

INTER-OFFICE CORRESPONDENCE

FROM: LCD

DATE: 3/21/49

TO:

ER		EL
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M.S.		
J.H.P.		

COMMENTS:

The kind of honest, frank & constructive criticism that mature policy. Lets pass on quickly & discuss at next staff meeting. JHW

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SUBJECT: Comments on a Policy of SS Operational Programs in the World's Backward Areas

I am puzzled and troubled about the proposal that SS institute a policy of conducting operational programs in the world's backward areas, and about some of the points which arise in discussions of the proposal. To facilitate staff discussion of this question, I should like to try to state some of my thoughts on a number of these points.

1. The Foundation's statement of purpose, "to promote the well-being of mankind throughout the world," does not necessarily argue, it seems to me, in favor of carrying on activities in all or even in several parts of the globe. Activities confined wholly to the United States, or to New York City, could entirely fulfill the purpose. Under certain circumstances they might do so far more effectively than much more widespread activities. To illustrate the point with hypothetical but by no means fanciful examples, let us consider the following alternatives:

- Concentration of funds in support of fundamental research leading to the discoveries of Pasteur and their application to the development of medical science versus applying the same amount to the recruitment and support of healers throughout the world before Pasteur's work had been done.
- Concentration of funds in support of the work that developed hybrid corn versus applying the same amount to the placement of county agents throughout the world before hybrid corn was known.
- Concentration of funds on research which will develop specific cures for different types of insanity versus applying the same amount to the establishment of insane asylums throughout the world in the absence of such knowledge.

Obviously many criticisms can be made of these hypothetical cases. Their only purpose is to illustrate the point that under many circumstances the well-being of mankind throughout the world may be far better served indirectly than directly. The alternative view leads necessarily, it seems to me, to the conclusion that the

Foundation's purpose is not fulfilled until activities are established in all parts of the world.

I should suppose that we all agree about this and that I have been attacking a straw man. Our problem is so to utilize scarce resources as to maximize their contribution to the well-being of mankind when viewed in long-run terms. But if we are agreed on this, I do not think we can use the Foundation's statement of purpose to weight in either direction discussions about what areas we should operate in.

Perhaps it will be argued that there are political or tactical reasons for far-flung operations because of the possible tendency for some members of the family, trustees, and top-level officers (not to mention ourselves) to swell with pride at the thought that the sun never sets on the good works of the Rockefeller Foundation. I suspect we need to be alert to detect and resist such imperialist motivations.

2. If we are to maximize the productivity of scarce resources, I do not see how the needs of backward areas can rank high among the considerations on which we base our decisions. As JHW points out, needs among backward peoples are unlimited. Moreover, we do not at present have any effective means for dealing with many of these needs. Our means for dealing with others are highly uncertain or of very limited effectiveness. Our attention should be focussed, I believe, on considerations of whether prospective areas and problems offer clear possibilities of making fundamental and enduring advances under existing limitations of knowledge, personnel, and technology. To be sure we must do some gambling but we should learn not to dissipate our stakes on impossible odds or stacked decks. One of the most beguiling snares we face is the ever-present argument that needs are so great, conditions so bad, or levels so low, that anything we do will result in such relative improvement as to justify the investment. I would be inclined to hazard the assertion that, in terms of long-run perspectives, responses to appeals of that type are far more often wasteful than otherwise.

I believe that RF's general policy decision is wise—that it can contribute more significantly to the well-being of mankind indirectly through helping to add to knowledge than directly through operating programs. (More on this later.) But here, too, we face the problem of maximizing the productivity of scarce resources through alternative allocations to relatively backward or to more advanced areas. And here, too, I think we face the continuous temptation to respond to appeals of great need by scattering grants which may slightly improve the immediate situation but which, in terms of the major objective, are completely or largely dissipated.

When considering new areas of operation, I believe that in general we should be unwilling to undertake any program activity except where we can find in combination all the conditions necessary to provide good probability that a really effective training and research center will result from the amount we are able and willing to invest. If we find the necessary conditions in a given place, we should still undertake no program unless we are prepared to make an investment of sufficient size and duration to insure the development of training and research to

a thoroughly respectable level by American standards. We should refuse to dissipate SS resources through grants to mediocre or poorly trained people even though they are the best ones there are, or to able and well-trained people in locations where the total situation is not promising. Such grants would undoubtedly improve a little the situation in the place where they are made. But they would in the long run be far less productive of new knowledge or really effective training than grants more strategically placed.

Whether or not we always fully achieve these standards, I believe they are good ones to strive toward. Of course, what conditions are necessary to provide adequate probability of respectable levels of training and research will vary somewhat with subject matter. But the test of productive promise in terms of a single standard of excellence should always apply if our aim is to maximize the yield of new knowledge from our resources.

3. I am not sure that I always understand exactly what is meant by the statement that we should work at the grass roots and that RF operations tend to be ineffective because they work from the top down. I, of course, agree that some kinds of research require direct and detailed acquaintance with people and conditions in lower classes and hinterlands, and not merely with leaders and population centers. But in so far as the statement implies, as it sometimes seems to me it does, that RF operations in backward areas should always be directed primarily to the grass roots, I believe it is highly dubious. In a country where there are 450,000,000 grass roots scattered over thousands of square miles, it seems likely that it will be highly inefficient if not almost entirely fruitless to focus operations exclusively or primarily on the grass roots. An abundance of evidence from extensive research indicates pretty clearly that social changes typically arise in population centers and diffuse outward from those centers, and very frequently arise in elite groups and diffuse to the masses. In so far as RF operations seek to produce social change, it would seem sensible to work along these natural lines.

Whether, and to what extent, resources and energy should be expended directly at the grass roots depend, I should think, on the character of the operation. If the aim is the reduction of malaria through mosquito control, the most efficient procedure may well be direct operations by RF staff in cooperation with governmental agencies of infested areas. When the changes sought require changes in attitudes and habits of local populations, as in efforts to improve sanitary practices, more attention must be paid to the grass roots. But here, too, efficient operation requires working through the normal channels of influence and control and depending on them for any wide diffusion of results. Direct grass-roots operations should be limited, I believe, to experimental and demonstration projects carried on in cooperation with the agencies of the country who will be expected to carry them into other areas. If the aim is to improve medical training and practice in a backward country, it seems probable that concentration of resources on the development of one really first-class medical school at a major center will be more effective in the long run than scattering those resources in minor improvements throughout the hinterland. Obviously, twelve first-class medical schools in twelve leading centers throughout the country would be better. But that is still no grass roots operation; and it requires twelve times the investment. I believe one good medical school would be more worthwhile than twelve, all no better than could be

provided by one twelfth the cost of one good one. Obviously, too, investment in one leading medical school may not prove very effective. But it is difficult for me to imagine any grass roots operation which could make an investment of similar size as effective in improving medical training and practice as could one good medical school.

I do not think I am wanting in eagerness for the improvement of life among the humblest groups and in the remotest regions of the world. But at best our resources can contribute but little to that end. If we are to make them count the most toward fundamental and enduring improvement, we must use great care to apply them strategically. And I believe this will mean applying them to influential centers or groups much more often than directly to the grass roots.

4. Another statement which I am never quite sure I fully understand is that we too often err by giving backward areas what we think they want, or what we believe they ought to have, rather than finding out what they do want and giving them that. I am sure we all agree that RF will be ineffective in its efforts to aid the advancement of backward peoples if we do not discover the real interests and motivations of those peoples and learn how to relate our programs to those interests and motivations or else how to develop new interests and motivations relevant to our programs. But the statement sometimes sounds as if it means that RF program should literally be directed at learning what people in backward areas want and helping them get it. This view seems to me mistaken and dangerous. No one would argue for it in extreme - and hence obvious - cases, such as a tribe which has no interest in learning improved agricultural methods but wants improved weapons with which to prey on its agricultural neighbors, or medicine men who are uninterested in knowledge about sanitation but want more hideous costumes with which to exorcise demons. But I think in less obvious cases too we cannot escape the responsibility of making decisions about what will be useful in improving the well-being of mankind and making RF policy decisions on the basis of our own best judgments. This is not to imply that our own judgments are infallible or that they can even be intelligent in the absence of the fullest and most sympathetic understanding of the points of view and interests of those whom we would assist. But I do not see how we can avoid the hard necessity for making our own decisions in terms of a coherent program for advancing basic knowledge. And this forces us, much more frequently than we would like, I fear, to the uncomfortable position of having to decide that the kind of assistance people would like is not the kind which will be sufficiently effective in advancing knowledge to justify our diverting RF funds to it. In such cases I think we must decide that no RF program is indicated unless we can find means of developing interests and felt needs in more fruitful directions.

5. The proposal that RF should undertake SS operational programs in backward areas to help raise the general level of life in those areas by demonstrating how the social science knowledge and skills of the West may be applied most effectively to the service of backward peoples, seems to me to be unwise for the reason that I do not think we can deliver on it successfully. I seriously believe we are more likely to discredit the social sciences by the undertaking than we are to effect important improvements in backward areas. The reason is that we do not have the knowledge and skills which can be applied in specific ways to specific situations with reasonably assured specific results. The models which we must admittedly seek to parallel are the I. H. D. and NS operations. But what parallels can SS undertake?

Both I. H. D. and NS can tackle specific problems with full assurance that their knowledge and techniques are adequate to produce visible results. They can state in advance what problem they will tackle, what their procedure will be, what result it can be expected to produce, and how they know this to be true. (In the case of experimental ventures that are less clear cut than this, they still have a large store of scientific knowledge of wide applicability on which to draw with confidence.) I think SS should not launch operational programs until it, too, can approximate these conditions. At present I believe the social sciences are in about the same position as medicine a century or more ago when operators had to depend on empirical knowledge and personal wisdom because there existed no basic knowledge of the causes or cures of specific diseases. And the empirical knowledge and personal wisdom which we now have in the social sciences are of very limited and uncertain utility in solving specific problems in our own society. They are even less applicable to backward areas since they are largely derived from observation and experience in our own society.

I think we are compelled to be extremely humble about what we can do in the way of operations in the present state of knowledge in the social sciences -- and inflexibly persevering in our efforts to improve that state of knowledge, especially by seeking progressively to displace empirical wisdom by specific scientific principles of known applicability. And I think we should be careful not to discredit the pursuit of more secure knowledge by too early or too sanguine efforts to exploit inadequate knowledge.

6. The net of all this is:

- a. I believe we should announce or launch no operating program in SS unless and until we can be quite explicit and reasonably confident about what problem we will attack, how we will operate on it, and what results are to be expected. In addition, we should be sure that the importance of the results to be expected and our confidence about producing them are sufficient to warrant the cost of the program -- realistically estimated. I am very skeptical about our being in a position to do this either now or soon.
- b. I believe we should concentrate on the most effective programs we can devise for advancing basic knowledge in the social sciences.
- c. This would confine SS activities in backward areas to:
 - (1) Helping to develop centers in backward areas where native groups can carry on significant social research and train effective research people. This should be attempted, in my view, only where the combined elements of strength in the local situation are such that a really competent center can be expected to result from what we can and will invest -- and where continued local support of the center may be expected after our support is to cease.

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(2) Backing research projects by qualified personnel which call for work in backward areas directed toward adding to basic knowledge in the social sciences. The relevant criterion here, however, would be the importance of the scientific problem and the feasibility of the project rather than expected benefits for the backward area. IHD and NS operations in backward areas may provide unusual opportunities for significant study of important social science problems. This possibility should be explored and exploited. Here too, however, the criterion would be the same as stated above, not expected gains for the RF operations.

d. In addition there may be instances, and we might seek a few, where there is hope that an SS backed applied research project in a backward area could prove useful to the country or to one of RF's operating programs. But here I think we should proceed with great care and undertake a project only under the following circumstances:

- (1) The aim is to secure information, not effect social change.
- (2) The RF operating group, or some agency in the country, will agree that the information, if secured, would be useful in some specific way to its operations.
- (3) The information to be sought is clearly specified and the necessary techniques and personnel are available for the undertaking.

For example, social scientists might be able to conduct a study which would show some of the ways and extent to which RF operations might be having unintended disruptive effects in a backward area. It is very much more uncertain that present techniques could lead to any secure information about how to modify the operation in such a way as to achieve the results it is seeking, but with the elimination of the unintended consequences. It is probable that studies of economic problems involved in the attempt to raise the level of life in backward areas could provide significant information about limitations and obstacles which should be taken into account. It is much less clear that they could provide secure information about how to achieve the desired results in the face of ^{the} limitations and obstacles. And it is practically certain that they could not provide convincing proof of their conclusions to any responsible operators who are inclined not to be convinced by the report. Such studies are likely to be useful, therefore, only where some operating group in the area involved is inclined to be impressed by the information provided and to be in a position to try to use that information in some actual program.

e. Providing opportunities for grass roots training of social science personnel in connection with RF operating programs in backward areas may be useful in special instances, but I am dubious about the usefulness or possibility of a large-scale general program along this line.

If the aim is merely to give a large number of social scientists some personal experience in and acquaintance with a variety of backward areas around the world, it seems to me that Carnegie's area fellowship program is already undertaking this in a very sensible way and in relation to a program for utilizing such experience in its area projects. If, as I would argue, RF should undertake such activity only where it is clearly and specifically useful for carrying out needed research or developing specified research skills, I am not at all clear about:

- (1) Where we could possibly find several hundred qualified people during the next five years to spend from one to three years in backward areas, or,
- (2) What they would do during this period in the backward areas or what specific research techniques they would thereby acquire.

In instances where we do find people who need and will accept such training in order to increase their research capacity, and where a clear program serving this purpose can be defined, I am sure we should proceed. But I cannot see how this can be expected to develop into a very extensive activity if we hold to a criterion of importance for a program of increasing basic knowledge.

- f. This leaves us, I realize, without any impressive program for expanding operations or research in backward areas. All it does is commit us to be on the alert to find and back research projects in our basic research program which call for research in backward areas, or which seek to provide knowledge which may ultimately prove applicable to the solution of some of the problems in backward areas. And, with special caution and modesty, possibly to undertake to find personnel for a very few applied research projects which may prove helpful to others in their operating programs. These we would undertake only where the problems are explicit, the personnel and techniques available, and the risks fully understood and accepted by the clients. The advantage of this limited proposal is that it does not overextend us by arousing unrealistic expectations which we have little likelihood of being able to fulfill.

LCD.

3/22/49.