

*Social Research in Nepal: A Critique and A Proposal **

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The basic objective of this paper is two-fold. The first is to sketch certain dominant substantive and epistemological biases embedded in the institution and practice of social research in Nepal. The second is to forward a few tentative suggestions towards making social research speak more directly on the limitations, hopes and fears of our people, and of ourselves.

The paper consists of five sections. This section focuses on the preliminaries. The second section endeavours to sketch some of the dominant substantive biases of social research. The third section sketches the dominant epistemological biases of social research. The fourth section tries to provide a few suggestions on research priority alternatives. Finally, the concluding section briefly speculates on the relationship between social research and university teaching in Nepal.

Before I begin, I think I will do well to provide a few clarifications. The first clarification — and admission — concerns the use of the phrase "social research." I intend the phrase to denote most of the research endeavours which recognize and work upon the essential sociality of all human practices, whether they be at the familial or national level, or whether they be primarily political, economic or ideological. Lest this definition appear unnecessarily abstruse, I use the term "social research" broadly to encompass research works (on Nepal) conventionally attached to the disciplines of history, political science, economics, sociology and anthropology. I must admit that I am more of a layman — a curious one at that — than an expert in most, if not all, of these research areas. In a sense, therefore, it is as a layman that I want to assess the biases in social research in Nepal. However, I also have had the benefit of learning from authorities in the respective fields (cf. Sharma 1974, Malla et al. 1978, Nessrec 1981, Institute of Humanities and Social Sciences, 1983). Lastly, assuming the commonality of the various research areas with respect to "the social", I expect to be fairly general and abstract in my exposition such that I may be able to transcend certain "disciplinary specificities."

The second clarification relates to the fact that I will be talking about "social research in Nepal" rather than about "social research at Tribhuvan University." I do this for two interrelated reasons. First, a substantial bulk of social research being undertaken in Nepal is being carried out either at Tribhuvan University or by Tribhuvan University personnel and/or graduates. This will probably continue to be the case for a long time. Second, the University as well as the other research

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establishments work under more or less similar constraints, and are engaged in producing more or less similar research works. It is perhaps for these reasons that the contributors to the Malla et al. volume mentioned earlier make an effort to inventory research efforts beyond the University setting.

Finally, the slimness of this paper probably merits clarification. I justify it on two counts. First, I was asked that this paper be brief, short enough to hold attention at a reading session. Second, as I noted at the beginning, this paper is intended only as a sketch rather than detailed and authoritative treatment of the topic.

II

If I may be allowed to plunge head on, the first criticism that can be levelled against the institution and the practice of social research in Nepal is that it has, by and large, proved itself to be incapable of penetrating the centre. It is not my intention here to go explicitly into the 'why' of this problem, although it needs to be, and can be addressed also. We could possibly utilize another reading session for this purpose. However, I merely want to elaborate my point here. The institution and practice of social research has, in general, failed to deal squarely on most of the fundamental issues perplexing the everyday lives of the masses of Nepalese people. Research programmes have sidestepped their mundane woes, often opting out for fancier topics. As I make a survey of social research efforts, only a few researchers appear to have made sustained efforts at examining the problems of landlessness (or landlessness for that matter), labour, poverty, tenancy, exploitation and the gross socio-economic inequalities that our society is heir to (cf. Regmi 1963-68, 1971, however). Only a few of our researchers have engaged themselves in systematically elaborating the relationship between the society on the one hand, and nature on the other, and on the fantastically active rate at which we are losing and/or destroying our natural resources. Few of us are engaged in specifying the parameters of population growth. Even fewer, if at all, are engaged in investigating corruption and the gross political-administrative-professional inefficiency we know is about us. It hardly merits emphasizing that these are our concrete experiences. These are some of our most central concerns. Yet, we do not seem to be able to confront them. Monumental though these problems are, we let them pass by. There can be little doubt, that in the process, we are alienating ourselves from making contributions consonant with long-range national interests.

Second — and this is a corollary to the point made above — the institution of social research has generally come to enshrine slogans, cliches, sycophancy, and the relatively insignificant into its domain. What clearly comes out from a perusal of research efforts in economics is that we are predominantly enchanted with "feasibility and impact studies" and with the ubiquitous "socio-economic survey" (es. Shrestha and Dahal 1978) despite their limited overall significance. Going through

the economic research list compiled by Shrestha and Dahal, one cannot but arrive at the conclusion that most of our economists are engaged in what may be called "the economics of and for the privileged" fundamentally oriented towards problems related to the generation of exchange values. This, despite the fact that the overwhelming majority among us are engaged in the creation of use values, of subsistence, household economics, Commodity economics, not social, human relationship, defines the framework of contemporary economic research.

The situation appears hardly brighter in other disciplinary areas. Research in political science has just about consistently managed to evade problems of generation and distribution of power (cf. Baral 1977, however) and of relationships of dominance and submission. That this is surprising is evident when we consider the fact that by almost all accounts ours is one of the most intensely hierarchized societal forms in existence in the contemporary world. Notwithstanding, one learns that these vital experiences of our daily lives fail to register on the "priority list" of political science research (cf. Baral et al. 1978).

Historical research too, despite its considerable upsurge and promise of the recent years (cf. Regmi 1963, 1971; Bajracharya 1974; Stiller 1973, 1980; and the unfailingly lively issues of Purnima) has taken occasional and halting steps at the provision of a historically coherent and meaningful account of aspects of our social past. Personal-political histories — which probably provide the historian with the whiff of grandeur and power — have continued to dominate the interest of most of our historians. Much of History today remains a fetishized account of genealogy, chronology, inscriptions, myths, events, and above all, of personalities. It is as if, in the eyes of most of our historians, the people were non-persons and their social forms non-entities.

Research in sociology/anthropology has had its share of being preoccupied with the less significant and the peripheral. Mired in discrete and spurious ethnographic accounts and, once again, on "socio-economic survey" and "impact studies", sociology/anthropology has failed to elaborate on the fundamental bases of our social structure and of its cultural manifestations. It might be symptomatic of sociology's unconcern for the mundane that one of the first graduate seminars in sociology at Tribhuvan University focussed on "Spirit Possession in Nepal" (Macdonald 1974: 29). Given its traditional preoccupation with the ideological-spiritual realms as manifested in the ethnic contexts, it comes as no surprise that the very legitimacy of the practice of sociology/anthropology has fallen under the shadow of a soul-searching question mark. How is research in sociology/anthropology to justify itself when the basic problems facing us at large relate to food and clothing (cf. Bista 1974, also cf. Rai 1974)? Clearly, this is not a problem haunting sociology/anthropology alone. Indeed, how is the institution of social research to rationalize its existence given that it continues to address itself to problems peripheral to our social-national existence?

Third, the institution of social research thrives, and operates on, the basis of a more or less explicit collusion with national and international political-administrative-ideological interests. I am not, of course, referring here to an isolated instance of a department or an agency carrying out a particular research project on behalf of one of these interests. What I am referring to relates to the mutual exchanges of legitimacy between these interests on the one hand, and the institution of social research on the other. As we witness the booming business of sponsored research, we cannot but remain queasy at their distorted research priority agenda. Unless we have a firm belief on the cohesive identify of interests between these research organs on the one hand, and the people who remain the passive subjects of political, economic and sociological/anthropological research studies on the other, this queasiness will persist. As it is, we are slowly but inexorably succumbing to these organizations. We let them define and elaborate our basic paradigm: they do the thinking and we work for them. This surrender — or usurpation — is nowhere more blatantly exhibited than in the official declaration of objectives of the Centre for Economic Development and Administration (cf. CEDA 1978), our premier research institution. It is not my intention here to quote the text of the objectives; suffice it to note that the key reference person of CEDA's research efforts is "the policy maker". CEDA's research efforts, the document assures us in explicit sequence, are meant to help the policy maker in the formulation, development, implementation and evaluation of various plans, projects and policies, and in the reformation of the administration. In addition, we are told, CEDA's research efforts are also directed at recommending alternative ideas, experiences and decisions to the policy maker. CEDA, of course, is by no means unique in its submission to the omnipresent policy maker. One vainly looks for the otherwise familiar old-fashioned adage "service to the people" as one goes through the scores of brochures put out by the various, recently proliferating research establishments. This surrender assumes a greater significance when we examine it against the often voiced criticism of the institution of social research that outstretched upwards as its palms are for research funds and research issues, it incessantly looks down on the people below rarely, if ever, taking time out to judge the hands that dole it out (cf. Nicolaus 1972, Shaw 1972).

To venture on epistemological biases, the first criticism that can be levelled against the institution of social research is that it has sold itself to the atheoretical, epiricistic mode of research. We appear to have arrived at an unanimous consensus on the quintessential image of the social researcher: He is a person who goes to "the field" in "search of facts." We have come to regard facts as given, lying in the villages out there waiting eagerly to be searched out. We remain unconcerned that it is a particular theoretical or metatheoretical standpoint that gives meaning, indeed birth, to a search for facts. Facts are meaningless unless they draw on, and are illuminated by, a particular theoretical/metatheoretical framework. That is, facts are only as good as the theoretical framework that underlies them if only because research categories cannot be independent of theoretical categories. As it is, we bestow

unserving loyalty on facts: We believe that facts, once searched out, will speak for themselves. It is ironic that we continue to work under this presumption at a time when the very legitimacy of facts in testing theories is being seriously questioned (cf. Habermas 1974).

Second, the institution of social research can be criticized for having failed to assume — let alone practice — that the society we live and carry out our research in, is an interrelated whole, a totality. I hardly need to belabour the point; it is much too obvious to those of us engaged in social research. Yet the institution, and the practice, of social research almost invariably negates this common assumption. Some of us are interested in "economic research", some in "political research", some in "historical research" and still some others in "sociological and anthropological research." We are somehow aware that these boundaries are artificial but because we have been trained, and continue to work under a system that nourishes such a division, we are unable to explore on the essential coherence of our social life. Unable as we are to deal with the whole, we make a virtue out of dealing with the parts. Even worse, many of us mistake the part for the whole. In the process what we lose is a certain meaningfulness, a certain coherence. What we gain is a misrepresentation, a distortion.

Third, it is not at all certain whether the institutionalization of village or micro studies will bear fruit on the long run. The rush for such studies, of course, is evident in the current practice of social research. I do not doubt that when efficiently carried out, such studies will be able to provide useful information on the locale to the various interested sponsors. However, what is of concern here is whether a collection of discrete micro studies can legitimately be taken to have laid the groundwork for a study of similar issues at other levels, e.g., the national level. Clearly there is a serious risk of running into ecological fallacy here. This risk could be minimized to a certain extent, were we to carry out micro studies within a broader macro theoretical-substantive frame of reference which ultimately gives meaning to micro studies. It is apparent, however, that most of us consistently skirt the essential problem of establishing correspondence between a macro-theoretical-substantive framework and the particular micro research study we are engaging in.

Fourth, the institution of social research is generally wedded to research designs which are non-comparative and ahistorical. This is largely attributable to the ad hoc and one-shot nature of most of our research efforts. In any case, it is apparent that research efforts which are neither historically-informed nor cross-culturally comparative, are prone to produce outcomes which are weak with respect to validity as well as generality and significance. A study which can adduce neither historical nor cross-cultural evidence to buttress its arguments is necessarily weak. A good research study, by definition, is one which is historically-and cross-culturally-informed. Most of our studies, however, are sadly lacking on these counts.

Fifth, the current emphasis on 'methodology' — methodological technology to be precise — in social research may not have been entirely to the good. It has certainly aided in the standardization of research efforts, and in the provision of a rather facile means for evaluating them. However, it also has had the consequence of blunting creativity and of creating the impression that methodology is independent of theoretical-substantive interests. This lure of technicism has probably also had the effect of diverting attention from substantive issues. I do not think this is a serious problem with us as yet, but it has been a persistent criticism of social research in the West. Often, certain issues are investigated more with a view to exploiting the "room" these issues allow for a display of elegant methodological technology, and less for an illumination of the issues themselves. A highly-reputed American sociologist once said that as long as you have a problem and you are sufficiently interested in it, you are bound to develop an appropriate methodology to deal with it. A certain degree of methodological flexibility, may not be without its benefits.

Finally, the existing institution of social research, oriented as it is to the scientific notions of "objectivity" and "value neutrality", has largely emasculated itself by abandoning the role of critical research. Most of us have failed to see, and utilize, research as a practice of criticism. I need not dwell on this any further as it forms the dominant theme of Professor K.P. Malla's (1979: especially pp. 179-206) recently published collection of brilliantly cynical essays. Critical research, however, should not stop at the mere criticism of the phenomenon, of the appearance. Critical research, instead, should try to demystify the phenomenon by continually interrogating on the whereabouts of its roots (cf. Smart 1976: 174-84, Lukacs 1971: 149-59). However, given our attachment to the popular and immensely powerful ideology of scientific research, it is not likely that many of us will work to exploit the potential of critical research.

IV

Clearly, the institution is facing some serious problems. Can we do something about it? I personally would like to forward an explicitly equivocal answer: "No" and "Yes" because, as most of us are aware, many of the problems of social research are intimately related to, and indeed emanate from, currently-existing political-economic and ideological-administrative structures at the national and international levels. "Yes" because we are aware of many of these problems and may eventually have to work towards their solution. In making the following brief suggestions — suggestions which follow from my earlier criticisms — I have merely tried to expand on the optimistic side in the belief that we, particularly the University social research institution, can do something about these problems.

First, I believe that the institution of social research should make a conscious and concerted attempt at delineating the crucial problems facing the Nepalese population at large. It is only then, that we

can be institutionally capable of exploring on the mainstream of Nepalese life, and of dealing with our central concerns. I think we will do well not to commit ourselves to the laissez faire practice of social research, particularly because the social organization of the Nepalese society at large—and of social research as well—is not framed around the laissez faire model. Embedded in this plea is the note that research efforts be directly framed around real, live problems haunting the lives of most of us. As indicated in the earlier section, problems related to the social organization of production and distribution (agrarian production and distribution in particular), poverty and inequality, and power and the State may be regarded as some of the crucial problem areas facing the institution of social research. Far-fetched as it may seem in the present context, social research will be only successful to the extent that it can directly address itself to problems related to our genuine basic needs.

Second, the institution of social research should make an effort to make the practice of research truly interdisciplinary. The transplanted, fossilized disciplinary cocoons we inhabit cannot meaningfully grasp the realities we have to address ourselves to. Indeed, I strongly suspect that these cocoons have certain built-in mechanisms which work very effectively at hiding, or at deemphasizing, the very problems which have the greatest need for illumination.

Third, the institution of social research should make efforts to encourage theoretically-conscious research. Hungry as we are for facts, for information, for data, these will probably be of very little use in the long run—unless of course we continue to move along the current course. To the extent that our research efforts are framed around real problems, the conventional counterposition of "theoretical" and "applied" research would bear little sense. That is, to the extent that theory relates to practice, to the practice of our life, the image of the "theoretician" as one dealing with the irrelevant and the nonexistent deserves little merit. On the other hand, it is only a theoretically-conscious research that can adequately grasp the various manifestations and meanings of our practice. In other words, only a theoretically-conscious approach, because of its totalizing and generalizing focus, can adequately comprehend a problem.

Lastly, the institution of social research must make serious efforts at enlivening the practice of criticism as an integral part of its research undertaking. There must be a realization on our part, that genuine social research necessarily entails a re-examination of the existing social and intellectual order. Critical research achieves this by carrying out a critique of the present, by venturing beyond the present. I think this is the only course open to us if we desire a better future.

V

It is commonsense that research and teaching are interrelated activities. This is fundamentally based upon the recognition that research can reproduce positively only by teaching. Teaching itself, of course,

must be based on research. It is in this context that we, as teachers, should try to visualise the interaction between teaching and research.

It is not my intention to examine this interaction in a great detail. However, even a cursory glance at our teaching practice is enough to indicate that we may not be producing "good" research capability. Admittedly, this is a complex problem and needs to be examined at a greater depth. I feel, however, that the basic reason for this state of affairs is the obvious lack of correspondence between what our students learn in class, and what they experience in life. This disjunction between classroom learning and vital life experiences contributes to a distinct sense of alienation. This in turn serves to nourish "the student problem"—indiscipline, lack of motivation to learn, and lack of commitment.

The major—but by no means the only—constraint with respect to this creation of relevance lies, I think, within the design of the syllabi. I would like to end my presentation by forwarding five interconnected avenues to improve the design of social science syllabi at the University. First, to reiterate, the syllabi should be intimately connected with everyday reality. This is not a mere cliché. A close examination of the currently operating syllabi, I am certain, will bring out a number of grotesque, as well as rather subtle mis-emphases. Agricultural economics and population economics, for example, are regarded as "optional subjects" in the Master's course in economics. Local, district and regional politics, for example, are hardly discussed in the teaching of political science. The situation is not much different in other social science disciplines. Second, Master's level teaching in all social science disciplines should focus explicitly on the Nepali experience—albeit in the world theoretical context. Theories and theoretical models per se should be taught at the intermediate and the bachelor's levels, even if in a rather simplified manner. Third, and as far as it is practicable, repetitions in topics covered at various levels must be avoided. Fourth, courses should explicitly be built around issues which bear current relevance, e.g., ecology, water resources, people's participation and national integration, for a specific body of small, concerned student participants. Such an arrangement, apart from generating interest among students, would also create more specialized and employable manpower while at the same time avoiding the redundancy in graduate expertise. Finally, the courses of study at the Master's level should concurrently allow room for the student's exposure to practical work in the area the student is interested in.

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