward unresponsive and impersonal forces such as transnational corporations.³²

Neither do the local rural people, who comprise the vast majorities in Melanesian states, have all the answers. In some cases, the activities of transnationals have become less visible to local people as the corporations hire and train more local middle-level managers. Bishop and Wigglesworth reveal:

Past criticism of these companies for their economic domination of the island economies is being reduced because of a changed corporate structure and image but Burns Philp, for example, rather than reducing its operations is establishing its stronghold in the development of a regional network through agency agreements and by concentrating on areas such as hardware and construction where it can be less criticized for competing with local entrepreneurs.³³

On the other hand, when some situations become visible local people increasingly react or even resist. As capitalist and transnational forces hold out the possibility of riches for all while aid projects also promote ideologies that they are for the eventual good of all, local people assimilate and come to use these ideas. If this is for the good of all, and I am part of the all, how come I am being exploited?

Where then does all this leave the participatory researcher--if she is an outsider?

I recall Foucault:

The reason this kind of struggle tends to prevail in our society is due to the fact that since the sixteenth century, a new political form of power has been continuously developing. This new political structure, as everybody knows is the state. But most of the time, the state is envisioned as a kind of political power which ignores individuals, looking only at the interests of the totality or, I should say, of a class or a group among citizens... But I'd like to underline the fact that the states' power (and that's one of the reasons for its strength) is both an individualizing and a totalizing form of power... We have to promote new forms of subjectivity through the refusal of this kind of individuality which has been imposed on us for several centuries.³⁴

"New forms of subjectivity," seems like the perfect theoretical justification for doing participatory research. But part of this new subjectivity should make it problematic that an individual First World researcher can just go and do their research to others. Unless a powerful invitational element is involved, when an outside researcher engages with local people--even with some elements of participatory or action research--the proprietorship of the effort remains in the hands of the outsiders. By invitational is meant that the research population has already engaged in definition of the problem and that they have actively sought outside engagement.

I fully realize that this is very hard on the young university scholar who genuinely and sympathetically seeks to learn more about people in other cultures or communities. How would such a person at the University of Hawaii or Australian National University engage with people like the Kwato in the Solomons?

What this person can do is contact various peoples' organizations. For example, in the South Pacific the YWCA (Young Women's Christian Association) is completely localized and works on locally relevant issues. The impetus for the movement for the Nuclear Free and Independent Pacific first came out of their Fijian members. There are very simple tasks that outsiders often can do for such organizations while learning enough about local conditions in order to collaborate on any deeper engagements.

For the outsider, this seems incredibly time consuming. One could not start one's real research for several years in many cases, and even then how does one know that local people will be interested in the same questions as the outsider?

On the one hand, I do not want to make the case that all research about local people has to be participatory research. But I recognize that saying this obscures some very serious ethical and political issues. As one islander explained to me, "Why should I be expected to cooperate in any way with some rich person who thinks they have the right to dig around my house for treasure. And then tell me why I shouldn't object when they take the treasure away!" What I call for then is some sustained and deliberate consciousness raising of First World researchers.

If First World researchers go over "there" on government and other grants, then why don't the people from over there come here and research us. Of course, the official response would be, "because they aren't trained experts." Then one suggests the exchange of citizen groups seeking information on common problems. In practice to date this is difficult to fund, while funding Third Worlders to come to the First to learn to be experts is not nearly so difficult.

The fourth chapter's discussion of the participatory researcher's role is recalled. What then, one asks, if national governments are oppressive towards tribal groups such as the Kwaio, would evading national research strictures be justified? This is a difficult question, but certainly more difficult for those junior scholars and practitioners from

outside the Pacific who although keen to learn about the Pacific have no experiential basis nor local contacts on which to make such judgements and risks.

But the preceding is still speaking from the point of view of outsiders wishing to act on Pacific Islanders. If the vantage is turned around, outsiders will have to wait until insiders--Pacific Islanders--make their own determinations. For those Westerners familiar with Melanesia it would then seem that "there is nothing to do, but wait." This only demonstrates the depth of the interventionist imperative to act on others. Yes, acceptance of the ethical implications of research critiques in this work would limit the range of activities of outside researchers in the Pacific. Yet, there are still many tasks to be undertaken if one supports the resolutions of the Young Nations Conference. The call to research into the relations between and among corporations, churches, and various nation states and their effects on rural people has not yet been fully answered. Bishop's case study of Burns Philp's activities cited in this chapter is one of just a handful of good, readable studies that have been done. Winkler's Losing Control cited, also documents transnational corporations in the Pacific context in a form and English accessible to Pacific Islanders.

Given a state shaped by complex class struggle and increasingly penetrated by transnational influences, it is safe only to say that research access will continue, although its form may change as the state technocracy seeks legitimacy with groups and classes who seek greater autonomy. Paulo Freire has said, "The right cannot unmask itself, nor can it sponsor the means for the people to unmask it more than it is willing to be unmasked."³⁵ Depressing as this seems at first glance, there are spaces for oppressed people and their knowledge to be heard, precisely where their governments seek to legitimate themselves. There are roles for research outsiders if they are willing to refuse to make local people into objects.

NOTES FOR CHAPTER VI

¹Song lyrics and translation as supplied by Sabet Cox, director of East Sepik University Centre, East Sepik Province, Papua New Guinea, April 1989.

²See Immanuel Wallerstein, The Capitalist World-Economy (Cambridge: Cambridge University Press, 1979) and Historical Capitalism (London: Verso, 1984b).

³Seventh Pacific Science Congress, "Division - Organization of Research," United States Educational Foundation in New Zealand, 1949, p. 4.

⁴*Ibid.*, p. 4.

⁵For an excellent discussion of this topic see, Bruce Koppel and Edward Oasa, "Trans-global Science Ideology," Development and Change (1987). The theme is that trans-global science ideology is used to justify political practice and further promote capitalism.

⁶Alexander Mamak and Grant McCall, eds., Paradise Postponed (Rushcutters Bay, New South Wales, Australia: Pergamon Press, 1978).

⁷Roger Keesing, "The Politics of Fieldwork: Solomon Islands 1962-1984," paper (Canberra: Australia National University, n. d.), p. 1.

⁸In my experience throughout Melanesia, I find village people blaming anthropologists for quite a lot. I've finally come to realize that in many cases "anthropologist" has become the generic term for any social scientist who works at the village level.

⁹University of Papua New Guinea, Educational Research Unit Research in Melanesia 9 (1984): 66.

¹⁰Cover report, "Studying the Future," Islands Business, January 1989 on publication of: Ron Crocombe and Malama Meleisea, Pacific Universities - Achievements, Problems and Prospects (Suva, Fiji: Institute of Pacific Studies, University of the South Pacific, 1989).

¹¹Mike Bishop and Ann Wigglesworth, A Touch of Australian Enterprise: The Vanuatu Experience (Fitzroy, Victoria, Australia: International Development Action, 1982), p. 34. In this excellent and accessible book, the authors have used the example of Burns Philp to explain the workings of transnational corporations in the Pacific.

¹²Azeem Amarshi, Kenneth Good, and Rex Mortimer, Development and Dependency: The Political Economy of Papua New Guinea (Melbourne: Oxford University Press, 1979), p. 153.

¹³*Ibid.*, pp. 157-158. Amarshi, et. al. point out in their conclusions: "Few truly dynamic influences are now at work within the class structures of Papua New Guinea. No radical educated petty bourgeoisie or educated working class has emerged ready to join urban manual workers and villagers on an organized and continuing basis. But given the maintenance of low-level, dependent capitalism (with few new jobs available in the bureaucracy, and skills being imported from abroad), this could change in the

future. The average level of education among the unemployed is rising, and it is estimated that by 1984 about 30 000 Form 4 (Grade 10) school leavers will be unable to find skilled employment within the country... This development is dramatically different from the situation which applied in the 1960's and 1970's, and represents a potential threat to the rather fragile stability of the post-colonial socio-political arrangements. The threat could be contained by cutting back on educational opportunities and by a greater resort to force, but at the cost of widening frustrations and disillusionment. And it could also bring more radical influence to bear on the urban trade union movement, and help to provide vital direction to the present circle of crime and punishment in the countryside."

¹⁴See Terence Wesley-Smith, "Melanesians and Modes of Production" (Ph.D. dissertation in political science, University of Hawaii at Manoa, 1988).

¹⁵Oswaldo Sunkel and Edmundo F. Funezalida, "Transnationalization and its National Consequences," in Transnational Capitalism and National Development: New Perspectives on Dependence, ed. Jose J. Villamil (Atlantic Highlands, NJ: Humanities Press, Inc., 1979).

¹⁶Fred Block, "The Ruling Class Does not Rule," in The Political Economy, ed. Ferguson and Rogers (Armonk, New York: M. E. Sharpe, Inc., 1984), pp. 32-46.

¹⁷*Ibid.*, p. 37.

¹⁸Te'o I.J. Fairbairn, Island Economies: Studies from the South Pacific (Suva, Fiji: University of the South Pacific, 1985), p. 202.

¹⁹*Ibid.*, p. 205.

²⁰*Ibid.*, p. 207.

²¹Jean-Marc Philibert, "Living Under Two Flags: Selective Modernization in Erakor Village, Efate" in Vanuatu: Politics, Economics and Ritual in Island Melanesia ed. Michael Allen (Sydney: Academic press, 1981), p. 333.

²²Margaret Rodman, Masters of Tradition: Consequences of Customary Land Tenure in Longana, Vanuatu (Vancouver: The University of British Columbia Press, 1987).

²⁴Magnus Blomstrom and Bjorn Hettne, "Beyond Dependency: New Trends in Development Theory," in Development Theory in Transition: The Dependency Debate and Beyond, Third World Responses (London: Zed Books, 1984), p. 174.

²⁵Jim Lamont, "A Little Bit of Everything: A Study of Rural Entrepreneurs Financed by the Development Bank of Vanuatu," paper (Nuku'Alofa, Tonga: University of the South Pacific Rural Development Centre, 1984). See also J. Crocker, The Vanuatu Village Small Business Training Program (Port Vila, Vanuatu: International Human Assistance Programs, Inc. 1985 a).

²⁶My vantage point is also informed from direct involvement in both the Vanuatu Chamber of Commerce and the South Pacific Chamber of Commerce up to 1985.

During the period 1985-current, I have been involved in several key South Pacific conferences where business relations and foreign investment were the main topics.

²⁷Joanna Crocker, "The Nature of Dependency in Vanuatu," paper (the political science department of the University of Hawaii, 1985c).

²⁸Primary source: First National Development Plan of Vanuatu; secondary: OECD 1984 Report; tertiary: Hamnett, Surber, Surber, "Economic Vulnerability in the Pacific, Pacific Islands Yearbook, 15th ed.

²⁹This interview was conducted in the context of a regional review of program planning for IHAP (International Human Assistance Programs, Inc.), a voluntary agency by which I was then employed.

³⁰Wally Hiambohn, "Landowners resort to sabotage in Panguna," Pacific Islands Monthly 59 (January 1989): p. 17. Throughout 1989 the various Pacific news services have carried stories on the armed resistance of Bougainville landowners to the mining operations.

³¹Solomon Islands Development Trust, "Points of Discussion," Link 11 (June-July 1989): p. 2.

³²James E. Winkler, Losing Control: Towards an Understanding of Transnational Corporations in the Pacific Islands Context (Suva, Fiji: Pacific Island Conference of Churches, 1982), p. 69.

³³Bishop and Wigglesworth, A Touch of Australian Enterprise.

³⁴Michel Foucault, "The Subject and Power," in Michel Foucault: Beyond Structuralism and Hermeneutics, ed. Hubert L. Dreyfuss and Paul Rabinow (Chicago: University of Chicago Press, 1982).

³⁵Paulo Freire, The Politics of Education: Culture, Power and Liberation, introduction Henry A. Giroux, trans. Donald Macedo (Mass: Bergin & Garvey Publishers, Inc., 1985), p. 85.

CHAPTER VII

SUMMARY AND SYNTHESIS

When the Nakety decision was made known, people were ready in all the tribes ... They had already counted their children. So if we had been asked right then how many children we had for the EPK we could have already given an idea - we knew how many children we had, how many helpers we had and which tribe they were in. We knew that ...

(Marie-Adele Nechero-Joredie on setting up a Kanak People's School)¹

I

Participatory research where it is labelled as such and carried out as in the Appalachia case is not wholly coopted yet. However, with increasing use of the label by individuals supported by large organizations from the World Bank to the International Development Research Centre of Canada, cooptation occurs despite individual intentions. Such large organizations are institutionally incapable both in practice and language of fully supporting local control or local proprietorship of research and development efforts. The key is in local control.

Cooptation lies in the development and/or modernization imperatives to do something to people. As long as this imperative is not changed, researched people may be viewed as "poor," "incapable," in short lacking something and hence needful of operations by outsiders.

These relations continue to be reproduced in research practices through institutional sponsorship of research, and those organizations' relations to the world economic order which includes current practices of science and applications of technology.

Participatory research, wherein local control is a central value, is proposed as an alternative to usual research practice. Although participatory research is proposed as a worthwhile alternative practice, ideal practice is subject to preemptive and cooptive forces. In order not to destroy the task, it is important to recognize the limits of and limitations on participatory research.

Ernest Mandel in his Late Capitalism makes extensive critique of late capitalism as a whole, discussing the commodification of everything.² This includes knowledge as a product subject to market forces and manipulation. In view of an increasingly commodified world where "Belief in the omnipotence of technology is the specific form of bourgeois ideology," respect for local knowledge seems less likely than ever.³ Mandel states:

To the captive individual, whose entire life is subordinated to the laws of the market - not only (as in the 19th century) in the sphere of production, but also in the sphere of consumption, recreation, culture, art, education, and personal relations, it appears impossible to break out of the social prison ... In reality, however, late capitalism is not a completely organized society at all. It is merely a hybrid and bastardized combination of organization and anarchy.⁴

Mandel is not exceptional in seeing the forces of late capitalism grinding away. Various writers of our time, including Jurgen Habermas, see crises of legitimation appearing.⁵

The preceding chapters, treat in turn the subjects of closure of research access, through the cooptation of participatory research as an alternative practice, and general academic debates on the validity of usual social science inquiry. These chapters were written with the above contradictions in mind. It is precisely in the areas of intellectual or knowledge production that these crises are most manifest. Likewise, it is possible that these are the areas where the potential for creative social change lies.

Again, it is crises that provide opportunities for renegotiation or spaces for politicization. Mandel in his concluding pages puts it most clearly:

This contradiction, however, is an Achilles Heel of late capitalism, even in times of the 'most favorable upswing', 'fastest' growth, and 'broadest' mass consumption. For the more that labour becomes objectively socialized and dependent on conscious cooperation, the more that immediate shortages disappear, and the higher are the educational level and average qualification of the typical producer - all the more intolerable will the direct organization and technical subsumption of labour under capital become to the mass of wage-earners, and with it their social and economic subordination.⁶

In other words, in order to sell itself, the capitalist system produces the ideology of the "American dream" and even "liberatory democracy." Concurrently, the more effective the sell, the higher expectations are raised, whether the expectations involve the persistence of the American dream in the USA or the cargo cult mentality in Melanesia. Thus, perception of the gap between reality and the sell becomes clearer and stronger and frustration increases. Again, it is precisely this intersection of contradiction and crises that opens spaces for alternative practices.⁷

Given the power of homogenizing forces, it is possible to see local knowledge as doomed; however, this is the case only if one wishes to keep cultures perfectly intact and untainted. If this were the case, there would be no point in doing participatory research. In that event, one would engage in participatory research only as an idealistic ethical exercise. Because there are crises and contradictions in our world, there are opportunities for political change. However, to pretend to know the prescription for all of a social problem is a mistake, and anyway, impossible. Thus participatory research should not be seen as a cure or a tool in achieving a grand hegemony over all current research practices.

Discussions of relevant theory, particular cases, institutional limits, and the relation of the state to research have helped to elicit some basic tactics that can be useful in participatory research. Before synthesizing and expanding on those tactics, it is first necessary to reflect on the persistence of certain theoretical practices that lead to exploitative research and to note how the persistent practices are also changing.

II

Where does participatory research reconnect to theory and what good does it do for more than a limited number of participatory researchers, communities, and movements? Can this be any more than an end-run around the huge middle of modernity and late capitalism?

While this does not suggest that localized tactics and strategies are part of a total hegemonic takeover, it does mean that they must be analyzed in a larger context. A great part of that context is a major paradigm shift. Oftentimes competing theories in political economy and development studies have been as hotly debated and labelled as though they evidenced a major paradigm shift, but such shifts have been rare. Certainly, many authors, ranging from radical Marxists to ultra-rightists have claimed to put forth new and competing theories, but they are still working within assumptions about unlimited production and consumption and the power of science and technology to resolve crises. Where these assumptions are unchallenged, there can be significant paradigm shift. Yet, other changes are occurring that are important enough to be considered as paradigm shifts.

Radical theologian Elisabeth Schussler Florenza puts forth a very useful definition of a paradigm:

A paradigm defines the type of problems to be researched, interpretations to be given and interpretative systems to be constructed. Thus a scientific paradigm determines all aspects of scientific research: observations, theories and interpretative models, research traditions and exemplars, as well as the philosophical-theoretical assumptions about the nature of the world and its total world view. All data and recorded observations are theory laden, no bare uninterpreted data and sources exist. Equally there are no criteria and research models that are not dependent on the scientific paradigm in which they were developed.⁸

The old paradigm is going, yet still clinging tenaciously to its bases of legitimation, while change is coming and in many ways already here. To paraphrase Thomas Kuhn, when enough of the old guard die off a paradigm shift may occur.⁹

There is now a distinct paradigm shift. Most recently and notably for participatory researchers, Peter Reason in Human Inquiry in Action has declared the paradigm's arrival:

My personal belief is that our basic philosophical stance for a new approach to human inquiry has been established. It is part of a new world-view which is emerging through systems thinking, ecological concerns and awareness,

feminism, education, as well as in the philosophy of human inquiry. It has been articulated partly by those associated with the development of co-operative inquiry; partly by authors such as Lincoln and Guba (1985), who have provided a quite excellent summary of 'post-positivist' thinking; and partly by books and articles by a whole range of critics of the mechanical and reductionist scientific world-view.¹⁰

The indicators of the arrival of the post-positivist aspects of the paradigm are many. The indicators briefly cited in this work are found in works of liberation theologians, Greens, eco-holistic-spiritualists, quantum physics, new thinking about power and its relation to knowledge such as found in the works of Foucault, and of course, some participatory researchers.

What are the implications of this new respect for local voices and knowledge? This respect does not imply a desire for a return to Rousseau's noble savage, although that sort of past or future utopian dream has been an anchor and point of intelligibility. This paradigm shift surrounds several contested areas, including:

1. person/collectivity
2. relation to nature and others
3. interconnectedness of the preceding
4. time/non-linear time
5. space/geography as that which sustains us
6. purpose of work and play
7. relations of cause and effect and rationality to feeling and myth
8. relations of knowledge and power

So far, these contested areas have not always been connected. After the categorizing of modernity, it is no wonder that the people working and writing in these contested areas resist making the connections where they might lead to homogenization. Here, however it is useful to make a few linkages.

In this inquiry, the key areas are the relation of knowledge to being and thinking and doing, and, as Foucault called it, "the relation of local and erudite knowledges."

Important linkages also occur in the relations between the rational, the emotional, and the mythological.

This is no simple, clear-cut process whereby the new way will suddenly take over all aspects of modernity that are destructive of popular and local knowledge. Yet, what Wallerstein has called the "commodification of everything," and Pacific Islanders might see as the selling of traditional knowledge in the form of dances and festivals staged for tourists, is being contested.

There is a more complementary understanding of these indicators of the larger change that is taking place. To date this is best described by Willis Harman in "The Need for a Restructuring of Science." Harman proposes:

[t]hat science be reformulated. The proposed basis for the reformulation is "the redefinition of the possible." Instead of starting with a limiting bias and having to defend against the anomalous, let us start with the assumption that any class of inner experiences that have been reported, or of phenomena that have been observed, down through the ages and across cultures apparently in some sense exist and have a face validity that cannot be denied.¹¹

Parallel to earlier discussions of scientism in this dissertation, Harman likens enculturation to hypnosis:

Experienced reality is, to an undetermined extent, socially constructed ... Science, then is a cultural artifact of Western society ... One of the consequences, however, has been the necessity to deny the validity of a host of phenomena that don't fit within those limits. As a result, a tremendous amount of effort has gone into defending the barricades against, or explaining away, these outcasts ...¹²

Harman attributes three main movements to a restructured science. The three movements are:

1. From objectivism to participation, wherein one gains knowledge by identifying with the observed.
2. From reductionism to holism, wherein one deals with a whole ecological system or a whole human organism.

3. From positivism to consciousness as causal reality, wherein "downward causation, according to which things are controlled not only from below upward by atomic and molecular action but also from above downward by mental, social, political, and other macro properties."¹³

The nature of the new paradigm shift is such that its overarching aspects are difficult to put into perspective. Certainly, an important aspect of the old paradigm of science is that there is a single underlying Truth and hence a single model of the world. Again, one might seek theoretical victory of the new paradigm over the old. This obscures what is happening.

To put the paradigm shift into perspective, one might turn to some of the recent discussions about soft systems analysis. In the soft systems approach to research and analysis, instead of looking for key problems and subsequent solutions, the approach is to look at situations rather than problems. Soft systems analyst John Naughton explains the approach as one where social systems are perceived in subjectively different ways by their individual members and there is a simultaneous legitimacy to these different ways of seeing.¹⁴

In simpler terms, what is changing most is the underlying assumption that there must be one main Truth at a time, or a "golden key."

Naughton, cites what he sees as the present condition and context of soft systems analysis:

In those social sciences whose concerns are closest to those of the systems movement, for example, rival and incommensurable paradigms continue to co-exist with no sign of a revolutionary triumph in sight. This could well be what happens in systems also - i.e. the two traditions [hard and soft systems] continue to exist, and to attract adherents and practitioners, but with an evolving consensus about what is the proper domain of applicability of each.¹⁵

It is this co-existence of what is seemingly contradictory that is evidence of an important shift. These very tentative first signs of the shift suggest possibilities for new

ways of approaching research. But again, seeking one approach or system triumphant over others obscures the emerging situation.

IV

How can we write about this new paradigm? By excising from our discourse (both practice and writing which is itself practice) the words "development," "progress," "modernization," "neutral technology," and "objective research." "But!" one interjects, "there must after all be some good development and that will need some good research by experts; people do have real problems, and the world is quite a mess!"

Nevertheless, the way these categories are constructed is so fraught with the danger of cooptation--as we have seen in the very definition of participatory research--that it may be better to abandon these categories. Retaining these labels and categories, even with qualifications, is dangerous, because it is too easy. One sees the community developer, who has finally realized that her well intended interventions have done nothing but to make the community more dependent on outsiders, saying, "Participatory action research, at last, a way to do things in the community!" Where the doing is not necessarily negative, the underlying impatience with listening, waiting for people to do their own analysis, waiting to be called is experienced as extremely negative.

On a theoretical level, one finds more and more scholars engaged in post-structural analysis. On realizing how very dangerous their own former notions of Truth (and that includes their own research approaches) and so on have become, they see everything as dangerous--or at least not discernibly ordered outside of our own ordering. Any ordering of things, to paraphrase Foucault, or searching for underlying causes, is heavily laden with a will to power. At this juncture, such theorists might characterize the nature of the universe as chaotic. This characterization in turn has its

own totalizing effect as it explains away and invalidates choice-making and action, patterns and linkages.

As I began to write this dissertation, one important aspect of my theoretical and methodological approach was to analyze social science research on the Third World by focusing on participatory research. I believed that Foucault's genealogical approach would be useful for purposes of critique and analysis, but like many of Foucault's critics I thought it would have to be abandoned when it became necessary to propose tactics and actions.

After continuing through the dissertation, I have come to see that within Foucault's work much is implicit, and it is worthwhile to use these implicit indications as a launching ground. More precisely, Foucault's call for a realignment of erudite and local knowledge holds potential where it is not disconnected from his statement that "the greatest indignity is to speak for the other."¹⁶ This does not preclude local actions based on both local and global analysis and reflection and action cycles, nor does it invalidate the work of academics where they take their own centers of learning as worthy of analysis and make themselves subjects in that process. Foucault addresses this also:

But if we are not to settle for the affirmation or the empty dream of freedom it seems to me that this historico-critical attitude must also be an experimental one. I mean that this work done at the limits of ourselves must, on the one hand, open up a realm of historical inquiry and, on the other, put itself to the test of reality, of contemporary reality, both to grasp the points where change is possible and desirable, and to determine the precise form this change should take. This means that the historical ontology of ourselves must turn away from all projects that claim to be global or radical.¹⁷

Thus what is again proposed here is a stronger emphasis in action-oriented research on the following:

1. Moving freely from the specific and local to the general and global and back again.

2. Linking practice and reflection and making every effort to resist the delinking that creates separate categories of theory and action done by separate people.

3. Going beyond listening to the other, and instead making spaces where the other may be heard, or better yet, listened to.

The goal of the above practices is letting otherness be. Foucault's approach seems crucial in this respect, because it denies one truth for everyone.

However, for the university-based participatory researcher and the intellectual, discontinuities appear when Foucault's approach is applied to some cases. Foucault has predicted that most anti-authority struggles would be immediate; citizens would see the enemies as those close by rather than as structural causes. Citizens would also tend not to seek change in larger-scale actions. As we have seen in the cases in the fourth chapter, people were able to organize and control strategic activities that led to some more than immediate changes. This affirms that politics as a positive practice is possible, and subverting institutional practices, while necessary, may not be all that is required.

Here we return to the original query of the dissertation: Are there strategies available for sustaining critique and altering existing dominating practices of First World research in the Third World? This question may be answered, but first it is important to describe how it should not be answered.

There are two insidious practices and assumptions in most research. The first is the will to power which leads to constant attempts to derive action guides from theory. The second is the imperative to intervene in the lives of those people we have divided from us, which results in our seeking solutions to their problems with them.

If I refuse to talk too much about what I would do to those people out there, then it must be that I think the problems are also where I am as a First Worlder. Originally, I had not planned on discussing the role of the First World research

facilitator, because so much has already been written about pedagogical techniques-- techniques that are manipulations to get those people to perform the prescribed actions. As work progressed, I realized it was not enough to suggest that the facilitator take an ethical position; something more specific was required to make clear the nature of the role.

For the above reasons, I have proposed a double helix of actions. In the first strand of the helix, the participatory researcher and the community with whom she works do need to engage in critique of the existing situation. This activity may entail information gathering, analysis, and subsequent action. In the first half of the second strand, the outsider or leader(s) of the process may bring in their own specialized knowledge and skills, but this is to assist community members in getting the information and actions they want, as well as to keep outside institutions out of the way. The second half of the second strand must be left open; this is where the community members decide what to do based on their own subjective experience of their problems. An outsider simply cannot theorize on any encompassing level about how to guide a community along the second half of the second strand. This knowledge and even wisdom can only come out of specific lived relations and experiences.

The activities of such a research facilitator are based on a willingness to accept the validity of the other's life experience. During learning and analysis processes the facilitator must be willing to let people experiment and make mistakes. "Why should we have to listen to the advice of those who never have to live with the results?" a Ni-Vanuatu leader observed?¹⁸

Echoing previous calls in this dissertation (as well as in others from Harman to Freire), a restructured science would then be characterized by participatory scientists in terms of "a willingness to be transformed" and living in a universe of infinite choices rather than in chaos. Earlier calls to deal with subjective experience have been

repeatedly rejected by the main body of scientists as at best personalizing. As Harman puts it:

Perhaps it will come forward now in more sophisticated form. At the level of "physical reality," admissible data is primarily in the form of quantifiable physical observation. At the organism level, somewhat more holistic kinds of observations (requiring subjective judgment) become important ...¹⁹

This new social scientist would also work in a consciousness characterized by Gramsci as:

[T]he philosophy of praxis ... a philosophy that has been liberated (or is attempting to liberate itself) from any unilateral and fanatical ideological elements; it is consciousness full of contradictions, in which the philosopher himself, understood both individually and as an entire social group, not only grasps the contradictions, but posits himself as an element of the contradiction and elevates this element to a principle of knowledge and therefore of action.²⁰ [Emphasis added].

Finally, in positing that a clear movement toward a "restructuring of science" exists, one needs to beware of the dangers of taking this position. Taken too easily, it may merely contribute to the continuing legitimization of manipulation of knowledge that harms many people, and even the earth itself. After all, as we have seen, there are increasing numbers of social science researchers and community development workers hopping on the bandwagon of "participatory research" where that research continues to legitimize intervention and interference in local communities rather than locally initiated self-analysis, action, and control. Nevertheless, worthwhile choices do need to be made and the supporting arguments need to be made intelligible.

As this dissertation progressed it has still been necessary to argue forcefully, as does Adrian Hnangan in "Kanak Aspirations:"

We're not savages, we're human beings first and foremost. If they let us sort out our own land, they'll have more than they had before. People have refused to understand this. But there's no point in discussions if they still think of us as inferior. When you talk with a Caldoche [white French inhabitant of New Caledonia], even if you're a university graduate, you're still a Kanak, a piece of dirt. I'm sorry to have to talk like that, but there you are.²¹

Concurrently with the production of this dissertation, the author, as mentioned in the preface, had a major involvement in a year-long series of seminars and forums that led to a major international workshop, "People's Initiatives to Overcome Poverty." The broad background of most of the participants included extensive experience in participatory research and development, yet one host institution evaluator of the workshop proposal commented:

In societies that approach a state of equality of opportunity, the people at the bottom of society tend to be those who lack whatever abilities it takes to get ahead ... This line of inquiry leads one into the role of genetics in poverty.²²

Over and over again during the production of this dissertation, through surveys of literature and actual interactions with so-called poor people, structural causes were far more important than other aspects of the so-called "poverty" problem. Although marginalized people frequently internalize the values of their oppressors, once drawn into an analysis process most are quite capable of making sense of the linkages and structures that keep them marginalized. It is this that is represented in the opening epigraph of this chapter; the people in New Caledonia did not need a lot of fancy experts from Paris telling them how to set up their school. Their own research was exactly adequate to their objectives; "they knew that." In the final analysis, no one knows about his or her situation as well as the person experiencing it. Thus, people have a right to their own self-determination and control over their own knowledge base and access to knowledge that affects them is part of that self-determination.

Discussing action research work in factories, William Whyte affirms:

one tries to map the employees' theory onto the managers' theory, one finds that at the center of each theory is the same complex of problems. Its implications however are much more developed in the employees' theory: theirs is richer more complex and more extensive. The important result concerning action potential is that the employees' theory included the two action possibilities suggested by management, plus numerous additional possibilities ... The main point is that with only minimal external support people can develop a quite adequate theory of their own situation [emphasis added].²³

My sources overwhelmingly come to the same conclusion: people can do their own information-gathering and analysis. Apparent lack of knowledge should not be turned into a deficit theory of knowledge where "we know, they don't," because this inevitably leads to acting on peoples as if they were inert objects. There are varying degrees of objectification of researched populations, but they tend to move from utilization of limited resources for research not deemed useful by the researched population, to creating dependency, and/or undermining self-analysis and self-determination.

There is a role for outsiders or insiders. These roles are often quite modest in terms of skills or basic support. What outside intellectuals can do, which is very important, is to take seriously the critique side of my helix model and help make space by revealing, unsettling, or otherwise changing their own practices and those in their own institutions. This is what I mean by "studying up."

V

Finally, is participatory research social science? What happens to science if peasants or local people make an analysis--especially when no outsider is involved? If there is no "real scientist," is it still science? If one holds to the type of participatory research that is discussed in the cases of the Appalachian land survey and the Vanuatu culture survey while accepting the emerging paradigm outlined here, yes. If the notion of science is expanded to include all forms of knowledge, most definitely yes.

Nevertheless, this proposition will remain most unattractive to many mainstream researchers for reasons discussed throughout this work, which amount to accepting the essential connection between knowledge and power. Accepting this proposition entails accepting a degree of self-analysis and patience with process and local description, accepted in its own words, which is almost totally foreign to those raised where the rational is supreme and the emotional and mythological only

peripheral. Further, the traditional researcher's very legitimation to continue working is undermined. If local knowledge is not inferior, then much of the underlying rationale for intervention is undermined.

The cure then is not to attempt a radical revolution towards achieving hegemony over all the current operations. Rather, what is attempted is an unsettling of the usual categories and a search for places where the ideological structure of the scientific world manifests the strain of repressing social change. Then one works on those places where they are labelled as "good." The Appalachian land use survey again presents itself; when the ARC said its research was neutral, that enabled the citizens' Task Force, to in effect, say, "Good, that means you couldn't possibly mind if we take the research topic to be land use and its relations to taxation."

I would also suggest that a partial hegemony might be useful for letting otherness be. That partial hegemony would be over the definition of participatory research; it could more clearly be based on local control.

One reviewer has repeatedly asked me, "If all these local cultures are going to be homogenized into the world-economy, what's the point of respecting local knowledge?"

If you conceive of culture as a changing pattern of practices that moves in response to specific groups of people's perceived ideas about the total environment, then yes, some of those local cultures will change, even disappear. They will disappear most quickly where a culture is attached to relatedness to a specific piece of land and the people living in it do so in isolation.

But the totalizing practices and effects of Western thinking are so great as to make invisible the practices that are going on side by side. Conceptualizing a global, world-economy is a useful model for critique, but tends to have one looking for a single, counter-hegemonic project, a golden key as it were. Since no convincing counter-

hegemonic project exists the many other practices going on side by side become less visible; we are again mesmerized by the culture of science and a single truth.

As quickly as the world-economy is erasing old, primal, local cultures, new ones emerge. New cultures are built around not a culture of the land, as in Melanesia, but of work. One might include truckers or computer hackers. The difference is that these new cultures are always fluent in two cultures instead of the one primal culture.

Several truths exist simultaneously. Participatory researchers can choose to treat and interact with other cultures as if they mattered, rather than attempting to "develop" them towards a goal pre-selected by mainstream ideologies and institutions.

Foucault has said:

Intellectuals must renounce their old prophetic function. In the same way, I am not thinking only of their pretension to tell what is going on, but their legislative function to which they have so long aspired... I dream of the intellectual destroyer of evidence and universalities.²⁴

As a participatory researcher and university-based intellectual, I too have a dream. I think of the fisherman who was too embarrassed by his lack of education to say a single word in front of lawyers and experts about his threatened livelihood. Then, I dream of a world where specialists would be embarrassed to make pronouncements about communities without first deeply engaging with that community.

NOTES FOR CHAPTER VII

¹ Marie-Adele Nechero-Joredie, "A Kanak People's School," interview in New Caledonia: Essays in Nationalism and Dependency, ed. Michael Spencer, Alan Ward, and John Connell (St. Lucia, Queensland, Australia: University of Queensland Press, 1988), p. 202.

² Ernest Mandel, "Ideology in Late Capitalism," in Late Capitalism, (London: Verso, 1978).

³ *Ibid.*, p. 501.

⁴ *Ibid.*, p. 502.

⁵ Anthony Giddens, "Habermas," in The Return of Grand Theory, ed. Quentin Skinner (Cambridge: Cambridge University Press, 1985), p. 136.

⁶ *Ibid.*, p. 584.

⁷ I would here note that, although Mandel's critique and explication are extensive and eloquent, they are limited in two major ways. First of all the instrumental nature of the bourgeois class and second the totalizing effect of describing all problems rooted in the proprietorship of means of production. I am well aware that this totalizing effect is equally operating in a description of current research activity as rooted in proprietorship of means of production of knowledge. Nevertheless, the work at hand is more self-conscious and by using genealogical approaches attempts to counter the worst of the effect.

⁸ Elisabeth Schussler Fiorenza, In Memory of Her, (New York: Crossroad, 1986), p. xxi.

⁹ Thomas Kuhn, The Structure of Scientific Revolutions, 2nd ed. enlarged (Chicago: University of Chicago Press, 1970).

¹⁰ Peter Reason, Human Inquiry in Action: Developments in New Paradigm Research (London: Sage, 1988), p. 3.

¹¹ Willis W. Harman, "The Transpersonal Challenge to the Scientific Paradigm: The Need for a Restructuring of Science," in ReVision 11 (Fall 1988): 15. Note that both Peter Reason and Willis Harman draw heavily on the writing of Morris Berman who is cited at length in this work in earlier chapters.

¹² *Ibid.*, pp. 14-15.

¹³ Harman, "The Need for Restructuring Science," p. 17.

¹⁴ John Naughton, Soft Systems Analysis: An Introductory Guide (London: The Open University Press, 1984), p. 13. Key soft systems analysis documents that could be useful for action researchers include: 1) Richard J. Bawden, "Systems Agriculture: A State-of-the-Art Assessment," Hawkesbury Agricultural College, Richmond, Australia; 2) Peter B. Checkland, "Rethinking a Systems Approach," Journal of Applied Systems

Analysis 8 (1981); 3) Peter B. Checkland, "From Optimizing to Learning: A Development of Systems Thinking for the 1990s," University of Lancaster, U.K.; 4) Kathy Wilson, "Looking at Situations Rather than for Problems," Department of Urban and Regional Planning, University of Hawaii, Honolulu, Hawaii; and 5) Kathy Wilson and George Morren, Systems Approaches to Food and Agricultural Situations (New York: John Wiley and Sons, 1989 in press).

¹⁵Ibid., p. 16.

¹⁶Michel Foucault and Gilles Deleuze, "Intellectuals and Power," in Language Counter-memory and Practice: Selected Essays and Interviews, ed. Donald F. Bouchard (Ithaca, NY: Cornell University Press, 1977).

¹⁷Michel Foucault, "Truth and Method," in The Foucault Reader, ed. Paul Rabinow (New York: Pantheon Books, 1984), pp. 45-46.

¹⁸Deputy Prime Minister Sathy Regenvanu to me in April 1985, Port Vila, Vanuatu, discussion about a woman's club that was "making mistakes."

¹⁹Harman, "The Need for a Restructuring of Science," p. 18.

²⁰Antonio Gramsci, Selections from the Prison Notebooks, ed. Quintin Hoare (New York: International Publishers, 1983), pp. 404-405.

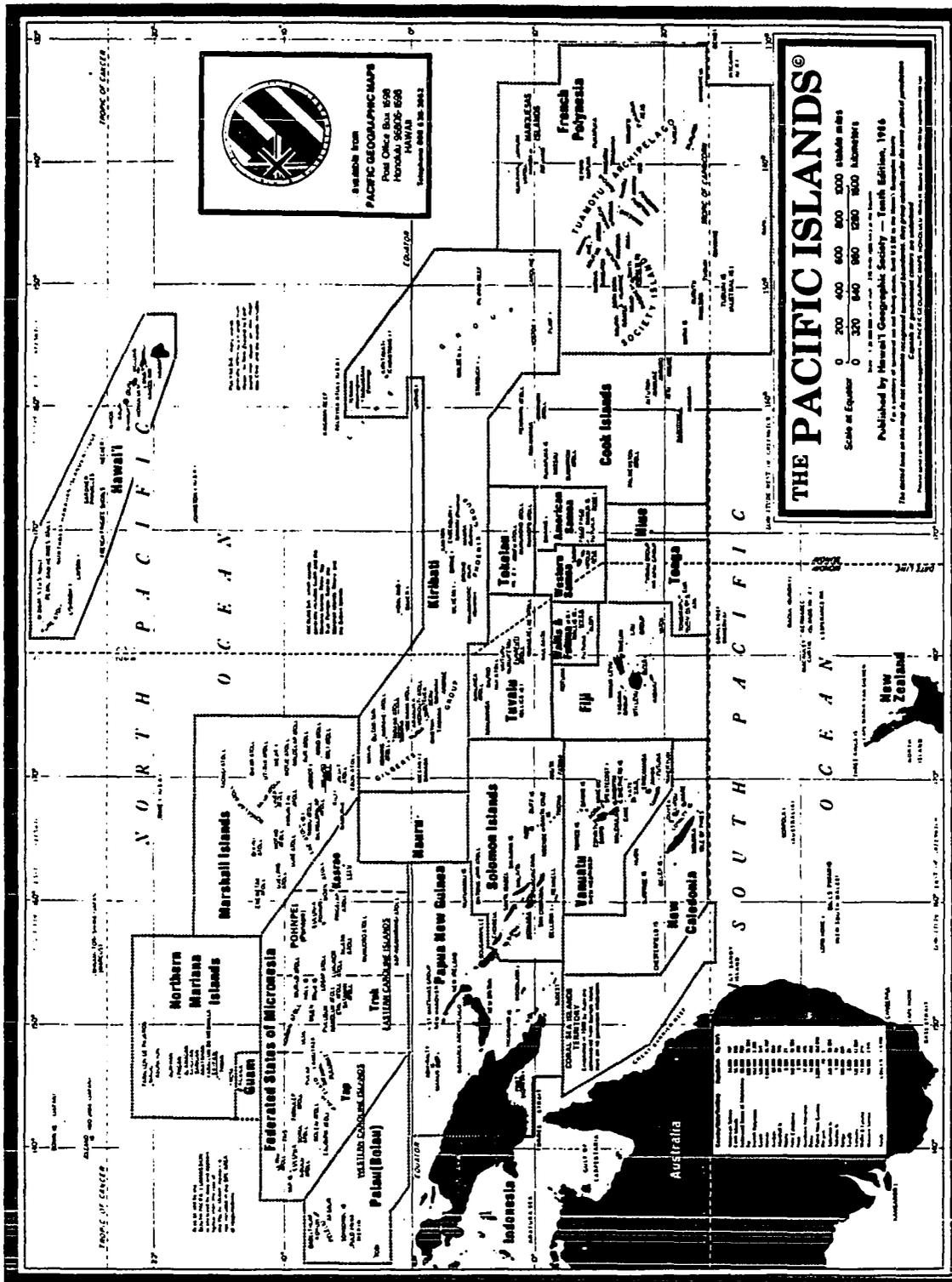
²¹Adrian Hangan, "Kanak Aspirations," in New Caledonia: Essays, p. 226.

²²East-West Center, internal memo to the project leader of the workshop "People's Initiatives to Overcome Poverty," held in Honolulu, Hawaii March 25-April 5, 1989. Here one is tempted to suggest research into the genetic causes of the frequent practice of middle-class, middle-aged, European descended males that totally negates the dignity and self-respect of populations that they purport to be helping.

²³William F. Whyte with collaboration of Kathleen King Whyte, "Types of Applied Social Research," chapter 10 in Learning from the Field: A Guide from Experience (Beverly Hills: Sage, 1984), p. 172.

²⁴Jean-Marie Charpentier, Henri Israel, and Christophe Paris, coordinators, Michel Foucault: Une Histoire de la Verite (Paris: Syros, 1985). Michel Foucault in conversation with Jean Daniel.

APPENDIX A



APPENDIX B. 1.



MINISTRY OF HOME AFFAIRS

P O Box 2349
Government Buildings
SUVA

TEL NO. 312622
TELEX: 2342 HOMSEC FJ

OUR REF. NO.
IM/G/1-88
YOUR REF. NO.

2nd August, 1989

Joanna Croker Fukino
East-West Centre
Box 1062
Honolulu
HAWAII 96848

Dear Nadam

Your letter dated 12/07/1989 is acknowledged.

You will have to first seek the decision of the Permanent Secretary for the Education Research Committee who will be kind enough to furnish you with relevant details for your preferred reseaych. For your information this office considers decisions on the application and issue of research permits, concurrent with the above committee's submissions and recommendations.

Permit forms are available at this office at a fee of one dollar Fijian (F\$1.00). On application research permits are levied one hundred dollars Fijian. (F\$100.00) submitted together with 2 passport size photographs, medical report, police report and certified copies of your acquired qualifications or vitae. Once approved research permits are normally valid for six months, extenable, only after you have permission from this office and conditionally after you have submitted a detailed report likewise.

I look forward to hearing from you soon.

Yours faithfully



Permanent Secretary for Home Affairs

CT/as

215

APPENDIX B. 2.



**SOLOMON ISLANDS
GOVERNMENT**

Telegrams: EDUCATION HONIARA

Ministry of Education & Human
Resources Development
P.O. Box G28
Honiara
Solomon Islands
Telephone:

Joanna Crocker Fukino
East-West Center
Box 1062
Honolulu
HAWAII 96848

Your Ref:

Our Ref: MEHRD/R/1/1

Date: 1/8/89

Dear Madam,

RESEARCH PERMIT APPLICATION

Thankyou for your still interest in Solomon Islands.

Not all research banned and not at all. But since the New Government took over from the Alebua Government the Research Division in this Ministry established new post and the ~~New Govern~~ Research Committee to screen properly *all* the incoming research applications.

If you still interest in Solomon Islands to do some research in your area of interest ~~then~~ don't hesitate to complete the attached application and return to me for my Committee screening.

Yours faithfully,

A handwritten signature in black ink, appearing to be 'J. Solomon', written over a horizontal line.

J. Solomon
Research Officer (Ag)
for: Permanent Secretary/MEHRD

APPENDIX B. 3.

FROM: R.A.

RESEARCH APPLICATION

1. Name:(Underline Surname)
.....
2. Address(es) (if more than one give all)
.....
3. Curriculum Vitae: (Please include copies of previous research)
.....
4. Subject(s) to be studied:- (provide in detail)
5. Areas where research work (specify by giving name(s) of area
where intend to carry out Research work) to be conducted:-
.....
6. Details of Funds:(level and
..... sources of
..... funding eg:
..... private or from
..... a foundation)
7. Methods of Research/Filming/Recording:
.....
(Details of the methods used)
8. Arrangements for accommodation in the area(s) of Research:
.....
9. List all possible ideas regarding the utilization of all research
results:.....
.....
.....
.....

10. Benefits to Solomon Islands: (List them)
-
-
11. Name and address of my person(s) in the area(s) willing to help you (a letter from the person(s) should accompany this application):
-
-
12. Do you intend to make collections of material culture or scientific specimens? (If Yes, Please list all specimen(s):
-
-
-
13. How long will the research take?
- (specify dates if possible):.....
-
-
14. Do you want to make any other additional statements?
-
-
-
-
-
-

15. Two referees who can certify your background by letters:

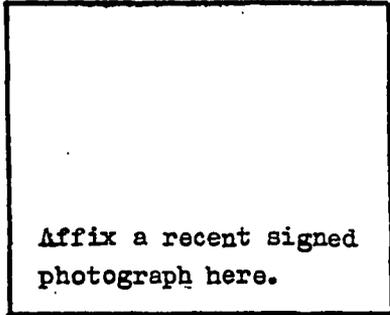
(a) Name:

Address:

(b) Name:

Address:

16. Signature: Date:



cc:

APPENDIX B. 4.

REPUBLIC OF VANUATU
NATIONAL PLANNING AND
STATISTICS OFFICE
Office of the Prime Minister

PO Box 741, Port Vila
Vanuatu (South West Pacific)



REPUBLIQUE DE VANUATU
DIRECTION DU PLAN
ET DE LA STATISTIQUE
Bureau du Premier Ministre

B.P. 741, Port-Vila,
Vanuatu (Pacifique Sud Ouest)

Our/Notre Ref : 042/1/3/LF

Your/Notre Ref :

Date : 20 June 1989

Ms. J.C. Fukino
East West Centre
Box 1062
Honolulu, HI 96848
U.S.A.

Dear Ms. Fukino,

I refer to your letter of 14 March. My apologies for this very late response.

In response to your queries on research there are no hard and fast rules. Government assesses each case on its own merits.

Should you or your colleagues wish to undertake research in Vanuatu I suggest you write to:

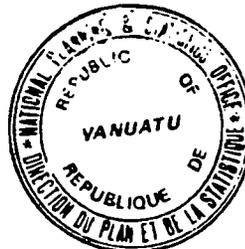
The Secretary for Foreign Affairs and
Judicial Services
Private Mail Bag 051
PORT VILA (Vanuatu).

with an outline description of the field of work you wish to pursue and a request to undertake the work.

Yours sincerely,

A handwritten signature in dark ink, appearing to read 'J.A. Ellis'.

for: Jules A. ELLIS
Acting Director, NPSO.



APPENDIX C

RESOLUTIONS FROM YOUNG NATIONS CONFERENCE

Resolutions and recommendations discussed and endorsed at the Plenary Session of the Young Nations Conference, University of New South Wales, Australia, August 27, 1976.

1. That all social scientists working in the Pacific recognise the need to communicate knowledge and assist politicians and/or advisers to understand how problems of Pacific people are related to local and international political conditions and also to examine ways in which local people can become more involved in political processes so as to bring about widespread and beneficial change.
2. That researchers recognise the need to examine the relationship between systems of domination and exploitation on the one hand, and conditions of Pacific peoples who are oppressed and exploited on the other.
3. That foreign researchers be encouraged to study their own social and cultural systems, power structures, and elites, rather than those of South Pacific countries alone. Three areas which are in need of research are:
 - (a) The involvement of Christian Missions in the Pacific, including Church investment in foreign-directed business enterprises operating in the Pacific region.
 - (b) The motivations and aspirations of universities and their links with the power structure.
 - (c) Resesarch into policies of multinational firms whose decisions affect island nations, including activities and impact of these foreign-based business organisations in the Pacific.
4. That all governments, universities, and other organisations acknowledge Pacific island researchers as well-qualified to study change and development in the Pacific.
5. That researchers working in the Pacific be required to remain long enough so as to familiarise themselves with local conditions (social, economic, and political), and to engage in resesarch involving the active participation of local people.
6. That Pacific governments be encouraged to provide clear guidelines for Pacific research, and to review these guidelines periodically.
7. That researchers be encouraged to translate their findings into simple language and/or into the vernacular.
8. That Conference participants express concern over any criteria adopted by various research and academic institutions for research choice and research appointments which inadvertently act to desensitise researchers to the genuine needs and concerns of the research population.
9. That research on the social and environmental impact of multi-national corporation activities in the Pacific be encouraged.

10. That the products of research contain information on the methods of investigation and other useful material so as to assist in the training of potential researchers from the host countries.
11. That researchers avoid topics that have been heavily researched.
12. That positive guarantees be given by researchers and institutions supporting researchers that the products of research be made readily available to members of the host country, and in particular the research population.
13. That foreign researchers state clearly, both before and after completion of field-work, their academic and non-academic reasons for engaging in Pacific research.
14. That more attention should be given to research in metropolitan countries, including studies of the social, political, and economic implications of their government's policies on the Pacific, and that the products of such research be provided to people living in the Pacific.
15. That researchers be encouraged to investigate what is being done in China, Vietnam, and other countries to cope with the problems of balancing economic needs and interests.
16. That researchers make every attempt to encourage the research population to express their needs and aspirations, and to incorporate these in their work.
17. That Pacific Island governments encourage their own people to recognise the need to become more self-reliant, both economically and culturally.
18. That conference participants recognise the social and economic implications of almost total dependence by many Pacific peoples on imported foodstuffs.
19. That the spread of Pacific elitism be halted and Pacific elites be made more aware of the consequences of their policies and actions.
20. That Pacific Island Conference members do all they can to impress on their governments the need to introduce political change when necessary in order to ensure a more responsible system of government.
21. That appropriate technology programs be encouraged at the village level with village people taking a major role in the planning of such programs, and incorporating indigenous skills and thought processes.
22. That researchers should communicate to others about the negative and positive effects of technology.
23. That metropolitan countries be encouraged to re-examine the migration process and modify their policies so as to provide equal opportunity to Pacific Island migrants.
24. That every encouragement be given to an exchange of peoples (students, workers, etc.) between countries in the South Pacific, and between the South Pacific and metropolitan countries.

25. That this Conference deplores the continued existence of multi-national corporations in the Pacific without promoting even and widespread development and assisting locally-initiated business enterprises.
26. That information about the consequences of various foreign-aid initiated projects be made more widely available to Pacific peoples and Pacific researchers.
27. That Conference participants endorse the resolution adopted by the recently held conference of UNESCO national commission of Asian and Oceania for the establishment of a social science research training and documentation centre for the region. Further, participants welcome the UNESCO effort to organise a world conference on educational information and communication and hope that the Pacific region will be adequately represented in the conference.
28. That Conference participants recognise the urgent need for the establishment of a centre to collate data and disseminate information on past and present research activity in the Pacific, and request consideration be given to establishing such a centre independently or through an established organisation such as the South Pacific Commission, in each region.
29. That a radio service be established between universities and other educational and research institutions throughout the world, and that each government be requested to seek frequency allocations for this purpose through the international telecommunications union. Frequencies to be included are parts of the high frequency and very high frequency range including provision for very high frequency facilities for space communications similar to those used in the Pacific PEACESAT experiment.
30. That Conference members recognise the need to communicate more frequently with our neighbours in Micronesia, and that Micronesians be included in future meetings of this type.
31. That the Conference urges the Director of SPEC to arrange for wide distribution and discussion of the task force review (More Effective Aid) prepared for the South Pacific Forum governments.
32. That as a follow-up to the Young Nations Conference, workshops and a further conference on specific issues to be suggested by Conference members be held in the South Pacific.

Resolutions Committee

Epilia Hau'ofa (Tonga)
 Grant McCall (Australia)
 Ruth Lechte (Fiji)
 Boio Daro (Papua New Guinea)
 Filimone Jitoko (Fiji)
 Alexander Mamak (Australia)
 Stewart Kingan (Cook Islands)
 Thomas Anis (Bougainville Island, Papua New Guinea)

APPENDIX D

INTERNATIONAL SCHOLARLY EXCHANGE:

A STATEMENT OF PRINCIPLES

[Issued by participants of the East-West Center Conference on Research, Access, Equity, and Collaboration, held in Honolulu during December 1985.]

The generation of knowledge benefits all societies by increasing capacities for choice, enriching diversity of cultures, strengthening human values, and preserving the integrity of our environment. Scholars contribute to their societies by the systematic quest for knowledge and discovery.

Although knowledge flows from the insights of individual scholars, the generation, validation, and diffusion of that knowledge is greatly expanded through international intellectual collaboration and deliberate scholarly exchange. The special concern of this statement is with scholarly exchange and cooperation across national boundaries to enhance the development and scholarship of all nations. We urge all scholars and those who facilitate international exchanges to support the principles expressed in this statement.

I. The Spirit Scholarship:

The fundamental objective in international scholarly exchange should be the advancement and diffusion of knowledge.

Access: To this end maximum possible access to information and research settings should be provided to all scholars within the limits of appropriate constraints.

Equity: All involved in the pursuit of knowledge deserve mutual respect.

Collaboration: Cooperation and collaboration are appropriate means for many foreign researchers to relate to local scholars and institutions. Collaboration proceeds from productive partnerships among individuals and sustained linkages among their institutions, respect for the social norms and cultural values of societies under study and the conduct of research which results in mutual benefit. Even when formal collaboration is not feasible, foreign researchers have an obligation to interact responsibly with scholars and other host country nationals.

II. Elements of International Scholarly Exchange:

A. Foreign Researchers: The respect of foreign researchers for their host countries is crucial to the success of scholarly exchange. Foreign researchers should seek to build mutually rewarding relationships with local counterparts and to involve them wherever feasible in the research process. Foreign researchers should share research skills with indigenous scholars and be willing to contribute to the development of scholarly resources and academic programs in host countries. Foreign researchers should become familiar with local research and development goals, and, where possible, orient their research to contribute to host country needs.

B. Host Governments: Agencies of the host country which frame the general conditions under which foreign researchers work in their countries should implement regulations consistently, act promptly on applications to do research, and make clear to foreign scholars the reasons for their denying access. National policies on access should be disseminated widely. Facilitating agencies can play invaluable roles in supporting collaborative arrangements with local counterparts, interpreting the role of foreign researchers to other domestic agencies, and making the results of research available to relevant agencies and interested audiences in their countries.

Sponsoring Agencies: Sponsoring agencies enable researchers to do cross-national research. These organizations should, where appropriate, encourage and select research projects that are genuinely collaborative in conception and implementation. They should suggest ways to harmonize foreign research projects with the research agenda of host nations. Such agencies should also provide financial support for supplemental services to local nationals to facilitate scholarly interaction. These agencies should increase flexibility in research projects by reducing funding restrictions for grants, particularly with respect to timeframe.

D. National Counterparts: Local scholars are urged to assist foreign researchers to gain access to sources of information and provide guidance in appropriate scholarly behavior and expression in the host country. They can also be helpful by interpreting to communities the nature, purpose and value of the research undertaken by foreign scholars. Where possible and appropriate, each researcher should work closely with a national counterpart.

E. Graduate Students: Young scholars may also be able to contribute to international scholarly research, independently or as members of teams. Where feasible, senior researchers in foreign settings should engage scholars-in-training from both their home and host countries to develop the younger generation of researchers.

F. Data: Scholars in the host country should have access to all data collected by foreign scholars in the course of their research, since such data can be of considerable value to future researchers, both local and foreign.

G. Research, Publications and Information Exchange: Credit for discovery and royalties, if any, should be allocated according to contribution without distinction of country of status. Research findings should be in the public domain, both locally and internationally. There should be strong support for libraries, archives, databanks and publishing facilities among nations. It is particularly important to disseminate relevant research findings from developing countries.

III. Professional Standards for Openness of Inquiry in International Scholarly Research: Conducting their research in new social and intellectual environments, foreign scholars must appreciate the norms for appropriate scholarly behavior in the host society. While respecting the universal value of academic freedom, foreign scholars should recognize that responsible exercise of academic freedom must be sensitive to other values that serve the public good of the host society. These values include:

A. Preservation of National Security: The work of foreign scholars should be consistent with the requirements of national security in the host country. They should not violate guidelines restricting access to regions or data. Foreign scholars should also refrain from participating in the politics of the host country. Nor should they circumvent domestic regulations or laws, such as those relating to the export of cultural artifacts. The scholarly intentions of the foreign researcher must be genuine.

B. Respect for Cultural Norms: Foreign researchers should inform themselves about and respect local cultural and social mores of the host country. They should also protect the rights of human subjects involved in research through appropriate safeguards.

C. Preservation of Research Material and Opportunity: Mindful that the observer inevitably influences the observed, researchers should respect cultural and natural systems under study to avoid destroying future opportunities for research. Foreign scholars should endeavor to leave research settings more open and responsive to subsequent inquiry than when they arrived.

This statement of principles was drafted and endorsed by an international conference on Access, Equity and Collaboration in East-West Scholarly Exchange held at the East-West Center in Honolulu, Hawaii on December 11-15, 1985.

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