THE INTEGRATION OF PURE AND APPLIED RESEARCH AND ITS APPLICATION TO THE DESIGN OF NATIONAL DEVELOPMENT PROGRAMMES[1]

Aroldo Rodrigues [2]
Catholic University of Rio de Janeiro
Brazil

Perhaps no other time in the history of mankind has given men an opportunity to witness such a strong and widespread concern with urban, national and world development and welfare programmes. The improvement of the means of communication and, possibly, a development of man's moral conscience and sense of social responsibility, have led the world to very significant changes in the last two decades. For the sake of concision, and also for objectivity's sake, let me concentrate upon only one of these significant changes, namely the change that took place in the social sciences in general, and specifically in psychology, and then tie this change to the role of applied psychology in a national development program. As an anticipation of the main theme of this paper, and also as an "entrée au matiere", I wish to state at this point that one of the main changes I see in contemporary psychology, as compared with psychology of previous times, is an extraordinary concern with *socially relevant science* and with the responsibility of psychologists in *promoting human welfare*. If one opens the December 1969 issue of the American Psychologist, he will find the first two papers exhibiting the words I intentionally used in the previous sentence, namely, "socially relevant science" and "promoting human welfare", in the titles of Morton Deutsch and George Miller's Presidential Addresses to the Eastern Psychological Association and to the American Psychological Association, respectively. The concern with the relevance of psychology to societal problems shown in these outstanding papers, plus the emphasis given by the *Journal of Social Issues* to applied social problems, plus a recent publication of the American Psychological Association entitled *Psychology and the Problems of Society*, plus the basic content of most contemporary publications in psychology, demonstrate quite convincingly the current trend prevailing in this field, and brings into focus the issue of pure and applied psychological research. Let me say a few words about these two aspects of psychological research, and then show how they complement each other and how their results blend into a useful product when ingeniously utilized by the *social science tech-*

nologist, as shown by Varela (1971) in his introduction to social science technology.

PURE AND APPLIED RESEARCH IN PSYCHOLOGY

We rather frequently hear people saying that underdeveloped countries cannot afford the luxury of pure research in psychology. I do not agree with this statement, at least in its broadest meaning. I concede that these countries have difficulty in allocating money to sponsor such research, but I strongly disagree with those who state that pure research should rank lower in priority than applied research, from any standpoint one chooses to look at the matter. More specifically: pure research should not, and must not, be placed in lower priority order as compared with applied research. I think it appropriate to here quote Underwood, who says in his book *Psychological Research* (1957):

> When the scientist may pursue his work, wherever it may lead him (providing no harm befalls others during the pursuit), without having to answer the question, 'what good is this?', that is what I mean by freedom of inquiry . . . never once have I heard of research being questioned by a dean, or other administrative officer, or a colleague, because the research worker had no answer to the question 'what good is this?': indeed, the question is never asked. The research might be questioned on a number of grounds, such as methodological adequacy; but never does the man have to defend his work against the charge that it has no immediately forseeable application (p. 10).

And again, from the same source:

> I use the terms "pure" and "applied" merely to identify the ends of a crude continuum. This continuum is defined by the attitude of the research worker. At the applied end of the continuum, we have the research worker who asks himself questions about the manner in which the world (nature or social order) is functioning and does research concerned with these questions only if it appears that the product of his research will clearly and immediately modify the way in which the world is functioning. At the other extreme is the investigator who asks himself questions about why nature behaves as it does, and sets about to get the answers without any concern that they may be used to change the world. All this pure research worker wants to do is understand the world. In between these extremes, of course, are gradations. Without doubt there are many research workers who ask themselves research questions as a result of a basic curiosity about nature and then further ask what relevance the answers to such questions might have in changing the world. Whether they proceed with the research or not depends

> on the values they place on the two aspects of the problem. And, of course, a man need not occupy a static position on the continuum; he may range as his interests and values change or, as during a war, when emergencies demand it (p. 9).

I do not think we are in such an emergency that requires giving priority to applied over pure research.

The position stated above has been, with very few exceptions, well understood by academic personnel involved in scientific activity. Obviously, it has nearly always been misunderstood by laymen. However, owing to the emphasis that has recently been placed on social relevance, the issue became a heated one even among scientists. Bernard Baumrin entitled his invited address to the Division of Clinical Psychology of the APA in 1969 in the following way: "The Immorality of Irrelevance: The Social Role of Science". In this paper the author argues strongly against research designed for the sheer desire to know things, without immediate bearing on the solution of important problems men are presently faced with. Therefore, one might infer that Baumrin, and a host of other people who would easily subscribe to his position, would call immoral a piece of research to which the researcher could not answer the question that Underwood claimed not to have ever been asked of someone's research in a decent University, that is, the question "What good is this?" Baumrin (1970) says that:

> to choose to do science for its own sake, or to choose to do science as if one were taking a hand in constructing a beautiful edifice, or to do it for one's own simple pleasure is to turn one's back on the world, to leave application to those who may be able to make something out of one's work never oneself knowing whether there is anything there to be useful. In short, to do irrelevant science is to make a moral choice; and if one can claim that ignoring the problems of one's fellow man is immoral, then doing science for its own sake is immoral. To take away from others one's own contribution to their well-being is immoral, particularly when it is done by deliberate choice (p. 82).

This assertion is, to my judgement, filled with emotional overtones and has little, if any, bearing on truly scientific enterprises. No one engages in a scientific endeavor just for the fun of it, without having in mind some more noble aim. A serious scientific work hopes to accomplish at least one basic thing: to understand better the world we live in. Is it conceivable that one would disregard or greatly minimize the beneficial implications for all mankind that stem from a better understanding of our reality? It seems to me, therefore, that Baumrin goes too far in his critique of non-immediate-applied research. He himself defines the degree of relevance

of an act as being the "degree of articulable forseeable consequences of that act with respect to substantial human problems" (Baurim, 1970, p. 81). Every piece of honest scientific research has some degree of articulable forseeable consequences with respect to substantial human problems. Baumrin is correct, however, in pointing out that once research is sponsored by funds that come from people it is only reasonable and fair that these funds be used in research that gives maximum priority to people's welfare. The allocation of funds for research may very well be based on the immediate relevance of the scientific product hoped to be derived from that research. This is not to say, however, that scientific activities of more remote social relevance deserve to be called immoral.

I wish to close this section with a quotation that, in my opinion, establishes with admirable precision and wisdom the guideline to be followed whenever the issue of pure and applied research is at stake. I refer to Morton Deutsch's remarkable address to the Eastern Psychological Association in 1969, when he said: "A focus on 'science' that excludes 'social relevance' as a distraction or on 'social relevance' that excludes 'science' as irrelevant will in the long run be destructive to both" (p. 1081).

SOCIAL SCIENCE TECHNOLOGY

As Varela (1969; 1970; 1971) has frequently pointed out, numerous discoveries derived from pure research have been later used by technologists in ways never dreamed of by the original discoverers. The basic work on plastics, on rubber, on lenses, on steel, together with the discoveries of the physical and the chemical properties of a variety of elements, have been later used by the technologists for the construction of a *product* of far-reaching social relevance. Just think of all the basic research behind the automobile we drive, the typewriter we write with, the camera with which we take pictures, and the space train that has carried 6 men to the moon and brought back lunar material for further discoveries. The same routine followed in the natural sciences is to be followed in the social sciences. We do need both scientists and technologists, so that we can have psychological products that are indispensable for the welfare of mankind. We have accumulated a fair amount of knowledge in regard to the psychological make-up of human beings, to the dynamics of their personalities, to the social factors that have some sort of bearing in their behavior. We know, for instance, many factors that generate violence, prejudice, mental stress, group satisfaction, resistance to change, persuasion, conflict resolution, decision making, social distortions, learning mechanisms, etc. Time is ripe for social science technology. We need the ingenuity of technologists to capitalize on the existent knowledge and make significant

contributions to the world so that we have a better world to live in. As Miller (1969) very well said, "the important thing is not to control the system, but to understand it" (p. 1071). And he goes on to say:

> In my optimistic moments I am able to convince myself that understanding is attainable and that social science is already at a stage where successful applications are possible. Careful diagnosis and astute planning based on what we already know can often resolve problems that at first glance seemed insurmountable. Many social, clinical, and industrial psychologists have already demonstrated the power of diagnosis and planning based on sound psychological principles (p. 1072).

Basic research, applied research, and social science technology ought to march together, complementing each other and blending into what is bound to become a treasure of invaluable potential to the well being of all men.

Varela (1971) shows brilliantly the appropriateness, the need, and the timeliness of social science technology. He says that a new paradigm about the social nature of man is warranted in view of the discoveries based on psychological research. As Varela puts it, these paradigms "have been gradually modified in working and in conception as time has gone by with the assistance of many persons, and may suffer even further modification and additions. The before and after paradigms can each be formulated in eleven statements."

I shall list now Varela's paradigms I and II, that is, the prevailing paradigm and that based on Social Science findings.

Here is the first:

1. All men are created equal.
2. When something goes wrong, someone is to blame.
3. The guilty should be punished.
4. Time and effort should not be spared to establish guilt.
5. The guilty are largely responsible for their own misbehavior and for their own improvement.
6. Unreasonableness can be countered by facts and logic.
7. One truth underlies all controversy.
8. Conflict is in general inevitable.
9. Most behavior is economically motivated.
10. Capable supervisors should devise solutions and see to it that their subordinates carry them out.
11. Supervisors are too busy and have no time to become experts in social science.

The second reads as follows:

1. There are great individual differences between humans.

2. Positive incitement is a better modifier of behavior than punishment or threat.
3. Social problems are solved by correcting causes, not symptoms.
4. Human conflict is no more inevitable than disease and can be solved or, even better, prevented.
5. Irrational feelings must be toned down before people can reason.
6. Human motivation is complex; no one does or fails to do something for only one reason.
7. Problems are solved more effectively in groups than individually.
8. Perceptions are more relevant to social problems than "true facts".
9. Time and effort are not unlimitedly available for problem solving.
10. Responsibility for individual improvement of subordinates should shift largely from subordinates and students to supervisors and teachers.
11. Supervisors and teachers should receive intensive training in social science technology.

Miller (1969), in commenting upon Varela's two paradigms, asks this very pertinent question:

> How can we foster a social climate in which some such new public conception of man based on psychology can take root and flourish? In my opinion, this is the proper translation of our more familiar question about how psychology might contribute to the promotion of human welfare (p. 1070).

The promotion of human welfare rests, to a great extent, in the ability of social science technologists in using the discoveries about the nature of man to make beneficial products to the betterment of interpersonal relations and human understanding and progress. This is the contribution psychology has to offer to, among other things, national development programmes.

PSYCHOLOGY AND NATIONAL DEVELOPMENT PROGRAMMES

Paradoxically, we are presently living in a world of great wonders and of great tragedies. Our generation has witnessed spectacular achievements, like the placement of men on the moon, and yet wars break out every now and then, poverty and even misery afflicts an enormous portion of our fellow men. One can take either a pessimistic or an optimistic view of the years ahead of us in regard to the world's problems: Advocators of both positions can easily list a host of facts that justify them. In favor of the optimistic side, I find one piece of evidence to which I allocate a good deal of weight. I think that in our times, more than in any other time in history, men in general are aware of, and concerned about, other men's

problems, vital necessities, and need for better living conditions. It is true that a lot of disagreement exists in regard to how to promote human welfare and fulfillment, and in connection with the way of applying distributve justice. It is also true that many atrocities have been committed under the guise of seeking human welfare and justice. Nevertheless, the concern with justice and with improving the living conditions of every human being seems to be reasonably widespread. This is a very significant fact and one that gives us all great hopes for the future.

National development programs have, as their ultimate goal, the attainment of conditions of prosperity and organization that will enable each country to promote better living conditions for all its citizens. The achievement of such an objective is not an easy task and requires manifold resources and joint efforts of governmental officials, scientists, professionals of all ranks, and the people themselves. A good deal of the burden in this enterprise lies upon the social scientist and particularly upon the psychologist. It is customary for a professional to consider his field of specialization as *the* most important. I do not wish to have this attribution made to me. I do not consider psychology to be any more or less important than any other science. What I mean to say is that, in trying to set up a program for national development, one is bound to face situations in which change is necessary. Most people know, through observation and/or personal experience, that to change one's habits, beliefs, attitudes, customs, traditions, and idiosyncrasies is not at all easy. Developed and underdeveloped countries in the world today are coping with problems of such relevance that change in one way or another is absolutely necessary. Here psychology is called upon for help. This science provides the social science technologist a good amount of research findings from which he can make concrete products to be properly used by governmental officials. Let me be more specific through some examples. As Miller (1969) and Varela (1971) have said, the main step in introducing any change into a system is to understand the system. Psychology has accumulated several techniques for diagnosis, such as public opinion surveys, psychological tests, techniques of interviewing, etc.; it also has accumulated a fair amount of understanding of how humans function psychologically, e.g., appropriateness of different persuasive techniques; utilization of the mass media of communication; personality proccesses; etc.

Thus, it is only natural that we put this knowledge in the service of men by advising government officials in the changes they plan to introduce for the development of the country. In the developed countries, psychological advice is called for very frequently in a variety of projects. Unfortunately, this does not seem to be true among the underdeveloped countries. There

is a general tendency in underdeveloped countries in the direction of giving more weight to the contributions of fields of inquiry such as physics, chemistry, engineering, medicine and economics. These are seen as more directly related to progress and welfare. This is a great mistake. All these branches of knowledge are remarkably important, but they are used by men and for men. There is one branch of knowledge whose specific focus of analysis and concentration is man, and this is psychology. It seems, therefore, rather odd that psychological knowledge be placed in second order of priority when we are engaged in an enterprise whose ultimate goal is man's welfare. It is psychological knowledge that will help preventing wars, educating people, changing prejudiced attitudes, reducing social conflicts in general, making people more capable of using their potentialities fully, motivating them to engage in beneficial causes and being more productive. Any national development program must have these goals in mind. The role of psychology in such programs is easily visualized in the light of what has been said.

I have avoided the term *applied* in most of this paper, and particularly in this last section, and have deliberately referred to the role of psychology *tout court* in such programmes. I did so based on the ideas laid out in this paper, according to which I believe that both pure and applied psychology are complementary to each other, and that both provide the social science technologist with the tools needed for making a product to be used for the welfare of man. The concern with socially relevant science to which I referred at the outset of this presentation seems to me to reflect a concern with the utilization of psychological knowledge for the betterment of the human condition. It would be inappropriate, I think, to draw a line between basic and applied research, and call futile and even immoral the former, and relevant and moral the latter. Both are relevant and moral, and the discoveries derived from both must be given away and used by all for the making of a better world.

REFERENCES

Baurium, Bernard H. The immorality of irrelevance: The social role of science. *Psychology and the Problems of Society*. APA, Washington, D.C., 1970.

Deutsch, Morton. Socially relevant science: Reflection on some studies of interpersonal conflict. *American Psychologist*, 1969, 12, 1076-1092.

Miller, G. Psychology as a means of promoting human welfare. *American Psychologist*, 1969, 12, 1063-1075.

Underwood, B. J. *Psychological Research*. New York: Appleton-Century-Crofts, 1957.

Vareh, J. A. Aspectos práticos de la psicologia social. Paper delivered at the *XII Congress of the Interamerican Society of Psychology*. Montevideo, 1969.

Varela, J. A. A psicologia social na sociedade contemporânea: Considerações em tôrno

de um novo sistema social. Paper delivered at the *Forum on Social Psychology* organized by the Brazilian Association of Applied Psychology. Rio de Janeiro, 1970.

Varela, J. A. *Psychological Solutions to Social Problems: An Introduction to Social Science Technology*. New York: Academic Press, 1971.

FOOTNOTES

1. Paper delivered at a Symposium about "The Role of Applied Psychology in National Development Programmes" during the meetings of the XVIIth International Congress of Applied Psychology held in Liège, Belgium, July of 1971.

2. The author wishes to acknowledge his indebtedness to the Rio de Janiero (Brazil) office of the Ford Foundation for having made possible the presentation of this paper.