ON THE RELATION OF ANALYSIS TO THE SITUATIONAL FACTORS IN CASE STUDIES

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This memorandum should not be considered a full-fledged paper. It is the draft of an argument designed to serve as a basis for discussion.

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I.

The following observations -- which, incidentally, are not the only ones I had an opportunity to make during my activities as a "roving" consultant to the Bureau since July 1957 -- center around two topics:

1. The degree of awareness of the generality level attained in case studies and multiple case studies.

Instances of such studies are the drug study, several medical profession studies, etc. To simplify matters, the term "case study" will also be applied to research on multiple cases wherever a misunderstanding is unlikely.

2. The degree of consideration given in analysis[*] to "situational" factors.

"Situational" factors or determinants are factors which account for the peculiar character of the case or the cases analyzed. This concept refers mainly, if not exclusively, to "sociological" factors -- determinants, that is, which comprise structural or functional characteristics of the case on hand, economic and political conditions obtaining at the time of data collection, historical influences, etc. Often these determinants are not traceable to the properties, psychological or otherwise, of the individual respondents, in which case they seem to be identical with what Lazarsfeld-Menzel in their paper, "On the Relation Between Individual and Collective Properties," call "global collective properties."

The sum of situational factors relevant to a case may loosely be called the "total situation" at the time and place from which the

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[*]When referring to "analysis" I assume throughout that all the procedures it involves are carried out competently.

material issues. (The total situation plays a role not only in case studies proper but in national surveys as well. This is illustrated by comparative studies on an international scale, such as Lipset-Rogoff's planned study of social mobility in the U. S. and Europe; they tend to bring out, for each of the nations compared, situational determinants not considered in the analysis of the national samples themselves.)

My basic assumption is that, in order to fully define a case under investigation, attention must be paid to the situational factors bearing on it; altogether they determine its position in the social process.

II.

There are case studies which manifest a strange ambiguity regarding the level of generality reached in them. An interesting example is the drug study. Of course, Coleman-Menzel sharply define the range of their data -- four Illinois cities east of the Mississippi and north of the Mason and Dixon line, none under the shadow of a leading medical center and the largest not exceeding the 100,000 mark. Moreover, the investigators never forget to localize their material (and, I should like to add, they remain acutely aware of the practical purpose analysis is to serve). So far the study is clearly circumscribed and well in focus. All the more striking is the vague manner in which the issue of generality is treated. Coleman-Menzel declare that

> the major reason for carrying out a study like this is to give knowledge about the way doctors generally come to incorporate a medical innovation into their practice. Yet the study was necessarily restricted to a single group of doctors and a single medical innovation. Generalization to other doctors and to other medical

> innovations must be done with caution. From the present study it is impossible to know just how generally the results found here do hold true for other doctors and other innovations. (Intr.-15)

Then they say that, thanks to multi-variate analysis with its emphasis on relationships between variables, their findings

> are quite likely to be valid for other generally similar populations and situations. (Ibid.)

What exactly does this mean? If I am not mistaken, it means that their findings do not apply generally or do apply only under very specific conditions -- a certain geographical region, no leading medical center nearby, cities with 100,000 inhabitants at most, etc. In other words, the hoped-for generality turns out to be an illusion; it covers only counterparts of the sample itself. Nevertheless, Coleman-Menzel call generality the major reason for carrying out their study. On the one hand, they believe in generalizations if they are done with "caution" -- a somewhat nebulous directive; on the other, they cut down generality to a minimum.

III.

Their wavering in this respect is symptomatic of many a case study. But before submitting further examples, I wish to advance the following proposition: In case studies the degree of awareness of the generality level is contingent on the degree of consideration given to the sociological factors. The less the latter are taken into account -- what this means will be seen shortly -- the more the issue of generality is likely to be blurred. (Of course, the reverse holds true also.) This

proposition is almost self-evident; roughly speaking, a researcher analyzing the data of a case or a group of cases cannot ascertain the extent to which his findings are more generally valid unless he knows something, and does something, about the determinants operative at the locus of his material. Lack of regard for them inevitably entails the kind of haziness instanced above. At this point a new terminological distinction suggests itself -- that between "oriented" and "unoriented" case studies. The former try to relate their variables to the total situation, whereas the latter do not sufficiently consider the impact of local influences and in consequence leave the degree of generality of their results in the open. Whenever the tendency toward unoriented research asserts itself, it is as if research evolved in a vacuum.

At first glance, the drug study does not seem to confirm my proposition. As I mentioned above, Coleman-Menzel refer throughout to the locus of their material (and, in addition, gear analysis to a practical purpose); and yet, contrary to what should be expected if the proposition were true, they make ambivalent statements about the range of validity of their findings. Now I believe it possible to show that, appearances notwithstanding, their wavering results exactly from the reason I indicated -- a neglect of the situational determinants. To be sure, the investigators are constantly aware of their point of departure; their study is and remains a study of four smaller cities remote from a leading medical school. But the fact that they keep the circumstances conditioning their sample in mind does not necessarily imply that they also consider, and explore for their research operations, relevant components of the total situation. There is more psychology than sociology in the study. Just to illustrate what I mean, the

variable "social friendships between doctors" might have been related to an assumption about a "collective property" of cities which have only 100,000 inhabitants or less. Perhaps in such cities of limited size the professionals of this or that denomination are more in need of informal relations among each other than those in the larger cities with their multiple social outlets. Consideration of this possibility would have established a connection between the size of the population and the importance of social friendships and thus helped localize the latter in sociological terms. So far as I remember, Coleman-Menzel in their description of the sample do not attempt to account for the ratio between the number of doctors and the size of the population. Are there, according to commonly accepted standards, too many doctors, enough doctors, or not enough doctors in each of the four cities? More likely than not, the degree of saturation plays a role in the flow of information about a new drug; consequently, it might have paid to "operationalize" this sociological rather than psychological concept.

Other studies are even less "local-minded" than the technically superb drug study. Take Wagner Thielens' "Some Comparisons of Entrants to Medical and Law School," based on a sample from one Eastern medical and one Eastern law school:

> The findings reported here, says Wagner Thielens, cannot, of course, be assumed to hold beyond the scope of the two schools, since without additional comparisons we do not know to what extent they must be interpreted only in terms of local situations within the two schools. But it is tentatively assumed that conditions obtaining more generally are reflected at least in part of these findings. ("The Student-Physician," 131)

In contrast to the accuracy of his comparisons, Wagner Thielens' "tentative" assumption is surprisingly casual and vague. Add to this that he does not really care about the "local situations within the two schools." In the summary of his findings no mention is anymore made of their limited generality; as a matter of fact, they are represented in such a way that they almost look like results of general validity. -- To stay with "The Student-Physician," I would also count here, for instance, such case studies as Huntington's "The Development of a Professional Self-Image," or Martin's "Preferences for Types of Patients." Kendall-Selvin's paper, "Tendencies Toward Specialization in Medical Training," on its part differs from the just-mentioned studies in that it not only shows an appreciable awareness of the source of the data and thus avoids inadvertently gliding into loose generalizations but traces one of its statistical findings to the presence of a nonpsychological situational factor, namely, the possible influence of the staff of Cornell Medical College on the decisions of fourth-year students. However, such references from psychological variables back to sociological determinants are very rare indeed, I guess.

To sum up, numbers of case studies are in the nature of unoriented research. They treat the major issue of generality in a cursory and ambiguous manner which contrasts with the internal accuracy of the quantifications and therefore all the more suggests that there is something wrong in the sub-structure. If my proposition holds, the vagueness about this issue is inextricably bound up with the reluctance to explore and utilize the situational factors which frame and condition the material analyzed. Unoriented research turns its back on the total

situation. This explains why the studies I have in mind give a see-saw impression; they undecidedly waver between specifying and generalizing statements -- whereby it should be noted that (unwarranted) preference for the latter is conspicuous in those case studies which are intended from the outset to illustrate something general, say, the drug behavior of doctors, processes of socialization, etc. They have a natural bent for hurrying away from the locale of their sample and plunging into the unlocalizable limbo of psychological extrapolations from which, as may be anticipated, a return to sociological categories proves extremely difficult.

IV.

After having roughly outlined my position I had better pause a little and discuss at least one of the objections that may be raised against it.

This objection runs as follows: For analysis in case studies to start with a consideration of situational factors would mean to put the cart before the horse. The task of uncovering these factors can be carried out only in the analytical process itself. And the analytical process extends far beyond the single case study. Any such study calls for supplementation by a series of comparative studies which alone are able to shed light on the peculiarities of each of the cases under investigation, thereby permitting the researchers to sift out the more generally applicable findings. (I learned from Patty Kendall that, with this purpose in mind, questionnaires have been sent to all medical schools in the U. S., the intention being to achieve, in the medical

profession studies, the high generality of a nation-wide survey.) To epitomise the argument, definition of the determinants of a case is contingent on a sequence of comparisons which can be expected successively to reveal all differential local influences. The total situation, that is, is not so much a starting-point (in the form of hypotheses) as a result (in the form of precise description). The indefinite generality of case studies is therefore nothing to worry about; it is just a sign that they are something like stepping-stones.

No doubt this argument carries weight. However, there is a methodological snag in it. (In speaking of a methodological snag, I immediately discount the fact that actually case studies are rarely followed up by systematic comparative research. For a variety of reasons, economic or otherwise, social research proceeds more haphazardly in this respect than research in the natural sciences.) Now what is methodologically wrong with too great a reliance on comparative studies is the following: If, as I assume, the situational factors have a bearing on the analysis of the situation to which they belong, then the variables selected for analytical treatment must refer, somehow, to these factors in order really to describe and interpret the situation under consideration. If they do not, analysis, in interrelating them, always runs the risk of reflecting obliquely the case investigated.

> In the drug study Coleman-Menzel say that "scientifically-oriented doctors" were among the first to receive information on tetracycline and introduced it at an early stage, whereas graduates of competitive medical schools were relatively slow in introducing the drug. This statement, contradictory by implication, is commented upon as follows: "...the slower introduction of tetracycline by graduates of competitive schools is evidently not due to the greater scientific caution of these competitive school graduates but to some

> other variable. In the data which we have this variable is not evident; it may be some <u>spurious factor</u> (italics mine) existing in the particular sample of doctors examined. In any case, the relation, which is not a large one, remains unexplained." (IV-19)

The summary characterization of the behavior of graduates from competitive medical schools as a "spurious factor" may be due to the obliqueness of the variables chosen. I have definite misgivings about the consistency (and adequacy) of the concept "scientifically-oriented doctors." It is distilled from sundry data (Appendix D, 1-2) which in my opinion do not fully warrant its construction. Sometimes the desire for establishing, as fast as possible, statistically manipulable variables seems to get the better of the concern for their substantiality. Of course, frequently the variables are solid and virtually relevant to the local contexts. As mentioned previously, in the drug study the category "social friendships between doctors" might easily be related to a property of smaller cities. And in the previously mentioned Kendall-Selvin study analysis is so conducted that it directly brings a situational factor -- faculty influence on fourth-year students -- into focus. (Interestingly, the research design there is such that the assumed existence of this factor comes almost as a surprise. This need not have been so. Perhaps it would have been possible to direct attention to the local determinants, including faculty influences, from the very outset and examine their bearing on the whole curve of student tendencies toward specialization, not only on the last phase of it.) Yet in case studies which are less sociologically-oriented than Kendall-Selvin's the adequacy of the variables to the given situation is by no means certain. And whenever the variables are chosen and utilized with little regard for the pertinent sociological

configuration so that analysis is apt partly to distort the reality it is supposed to describe no comparative study or sequence of such studies should be expected to remove the ensuing opaqueness of the analytical description.

V.

I now propose to circumscribe the concept of situational factors in more detail. The first thing to do is, naturally, to point to existing studies which gravitate toward oriented research. Contextual analysis in studies of a family of cases ranks high as a systematic attempt to lay hold on, and operationalize, local determinants of each of the cases analyzed; in the Teachers Apprehension project, for instance, size, wealth, and political preferences of diverse colleges are brought into play. To mention a few other relevant studies, contextual or not, the ITU study establishes meaningful relationships between individual properties and such collective properties as the ideological climate in larger and smaller shops or even, if my memory does not deceive me, the impact of tradition; the polio study takes structural features of national organizations into account; McPhee's program for research on television is crowded with assumptions on the total situation; and the study of the Religio-Psychiatric Clinic which is a single case study derives, with the aid of ideal-types and Parsonian dichotomic variable-patterns, its psychological units from estimates of the total situation based on transcriptions of group discussions and participant observation.

Examination of these examples leads to two conclusions, one concerning the range of situtional factors, the other the problem of their selection.

First, definition of the total situation involves various areas -- not only sociology proper and social psychology (whereby, in view of the current bias in favor of "psychology" the emphasis should be put on the sociological rather than psychological component of this discipline) but also economics, politics, anthropology, history. The area of history belongs among the most neglected for the simple reason that, because of their understandable aspiration to generality, case studies tend to care little about the place which their case occupies in the historical process; they forget the longitudinal section in the cross section. But this oversight might endanger the precision of analytical description. The operational significance of the historical factor is prominent in research on institutions or groups with a live tradition and a history which palpably conditions their present shape and functioning. Take a study of church congregations: it requires the researcher to deal in images, concepts, and issues saturated with values and meanings which have accrued to them in the course of history. Accordingly, the contents of these notions are important in their own right; they figure as such and such contents in the minds of the parishioners. Which in turn means that analysis is adequate only if it considers the possible implications of the contents themselves no less attentively than the attitudes toward them. A complete isolation, for analytical purposes, of attitudes from traditional notions whose content carries weight as this particular content may falsify the picture. At any rate, the authors of "The Authoritarian Personality" would have come closer to the truth if they had related their personality syndrome to the historical and national contexts in which it actually appears. It would also have

been better psychology for being less of it. -- A companion project to Benson's exemplifying the usefulness of the historical approach for social research under certain circumstances might prove extremely interesting.

Second, the selection of possibly relevant situational determinants depends upon the case investigated. Lowenthal in his study of biographies in popular magazines over a period of time orients research toward a historico-economic factor -- the ongoing transition from a production economy to a consumption economy. When devising the report on Satellite mentality, Berkman and I introduced,on the basis of a careful reading of the interviews, a number of politically important topics which then served us as a starting-point for analysis proper. Incidentally, a comparison between this report and the INRA report on the same interviews -- our qualitative analysis was to supplement, or complement, the quantitative analysis of the latter -- makes it quite clear that the situational factors must be hypothesized at the outset in order to affect the analytical process. INRA's report was relatively unoriented; the researchers gave the material a routine workout without, at the beginning, culling from it clues to (significant) components of the given total situation and proceeding accordingly. But this precisely is what Berkman and I tried to do. So we felt justified in exploring the interviews for such topics as the "unreality" of the Satellite world, the distinctions made by Satellite non-Communists between several types of Communists, the different attitudes of non-Communists toward resistance in times of relative calm and times of acute crisis, etc. -- topics referred to in the material but left unmentioned in the INRA report. The moral is that you cannot extricate from analysis

what has not been tentatively put into it. To stress an important point, the decisive difference between the two reports is not that one prefers qualitative analysis (much of which might have easily been quantified) and the other indulges in quantifications; rather, the decisive difference must be sought in the research design: we made an attempt to utilize socio-political determinants, while IMRA did not.

As for the question of how to seize on relevant situational factors, I should like to suggest that it be taken up in the planned seminar;the best method is perhaps to discuss one or two unoriented case studies, with a view to reconstructing the total situation from which their samples stem.

VI.

It follows from what has been said till now that a case study is oriented only if its variables or units are related to the possibly relevant sociological determinants. Only then will research achieve "the precise description and analysis of social events" (quoted from Lazarsfeld-Barton's paper, "Social Research in the United States"). The task with which the researcher is thus confronted requires closer definition. Except for research on "global collective properties," practically all case studies analyze their sample for properties of the individuals and/or groups involved; it is the humans and their interrelationships that count after all. Altogether these properties -- attitudes, behavior-patterns, preferences, status aspirations, etc. -- belong to what may be called the "psychological dimension." Similarly, the situational factors on their part may be said to constitute the "sociological dimension." The necessity for case research to select

the psychological properties which appear as variables of analysis in such a way that they have a distinct bearing on the pertinent total situation can now be formulated as follows: The sociological dimension takes precedence over the psychological dimension. Or conversely, in the interest of a "precise description and analysis of social events" any psychological unit must be traceable to the ensemble of sociological characteristics framing it. Within social research even a seemingly self-sufficient pattern of personality traits is not an entity in its own right.

Among the studies which acknowledge the primacy of the sociological dimension Merton's recent paper, "Priorities in Scientific Discovery," is of special interest methodologically for two reasons. First, Merton shows that the recurrent priority disputes call for a sociological rather than psychological explanation; they persistently flare up because the code sanctioned by the fraternity of scientists requires that new findings be attributed to their discoverers. A phenomenon which on the surface appears to be motivated psychologically thus turns out to be a genuinely sociological phenomenon. (Had Merton been less aware of the dependence of psychological properties on sociological determinants, he might have "explained" the fact that in priority disputes scientists take up the cudgels for themselves as well as their confrères from some pattern of personality traits in which, at critical moments, the desire for recognition and prestige scores higher than self-denying modesty; patterns of this kind can be made to fit any event or situation; yet in insisting on the sociological nature of the phenomenon, he judiciously avoids the pitfalls of purely psychological constructs, which also permits him to leave the

eternally fluctuating relation between modesty and prestige aspirations undefined.) Second, Merton arrives at his conclusions by way of a comparative survey of relevant cases which is oriented toward situational determinants from the beginning. The same applies to all the units of analysis as a matter of course. He does not rely, that is, on unoriented comparative research to define the sociological character of priority disputes; rather, he finds out about it in the course of comparisons which are permeated with sociological considerations. This highlights a point made in earlier contexts: that comparative studies whose units are impermeable to the sociological dimension stand little chance of detecting situational factors in the process. -- As has already been indicated above, Klausner's study of the Religio-Psychiatric Clinic belongs here also. True, it concentrates on the divergent attitudes toward psychotherapy of a number of cooperating ministers and psychiatrists, but attitudinal analysis is framed by a phenomenological description of the outlook of these people: They are identified as deviants; and their opinions about the functions of the church and the significance of therapy are confronted with the institutionalized views of the clergy and the medical profession. In other words, Klausner locates his variables in sociological space by relating them to the collective properties of two social systems. -- In my own book, "From Caligari to Hitler," I hypothesized, on the basis of my material, the existence of certain inner dispositions among the Germans of the Weimar Republic; instead of passing them off as independent entities, however, I tried to embed them in the sociological contexts of the period and to trace changes in collective psychological behavior to the changing economic, social and political conditions. It was psychology in the

sociological dimension and sociology derived from psychological constants. (After my book had appeared, a psychoanalyst told me: "Your psychological analysis is fairly correct so far as it goes. But it doesn't go far enough." This exactly was what I wanted to avoid.)

VII.

The current style of social research does not seem to encourage consideration of the situational factors. I infer that much from the fact that numbers of researchers shy away from the sociological dimension even in cases where it is fully exposed to view. They succumb to what may be called the "psychological fallacy."

Take Anderson's paper, "Some Contributions to the Study of Social Perception": based on data of the Teachers Apprehension study, it aims, among other things, at determining the impact of the college teachers' "caution," "worry," and "permissiveness" on their perception of "incidents" at the respective colleges. (The contextual part of analysis may be neglected here.) The underlying assumption is, of course, that the three variables "caution," "worry," and "permissiveness" are psychological properties and as such of one and the same nature. Yet what, perhaps, holds true of "caution" and "worry" -- by the way, how differentiate between them? -- does certainly not apply to the concept "permissiveness." It is no genuinely psychological concept. The six indices from which it is built amount to value statements about situations which would have in varying degrees involved political risks at the time of the data collection. Consequently, these indices bear not so much on the respondent's psychological make-up as on the range

of his political judgment. Far from suggesting a psychological disposition like "caution," or "worry," or "tolerance," they at most denote a temporary attitude toward Communist infiltration, as it affects the campus and the traditional notions of academic freedom. Whether or not "permissiveness" is a substantial unit is at least controversial. But once it has been constructed, emphasis on its threadbare psychological component is hardly justified. The sociological connotations of this problematic unit are indeed so obtrusive that unconcern for them must be laid to an ingrained habit of severing the umbilical cord between the psychological dimension and the sociological dimension at the expense of the latter.

Professor Brunner and David Wilder told me that they have come across the same habit in their survey of existing research on adult education. In accordance with the declared preferences of the educators themselves, most studies investigate reasons for enrollment, student attitudes -- do you like the course? -- changes in attitudes, the kinds of gratifications derived from attendance, etc. The bulk of research -- much of it consists of doctoral dissertations free from nonacademic obligations -- centers around motivations, human interrelationships, group dynamics, and the like. Which is to say that, all in all, these studies give a wide berth to adult education as a movement which raises sociologically important issues. Adult education has the function of providing broad strata of the population with knowledge in the areas of their concern. Well, does it fulfill this function? The run of the studies fails to broach the problem of communications content. And yet it would not be too difficult to ascertain and measure the gain in knowledge achieved by those who attend the courses. Another relevant

issue pertains to the sociology of culture: what happens to high-level knowledge if it is passed on to relatively untrained students? The significance of this issue for adult education and its cultural objectives is obvious. An evaluation of transcripts of lectures by authorities on the subjects treated might yield yardsticks for the analysis of teaching performances and enable the researchers to score their adequacy. And this leads to a consideration of the role which the educator's training plays in the communications process. Etc. The point I wish to make is simply that adult education poses problems which reach deep into the sociological dimension; that these problems stand out glaringly; and that nevertheless a great deal of autonomous research shows little regard for them. It is as if the "psychological fallacy" blinded many a researcher to their existence and at the same time compelled him to proceed along lines which virtually serve purely manipulative purposes.

VIII.

Let us look more closely into the structure of unoriented case studies. They share characteristics which result from their indifference to the situational factors and their concomitant tendency to blur the level of generality. Analysis in such studies comprises a series of steps most of which do not directly bear on my argument and will therefore be omitted. Of interest here are only the two subsequent major operations.

1. Establishment of variables or units. According to premise, these variables are not identical with situational factors; they are "internal" variables. (Of course, the same applies to the hypotheses underlying them.) As a rule, the variables selected designate properties

of individuals or groups -- attitudes toward a minority, preferences for a political party or a musical genre, personality traits, prestige aspirations, and what not. These examples show that the units used in case studies mostly consist of a sociological and a psychological component; for instance, preference for a political party points to both the social entity preferred and an inclination of the individual preferring it. Even though the variables do not lie in the sociological dimension itself -- amounting, say to a "global" property of smaller cities or the content of a tradition-laden notion -- many of them might nevertheless permit the researcher to follow up the implications of their sociological component and thus work his way toward the situational influences at the time and place of the data. In unoriented research, however, these communication lines are largely blocked. There the emphasis is on the self-sufficiency of the variables, their independence of the local determinants. Otherwise expressed, their psychological component is made to overshadow what they include in "system references." A major reason for their isolation from the total situation is presumably the concern, at the outset of analysis, for exact measurements; attempts to account for the situational factors would introduce considerations apt to hamper, or even obstruct, the quantification processes. So the attitudes toward a minority are essentially valued as a psychological property of individuals. Note that this preoccupation with the psychological component of the variables not only tends to obscure parts of social reality which they potentially cover also, but may well lead to problematic psychology. It is by no means certain whether all the attitudes, preferences,behavior-patterns, etc., elicited from the data for analytical treatment represent genuine properties of

the individuals with whom they are associated. Individuals whom the evidence reveals to be biased against a minority may manifest such a bias only within the given social contexts. To be sure, one might argue that an unoriented case study dealing with antagonistic attitudes toward a minority does not assert more than precisely this; but by disregarding the social contexts relevant to the case, the study creates the impression that the bias is, so to speak, a quality of the individuals showing it -- an impression which may be deceptive.

To sum up, unoriented case studies tend to resort to variables which are not derived from hypotheses about the situational factors. Although numbers of these variables are transparent to the sociological dimension, they are often used with manifest disregard for their references to that dimension. Under the impact of the "psychological fallacy" all the light falls on their psychological component. Yet this does not necessarily transform them into intrinsically psychological units either. It is as if those which do not belong here nor there occupied a sort of twilight region. Generally speaking, we are confronted with the problem of whether the variables of unoriented research really facilitate "the precise description and analysis of social events." (Incidentally, it is a problem which also concerns contextual analysis. In order to get hold of the total situation contextual analysis may either start from sociological assumptions and construct its variables accordingly, or avail itself of the kind of variables characterized just above and reinsert them into sociological contexts. On principle, the second alternative should be expected eventually to yield as much information about relevant sociological determinants as the first. However, my guess is that the first

alternative -- selection of sociological variables at the outset -- is more likely than the second to attain the goal of contextual analysis. For in operating with variables relatively remote from the sociological dimension, this type of analysis runs the risk of tapering off into an asymptotic approach to the situational factors or at least becoming increasingly cumbersome. Of course, this need not be so. But even supposing that the second alternative fully serves the purpose, I cannot help feeling that it relates to the first in about the same way as the Ptolemaic system to the Copernican.)

Statistical analysis. The statistical processing of case material establishes, as accurately as possible, the existing relationships between the occurrences which fall under the variables selected. It may be taken for granted that quantitative analysis results in a refined description of any case analyzed -- a description which, I hasten to add, is naturally limited to such aspects of the case as the variables denote. The problem I want to raise here has nothing whatsoever to do with these mathematical operations themselves but bears on the following matter: does, in unoriented case research, the analytical treatment to which the data are subjected affect the signifying power of the variables covering them? My tentative proposition is that it does. To be more precise, it appears that in the studies under consideration the statistical elaborations tend further to divert attention from what the guiding units include in sociological references. The reason is this: As has been submitted above, many units or variables point to both the psychological and the sociological dimension. And even though the researcher indifferent to the latter actually concentrates on the psychological component of the variables,

he is theoretically still at liberty to look about and follow up the clues they offer to the situational factors. The variables themselves preserve their double character; they are not yet isolated from the sociological determinants but continue to be in a measure suggestive of the total situation. Now notice what happens when the data defined by these variables are statistically analyzed for correlations, other kinds of relationships, deviations from observed regularities, etc. In unoriented research the operations which are then taking place involve the variables not as units belonging to the sociological as well as the psychological dimension but as elements independent of any allegiance that might impinge on the researcher's freedom to combine and interlink them at will. They must be considered self-sufficient or else they cannot be correlated freely. (It is understood that I do not think here of large-scale surveys in which statistical regularities are the point of departure for interpretation; nor do I speak of such statistical operations as may be called for within the framework of case research which incorporates the sociological dimension.) Due to an unrestrained emphasis on statistical analysis, the psychological component of each variable becomes automatically all-important and eventually stands for the whole of it. In consequence, the mathematical workout given the variables does not increase our awareness of their references to the sociological determinants but on the contrary leads even farther away from them. Instead of bringing the social influences at the locus of the material again into view, it renders their reconstruction more difficult.

IX.

And what about the findings of unoriented case studies? To begin with, they are ambiguous as to location. On the one hand, it is obvious that they relate to data assembled at a certain time and place; to this extent they are well-localized. On the other hand, they are not embedded in the social contexts from which the material issues so that their location remains indistinct; and this being so, they tend to evoke the illusion of a generality similar to that of the natural sciences. Hence the haziness about the generality level discussed above. Suffice it again to mention this ambiguity which is only in the nature of a symptom after all.

It is symptomatic of the way in which the findings describe the social events on which they bear. There is no doubt, of course, that all the relationships discovered and established in the course of analysis reflect actual relationships. The findings portray accurately existing occurrences and expose to view much of their otherwise inaccessible interplay. The question is only what part of the social events do they cover? Since analysis not initially framed by sociological considerations disregards the situational factors, its results will not take them into account either. So the findings are likely to neglect essential aspects of the case they are intended to describe. They amount to a fine-spun texture of correlations and relationships all of which, however, are left undefined in terms of the social circumstances generating them. Once again, the configurations revealed by unoriented analysis do exist; yet they render not so much the social events in their fullness as the sediments which these events deposit in the psychological dimension or somewhere between it and the sociological

dimension. Otherwise expressed, it is the shadows of the events rather than the events themselves which are summoned by analysis unaware of the sociological determinants. This corroborates an observation advanced above -- that unoriented case studies are threatened with rendering their cases obliquely. To be sure, the relationships established in any such study are true to fact; but from the angle of hypotheses about the total situation it may well prove necessary (1) to assign to these relationships new weights, and (2) to introduce sociological variables. Both measures would completely alter the picture, without for that reason giving the lie to the factuality of the previously established findings. The accuracy of statistical analysis should not be confused with precision in the description of social events. (My POQ article, "The Challenge of Qualitative Analysis," includes some remarks on this point.)

Unoriented research, then, yields findings of indeterminate generality and doubtful relevance to the cases at issue. Because of their unrelatedness to social reality proper they lack a frame of reference. Its absence is of consequence. It accounts for the unhampered sprawling of analysis, the emergence, among the findings, of relationships which, it is true, grow out of the analytical process but have no other merit than to occur at the place of the data. They are just transient happenings. This indiscriminate exhibition of all that can be brought out by mathematical operations may be traced to two reasons. First, it almost is as if the accumulation of findings of problematic significance, and the preoccupation with utmost accuracy in securing them had the function of substituting for the omitted situational factors; as if the researcher felt there is a gap that must

be filled and then filled it with everything he is able to wrest from the material. Second, his insistence on doing so may also spring from the belief that procedures in social research are identical with those in the natural sciences and that therefore seemingly insignificant results of analysis stand as good a chance as significant ones of being recognized, some day, as indispensable contributions. Yet is there really an identity of method and goal? Let alone other integrant differences between the two approaches, the findings of social case studies cannot lay claim to the genuine generality inseparable from all findings in physics. So it happens that at least part of unoriented research appears to be in excess of what would be needed were research framed by considerations of the possibly relevant sociological determinants. Or to say the same in the reverse way: if there were more analysis guided by assumptions about the total situation, much of what now hangs loosely in mid-air would automatically disappear. Orientation would dispose of the sprawling: sociology would do away with the "psychological fallacy."

X.

At the end I wish to submit a guess for what it is worth -- that one of the origins of unoriented research is market research. Most of the foregoing comment on the former does not apply to the latter. Being as a rule concerned with short-term causes and effects, market research need not inquire into the total situation; and it is oriented inasmuch as its analytical efforts are geared to, and thus limited by, a practical purpose, however insignificant from a sociological point of view. But

no sooner does case research lose sight of a practical purpose and inadvertently cease to confine itself to findings of short-term validity than it immediately assumes the character of unoriented research, provided it does not at the same time integrate into analysis the situational factors.