Socio-Cultural Factors and the Development of Sociology in Latin America

1. Introduction.

The sociology of sociology is a chapter in the general sociology of knowledge that should be of particular concern to the professional sociologist. In principle, it should tell us something about the conditions that further or impede what today is considered scientific sociology, and thus give us some insights that can be used for some practical social engineering, in casu to develop sociology as a science. For this to happen the most rigorous techniques of data analysis and the most imaginative theory formation would be required. Unfortunately, what we shall present here comes short of that goal, it is more in the nature of notes and observations for a real study.

What is presented here is a kind of prolegomenon to an investigation of that kind. The focus is on Latin American sociology. The technique is more that of the traditional anthropologist than of the modern sociologist: it is based on the notes taken by a participant observer, himself a sociologist, teaching graduate courses in a regional institution depending on the UNESCO, travelling in most countries in the region, talking with most of the active sociologists trying to find out how social science as a system functions. The data, consequently, are not standardized but informal, so what is presented is hardly more than a set of hypotheses. Broadly speaking, the method of analysis is simply this: the author knows something about the sociology developed in Northern Europe and USA on the one hand and in Southern Europe and Latin America on the other. There is a noticeable difference in achievement, for instance, as measured by some simple index of who is most quoted in articles and books. [1] (South quotes North and not vice versa. [2]) The question is why, and since a difference can only be explained by other differences, the question is what other differences seem to be related to this difference in the output of scientific sociology.

A point of departure is the self-analysis, presented by some Latin American sociologists, which can be subsumed under the headings of "the political argument", "the economic argument", and the manpower argument. The political argument runs as follows: sociology is meaningless without a relatively open society, since

it is an institutionalization of 1) a free search for relevant
data about the society, 2) free theory formation about the society
inclxding publication, since science by its requirement of inter-
subjectivity is bound to be a public institution. A scientific
product should be publicly accessible; if it is not it may still
satisfy other scientific requirements, but it is impossible to
assess whether it does so or not. Since empirical data and theory
construction are found <u>together</u> in the sociology of only relatively
few countries (in LA sociology one very often finds theory without
data and data without theory), and these countries tend to be among
the most open countries by other criteria, there is undoubtedly
something to the argument. [3)] An open country is, perhaps inevitably
so, a pluralist country with a relatively open exchange market of
ideologies. In the closed society the ideology market is also
closed, there is a dominant ideology defining what is right and
what is true in social affairs with which sociology would have to
compete, and there is no model of how deviant conceptions of social
reality can be accomodated in the opinion market. The stronger the
dominant ideology, the more "singularistic" the conception of
society, the more difficult the penetration of sociology. This may
also apply to the case of a very strong democratic ideology, as in
the case of Switzerland. [4)]

The difficulty with the argument is its failure to explain why
sociology and sociologists do not use the possibilities there are
to do interesting and important research and particularly the
inventiveness in defining <u>a priori</u> as impossible investigations
that with the same degree of inventiveness could easily have been
launched. "This does not work here in Latin America" is a favourite
expression the foreign sociologist will get from his Latin American
colleagues as many times a day as he wants if he suggests projects,
and the political argument will be among the many reasons given.
An interesting side effect of the argument is the wedge it drives
in between what one could loosely call the "Catholic" and the
"Marxist" traditions. With Cuba as the only exception Marxists
often define themselves as deviants, as outcasts, as persecuted
more often than objective circumstances should warrant, and the
image of empirical or free sociology as "impossible" is compatible
with their image of the existing social order as basically wrong,
to the extent that they may not even want a disconfirmation of their

own theory because it becomes more important to keep the image of
the social order than to develop sociology. The "Catholic" school,
often of the left wing demochristian variety also has its argu-
ments with the social order but less basic ones, and hence are more
prone to search for the feasible, and not to define it a priori as
impossible. This is one factor among many that may contribute to
more empiricism in left wing Catholic sociology.

However, one would have to be very blind to many aspects of
Latin American reality to deny a great deal of validity to the
political argument. It is easy to ridicule, in the standard gringo
way, the stereotyped image of the Latin American student: always
on strike, always intriguing for some restructuring of everything
from timetables to the universe, everything except his own
deplorable level of knowledge. A Cuba under Batista or Castro and [5)]
a Republica Dominicana under Trujillo did not promote empirical
sociology as it is known from the North-Eastern corner of the US
or the North-Western corner of Europe - and the major concern of
the student becomes the change, in action terms, of the regimes,
not the abstract study of the conditions for change. For this also
ties in with the political argument: the general feeling that other
things are more urgent, that the conditions in the North-East of
Brazil, or in the slums of the big capitals (not to mention the
small capitals) are such that every hour spent on academic sociology
is an hour taken from political activity. The equally obvious contra-
argument would perhaps run as follows: no social order succeeds by
everybody doing the same, for instance politics. Neither Kant, nor
the existensialists, if so interpreted, were right: if I choose to
become a sociologist I do not have to justify this choice by saying
that I want all others to do the same, or that I choose on behalf
of humanity. On the contrary, I may do it precisely because others
already go in for politics, and then base my judgment on relative
expendability or substitutability: imagine I can do for sociology
what few others can do but am highly replaceable in politics, would
this not be an argument for devoting my career to sociology?
Besides, who will argue against the position that the sociologist
can contribute to the creation of an image not only of the existing
social order, but also to a richer vision of a future order, and
who will argue against the potential political usefulness of this
(even though, unfortunately, few sociologists do this)?

Before we leave the field of argumentation let us look closer
at the economic argument which runs as follows: "sociology is a
luxury and it costs much money, in our economies we simply cannot
afford it". The argument is bolstered by reference to typical
North American research budgets, involving costly field operations,
big surveys or complicated comparative designs, much IBM work, a
team of assistants etc., in short, budgets running all the way from
$ 10,000 to even a million dollars. The cost is enormous, the
immediate social value of such research can be disputed, and the
argument has considerable validity in a strained economy, as evi-
denced by the fact that sociology has gained momentum in other
countries only after a certain general affluence in the economy.

But the argument, again, fails to explain why so much simple
and cheap empirical sociology is not carried out. It costs almost
nothing to do a content analysis of newspapers, or an analysis of
elite compositions (economic, political, military elites, for
instance, to see how they intermarry) based on membership lists, or
small sample surveys carefully done. Unfortunately, many sociolo-
gists seem to think that their own prestige is more proportionate
to the size of the research budget, than to the validity of their
theories, let many chances pass by waiting for the golden opportu-
nity to "get a grant", and spend much of their time making a project
"foundationable". As years pass by waiting, the initial training
deteriorates and the chances of getting the grant become smaller.
However, as will be spelt out in some detail, the structure of
Latin American sociology nevertheless offers opportunities for the
sociologist who has never done empirical work.

After these introductory remarks let us pass on to the factors
more in the focus of the sociologically trained analyst: the socio-
cultural frame in which sociology is supposed to develop, to see
whether there are manipulable variables within this frame of refe-
rence that could be used to stimulate sociology further, so that
one did not have to wait for the political millennium (or at least
decennium), the age of affluence, or the third factor: the return
of so and so many Ph.D.'s from USA and Europe. [6] Our basic assumption
can be spelt out at once: these three factors are not only not
sufficient, there are other factors that may be even more important.
As a point of departure, let us try a simple typology of styles of

sociological work.

2. A typology of sociological work.

"Sociology" is, of course, by no means a unitary concept having only one limited and recognizable interpretation. As often noticed, many sociologists have little else in common than the title "sociologist" and vague references to their field of interest as "sociology". But the important lines of division for a fruitful discussion are hardly in terms of "Catholic" vs. "Marxist", or "functional" vs. "structural" sociology. Instead, we suggest that a typology for classification of work presented as sociology can be meaningfully based on the following five dimensions:

1.	Object of analysis:	Society vs. sociology
2.	Mode of analysis:	Descriptive vs. normative
3.	Data:	Empirical vs. non-empirical
4.	Hypotheses:	Propositionalist vs. dimensionalist
5.	Theories:	Integrated vs. isolated

In principle, this gives a 32-fold typology, where the two extremes, represented by the five left-hand choices and the five right-hand choices mirror relatively well what is often called "modern" vs. "traditional" sociology. Since development is identified verbally as the change from traditional to modern, analysis in these terms is essentially a kind of development analysis.

The first dimension refers to what the sociologist chooses as his object of analysis. In principle, it is some aspect of social reality, but many sociologists, and in Latin America very many, focus on a very special part of social reality: sociology itself. That is, they comment on what other sociologists do, they write reviews, they criticize, they place a new concept or a new theory in its sociological context by tracing it back to earlier authors, they establish genealogies of concepts and perspectives and theories. In short: their style is the style of literary criticism. Their reality is not society but the sociological mirror of society, they write more (and better) about the works of a sociologist than of the empirical facts he refers to.

They are what Zetterberg very aptly has called the <u>commentators</u>

of our field : the housekeepers, the librarians, the stocktakers.
They are meta-sociologists, not sociologists. Nobody will deny
their usefulness up to a certain point, except when this becomes
the dominant concern and the sociologist deceives himself into
believing that he gains insight in social reality by these exer-
cises in the history of science and history of ideas. To analyze
how the word "class" is used by a number of sociologists is
interesting, and may contribute to the understanding of social
class as a mode of human existence, but the findings will concen-
trate on two foci: the biographies of individual sociologists, and
diffusion phenomena in the community of sociologists. The question
is whether this should warrant more than a cursory interest by a
minor fraction of active sociologists, and preferably in their
emeritus years when a kind of level of integration is attained
where ideas about the genealogy of sociological ideas have
crystallized in their minds. Besides, only rarely is it done in
an empirical way so as to really contribute to the sociology of
knowledge.

The second dimension is well-known: it is the problematic dis-
tinction between what is and what ought to be. This dimension is
highly correlated with the first one in the sense that the commen-
tator type of sociologist seems to be very prone to make judgments
as to what sociology ought to be like (the present article is an
example), not only to describe trends and developments. In our mind
this is all to the good provided two conditions are met: that a
thorough descriptive or theoretical job is done, and that advice
and facts are kept sufficiently apart to avoid confusion. The
sociologist should, we feel, take on much more responsibility, have
the same will. : to expose his theories to clinical tests as the
medical man by spelling out the   policy implications of his
findings. But many sociologists seem to forget that the title of
sociologist gives no carte blanche, the legitimacy of the advice,
if any, should lie with the validity of the data and the careful-
ness of the reasoning, not with the (imagined) status of the author.

The third dimension refers to the level relative to data at
which the sociologist is operating. The sociologist uses a system
of symbols, usually words, and most of them non-technical. Some
symbols refer to data, other symbols have no immediate data-

referent, or the referent is not made use of. Thus, one may write
an article discussing the concept of "interaction" in social
systems, or "status", with no data references, and in such a way
that no empirical finding will have any impact on the article.
Many confuse this with theory, but a theory is a set of inter-
related hypotheses that state something. A theory says that some-
thing is the case, and, consequently, that something else is not
the case. Non-empirical symbolic work may take the form of expli-
cation only, with no proposition construction. But it may also
involve proposition formation, as in the typical work presented
by econometricians where important variables are defined, mathe-
matical relations worked out and deductions made all at a purely
symbolic level with no reference to empirical data, but often to
a hypothetical world populated by homo oeconomicus. Due to the
intrinsic complexity of a mathematical sociology this stage is
almost unheard of in sociology, and it may well be that sociology
will bypass it and develop a mathematical sociology much more tied
to data than mathematical economics has been - that is, much less
linked to "ideal cases" in the Weberian sense (today often called
"base-line models"). Whether this is beneficial for the develop-
ment of sociological theories remains to be seen.

This then brings us to the fourth dimension: propositionalist
vs. dimensionalist sociology. The idea is taken from Zetterberg[8] and
is simply this: some sociologists produce propositions, they say
how things are. But more frequent in some sociological milieux is
the dimensionalist tradition: concepts are defined, even operationa-
lized, taxonomies produced by logical multiplication and the virtue of
conceiving of them as the author suggests and not in any other way
is made very explicit - but the variables are not related. That
is: the author takes no risks relative to social reality, he ex-
cludes no constellation in social reality as impossible or improb-
able. All he says is that he who wants to describe and explain
social reality would do better using certain concepts and dimen-
sions than others. It goes without saying that this kind of ana-
lysis very often becomes normative. Work of this kind is indis-
pensable, but at the beginning, not at the end of a scientific
process.

The last dimension presupposes a choice on the fourth dimen-
sion in favor of propositions (or hypotheses, i.e. unconfirmed

propositions). Propositions can be presented related to each other
by a relation of deducibility, which is the <u>integrated</u> form, or as
<u>isolated</u> "findings". Theory-formation, or the integration of a set
of propositions, is one of the landmarks of a progressing science.
It is an indispensable part of the combination of inductive and
deductive modes of reasoning known as the hypothetico-deductive
method, where the goal is a set of propositions (relating a set of
dimensions) such that all propositions, and only those that can be
derived from this set receive a reasonable degree of confirmation.

At this point one might perhaps make another distinction by
combining two of these dimensions, nos. 3 and 5. On the one hand
is the typical inductionist among the sociologists. He works with
empirical data, and establishes, by painstaking work, isolated
propositions with a high degree of confirmation. On the other hand
is the deductionist who starts from a theoretical perspective and
deduces, more or less explicitly, more or less explicit and
testable propositions. However, to do this he has to make assump-
tions and introduce dimensions that he has difficulties in opera-
tionalizing, and the result is a low degree, if any at all, of
confirmation. The consequence is the well known distinction between
relatively flat empiricism on the one hand and abstract theorizing
on the other.

It would be foolhardy to try to spell out all the possible
combinations in detail, and to try to make a sweeping characteri-
zation of Latin American sociology by forcing it into one cell in a
typology of the kind we have presented. Latin American sociology
is obviously in a transitional phase with a number of tendencies
criss-crossing each other in an interesting way.[9] For the outsider
from the North, at either side of the Atlantic, the emphasis on
the study of sociology, the normative mode of discourse, the non-
empirical tendencies, the tendencies to be dimensionalist but also
to produce grand theory stand out - relative to what he is used to
- with some remarkable exceptions that tend to be non-theoretical
and highly descriptive.[10] One could easily demonstrate this by means
of comparative content analysis of North and Latin American out-
put in sociology, but it is hardly necessary. It is more impor-
tant to pry into the factors conditioning sociology, and we are
here thinking of socio-cultural factors. We shall divide them into

three major themes: the status of the intellectual in his social context, the relation between intellectuals, and some characteristics of what is often referred to as the Latin culture.

3. The intellectual in his social context.

Much has been said in the literature about the status of the Latin American intellectual, and the general characteristics of his status certainly play an important role.[12] To draw on the distinction in social analysis between ascribed and achieved status, let us assume that the intellectual in Latin America, relative to countries in the North Atlantic area, has an ascribed status. What are the implications of this relevant to the kind of sociology he will produce?

It does not mean that he gets his status by birth, although correlations with background data known at birth probably are still rather high. He must attain and achieve in a certain phase. Concretely, he must get his diploma and his licencia, but the question is what happens after the degree has been achieved. Outsiders may be struck by the lack of reading, the lack of constant effort to be up to date after a position has been gained, the lack of internalization of the academic norm that an academic position, however secure, has to be attained again and again, that the possessor has to reaffirm himself. Legitimacy is based on continuous output, not on an output once in his early twenties, evidenced by the diploma. One can surmise that the more diplomas one finds on the wall, the less reading takes place between the walls, for the more ascribed is the status.[3]

One concrete and very simple consequence of this is the lack of input of new ideas in lecture courses and textbooks. In small countries with only one or two chairs in sociology, this may lead to a lag of at least two full generations of sociological thought. The aging professor may have studied in the US or in Europe in his twenties, with professors who were then in their sixties, lecturing on what they had developed themselves some twenty-thirty years earlier. If the university was good, the tutor excellent and the pupil willing to keep up to date one professor of sociology per country may be sufficient for many purposes in a country with a few million inhabitants - but these conditions are rarely met.

There is a cultural aspect that ties in with this enforced stability. The idea of a university man who expands his knowledge and his field of vision is natural in a society conditioned to a concept of knowledge as Becoming, not Being. Imagine a culture where knowledge is conceived of as essentially complete, apart from minor revisions and extensions. In such a culture the very fact that the professors transmit what was once transmitted to them is already a validation of the culture and the knowledge. To change it is to challenge ~~the validity of the idea of knowledge as continued advance~~ the validity of the wisdom of the preceding generation (and the validity of one's own training as mentioned above). This is obvious, but the corollary is more interesting: to challenge that wisdom, and to change it, is to defy the establishment and may for the opposition be a value in its own right regardless of scientific merits. This will be pursued later. Here we only want to point out that a strong position given to such normative disciplines as theology and jurisprudence will favor an image of knowledge as essentially stable, and even more real than the empirical world. The facts are wrong (sinful, criminal) relative to knowledge, it is not that knowledge is false relative to the facts. This is probaly inculcated in part already in primary school.

To the extent the status is ascribed, or semi-ascribed, to
that extent will criteria for evaluation of the intellectual be
tied to the paraphernalia of the status, not to the intellectual
achievement. There will always be a need for evaluation, for
differential ranking, but it will not necessarily take the form
of assessing the validity of the scientific output. Let us mention
some other criteria that can be used when directly relevant
scientific criteria are underdeveloped: age; general style, ability
to talk and write with the prescribed mannerisms of intellectuals;
knowledge of the intellectual tradition in the field (as distinct
from new contributions to the field); certified training in other
fields i.e. knowledge of everything but sociology; no. of years
abroad; reference to famous sociologists ("I worked with X, I
studied with Y")..An interesting variant: anthropological studies
are often evaluated according to duration of field work, not
according to validity. A person satisfying these criteria can have
his statements accepted even when the same statement much better
founded is rejected because it does not come from the socially
correct person. A special case is the case of the foreigner: if he
can demonstrate the right symbols, e.g. training at the right
university and recognition of the right places, he has a chance
of getting his ideas accepted that only very few locals will have
in this system.

For lack of real criteria to judge intellectual achievement a
certain atrophy in the system with discontinuous jerks in the
curve of development will result. Persons in power , i.e. the
professors, do not have to produce but often acquire more, not less
prestige with their adherents simply by growing older. In the
extreme case they will only accept as their successors younger
people who share their points of view and the result will be
complete stability over generations in intellectual outlook. This
model, however, does not cover the transitory stage of Latin
American sociology. The typical feature seems to be: a chairsis
conquered, the atrophy sets in relatively quickly for lack of
time or lack of necessity to keep really up to date; new ideas
accumulate in the younger generation; a polarization along genera-
tion lines sets in until there is a revolution where the young
get into power, or a secession where a new institute or new
university for that sake is established, expressing adequately

the point of view of the younger generation. For some time this
leads to renewal, but then the process is repeated with a new
period of atrophy. The scarcity of chairs makes the time intervals
between the jerks quite long, and the lack of contact (see below)
the generational conflict very brutal and absolute. Everything
old is defined as wrong and the virtues of the new are greatly
exaggerated. There develops a myth both about the futile past and
about the glorious future - just as in politics - and the new
leaders dig their own grave by promising far too much.[5]

The degree of ascription attributed here to the status of the
typical Latin-American intellectual is a reflection of the general
level of ascriptiveness in the societies in the region. Thus the
intellectual has remarkable difficulties with manual work. He
needs an office assistant to carry books for him from the library,
and what is more significant: he has difficulties undertaking such
"manual" jobs as operating an IBM sorter-counter, a calculating
machine or even a slide-rule. This forces him to buy assistants,
which is costly and often not rational and removes him from
close contact with his data. Even such important clerical opera-
tions as filling in questionnaires, revising them and coding them
are often defined as beneath his dignity. This, indeed, is also
the case in other corners of the world, but when it happens to
sociologists who have gone through a period of active training and
contact with data and done the data processing and the data ana-
lysis themselves it is less dangerous to the process of integrating
theory with data. It is hardly necessary to say that there exists
a solid body of arguments justifying this lack of contact, argu-
ing the futility of empirical research in general, and against
"mathematics" in particular. Arguments of this kind are too
sweeping to have general validity, and serve to draw an extra
wedge between empiricists and theorists.

An important side-effect of the ascribed nature of the status
of the intellectual lies in the relations between students and
professors. A professor has passed the magic line, he is on the
other side, and should have the paraphernalia of social rank.
Uncertainty, anxiety, lack of real criteria lead to excessive use
of criteria extrinsic to the field also when the students are
evaluating their teachers: an enormous emphasis on personal

qualities,   how well does he talk, what kind of training does
he have (candidates for fellowships almost invariably refer to
where their professors have been trained), to what "school"does
he belong, how old is he - almost anything except what sociology
does he teach. This is the pattern of expectations often directed
to the professors, and as mentioned above, it is complementary to
what the professor is trained to expect from himself. If he does
not keep himself as close to the research front as possible he
will have little else to offer than extrinsic criteria and will
try to maximize his achievement on these - one of them being in
terms of rhetoric. By the fact that he has his chair the pattern
will be institutionalized if it was not so in advance: the students
learn that it pays and will probably imitate the behavior. For
that reason this kind of professor is often preferred to the
really knowledgeable one who teaches them sociology but nothing
of the style of the professor caste.

Another side effect can be found in the relation the students
have to their examinations. To a foreign observer the level of
anxiety prior to an examination in the region is almost incredible
- unless this analysis in sociological terms has some validity. The
examination is really a transition ritual, it marks the transition
from one status to the next. He who succeeds totally, he gets a
status that gives him certain privileges in the semi-caste
society - and he who fails loses all access to these privileges.
More is at stake than in other countries where the student knows
he can make up for a failure by achievement later on, that he is
not so much earmarked for life. In an achievement-oriented society
they have also received more general training in being evaluated
in having to prove themselves for possible social mobility than in
a more traditional culture. In the latter the diploma counts, not
the knowledge - as when applicants refer to their diploma and are
astonished when they are asked to demonstrate their knowledge.

Let us then look at some implications of this semi-caste social
structure for the relation of the sociologist to his subject
matter, i.e. society. Much has been said about the differences
between physical and social sciences, and one of these differences
is of particular relevance in this connection. Confronted with the
enormous complexity of human affairs, with the theoretically and

practically challenging task of knowing more about the human con-
dition the social scientist is at one great advantage relative
to the physical scientist: he is himself part of that condition,
both as participant and observer, he can draw on his own experi-
ences; like the geologist he can examine the layers of social
experience sedimented in his own mind. The physical scientist
may report on experiences with the forces of gravity when he is
walking on a slippery road, but his own participation in physical
reality, or at least his ability to report on his own body, is
meager relative to the total insight in Nature revealed by
physical scientists. Of many social scientists it may still be
said that a considerable part of their literature is inside them
as experiences that form a reservoir for hypothesis-formation,
that they get as much out of their data as they have inside them-
selves. To rely uniquely on this would lead to a pre-scientific
kind of phenomenology oblivious of the public nature of science
- to discard this as a source of insight will at least imply a
waste of resources, at worst theoretical barrenness.

The immediate conclusion is that the richer the social exper-
ience of the social scientist, the richer his insight potential,
both in terms of hypothesis-formation and theory-formation. This
kind of insight can hardly be obtained vicariously, by means of
literature, movies, reading - for these sources give experiences
digested and moulded by other minds, often in a stereotyped way
that may blur social reality and perpetuate social myths. Since
social experience has to do with depth and width of social inter-
action, a very relevant question is simply: what is the inter-
action range covered by the community of social scientists?

Imagine a model of society trichotomized vertically in high,
middle and low class, and horizontally in primary, secondary and
tertiary sectors of production. Many other schemes could be sugges-
ted, but this one spans the social structure sufficiently well
for our purpose. The decision elite of the society is located in
the tertiary, high cell, highly influenced by primary and secon-
dary elite. Academic Man, among them sociologists, are probably -
for we do not have data on this for the region - to a consider-
able extent born by parents in the tertiary middle class,[16] raised
by them, married into other families in the same cell, and them-

selves by definition located in that cell. To the extent this is
true, social experience is limited to the urban upper middle class
tertiary sector, and even mainly to a narrow segment of that
sector. Thus, few intellectuals, in our experience, have first hand know-
hand kr ledge of the commercial sector. But the social distances are,
of course, considerably greater than in Northern Europe.

There are several ways of compensating for this one-sidedness
in social experience when exogamy or social mobility relative to
one's parents are less frequently used. Two such simple mechanisms
extremely well known to European and even much more to North
American students are travelling and summer-jobs, often combined.
The social importance of travelling in an open society where con-
tact is facilitated by wide dispersion of a fairly uniform idiom
and fairly uniform social customs can hardly be exaggerated. But
as is very well known, there are ways of travelling that protect
the traveler against new impressions so efficiently that he brings
the whole _ethos_ of his social cell with him, and only suffers a
certain displacement in space that permits him to enjoy other
views and other kinds of food. The more daring and imaginative
kind of travelling, such as hitch-hiking and summer-camps that
probably have served as eye-openers for a considerable fraction
of students in other parts of the world are often excluded
a priori because of the social costs involved. There is the
strong ideology that a vacation is a vacation and should in-
clude a beach. Because of lack of training in interaction across
great social distances in the social matrix the Latin American
students, in our experience, meet few people on an equal and
informal basis. Thus, it does not seem as if there are many
voluntary associations that have memberships from different
cells if the nine-fold division discussed above. And the se-
gregation goes deep down, through primary school and Kindergarten
to the birth clinic itself.

It may be said that the state of LA economy does not permit
the competition of experience-hungry students in factories, on
farms, in offices in summer time, since this would mean loss of
jobs for workers who have no alternatives. We shall not argue
against that argument, but rather point out that it does not
explain the many cases where socially adventurous experiences are
not utilized even though there would have been no such implication.
In fact, we have encountered more inventiveness in explaining why
it is impossible to gain more social experience than in gaining it.

The consequences of this pattern of under-interaction with
distant cells in the matrix are many, and we shall point out three

of them that to our mind are particularly important.

First of all, as mentioned above, it deprives the sociologist of a source of insight that is not equally denied to his colleague in certain other countries. This may reflect on hypothesis-formation as well as interpretation.

Secondly, and closely tied in with this: because of a certain alienation from other sectors of society, ideology has a more un-constrained and free play. Preconceived ideas, taken out of more or less codified ideological orientations, about how people are in other cells in the social structure are not sufficiently confronted and contrasted with direct impressions of social reality. To take a simple example: the applications of a simple profit model as mo-tivation theory for others. Sociologists who would never themselves subscribe to the idea that the only thing that motivated them was a desire to earn money feel no restraints in perceiving the capita-list as only interested in maximum money for mimimum work, or the worker as similarly motivated. A closer knowledge of them would invariably reveal nuances, more identification, more variation in motives, but patterns of interaction protect the sociologist against this knowledge. Hence a great interest in the alienation of lower classes: without denying the reality of this, a factor maintaining the image of working class alienation is the alienation of the intellectual himself, from his society in general and indeed from the working class.

Thirdly: lack of training in casual, informal association im-pedes data collection. The rules of the caste society prescribe a certain tone of authority, mixed with artificial camaraderie and paternalism when the intellectual associates with lower class respondents. As argued elsewhere, this is compatible with the typi-cal interview survey of lower class citizens - but not with elite investigations, nor with more empathic studies downwards in the social structure. In our own experience the social clumsiness and awkwardness in the data collection situation is a major factor when sociologists often turn against empirical methods, define them as impossible and unreliable and as not touching "what is really important". This is understandable: the task is considerably more difficult than in certain other countries where class antagonisms and sector differences have been more broken down in the process of

social development.

4. Divisions in the intellectual community.

So far we have treated the intellectuals as if they were a unified whole, and contrasted them with the rest of society. This, of course, fails to take into account the significant divisions of the intellectual community into schools, traditions, factions of all kinds. Some of the most important divisions of the social science community would be: according to political inclination; national vs. Catholic universities; according to methodology (empirical vs. non-empirical); according to place of training (Europe, US, at home); not to mention according to the name of the discipline. The basic question is how such schools relate to each other. One can conceive of the rich flora of social science orientations in Latin America as a reservoir of pluralism, where the fruitful interplay between the schools might be beneficial to all of them, and to social science in general. The problem is how to make use of this reservoir.

As a point of departure for an analysis let us describe two extreme modes of interaction between groups which we can call the conflict mode and the contact mode respectively. They can be seen as rules in a game of interaction, and in the ideal cases the groups have to choose either of the set of rules:

Rules for interaction between groups:

	Conflict model	Contact model
Relation to general image of other group	No common goals, goals mutually exclusive	A lot of common goals, and goals that look mutually exclusive can be redefined.
	To help him is to harm yourself. Zero-sum game model	To help him is also to help yourself. Non zero-sum cooperation model
Methodological implication	The other group is useless for yourself; the differences are so great that there is no need and no use for a dialogue	The other group is useful to yourself; precisely because of the great differences he may point out the short-comings in your own thinking.

	Conflict model	Contact model
Contact implications	Avoid contact, the other group does not deserve it, they represent something so inherently bad that they should not be helped. Distrust, hide your discoveries, the other party may steel them away from you.	Seek contact, in spite of differences you may promote the common goals, serve to improve each other and thereby serve some higher value.

These two modes of contact are well known to every human being. Together with a third model, where one goes actively in for damage to the other group, they mix in various proportions and define the atmosphere at which human interaction takes place. The conflict model is particularly well known from the institution of Politics, where the perception of the other group and the policy it advocates as detrimental to the general welfare is basic. From this, of course, it does not follow logically that contact should be avoided. One might also advocate contact on the grounds that it might lead to some kind of conversion of the enemy, or at least to know him better so as to be better able to beat him. Avoidance of contact follows only, as a consequence, in a culture defining it as a consequence. And this seems to be an element of the political culture in many countries: to have contact with the antagonist reveals lack of sincerity, lack of seriousness and belief in one's own convictions, and may even be indicative of treacherous motivations. At best it is incompatible with maintenance of one's own individuality, at worst it means submission.

In the institution of Science this kind of thinking has a particular methodological consequence. It is easy to talk about the scientific requirement of "intersubjectivity" - that the validity of propositions and theories should be confirmed by the widest possible variety of competent scientists - it is not so easy to institutionalize. In practice it presupposes contact at all stages in a research process, but particularly in the concluding phases, with other researchers - either personal contact or indirect contact, through the medium of one's publications. Direct contact is preferable, because it may lead to a pre-publication control, a feed-back at a stage in the process where there is still ample time to modify methods and hypotheses. It is trivial that this kind of contact is only valuable if it is not restricted to a narrow circle of similarly trained researchers, socialized into a collec-

tivity so as to affirm and reject in unison, and often subdued by
awesome respect for the leader of the school. It is the outside
criticism that is valuable - although at times the distance may
be so great that there are too few common references for a dia-
logue. This condition, however, stands in a vicious circle rela-
tion to lack of contact, and can only be broken by establishing
some kind of contact, direct or indirect (e.g. by providing
opportunities for the two camps to obtain the same kind of training).

No foreign observer will fail to notice the scarcity of inter-
school contact in Latin America, or the intensity of the rivalries
that are made manifest when efforts are made to break up this
precarious equilibrium by organizing common seminars,
periodicals, institutions. The result is a kind of aca-
demic inbreeding, often a kind of feudal system with
the feudal lord surrounded by an inner circle of yes-sayers,
and an outer circle of chronic no-sayers, and insufficient
criticism because the ideology prevents contact with the
critics. This pattern is particularly dysfunctional when
combined with the pattern of rapid
Institute-formation and splitting up of universities to accommodate
for the discontinuties in change of scientific orientation. One
gets an extreme number of universities, as in Colombia, functioning
as a horizontal caste system. The university at which a young
person will study, if any at all, can almost be predicted at birth,
a graduate from one of the universities does not become a professor
at one of the other universities, and most importantly: there are
very few points of academic contact (such as regular seminars,
guest lectures, mutual quotation, etc.). The consequence in this
age of technical assistance is particularly deplorable: knowledge
injected at one point in this academic structure, by means of a
home-coming Ph.D. or visiting professor, does not spread automa-
tically to the rest of the structure (by, for instance, arranging
common seminars to exploit fully the source of knowledge), but
is contained within the walls of one of these water-tight compart-
ments. The system functions more as a system of groups at war re-
ceiving intelligence they do not want the other party to obtain,
than as an academic community devoted to the promotion of know-
ledge. In one formulation: the model held to be adequate in the
field of politics is used in the field of science.

This formulation probably also leads one to some answers to
the problem of the origin of this way of organizing the academic

community. The self-analyses given by some LA sociologists point
to the relevance of the political model: the real difference be-
tween the academic groups is in political terms. Catholics and
Marxists, to use specific cases, want to use their sociology as
political weapons in their arsenals, hence scientific disputes are
only badly camouflaged political conflicts for legitimacy, power
to influence, power to stand out as Scientific, as holders of some
kind of objective truth. There is also the economic argument: means
for research in the region are so scarce that the competition forces
one into the conflict model of interaction, however undesirable
this may be.

Both of these arguments have a certain range of validity, but fail
to explain the many cases observed by the present author where the
groups are quite similar in political outlook and economic means are
not sufficiently scarce to explain the pattern. As a matter of
fact, it often looks as if the conflict model applies better the
closer the groups - for the closer they are, the more do they
strive for a kind of goal that certainly is scarce if not economic:
to dominate their sector of the field alone. Thus, sociologists
with a Marxist orientation and Catholic sociologists may meet on
the condition that they are not from the same country, sociologists
may meet with phenomenologically inclined psychologists but not
with more operationally oriented ones, etc.

One alternative explanation would run as follows. Both the
conflict model and the contact models are learnt, and will be
used according to past training, present stimuli and future rewards.
Since one is discussing intellectuals, it may be worth while look-
ing into the socialization of young students to see what kind of
model of human interaction he is socialized into. Two factors that
have come to the attention of at least the present author are
1) the relatively early age at which the Latin American student
enters university, 2) the extreme use of the conflict model as a
model of political activity for university students.[19] At an age when
his North American fellow students pass through college as a
transitional stage between adolescent and adult, with ample oppor-
tunity for experimentation, with the institutionalized opportunity
to postpone both choice of future occupation and choice of ideolo-
gical conviction, the Latin American student has to launch himself
on the opinion market, ready for sale, completely structured.[18] The

sign of maturity often seems to be having strong convictions
rather than deep convictions. To have identity often seems to
mean a willingness to display membership cards, if not literally
speaking at least to identify oneself as an X-ista. A dialogue
often seems to start with the identification, and then it either
stops or turns into a simultaneous presentation of the two plat-
forms with no real exchange. If a discussion or a seminar includes
somebody who has not yet been registered in the file of isms, his
presentation will be subjected to some kind of occurrence analysis
where the symbols are counted. Symbols, such as an IBM-table, a
slide-rule, a chi-square, a quotation of Lazarsfeld are put in one
compartment; C. Wright Mills, "class-struggle", "historic process"
in another; "the existential condition"or "insight in the indivi-
dual situation" in still another. The outsider may derive great
personal fun if he is machiavellian enough to mix the symbols by
talking approvingly of all of them, but all he will obtain is
probably to be classified as confused. To the polarized conflict
model belongs a strong pressure towards clearly defined lines,
a willingness to declare onself and to stand by it come what may.
Often, this declaration will be so loaded with political
symbolism that it is tantamount to a political declaration.
This does not mean that there are no cognitive elements
present, only that the form chosen belongs to another
context. Again, this is not strange considering the reward
patterns† there is immediate and ecplicit appreciation
of a clear political stand, whereas an academic achievement
is likely not to be understood, or to be misinterpreted
as struggle for power, or to be appreciated only by a ve ry
limited in-group.

Thus, we hypothesize the existence of a strong exposure to the
conflict model as the adequate social relationship between com-
peting groups. This explains the difficulty in organizing seminars
that bridge the gap between competing groups. To enter a seminar
room is tantamount to signing a membership card, it signifies
group allegiance. Thus a curious game starts: both groups try to
find out whether the other group dominates the game and do not
commit themselves before they feel that there is a sufficient
guarantee of balance of power. For this reason one of the groups
may take a first step by sending so few observers that the other
group, by mere default of the first group, becomes dominant - and
the next time even the Observers will be withdrawn and the seminar
defined as "completely X-ista". In our mind there is a straight
connection between this kind of behavior and the readiness Latin
American politicians show for the tactic of resignation from govern-
mental positions. When things threaten to go out of hand resig-
nation is made use of at a point where other cultures, perhaps,
would prescribe tenacity and fight. The extreme consequence of
this line is the voluntary asylum (always justified as being
compulsory).

It is interesting to compare this to the widely diffused
myth of Latin American individualism. The precise content of
this myth is difficult to catch. But one should not confuse a
pattern of "holier-than-thou"-ism, where secession, separation,
seclusion, compartmentalization are preferred to open exchange
of ideas and contact, for mutual benefit, with individualism.
One interpretation of individualism would be to stand by one's
convictions even in a context of opposition -- but the pattern
referred to here is escape from opposition into the safety and
purity of one's own group. Of course, science also develops
by starting new schools, by fresh starts in general - but the
start is not enough. No researcher has sufficient richness inside
himself to do his job alone. For continuity he needs exchange, at
least as much as he may have been in need of a first phase
of initial isolation to develop his identity. Eclecticism
must be institutionalised, not only a happy coincidence.

At this point it may be worth while to spell out some struc-
tural requisites for institutionalizing change and exchange in
the intellectual community. It is easy to say that one needs con-
tact between generations and schools, or that the professor shall
keep up to date and not limit himself to the perspectives he con-
quered in his early youth. But such changes are not brought about
by moralizing, they must be built into the academic structure, e.g.
by such requirements as competition for a chair every five years,
or a compulsory pattern of exchange of professors (the latter is
facilitated between nations in Latin America by means of the in-
stututions of political asylum and/or United Nations technical
assistance programs, both contribute to a circulation of elite).

Our analysis is brought out more clearly, however, by indica-
ting three kinds of policy implications when it comes to the con-
crete structure of academic life. The assumption is that science
is best in a milieu of diversity and pluralism, but only if the
diversity is made use of.

First of all: the present structure of self-contained
schools of sociology within the universities may train, in due
time, a number of good sociologists, but they become too uni-
form in their training. A more flexible system would permit the
student to pick and choose his curriculum more freely, so as to
open more for different perspectives.. Uniformity is a danger be-
cause even the most perfect curriculum designed by the staff will
be outdated quickly if one is not open to the rapid progress in the
field. Besides: the present structure does not give enough
opportunities for graduates in other fields, such as public ad-
ministration, law, public health, economics, to get a good training

in sociology so that they can function as adequate receivers of
what the sociologist has to offer.

Secondly: the present structure of in-breeding in the uni-
versities is as dangerous with students as with cattle. One
should provide for much more exchange by instituting substituta-
bility of examinations, i.e. graduating in one school but with
more possibility of including courses from different universities.
This, in turn, would provide bridge-heads for a pattern of
"exogamy", where the graduate becomes professor in some other uni-
versity than his own. In this way one could really draw upon the
pluralism provided by the present structure, for mutual exchange
and fertilization.

Thirdly: even with this pattern there would probably not
be enough exchange. People have to meet under positive and func-
tionally relevant conditions for exchange to take place. Regional
institutions for postgraduate or postdoctorate training may be
useful here provided they are really good and the recruitment
is done on a reasonably symmetric basis, and provided this contact
potential is used later by bringing them together again, for con-
ferences and comparative research. [5)

5. The cultural context.

So far we have tried to base our reasoning on what we might
call structural factors: the nature of the status of the intel-
lectual, the models for relations between intellectuals, and the
location of the intellectual in the social context. We now turn
to a purely cultural phenomenon, a certain intellectual style which
no doubt is of paramount importance in this connection. It is
often referred to as "scholasticism", "the tomist tradition","the
Spanish heritage", "the Latin heritage", etc. We shall not make
any effort to trace it back to historical origins, but rather try
to make more explicit the nature of this cultural ethos as it
impinges on the imagination of the sociologist and moulds his way
of thinking and acting as a researcher. More particularly, we
should like to disregard the connotation of scholasticism and
tomism, particularly since the tendencies, in our experience,
are equally pronounced among more marxist oriented sociologists.
Also, we would like to reiterate that we do not pass any judgments
as to what is good and what is bad, but rather try to shed some
light on certain interesting connections in the field of social
thought.

Intellectual activity takes place according to certain rules,
as does the subtype, the scientific activity which is intellec-
tual activity in a more systematic form. It would be easy to say

that the Latin American tradition is more deductive, the North
American and North European more inductive reflecting, respec-
tively, a basically more ideational and basically more sensate
orientation. But this is also far too general. It is not only that
much more emphasis is put on symbolic work than on empirical work,
both in the sense that it gives more prestige to produce impres-
sive theory than to produce valid data, and in the sense that all
symbolic work (concept-formation, hypothesis-formation, theory-
formation) should be done prior to the empirical work. More im-
portant is a set of assumptions about the relation of the symbolic
to the empirical.

The general problem can perhaps be stated by breaking the
scientific process up in certain steps, and search for positions
of relevance in this connection.

Imagine a social scientist has been socialized into a tradi-
tion that makes him believe the following:

<u>as to concept- formation</u>	1. each concept such as 'democracy', 'sociology', 'status' has one and only one real meaning which can be uncovered by tracing the origins of the word and constitutes the true or real meaning of that word.
	2. if the word stands for a social institution, then the real meaning stands for the essence of that institution (the necessary and suffi-cient condition for the existence of the institution)
<u>as to hypothesis- formation</u>	3. each phenomenon, such as poverty, class differences, social mobility can be described in one and only one way which can be uncovered by analysing the words that most accurately reflect the meaning of the phenomenon.
	4. in finding these unique meanings and unique hypotheses empirical work may be of some gui-dance, but the main burden has to be carried by symbolic work, especially by searching for the true interpretation of the words that are used.
<u>as to theory- formation</u>	5. each proposition has one and only one expla-nation which is the true explanation of the phenomenon, and which can be found by a thorough understanding of the essence of the phenomenon.
	6. a proposition derives more confirmation from its position in a theoretical framework than

from some empirical coincidence

> 7. a goal of scientific activity is the inte-
> gration of hypotheses into one pyramidal
> theory with perfect consistency, and prefer-
> ably only one axiom. If that axiom is found
> the rest is a problem of logic.

The general idea can be expressed as follows: according to this
tradition, there is a lot of work that has to be done before any
empirical investigation should be launched. Concepts have to be
defined and hypotheses have to be formulated and integrated into
theories. But this is not essential, for most staunch empiricists
would also agree to the intrinsic value of a thorough symbolic
job on all three levels, prior to the empirical investigation. What
is essential is the social consequence of this belief: a constant
postponement of the empirical work because the symbolic work has
still not been completed - till the point where the confronta-
tion of theory with data becomes unnecessary because the internal
consistency is so compelling that one falls into the trap of
a priorism, of rationalism. When confronted with data nevertheless
and with discrepancies between theory and data, there are three
lines of defense that are frequently used:

1. the theory was not intended to cover social reality, but
    an imagined reality, where the implications of certain
    assumptions have been spelt out (ideal case analysis, or
    base-line analysis).
2. the theory reflects social reality and the data not,
    a. because the data-collection was not valid,
    b. the data-collection was valid, but did not reflect
        true social reality, as in the famous analysis of
        "false consciousness".

By means of these techniques, singly or combined, the a priorism
can be defended ad infinitum.

To take an example: imagine we are concerned with such con-
cepts as "freedom of decision" or "justice". These belong to the
set of big words, the words that are used to span the coordinate
system of our civilization. We want to find out something about
them, e.g. by collecting cases where the words have been used. If
now our assumption is that what a word has brought together will
also conceptually hang together, a great deal of mental energy
will have to be used in finding a common denominator in the count-
less occurrences of these terms, and define as "wrong" the

occurrences that do not fit. A distinct approach would dispense
with the assumption of a one-one relation between word and mean-
ing and rather assume that big words also have big ranges in their
usage - and proceed by grouping together usages of the words that
seem to be related.[20]

This has a great deal to do with contemporary social science
methodology. The whole idea of "indicators" is based on a rejec-
tion of the assumption of uniqueness of meaning of words or state-
ments, and an acceptance of the idea that an attitude can only,
if at all, be reliably established by means of multiple questions
"tapping the same underlying dimension".

Another example: the tendency to see different theories as
mutually exclusive. Logically only mutually contradictory theories
are mutually exclusive; theories that are not contradictory and
refer to the same set of phenomena can be said to supplement each
other, to give different perspectives on the phenomena, to account
for one fraction each of the variance, etc. Later a metatheory may
be developed that integrates the various theories, but there will
always be a question of what to do in the meantime, in the short
run. A consequence of the way of thinking portrayed above is the
refusal to let different theories coexist mentally, and an insis-
tence on a choice. One is either for or against a dialectic ex-
planation of the historic process in terms of emergence and reso-
lution of class antagonisms - one cannot say: "OK, that is inte-
resting, let us see how far this carries us". Eclecticism, here
defined as the unintegrated coexistence of different theories
(hypotheses, methods, perspectives, etc.) is incompatible with
the monolitism of this intellectual culture.[21]

A third example: the concern with basic definitions prior to
research. This does not refer to the concern any social scientist
would accept, of defining the concepts that are particularly
central to his work in a reasonably precise way (although it may
be argued that this should happen parallel to the data-collection,
-processing and -analysis, not prior to it). It is the insistence
on illuminating clearly such old borderlines as the one between
sociology and psychology, or between sociology and history; or
clarifying the distinction between human beings and animals, the
<u>sui generis</u> meaning of social interaction, etc. All this is work

that can be best understood as efforts to start with the top of
the pyramid and working downwards. Psychologically it seems to
give a feeling of safety, of standing on firm ground, similar to
what the empiricist gets when he feels his dimensions are well
operationalized and his instruments valid. But it can also be seen
as an expression of the caste system within the intellectual com-
munity: by defining one's own field one also defines one's area
of pretended competence and autonomy - that no psychologist/
economist/anthropologist ever enter here! It is a kind of trade
unionism, like when an electrician has to call a carpenter to do
some work on the wall that he could just as well have done himself.

All this amounts to a self-sufficiency of the symbolic world.
The social scientist of this kind needs never leave his library
and his study. The empirical world becomes epiphenomenal and the
symbolic world real, the criterion of truth becomes consistency
and not correspondence, the criterion of excellence becomes formal
elegance and not well integrated and empirically confirmed pro-
positions.

6. Conclusion.

The connection between the structural situation of the Latin
American intellectual in general and sociologist in particular and
the type of sociology that will develop seems evident, perhaps too
evident. Compare the ease at which the sociologist can get data
about sociology with the difficulty in getting data about a semi-
caste society: the former he can find in his library, for the
latter he has to cross all kinds of social boundaries at great
social costs for himself and others,.In this way the system of
social thought can become self-sufficient, it can be brought to
feed on itself with occasional inputs on some data, frequently
dug up by others.

It may be objected that the sociologist could use his own
experience, write a sociology of sociology or a sociology of
university life or else of something where he already has social
experience. Why this happens so rarely we do not know, but it may
be because of a particularly strong resistance against objecti-
fying something which is a part of oneself. It is our experience
that only very few sociologists in the region are able to give a

sociological analysis of a situation where they themselves are
actors - they resort to personalizing, and to political and eco-
nomical arguments. The detached way of looking at a social system
as a thing is mainly applied across considerable social distance,
as in the descriptions of other ethnic groups produced in the
Andean republics, and of other social strata in the more industri-
alized nations. A simple explanation can also be given in terms
of the polarization in social interaction. To "sociologize" may
call to the attention some "unapplauded consequences" of social
structures, and to do this to one of one's own group may throw
doubt on where one stands. But to do it to a distant or antagonist
group may be socially correct.

The normative mode of social analysis, frequently encountered
in Latin America, is conditioned by the strong impact of three
sources of normative thinking: Church, Law and Ideology. In other
intellectual climates the distinction between "is" and "ought"
may be used to serve the function of isolating the scientist from
his social context, by outlawing normative discourse to the extent
that efforts to draw the policy implications of one's findings are
frowned upon. But there is considerable distance between this and
an obliteration of the distinction to the extent that descriptive
work is not done and value-propositions are presented as if they
satisfied the conditions for verified factual propositions. The
distinction between descriptive/cognitive and normative/evaluative
has been crucial in the development of empirical science and if it
is not inculcated in the students from the beginning the conse-
quence is that they fall back upon their reservoir of normative
culture when pressed for statements about something.

The conditions favoring non-empirical, symbolic work have al-
ready been spelt out in some detail: here social and cultural
conditions combine and produce a situation that is not easily
overcome. It is not only a question of lack of access to society,
but also of seeing empirical matters less relevant. And this ties
in with the last two dimensions: when the point of gravity is
located in the symbolic work the result is almost inevitably a
system, with considerable internal consistency but so few contacts
with data that the researcher is almost only constrained by the
rules of his symbolic game. The result is a kind of
verbiage that may be

in line with an intellectual tradition but hard to operationalize
for the promotion of social change.

At this point one may return for a moment to the two basic
factors in what we have presented as "native theory": the political
argument and the economic argument. We have based our reasoning on
three more mediate factors; the intellectual in his social context,
divisions in the intellectual community and a certain cultural con-
text. May it not be said that they all three derive from a certain
socio-economic syndrome, characterized as "developing countries"?
The rigidity of the social structure and lack of contact across
horizontal and vertical distances does not apply to intellectuals
alone but is a more general characteristic, and so is ascription
as a socio-cultural theme. The use of the conflict model and the
steep gradients in people's willingness to associate with other
people are general reflections of a highly polarized social situ-
ation with monopolization of power and low mobility. And the cul-
tural pattern with its anti-empiricism and strong rationalism is
closely related to a general ideology of stability, an "ideational"
orientation in Sorokin's sense.

There are probably not many sociologists who would deny
the massive consistency in the syndrome of traditional society.
Disagreement would be over the direction of causality. We know
of no evidence to refute a two-way model: modern society fosters
modern science, and modern science may help create modern society.
Development requires a sychronized attack in many corners of the
social complex - and we have indicated what to us looks like worth-
while points of attack in the institution of Science.

In conclusion: to break through this socio-cultural complex
is not easy. Nevertheless, it takes place - Latin American sociology
is in a phase of remarkable transition, on its way between the two
extreme in our discussion. It will be interesting to see whether
the change will be only in terms of cultural style or also in terms
of imitation of the North only, or in developing a sociology that
is really adequate to study the tremendous and dymanic richness of the
social fabric of Latin America. It would be sad if the desire to

develop something different from what is found in the North should
only lead to a perpetuation of meta-sociology of the kind discussed
here and not to fresh inventiveness in empirical sociology. For the
Latin-American social scientist has one advantage that colleagues in
most parts of the world will envy him: he has outside his doorsteps
societies so immensely rich - he is like physicist that has the whole
temperature tange at this disposal relative to a physicist forced to
work in an interval of 10°. But richness is not only stimulating,
it may also overwhelm - and we offer that as a final explanation and
challenge.

Also, it should be pointed out
that this is not intended as a factual description of the variety of
Latin American social science today, but as an analysis using the
"ideal type" technique. What is discussed is a special intellectual
style that might be termed "traditional". This style permeates Latin
American social science, but is not identical with it,- it only has
to be understood to understand more fully the difficulties that arise
in this transitional period.

1) This, of course, is no infallible criterion. On the contrary!
there are patterns of deference in science in general and sociology
in particular, as everywhere else. Since we do not have in our hands
the criteria of tomorrow (if we had, tomorrow would have been in our
hands) we shall not foreswear a change in the tide, from South to
North. The reasons why this seemes unlikely will be spelt out later.

2) A perusal of Latin-American sociological output suggest that the
percentage of references that are references to North-American and
North-European sociologists is U-shaped. There is a tendency for an
author either to work inside a Latin framework or is outside it. The
first case may include occasional references to North-American socio-
logists of undisputed reputation, often because of some verbal simi-
larity between a term used by the author and a term used by the aut-
hor quoted. The second case of cultural bridgeheads whose frame of re-
ference is outside their place of residence. Over time, with cultural
assimilation, the curve will of course become increasingly A-shaped,
intergrating Latin-American with North-American sociology.

3) International society, like other societies, tend toward rank-
equilibration. The country that has much of some scarce good tends to
have much of other goods too. There are many reasons for this, the sim-
plest one being the functional relation between the dimensions. Thus,
productivity, technical break-throughs and relative size of the popu-
lation of scientists will be closely related. Thus, 3 % of the GNP in
USA (per capita income above $ 3000) is spent on research and deve-
lopment - the corresponding figures for India are 0.2 % and $ 70.

But nothing is automatic; there can be productivity of natural
scientists without any real opening in empirical social science (the
case of the Soviet Union) or high productivity without inventiveness
(imitation). The countries most productive in modern social science
(the US, perhaps Canada, Argentina, the four Nordic countries, Britain,
France, Netherlands, Germany, Poland, India and Australia) are a few
exceptions among the richer countries and (with Poland as the only
possible example) all equipped with institutionalized pluralism in the
political market. It might be interesting to study why some countries
in this upper world bracket, e.g. Austria, Iceland, Switzerland, per-
haps Canada, Belgium and New Zealand, Italy, Ireland, Hungary and
USSR yield so little in modern sociology,   but there is hardly
any simple common formula. In some cases the size of the country may
be insufficient (Iceland), in other cases serious depletion of the
general stock of scientists due to persecution and emigration (Au-
stria); in still other cases closure of the society and too heavy com-
petition with existing explanations of the structure and function of

society. The most important exception at the other end of the scale, i.e. India, has probably to do with the size, pluralism and criss-cross; and British empiricism.

4) I am indebted to Peter Heintz for this observation.

5) Heintz Hartmann starts his review "Sociology in Cuba" (American Sociological Review, 1963, pp. 624-628) with the words "In present day Cuba, sociology in the Western sense of the world is rapidly dis-appearing" and concludes saying "The hard fact for the moment is that the odds are very much against sociology at the Cuban univer-sities", referring to the University Reform Plan of 1962. The im-pression is confirmed by the present author who has had occasion to get some impressions at various stages of the Cuban revolution (1960, 1962, and 1963). The general impression is a priorism and general lack of methodology.

6) Or from the region itself. The expansion is enormous, partly be-cause new faculties and departments in social science are created right now. Thus, it is reported that the number of students of social science in Brazil increased by over 400 percent in the 1950's (1963 Report on the World Social Situation, United Nations, New York, p. 69). The amount of accumulated disequilibrium that will result from this overproduction relative to the production of jobs will be an important source of social unrest in years to come. Quantity, even quality is not enough; scientists have to function in an adequate social setting.

7) Zetterberg, Hans; "Review of Bcker, Boskoff: Modern Sociological Theory in Continuty and Change", ASR, 1958, pp. 95.f. "The commenta-tors are the guardians of the classical traditions in sociology".

8) ibid., p. 95. The propisitionalists are called "modelists" by Zetterberg.

9) For an interesting account of this see Wood, B. and Wagley, C.: "The Social Sciences: Parochial or Cosmopolitan?" Items, December 1991, pp. 41-45, especiallly for the analysis of how the sociology developed has been adapted to the local situation, with rich anthro-pology in the andean repuplics and sociology of development further south.

10) This tradition is especially manifest among the ethnologists and ethnographers at the countries with a high proportion of indigenous population, and among the social planners of the more industrialized countries. The tremendous impact of the UN Commission on Latin Ame-rica, the ECLA, has probably been beneficial in terms of respect for data but possibly also harmful in terms of cultivating at times a kind of bare-foot empiricism.

11) For some indications, consider tables in the Appendix of Galtung, Johan: Teoria y Metodos de la Investigacion social (forthcoming). The tables are based on a content analysis of a number of recent issues of social science journals in North America, Latin America and Europe.

12) See, for instance, the article by Beals, R.L.: "Social Stratifi-cation in Latin America", American Journal of Sociology, 1953, pp. 327-339. Both for Peru and Brazil are "some intellectuals" placed in the very highest stratum, with the power elite.

13) "At the same time, the schools have been associated with an over-emphasis on formal qualifictions at all levels of employment, foten

confirmed by legislation, so that the possesin of a certificate or degree is more valued than the education attested by degree. The severest criticism of these defects is heard from Latin American statesmen and educators; they are agreed that the quantitative expansion of the schools envisaged for the coming decade must be accompanied by carefully planned reforms of the school systems and by a clearer conception of the purposes of education" (World Social Situation, p. 127). Also see background papers for the Conference on Education and Economic and Social Development, Santiago, March 1962. But the problem has deep roots in Latin American society and Latin American culture, and can hardly be attacked as an isolated problem.

14) Nobody will deny the importance of extremely low salaries that
    1. forces the acquisition of books down on the priority list,
and 2. forces the academic man into other jobs, often three or four at the time, so that his time-schedule permits no concentration, no prolonged stretch of time sufficient to develop original ideas through real concentration. Latin-American academic man, much more than his North-American counterpart, is always on the move, to and from jobs and meetings. This also cuts down on his leisure time. But again, the sociologist outsider may wonder. Is the achievement low because the pay is low, or is the pay too readily converted into the status symbols that correspond to the high rank academic man receives by virtue of his diploma? And if high rank absorbs too much of the income in order to live up to ascribed status and leaves too little for the tools of achievement, then that rank is perhaps too high?

In the transitional state fund today in most of Latin America there are other costs, according to our observations, that are more important than money and time when it comes to explaining why intellectuals often are poorly informed. Imagine a system where the asciption model was completely valid. In such a system to read books or journals after examinations would be not only irrelevant but even negative: it would be a sign of inner weakness, of lack of trust in the legitimacy of one's position. To study would be a regression to the pre-diploma phase. And even more importantly: it implies a defiance or the system, an undermining of one's own position and the position of one's colleagues. Thus, an element of disloyalty would enter, and the infractor of the norm would anticipate sanctions that would deter him to the extent the system was internalized. Thus, we would predict a tendency to do the reading, if at all, in secret or abroad, and present oneself after a new rite de passage, not in the humiliating student phase.

Finally, there is the tremendous cost involved in taking spare time studies, since it means family time. That the compromise point in such status conflicts is more to the Family end than in the North needs no further elaboration.

15) These famous and violent oscillations of the pendulum describing general scientific orientation are accentuated by the circumstance that Latin America, in general, is not located at the research front. Intellectuals returning from excursions abroad, to Europe and to the US can tell strange tales about the most recent vague in sociology in Paris or Berkeley or London and may turn the pendulum because they can communicate a relatively complete school of thought. The new will be resisted by 1. those who have thoroughly understood the old, and 2. those who are too old to change orientation. If the former are few in numbers and the new pretenders for cultural dominance do not have to invent much themselves but can import it (parly as books, partly as technical assitance) the old tradition has little chance since very few derived real gratification from it. But this makes

the swings of the pendulum less of an intellectual process and more of a struggle for cultural dominace.

16) ECLA documents repeatedly refer to them as being predominantly "urban middle class".

17) This is between themselves as well as in relation to the universi- ty. Student strikes are, however, also fairly well spread in the secondary schools.

18) One is reminded of Erik Homburger Eriksson's analysis of adole- scense as institutionalized experimentation in the US culture, in Childhood and Society New York: Norton, 1950) part IV.

19) The structure of the university system as one of the many factors that condition the growth of science has been studied by suprisingly few authors. Most of these studies are summarised in the articles by Joseph Ben-David, who has also made very important contributions. See "Scientific Productivity and Academic Organization in Nineteenth Century Medicine" (ASR, 1960, pp. 828-843), Roles and Innovation in Medicine" (AJS, pp. 557-568), "Scientific Endeavor in Israel and The United States" (The American Behavioral Scientist,, 1962, pp. 12-16) and "Universities and Academic Systems in Modern Societies",(European Journal of Sociology, 1962, pp. 45-84). A main theme of the latter article is the importance of competition between universities within a cultural sphere, which is used to explain the growth of German science (p. 62). One should probably emphasize here that high number of universities is not enough, for 1. they have to|see each other as competitors, not as essentially different (like technicum and a di- vinity school) and 2. they have to be fairly equal in size so that competition is meaningful. In a highly centralized country like France, where intellectual life is dominated by one university, scien- tific atrophy may come earlier than in a federal country (Germany, USA, USSR) with fairly equal size of the universities and a pattern of mobility between them. Mahalanobis, in a recent paper at the 12 Pugwash Conference at Udaipur, India January 1964 stresses the impor- tance of a "professional organization of scientific workers with al- most unrestricted freedom of movement" which is found in USA and USSR. In the colonial countries, that are typicaly dominated by one big ca- pital at the ocean side and equipped with an exploited interior this is much more difficult. Thus, according to this formula the most opti- mistic prediction of scientific growth in Latin America would befor Brasil, uless some of the Spanish speaking republics enter some kind of academic union to provide a sufficient market for talent and mobi- lity and competition. One big university will be satisfied with passing on old knowledge, but if it has to compete for its prestige it will have to invent something new. For the same reason an insti- tute or department ought probably to have at least full professors, with rotating chairmanship, to prevent atrophy from lack of competi- tion (Ben-David, 1962, EJS, p. 74 mentions this for the USA).

20) The tradition of empirical semantics introduced by Professor Arne Næss at the University of Oslo has done pioneering work in his field.

21) Or, more in detail; there is a close connection between a philoso- phy of science that stresses "one word - one meaning" and "one pheno- menon - one explanation" on the one hand and a political philosphy that emphasize that the adversary is totally wrong, one's own party monopolizes what is right. Theories are mutualy exclusive like politi- cal parties or associations, it has to be one or the other. Or, with a slight variation: there is little cultural distance between the po- litican who is unwilling to discuss or mention arguments contra what he has suggested and a scientist who is unwilling to discuss alterna- tive theories. Simple questions like "what is the weak point in your

theory?" (to the scientist) or "what is the weak point in your program?" (to the politician) are good tests of ability to tolerate ambivalence, to see scientific theories like political programs as something that is being created, not born complete. And from the coexistence of arguments for or against a theory or a program inside a person to association with persons with other opinions the distance is also, probably, small.

Just as we have argued that a style of interaction, which we have called the conflict model, dominates intellectual life in Latin America one may speculate whether the contact style, known from idealized scientific interaction, could dominate political life, But that would belong to an analysis of political styles.