

INTERESTS, PURPOSES AND PAY-OFFS

by Ronald P. Dore

I want to take as the text for my sermon this afternoon a question asked by Robert Cole yesterday in the course of our discussion of Dr. Iga's paper. The paper sought to answer the question: "Why is it that Japanese scholars are not much interested in social problems?" Dr. Cole's question, or meta-question, was: "Why should we be asking this question at all?" Perhaps the most useful thing I can do is to try and systematize some of the possible answers to this question about what we *ought* to study, what questions it is important to ask—answers which are sometimes made explicit, but are more often simply implicit in our research activities.

Perhaps the best way to start is by asking a different question. Has there ever been a meeting like this, convened for the purpose of assessing the state of the discipline and what should next be done, summoned by students of classical Greece, for example, or Alpine botanists, or the students of Turkic languages who were mentioned in yesterday's discussions? That is a rhetorical question, I may say, to which I will assume that the answer is "no." There are two possible explanations why this should be so. The first is that the students of such disciplines do not really feel the need to get together and evolve a collective view. The second is that nobody offers them money to get together. And, of course, the two are possibly related.

Let us take the first point. I think it likely that they would not feel the need to get together. The reason, I take it, is that one of the assumptions about life of the students of classical Greece is that they are in the business for the pursuit of truth, for the satisfaction of intellectual curiosity. They accept that each person follows his own intellectual demiurge and it is nobody's business what anybody else is doing. They are concerned—to adopt the categories used by Harumi Befu this morning—with being scientists rather than citizens. And, of course, this is indeed their justification for being in universities at all. Their presence in universities is, as it were, a symbolic reaffirmation of their society's belief that knowledge is a good thing: not that knowledge is power—but that knowledge is a good thing. This seems to me a reasonable justification; it *is* a good thing to have it reaffirmed occasionally that knowledge *is* a good thing.

Yet we, on the other hand, start from the assumption first of all that there

is some possibility and, secondly, that there is some point in trying to achieve some consensus about where we have got and where we ought to go from here.

One explanation of this might be the well-known phenomenon, mentioned by Dr. Lebra in his paper, that people tend to take on some of the characteristics of what they study. In Japan, as perhaps most of us have been guilty of saying in some form at some time or other, "The group is more important than the individual." And so, perhaps, among students of Japan. An alternative and possibly more likely explanation is that it is simply because, for reasons to be discussed later, in our case an obliging foundation is, in fact, prepared to pay for us to get together. And who are we to say "no" to the offer of two pleasant days in pleasant company? If this is the correct explanation, then we ought, perhaps, to consider the foundation's motives rather than our own.

But first let me summarize my initial starting point. If we define academic research as simply the pursuit of truth and understanding, then there is no available criterion by which one can say that one topic of study is more *important* than another. We all have intellectual curiosities that take us in different directions (partly depending perhaps on the way we started: Bernard Karsh said he was just bored with studying American workers; I applied for a Turkish course and was put on to a Japanese course by mistake). Both the initial stimulus to, and the subsequent directions of, our personal curiosities can be infinitely varied. Some people find it fascinating that the Japanese growth rate is higher than other people's and want to explain why that is; some people are curious about the incidence of matrilineal cross-cousin marriage; other people may want to know why it is that the price of *daikon* in Hakodate tripled when the average price level only doubled. Some are interested in Japanese emic patterns; others in emetic patterns. As "pure" research, these are all equally valid topics. In order to make the value judgment that one line of research is important and another is trivial, one needs some other criteria, and they must be criteria of "usefulness," of the extent to which the research serves some other end besides the simple discovery of truth.

Let me try to list some of the kinds of usefulness that have been either explicitly discussed or implicitly taken for granted in our discussions.

The first one is that by our efforts accurately to describe Japan in the English language to our fellow citizens we shall be doing good since we shall be diffusing a better understanding of the Japanese to Americans or Englishmen. This, in some way, is supposed to contribute to international understanding, good will, harmony between the nations, lessening the likelihood of war, and so on. Here, certainly, is a criterion which distinguishes us from the classicists because nobody really wants to get chummy with the ancient Greeks. If this is in fact the usefulness that we claim, clearly I think

we have to make our results readable, presentable, and appealing to the mass of our fellow countrymen whom we are trying to inform. On the whole, I would think we are not doing at all well in this line. A good Japanese film, a few translated novels, probably contribute more to this particular end than a dozen articles in *Ethnology* or *Economic Development and Cultural Change* or *Psychiatry*.

A second possible form of usefulness is the description of Japanese society not for the mass in general, but for the particular elites in our own societies who frame policy toward Japan. In the social sciences, of course, Ruth Benedict offers a paradigm of this activity. Her work in the Office of War Information was sponsored by the American government in the belief that her findings would make it possible to conduct the war more efficiently. (I was surprised, incidentally, that John Bennett should have said in his paper that one of the things we need is a reappraisal of Benedict. The shadow of Grandma Benedict seems to me to loom very large over the papers for this conference, many of which are devoted to chasing hares that she first flushed out of the undergrowth many years ago.)

How, then, would one rate our performance in pursuit of this kind of usefulness? Somehow, if this is the reason why the fund-giving establishment is sponsoring our endeavours, I don't think it is getting its money's worth, though my heart does not necessarily bleed for the fund-giving establishment on that account. I doubt that many foreign policy decisions have been taken on the basis of what any of us has written about the nature of the Japanese educational system, the Japanese need to achieve, their need to *amaeru* or whatever else. Sociologists, social anthropologists, and social psychologists differ in this respect from the political scientists and the economists who are much more likely to have influence on policy. We, if you like, get the spill-over; the spin-off. The economist students of Japan in their recent appraisal meeting in Washington are supposed to have said that in order to give good advice as economists they needed to know more about background social factors. It is this kind of notion, I suppose, which makes fund-givers consider us vaguely important, even if in practice they find our capital-output ratios rather high.

There is, however, a second subvariant of this kind of usefulness: not the mere proffering of background information or direct advice to receptive official ears, but protesting remonstrance offered to, or if necessary shouted at, unreceptive governments. Many Americans felt that there was such a moment in 1960, and the time might come again when, on the basis of our presumed competence in matters Japanese, we might need to speak out and speak out vociferously about some foreign policy of our government which seems to us to be based on completely false assumptions about the nature of Japanese society.

So much for the second possible kind of usefulness. A third reason for

trying accurately to chart and describe Japan is simply that it helps us to understand our own societies and ourselves. The point has been made several times in our discussions that almost any description of Japan or the Japanese has some implicit standard of comparison. In my view one can learn most if one makes that comparison explicit. Take the example of semantic differential tests which came up in our discussions—the difficulties implicit in making an equation between the word “*kōfun*” and the word “excitement.” The process of sorting out these difficulties is not just an exercise in exploring the connotations of *kōfun* in Japanese. It is equally and simultaneously an exploration of the concept “excitable” in English, and of the evaluations of volatility and sobriety in our own culture. I have personally found, for example, that when discussing British society with my own sociological colleagues, there are certain topics—patterns of individualism and individuation, class consciousness and so on—of which (though perhaps I flatter myself) I feel that I have a somewhat more sophisticated understanding precisely because I have had to use these English language concepts, these products of a British culture, to describe a society as very different from England as Japan.

This seems to me a form of usefulness by no means to be despised. Why not engage in the study of Japan explicitly in order to find out more about ourselves? By way of light relief, let me indulge my vanity by quoting from an article I once was asked to write on “The Japanese Personality.”¹ I still think this is something of a non-subject, but given all the warnings about modes and averages and the multiplicity of subcultures, perhaps there still is something one can usefully say about dominant tendencies. I tried to make the inevitable and usually implicit comparisons explicit, by contrasting the Japanese with the British. By trying also to give two alternative formulations to each characteristic I also discovered something—not so much something about Japan or about Britain (though one might well do so if one did some research on these lines) as about the pattern of my own ethnocentrism. The list came out something like this: The Japanese

are less self-confident and more neurotically preoccupied with retaining the good opinion of others.

are more imitative.

are more ambitious.

are more slavishly diligent.

are more submissive to superiors.

have a keener sense of personal honour and are less complacently self-righteous.

have a more realistic willingness to learn from others.

have a keener desire for self-improvement.

are less afraid of hard work.

have a more realistic appreciation of the need to co-operate in society.

are less willing to stand up for individual rights.	are less selfish.
are more dishonest and indirect in speech.	are more sensitive to, and less willing to offend, the feelings of others.
are less men of principle.	are more willing to forego the pleasures of self-assertion in the interests of social harmony.
have less sense of social responsibility to remove abuses in their own society.	are less busybody, with a more tolerant willingness to live and let live.
are more childishly naive.	have more good-humoured cheerfulness.
are more introverted.	are more shy about imposing their views and feelings on strangers.
are more sentimental.	show greater affectionate warmth and quicker emotional responses in intimate relations.

This, of course, is a special kind of comparative study, not the kind of studies to be discussed later, which are probably more useful if one takes not two, but three or four or more societies for one's comparison. The former requires that the counter-society with which one compares Japan should be one's own society. And it follows that if this is one's guiding purpose, one should choose one's particular research topics in the light of what seem to be important "problems" in one's own society. This was, indeed, what Herbert Passin was suggesting when he urged that we should look at student problems and the educational system in Japan in order better to understand our own. One might look at penal systems, too, industrial relations, divorce laws, almost anything, as a means of illuminating one's own society's problems.

A fourth possible kind of usefulness is the one which Dr. Iga's paper discussed, the application of our efforts to the investigation of social problems, of overt social problems, in Japan—problems of poverty, discrimination, deviance, and so on, drawing attention to them, trying to delineate their dimensions and urging solutions. I sympathize very much with the views Ezra Vogel expressed about this in our discussions, but the question clearly arises whether we as outsiders have a right to do this. The answer can only be subjective. Nowadays, by the time I have been in Japan for six months at a stretch, I find myself as capable of becoming indignant about things that I see around me as I do about things I see around me in England. I can feel as much concerned about the outcome of the next Japanese election as I do about the outcome of the next British

election. And it seems to me subjectively that this gives me at least an emotional and moral right to air my views about Japanese problems, and to engage in Japanese polemics. But one has to face the fact that other people may attach more importance to citizenship and the colour of the passport one holds than I do. And although I feel I have the right to become engaged, I do not really think I have a right to expect the Japanese to take me seriously when I do so. If they do, then one is lucky, but this is hardly the sort of aimed-for usefulness that can be universally recommended.

All four of the possible forms of usefulness I have tried to describe so far simply require description. They are ethnographic pursuits (or, as some who attach importance to the divisions between disciplines fortuitously produced by the growth of universities might prefer, sociographic pursuits). They can hardly be expected in themselves to produce contributions to sociology or to theoretical anthropology or to psychological theory. Yesterday in our discussions of kinship it was claimed that one contribution of students of Japan to general kinship theory was the notion of fictive kinship. That is perhaps going beyond simple description, but to me that is still not theory. Developing descriptive categories like "fictive kinship" is certainly the beginning of theory, but in my view it is not until one is concerned with formulating and testing propositions to the effect that fictive kinship is likely to occur in societies of type X, Y, and Z under conditions A, B, and C, that one can really claim to be making contributions to theory. And this, of course, requires comparison: comparison of different societies or comparison of different social situations within a society. I don't know who it was who said "comparative sociology" is really a tautology (all sociology requires comparison), but it seems to me a sensible remark.

However, to say that making contributions to theory is more intellectually respectable—a higher order of intellectual activity than mere ethnography or sociology—still does not solve the problem of usefulness. The world is full of theories: about stone age migration, about the conditions for entrepreneurial capitalism, about the relation between dating patterns and parental golf club membership, or whatever. If one approaches the study of Japan as an attempt to develop more and more sophisticated theory to which Japan simply contributes one example—a crucial example, it is hoped—one is still left with the problem of deciding *which* are the more important kinds of theory to develop, and the further problem of justifying their importance by some criteria of usefulness.

I would like to suggest two fields of theory for the usefulness of which one can make out a reasonable case: the first we have discussed a good deal, the second we have hardly mentioned in our discussions. The first is the study of contemporary Japan as one example of an industrial society, as a means of investigating what tendencies are common to industrial

societies in general. The second is the historical study of social change in Japan in the last century in comparison with the social change which has taken place and is taking place in other economically developing societies.

The first, of course, belongs to the general framework of ideas known as "convergence theory," about which Robert Cole has written a whole paper. I personally find this the most interesting framework within which to work. It has the additional advantage that most of us have friends in the Japanese academic world with whom we would like to work and it is a field where collaboration can provide absolutely equal pay-offs for both parties. The pay-offs are equal because insofar as we achieve a better understanding of anything, insofar as we can test doctrines of technological determinism, insofar as we can gain a better knowledge of the functional interdependence of institutions in industrial societies, we become better able to forecast, and so to control our future—not just the future of American or British society, but equally of Japanese society—or of other industrial societies too, for that matter. Of course, this is an area of shifting sands in which little is likely to be established with any certainty. But even if one does no more than slightly strengthen probabilities, it seems to me a worthwhile endeavour which can be justified as having considerable usefulness.

As for the second field, the study of Japan as a model of economic development, I find it rather surprising that it should have received so little attention at a meeting such as this in the year after the Meiji centennial, the year when, in Japan, the fanfares were blown with such gusto to celebrate a century of rapid transformation—though some, I know, are working on this theme: Ken'ichi Tominaga, for instance, has given me a copy of an interesting paper he has written on Japan and Thailand. I personally am very skeptical about the possibility of deriving any wholesale prescriptions for contemporary developing countries from the Japanese model. The world has changed enormously between 1870 and 1960 and an enormous number of things in the external environment of contemporary developing countries are *not* equal to the external environment of 19th century Japan—the technology or the patterns of industrial organization available to be imported, the international trade situation and the political and ideological pressures coming from outside, and so on. All of these things make a tremendous difference so that any wholesale prescription of "the Japanese model" as a package deal is ruled out from the start. Nevertheless, for all the large questions which are begged in our *ceteris paribus* clauses, there are some topics, like, for example, the relationship of universal literacy to labour commitment, or the relation of family structure or ideological systems to political stability, or entrepreneurial motivations, where the study of Japan can, I think, help to increase our understanding of the social preconditions for economic development, and so help to make the recommendations of those who scurry around the world as development

advisors for the U.N. or the World Bank a little more intelligent.

That concludes my tour around what you might call my map of usefulness. I still think, of course, that like classical scholars or students of Turkic languages, we are entitled to say: I too am a member of the academic profession, and as such part of my *raison d'être* is the symbolic reaffirmation of the importance of knowledge. We are still entitled to say that we are doing what we are interested in, because it's interesting, because it's interesting, because it's interesting. But, it seems to me, unless we take one or several of the criteria of usefulness which I have suggested above—or perhaps some others which I have missed—we are hardly entitled to say that one line of research activity is more *important* than another.

LONDON SCHOOL OF ECONOMICS

NOTE

1. *Asia, A Handbook*, ed. Guy Wint (London: Bland, 1966).