

WHY DO SOCIAL SCIENTISTS TEND TO SEE THE WORLD AS OVER-ORDERED?

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There is little doubt that the social sciences have always overestimated and still tend to overestimate social regularities. They always saw more unconditional and conditional laws, more unflexible trends than could actually be observed. I have tried elsewhere to document this point by a number of examples.¹ Economists have believed that inflation and employment should always vary in opposite directions, that increase in taxes should have always inflationary effects. Development specialists have believed that building leading industries, or alternatively that developing education, increasing overhead capital, injecting foreign capital, etc. were each a necessary and sufficient condition of development. Sociologists have believed that modernization should necessarily produce laïcization, the nuclearization of family structures, the end of ideologies. Of course many other examples could be given. On the whole 20th century social scientists give the impression that, although they are more prudent in their predictions than 19th century social philosophers, and their generalizations less sweeping, they still tend to overestimate the predictability of social processes.

Why? This is the question on which I would like to present some remarks in this paper. This question was raised notably by Popper in famous pages.² But I think his answer is insufficient. In a nutshell, he contends that social scientists tend to overestimate regularities because they fail to understand the nature of scientific work: writers as Stuart Mill or Marx would have missed an essential point, namely that science is by essence incompatible with the very notion of unconditional laws. I think the story is more complicated; this can be seen at the fact that the social sciences also produce in abundance conditional laws which they see as more unflexible than they actually are. How is this overorderliness of the social scientists world to be explained?

A first source of this overorderliness, a very important one,

derives from a very normal and unavoidable aspect of the theory-building process: a theory or model includes always a priori aspects, of which the scientist is not always conscious. Sometimes, these a priori can be harmless or beneficial. Thus, every theorist describes his theory by words. By so doing, he introduces a number of presumably harmless implicit and meta-conscious³ statements such as "the words used in the theory have an unequivocal meaning", "when a word appears twice, it appears with the same meaning", etc. But he can also unwillingly introduce invisible statements which neither himself nor his readers will immediately perceive. They can have the effect of leading him to derive from his theory statements which cannot actually be derived. He will see them as genuine consequences of his theory, however, since he will in all good faith assume that he has made clear all the assumptions and conditions under which his theory is valid. In symbols, suppose T is a theory, that T' is its explicit part and T'' its implicit part. It can very well occur that $T \rightarrow C$ while $T'' \rightarrow C'$. The theorist will normally assume that he has listed his main assumptions, i.e. he will believe that $T = T'$, and conclude $T \rightarrow C'$.

This process is a simple, but a very important one. It explains to a large extent why so many social scientist can see in the world much more law and order than there actually is, and why - in spite of the value granted by the scientific community to methodical criticism⁴ - they appear often as credule. This process is important because it rests upon very normal and common mechanisms. Moreover, there is no easy defense and protection against its negative effects. Before being able to see that a model rests upon some assumption, one has namely to discover this assumption. Seeing it is not only a matter of good will. It requires also attention and work. Secondly, one can never be sure that one has identified and listed all the assumptions implicitly present in a theory or a model. Once a number of assumptions have been listed, one can conclude that the main ones have been taken into account. But what means the main? There is seldom a clear and easily usable criterium by which the author of a theory could be sure that he can quietly ignore the assumptions present in his model which remains implicit.

To illustrate, I will take the case of a brilliant theory in the neo marxist tradition. This theory says that a society characterized by semi-feudal relationships between landowners and tenants⁵ is necessarily stagnant. A system of relationships is qualified as semi-feudal when the tenants can sell freely their labor force on the labor market, but are permanently dependent on the landowner because of their financial indebtedness toward him.

This situation occurs when the tenants' income is smaller than their consumption and when the landlord is the only possible source of credit. This situation is typical of many areas in the underdeveloped world.

Briefly summarized, the theory concluding to the inevitability of economic stagnation in such a system is the following: in a system characterized by semi-feudal relationships, the landlords draw their income from two sources: the sale of their part of the crop and the interests they draw from their loans to the tenants. In other words, their income is composed of a commercial and a financial part. Let us suppose now an innovation is available which would likely increase the productivity of the farms. If it were adopted, it would increase the commercial part of the landlords' income, since the crop would be more abundant. But it would also threaten the financial part of their income, since, as the tenants' income would raise, they could borrow smaller amounts of money. On the whole, the landlords would not know whether adopting the innovation would make their economic situation better or worse. So, they will tend to reject innovations, and the economy will remain stagnant.

When this theory is exposed by its proponents a number of assumptions on which it rests are generally made explicit. Thus, it is made clear that the theory is valid only if one can reasonably assume that the tenants will not consume all their additional income, i.e. that their marginal propensity to consume is not maximum. Another assumption is that the social conditions in the considered society are such that the tenants would see their income grow as a consequence of the increase in productivity. These assumptions and the other main assumptions included in the model which could be listed are easily acceptable: they describe realistically the type of society they consider. On the whole, the theory appears as convincing: it rests on easily acceptable psychological statements (e.g. the landlord does not want to see his income decrease, the tenant wants to survive, etc.). It describes a situation typical of many underdeveloped countries. It explains many actually observable situations where semi-feudal relationships and economic stagnation can both be observed. Finally, it suggests that the only way of drawing a society characterized by semi-feudal relationships from economic stagnation would be to change this system of relationships, by modifying the structure of land ownership.

Let us call T' the theory (more precisely, its explicit statements). The theorist who thinks he has made explicit the main assumptions of T will hold $T=T'$. Let us call C' the most spectacular conclusion of the theory: since the system of relationships is

the cause of stagnation, development supposes a transformation of this system. To the theorist $T' \rightarrow C'$, $T \rightarrow C'$.

A moment of thought shows however that beside its explicit statements T' , the theory contains also a number of implicit statements generally not presented when theories of this type are developed. Let us call T'' this set of implicit statements. Elements of T'' are for instance:

- 1) In the society S the power of adopting an innovation belongs exclusively to the landlords; otherwise, the tenants - to whom innovation is benefitful with certainty - would exert a pressure to the effect of introducing the innovation.
- 2) The administration is entirely passive or powerless; otherwise, it could for instance subvention innovation so that the landlords would be sure not to be losers, offer to the tenants access to other sources of credit, etc.
- 3) The landlords are characterized by a maximal aversion toward risk. The innovation threatens the financial part of their income, but does not lead to a loss with certainty: as an effect of the innovation, the increase in the commercial part of their income can exceed the eventual decline of the financial part. So that the theory holds only under the implicit assumption of a maximal risk-aversion of landlords, more precisely of all landlords.
- 4) The landlords refrain from - or are not in a situation of competing with one another; otherwise some of them could adopt the innovation, sell their crop on the market at a lower price and by so doing increase their overall commercial benefit.

Obviously, the assumptions T'' are strong. Nothing says that they can be considered as automatically satisfied in semi-feudal systems, nor that they are typical of such systems. Consider for instance assumption 1). It is true that when innovations are costly, the landlords will have in many circumstances the power of deciding whether it should be adopted or not. But many innovations are not costly. In that case, the tenants would have more likely their word to say. Obviously, the question whether assumption 2) is valid is an empirical one. Assumption 3) is a very extreme psychological statement which can be valid exclusively in some particular circumstances or of some social actors. Assumption 4) is an extreme sociological statement. On the whole, it is not the theory T' that leads to the conclusion C' , but the theory T , i.e. T' plus the set T'' of implicit statements such as 1) to 4). Now, T'' includes statements of which it would be adventurous to say that they are always or even frequently satisfied in semi-feudal systems. On the contrary, given the strength of the assumptions T'' , T describe a very particular type of semi-feudal system. So that the theory according to which a semi-

feudal system should remain stagnant until its structure is modified, which is a consequence of T, is not a consequence of T'. $T \rightarrow C'$ but $\sim(T' \rightarrow C')$.

This example illustrates a very typical process: in many cases, social sciences see more order in the social world than it contains because the theories they elaborate rest upon a number of implicit statements, in other words because they introduce what artificial intelligence specialists call the "closed world assumption". While T is different from T', they assimilate the two. Instead of seeing the set of assumptions as open and including further elements beyond the explicit ones, they treat it as closed. These implicit statements are not ignored for irrational reasons, however, but because extracting them suppose a positive work and sometimes a long discussion. It would be easy to give many examples, drawn from classical or modern works, of this process.

A second major reason for the overorderliness of the social and of the historical world as seen by the social sciences and history is also of a kantian character in the sense that it deals also with the a priori used in the social sciences. This reason resides in the fact that they often develop their theories within logical frameworks incongruent with the real world. As in the previous case, this inadequacy should not necessarily be considered as the product of some form of irrationality from the part of the social scientists. It can also be derive from a normal process in the growth of knowledge, for in many cases the questions as to which a priori should be used is a hard one.

To take an example, suppose I am interested in explaining a given social phenomenon, say Y. I can try to find an explanation of form "X is the cause of Y". Of course there can be no valid explanation of Y of this type. A more complex explanation would be "X1, X2 and X3 are the causes of Y". Still more complex would be the explanation "X1, X2, X3, and other variables caused Y". Let us now suppose these forms and other forms which could be devised can be ranked in complexity and call them F1, F2,... Very often, the overorderliness of the world as seen by the social sciences result from the fact that they often propose of a phenomenon, say Y, an explanation of form Fi while Fj ($i < j$) would correspond to the objective complexity of the real world.

Of course, it would be difficult to describe this hierarchy in detail. The notion that it does in fact exist is implicitly recognized even in ordinary knowledge, however. Thus, suppose that somebody suddenly leaves a party against expectation. Very normally, this will give birth to questions such as "what was the cause of his departure"? By so saying, one supposes that Y (the

departure of John) can be explained by a theory of complexity F1. In some circumstances, it will be obvious that this level of complexity is the good one. In other cases, it will be doubtful. In still other cases, it will be obvious that Y cannot be explained by a theory of complexity F1. Thus, a question such as "what was the cause of the first world war" would appear normally as inadequate. "What were the causes the first world war" would appear as more normal. So, in this case of ordinary knowledge, when we are concerned with explaining a phenomenon Y, we use at a metaconscious level a priori statements such as "the explanation is certainly of form Fi", "the explanation is certainly not of form Fi", "the explanation is possibly of form Fi", etc.

What is true of ordinary is also true of scientific knowledge. When a social scientist wants to explain some phenomenon Y, he will endorse - in most cases without knowing it - a priori logical statements as to the possible form of the explanation of Y. But very often the explanans will be downgraded with respect to the level of complexity of the explanandum. In most cases, this downgrading will appear only after a long discussion. The situation is in this respect the same as in the previous case. Becoming conscious of this downgrading, as well as of the eventual inadequacy of the closed world assumption is in no fashion a simple matter of good will. Reciprocally, this explains why scientific analysis as well as ordinary knowledge can very well produce a simplified and overordered view of the world.

As in the previous case, I will take an example to illustrate my point. Magic is often defined as a belief in ungrounded causal relationships. When Evans Pritchard's Azande⁶ walk in the bush and fall on a root, they attribute their fall to an invisible force. Evans Pritchard explains that, because of the fact that roots emerge frequently from the ground in the bush, the Zande is permanently careful and concerned with not falling on one of them as soon as he takes a walk out of his village. So, when he still staggers on a root, he looks for an explanation, and tries a theory of the simplest form, i.e. of form F1 ("there is a cause for the event which just occurred to me"). The right explanation would be much more difficult. Certainly, it does not belong to the F1 type. It is not very difficult to find in the social sciences illustrations where inadequate a priori statements are used in the explanation of a phenomenon Y. If I may do so, I will evoke some examples which I met when preparing my book on the philosophy of the social sciences and history, *Theory of Social Change*.⁷ When going through the literature on socioeconomic development, I was struck by the fact that most theories seemed to rest on the metaconscious principle that socioeconomic devel-

opment has one (essential) cause. Thus, to Leslie White⁹ technical change is the (main) cause of development. To others, this role is played by overhead capital. To others, underdevelopment is due to the narrowness of the markets in poor countries: when a market for a given product is small, producers are not incited to increase their productivity, since an eventual investment in capital would not likely be covered by an increase of sales. As the productivity will not increase, the economy will remain stagnant. To others, poverty means no saving capacity, no investment capacity, no increase of productivity and finally economic stagnation.

I could easily make this list much longer. But it is more interesting to look closely at the structure of one of these theories. For this purpose, White's theory can for instance be selected. What strikes the reader of White's fascinating book first is the quality of his examples. It is true that the technical innovations he presents have actually generated chain reactions of considerable length and produced on the whole tremendous transformations of the societies into which they were implanted. It is true that the introduction of the iron plough for instance has modified the Middle Ages societies in most of their aspects: it was the cause of drastic changes in the distribution of cultivated lands, in the stratification system, in the concentration of settlements, in the structure of the political system, etc. This example and several others which White brilliantly develops suggests that sometimes technical change (X) can produce drastic changes (Y) in areas at first sight very far away from the technical dimension. But the interesting fact is that White's conclusions are actually read not as they are (X among many other causes, in combination with other causes, etc. can produce Y), but in the downgraded form: Y is mainly produced by X; technical change is the main cause of social transformations. On the whole, White's book is often perceived as a confirmation of Marx's view according to which the changes in the forces of production would be responsible for the changes in the relationships of production. This translation is formally a rough logical mistake. Clearly "X is the cause of Y" and "X in combination with other causes contributed for Y" are logically different statements. Why then is the confusion between the two so frequent? My answer would be that the confusion is produced in the mind of the reader by the combination of the metaconscious a priori principle "Y can be explained by a theory of form F1" and of perfectly acceptable statements such as "X can be the cause of Y since, say, the iron plough was effectively the cause of drastic changes in the Middleages societies".

I have taken the example of socioeconomic development. Of course many other examples could easily be taken. To take just a single well known example from a very different field: to K. Popper (at least in one version of his thought), the main cause of the cumulative character of scientific knowledge lies in the falsification of scientific theories: scientist proposes a theory T. T implies that Q should be observed under the conditions K. Now, under these conditions not-Q rather than Q is observed. Hence, T must be either made better or replaced, even in the case where it explains a number of facts. It is now obvious that this theory of the growth of knowledge is an oversimplification. But as soon as the reader - metaconsciously - believes that the growth of knowledge has (basically) one cause, the popperian candidate is a good one.

As orthodox popperians can easily be found, one could find easily economists believing in the overhead capital theory of development, or in the law of the vicious circle of poverty, or in some other of the many theories of development. In other words, while popperians and anti-popperians disagree with one another, they tend to agree on one point: that there is a (basic) cause of the growth of scientific knowledge. In the same way, development economists agree that there is such a thing as the basic cause of development or underdevelopment. They disagree only on its identity. All these discussions are only possible if one supposes an implicit agreement among the discussants as to the validity of the a priori principle "Y can be explained by a theory of form F1".

This downgrading of the form of the explanation with regard to the real world has the power of leading the actor who endorses it to draw false conclusions from true premises without knowing it, or to distort the logical form of statements, as in the examples I have evoked, where its interference produces and legitimates the metaconscious transformation of "if X, (sometimes) Y" into "if X, Y" or even "if X, Y".

When a scientific inquiry is conducted for instance in sociology, history or economics, it occurs frequently that no valid explanation of form F1 ("X is the cause of Y") can be found. Often, even more powerful types of explanation such as "X1, X2, X3,.... are the causes of Y" can be invalid. For two cases occur frequently: 1) the case where a phenomenon Y is caused by so many causes, derives from a so complicated network of causes - including eventually circular causality -, that the question "what is the cause of Y?" has actually no meaning at all; 2) the case where the phenomenon Y has really no cause in the sense that it results from chance, or more precisely from the coincidence of

two or more causal sequences (recontre de séries causales indépendantes), to use Cournot's famous expression.¹⁰

Let us consider the Cournot case. It appears when a phenomenon Y is due to the coincidence of some causal sequences. In plain words, when Y is an accident. Obviously, there are cases where we recognize clearly an accident as such, e.g. when we say: "it could easily not have happened if I had not been there just at this very instant". But even familiar events can easily be ambiguous in this respect. Of a very same car crash I can say "it had to occur because the driver drove too fast" (explanation of type F1), as well as "it would not have occurred if at this very moment the car in front of him had not...." (explanation of the Cournot type). In spite of this ambiguity, the contingent side of this type of events is generally recognized, however. For this reason, the notion "car accident" has become a general conceptual label where the word "accident" has lost its original linguistic connection with the notion of chance. In many other cases by contrast, it is not clear at all whether a phenomenon should be considered as a Cournot effect or as the effect of a cause, a genuine cause, if I dare say so. In such cases, without knowing it, the interpret will often use metaconsciously the postulate "the explanation is of type F1", while in reality Y is a Cournot effect. In this case, a mental process similar to magical thinking will develop in his mind: while Y has no cause, he will see it as the effect of a cause, if he can find one. In the same way, according to Evans Pritchard, when a Zande falls when walking in the bush, he tends to see his fall as the effect of an invisible force.

Not only the Azande, but ourselves, not only laymen, but scientists can easily produce such "magical" interpretations. They are likely to occur when Y (the phenomenon to be explained) does not present itself intuitively as an accident. When e.g. Y is a stable phenomenon, i.e. a phenomenon which lasts over time, or when it appears as correlated with other phenomena, the analysts will unlikely accept the idea that it is without cause(s). This can actually be the case, however.

An example can illustrate this case: in a very interesting and informed study on the religious affiliations of American College professors, Steinberg¹¹ discovered fascinating (though expected) correlations between academic field and religion. Protestant appear as much more than proportionally represented in fields such as botany, chemistry and a number of other sciences but also in music. Catholics are overrepresented in most of the fields belonging to the humanities: German, English, French, etc. Jews are overrepresented in the social sciences, in the psychological

sciences, as well as in fields such as business and management. So, there is no doubt as to the existence of genuine correlations between field and religious affiliation. On the other hand, it seems that there is a *Familienähnlichkeit* between the disciplines predominantly chosen by each of the three main affiliations: respectively, the nature of sciences, the arts, and the human sciences. Some facts obviously do not fit into this pattern, as the overrepresentation of Protestants in music, or the overrepresentation of Jews in some hard sciences, biochemistry, bacteriology, etc. But does not a correlation - by the very essence, so to say, of the notion of correlation - always imply the existence of exceptions?

As many readers would probably do, Steinberg interpreted these correlations as due to genuine causes. Because of well known differences in the ethos characteristic of the three religions, Protestants, Catholics and Jews would be led in their choices by different value systems. The humanities would be more congenial to the values which are ordinarily those of Catholics. The disciplines including a human relations dimension would be palatable to Jews given the basic values they are exposed to by their socialization. And the Protestants would be more likely attracted by the disciplines including an ascetic dimension.

Such an interpretation will likely be easily accepted for three reasons. (1) The correlations are genuine and strong, and they are immediately readable in the sense that most of the disciplines where each of the groups is overrepresented have something in common (human relations for the Jews, asceticism for the Protestants, cultural dimension for the Catholics). Of course there are "exceptions" such as music, where according to the theory Catholics rather than Protestants would have been expected. Or biochemistry where Protestants rather than Jews should have been overrepresented. But again, correlations imply (except when they take the very rare and particular absolute value 1; the existence of "exceptions". (2) Correlations of the same type were often found in other areas of research. Thus, the influence of ascetic values on the behavior of people with a protestant education was ascertained by many studies, for instance by Weber's *Protestant Ethic* or Merton's classical study on the influence of puritanism on the development of modern science.¹² Weber had already noted that the choices of secondary school in Baden reflected religious values: catholic students were more attracted by the arts and protestant students by the sciences.

But there is a third reason to the credibility of this struc-

tural interpretation (I summarize by this expression the idea that the correlations are interpreted as having a genuine cause): it is not easy to think of an alternative interpretation. In particular, the idea that the correlation could be due to Cournot effects, i.e. be purely contingent does not easily present itself naturally to the mind. First, because the idea that this correlation could be due to chance appears easily as contradictory, especially in a case such as here where the same correlation was repeatedly observed. We accept without resistance the idea that an isolated event can be due to contingent factors; more difficultly the idea of a correlation being produced by chance. We expect normally of correlations that they be due to genuine causes; to causes which can eventually be hidden and merely apprehended by reconstruction of deduction, but which still have to be real. In other words, when the problem is to interpret a correlation, we normally perceive this problem within an a priori framework: the correlation has to be explained by variable or eventually be a set of variables corresponding to real factors.

As shown by D. Friedman¹³, it turns out that the best interpretation of the correlation(s), is the interpretation by Cournot effects, i.e. by the coincidence of independent causal sequences, however. Sketchily presented, this interpretation is the following. The time when it had become plausible for a youngster from a Jewish family - as a consequence of the increase of the social mobility of this group - the envision an academic career was also the time when the social sciences or business studies developed intensively and when a number of new chairs were created in these fields. The fact that the collective mobility of the Jewish minority increased was definitely not without causes nor contingent in itself. That chairs in business and social relations were created in increasing number was not without cause either; it is even rather easy to find out which causes generated this development. But the fact that the two causal sequences met at this point in time rather than at some other one is contingent in itself. As the correlation was generated by this coincidence at least for an important part, and as this coincidence was generated by no cause, the correlation is also without a cause, so strange as this idea may appear at first sight.

The "choices" of the Protestants may be explained in the same way, by Cournot effects. At the beginning, the main disciplines which were taught in the American university were the basic hard sciences, those which were directly useful, such as chemistry of the sciences related to the development of agriculture. Beside of this utilitarian considerations on which Parsons

has rightly insisted¹⁴, religious reasons played also their role. They explain why music was also very soon an important field. I need not insist on the importance, which stroke Weber so much at the beginning of this century, of religious values in America.¹⁵ Now, at the time when the American university was mainly oriented toward the "useful" sciences and toward the religious life, the only students likely to make an academic career were protestant. And when the American university system, because of the growing influence of the United States in all areas, expanded itself and developed departments of arts and literature, the immigrants from Poland or Italy notably had become sufficiently settled down in the country to envision an academic career.

So, a great part of the correlations can be explained by what economists or sociologists - sociologists of crime notably - have called the theory of opportunities. This explanation is better than the alternative structural interpretation for reasons to which the name of Popper is ordinarily associated. Let us call T_1 the interpretation by the intermediary variable of the collective ethos of the religious groups and T_2 the alternative interpretation by the distribution of opportunities (and by the complementary assumption of the relative inertia of social systems). T_2 explains many data which T_1 does not explain. Thus it explains easily why the Protestants are overrepresented, not only in the hard sciences, but also in music. Or why the Jews are overrepresented not only in the human relations or in business, but also, in bacteriology or biochemistry. These facts are easily explained by T_2 , while they have the status of "exceptions" according to T_1 , or in statistical terms the status of "errors".

T_1 appears nevertheless as more natural and more attractive because we tend to eliminate the Cournot type of explanation in a case such as this one and to consider as an evidence that the right theory is rather of form "X (eventually $X_1, X_2, X_3 \dots$) is (are) the causes of Y". For this reason, I have noticed that when I present T_1 and T_2 to students, they have often two reactions: 1) they appear as convinced of the fact that T_2 is true, or at least more acceptable than T_1 : they recognize easily that finding an explanation by Cournot effects can take more effort and time than finding an explanation of the structural type; 2) but they have also the strong feeling that T_2 does not represent a real explanation. The reason of this feeling lies evidently in the fact that to them as to most people, "explaining" means finding a real, substantial, if hidden, cause. Coincidences do exist, as in the case of car crashes. But seeing a phenomenon such as a correlation as an accident is currently perceived as unacceptable.

To summarize, the overorderliness of the social scientists

world is essentially due to the fact 1) that the theorization is possible without the mobilization of a number of a priori, 2) that the question as to what is the right type of a priori given an explanandum has not always a clear and distinct answer, 3) that, as a result, in scientific as well as in ordinary knowledge, these a priori appear very often as downgraded with respect with what they should be, given the complexity of the real world.

I leave aside the fact that this inadequation between the a priori and the world can take more complex forms than I indicate by this notion of downgrading. Thus, it occurs also that non random effects are interpreted as due to chance.¹⁶

Another type of a priori need to be mentioned. They are less general, and more connected with cultural traditions in the social sciences. While the a priori I have sketchily evoked above can be compared to kantian a priori in the sense that they appear in all acts of knowledge, the a priori I would like to mention now are closer to Kuhn's paradigms. They are connected with scientific "cultures" and subcultures. Thus, in sociology two more or less permanent polar traditions can be roughly distinguished.¹⁷ One of them - the positivistic one - minimizes in its analyses the consideration of the actors, their subjectivity and motivations. Durkheim exemplifies this tradition well. At the opposite, the other tradition - the tradition of the sociology of action, shortly actionist sociology - considers that any social phenomenon being the product of actions, explaining why actors behaved as they did is a crucial moment of any sociological analysis. Because of its very orientation, the second type of paradigm leads the social scientists who endorse it to devote more attention to contingency. It makes impossible to see the world as predictable and determined.

To see it, an example can be taken, the case of the pure interaction structures considered in game theory. Clearly, some of these structures have a very predictable outcome, while others have not. Thus, the outcome of a cooperative game is entirely predictable: the structure being such that two actors have a definite interest in cooperation, one can predict without risk that they will cooperate. The same can be true when the actors have divergent interests. So, the famous "prisoner's dilemma" structure¹⁸ although less predictable than purely cooperative structures is highly stable and often predictable. When a situation of interaction is characterized by this type of structures, it will be as predictable at the most predictable natural events. At the opposite, the outcome of a structure such as the famous "chicken game" is highly unpredictable. In this case, the payoff for each of the actors is highly dependent on what the

other will do in the sense that each of its alternative strategies can be either rewarding or fatal, this depending on the choice of the other, who for his own part is confronted with the same alternative. This type of situation is by its very structure volatile and unpredictable.

Thus, some situations are open and unpredictable, others closed and predictable. In other words, some interaction structures are objectively characterized by a high degree of contingency: they leave a large place to the actor's autonomy and, as they do not determine clearly what is best for him, they are unpredictable. Recognizing this point is crucial to understand the world as it is. For obvious reasons, scientists working within the actionist paradigm are more likely to see it.

These examples from the theory of games are important. They show, at a high level of generality, that, according to their structure, situations of interaction can contain various doses of objective contingency. The same idea can easily be illustrated by concrete rather than abstract examples. I will evoke an example from a development study: in a study on Japan, Dore¹⁹ writes that, according to an old tradition the landlords were expected to lower the rate of the rent the bad years, while they were not supposed to increase it the years where the crop had been particularly abundant. This created an objectively ambiguous situation when the landlords were confronted with the question as to whether they should adopt an innovation or not: "Why should I adopt the innovation, since the crop will grow, but not the rent? On the other hand, if I adopt it, I will avoid a reduction of the rent the bad years". Such a situation is obviously much less predictable than a situation where, say, an innovation would have only positive effects for all. Its objective ambiguity creates contingency in the world.

In this sketchy paper, I have tried to explore some of the main reasons as I see them as to why the social sciences see so often the world as overstructured. A main conclusion emerges from this examination: that this overstructuration is to a large extent the consequence of normal cognitive processes. We have to be Kantian and recognize that, in ordinary as well as in scientific knowledge, we approach the phenomena which intrigue us with a priori. Often, these a priori raise no problem. We can very well in many circumstances use the so-called "closed world assumption", or a statement such as "Y has one cause". In other cases, we use wrong a priori. But becoming aware of it often requires time, debate and effort. These natural cognitive processes tend to produce an underestimation of contingency, and an over-

structuration because, as they pay little attention to actors and action, they do not take into account the objective contingency which characterizes many situations. In the positivist tradition, even weak correlations between variables are interpreted in a deterministic fashion: "The correlation is low because there are a number of factors explaining Y which I do not know". In other words, indeterminacy is currently interpreted in a subjective fashion, in cases where it is objective. The distinction is crucial if one wants to understand the world as it is. Thus, in the above example on Japan, the correlation between any independent (explanans) variable and the dependent (explanandum) variable "acceptation/rejection of innovation" has to be low, because of the objective indeterminacy of the structure. In the same fashion, correlations between a dependent variable such as the frequency of strikes²⁰ and any independent variable are always low for objective reasons: union leaders and workers can have good reasons to start a strike, but also not to start it. As soon as one sees a strike as a strategic reaction to a situation, one should not expect a high correlation of strikes with any independent variable.

In my *Theories of Social Change*, have tried to indicate that recognizing objective contingency is as crucial to see the world as it is - i.e., to be scientific - as to recognize determinism. Because of the influence of a diffuse positivism, contingency is often seen as being by essence subjective, however. The present paper goes a step further. It tries to suggest that, beyond this influence of positivism, the underestimation of contingency and the overordered view of the world developed by the social sciences as well as history derive from very natural processes in human thinking.

On the whole, the discontinuity between the social sciences and ordinary knowledge - or even magical thinking - may be less clear than one thinks.

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NOTES

1. R. Boudon, *Theories of Social Change. A critical Appraisal*, Oxford, Polity Press, 1986 (1984).
2. K. Popper, *The Poverty of Historicism*, London, Routledge & Kegan Paul, 1963 (1944-1945).
3. I prefer "metaconscious" to "unconscious", this latter word being more theory-laden. I use "metaconscious" for instance

- in Hayek's sense in his *Law, Legislation and Liberty*, London Routledge & Kegan Paul, 1973.
4. As described by R. Merton in classical pages in *Social Theory and Social Structure*, Glencoe, III., The Free Press, 1957, 552-560. Merton speaks of the "organized criticism" of science.
 5. A particularly brilliant exposition of this theory in the case of West Bengal is provided by A. Bhaduri, "A study of Agricultural Backwardness under Semi-Feudalism", *Economic Journal*, LXXXIII, 329, 1976, 120-137. I lean heavily in all this passages on R. Boudon, *Theories of Social Change*, op.cit.
 6. E.E. Evans-Pritchard, *Witchcraft, Oracles and Magic among the Azande*, Oxford, Clarendon Press, 1968.
 7. R. Boudon, *Theories of Social Change*, op.cit.
 8. L. White, *Medieval Technology and Social Change*, Oxford, Clarendon Press, 1962.
 9. R. Nurske, *Les problèmes de la formation du capital dans les pays sous-développés*, Paris, Institut pour le Développement Economique, '963.
 10. A. Cournot, *Exposition de la théories des chances et des probabilités*, Paris, Hachette, 1843.
 11. S. Steinberg, *The American Melting Pot*, new York, McGraw Hill, 1974.
 12. M. Weber, *The Protestant Ethic and the Spirit of Capitalism*, New York, Scribner/London, Allen & Unwin, 1950 (1930). R. Merton, *Science, Technology and Society in Seventeenth Century England*, New York, Howard Feertif, 1970 (1938).
 13. D. Friedman, "Normative and Rational Explanations of a Classic Case: Religious Specialization in Academia", in M. Hechter (ed.), *The Microfoundations of Macrosociology*, Philadelphia, Temple University Press, 1983, 90-114.
 14. T. Parsons, "A Revisited Analytical Approach to the Theory of Social Stratification, in *Essays in Sociological Theory, Pure and Applied*, Glecoe, The Free Press, 1949.
 15. Weber, "The Protestant Sects and the Spirit of Capitalism", in H.H. Hertz, C.W. Mills (reds.), *From Max Weber: Essays in Sociology*, New York, Oxford University Press, 1946, 302-322.
 16. Examples of this type are analyzed by R. Boudon, *Education, Opportunity and Social Inequality*, New York, Wiley, 1974.
 17. See R. Boudon, "The Individualistic Tradition in Sociology", in H.C. Alexander, B. Giesen, R. Münch, N. Smelser (eds.), *The Micro-Macro Link*, Berkeley/Los Angeles/London, University of California Press, 1987, 45-70.
 18. A. Rapoport, M. Guyer, "A Taxonomy of 2 x 2 Games", *General Systems*, XI, 1966, 205-214.

19. R.P. Dore, *Land Reform in Japan*, London/New York/Toronto, Oxford University Press, 1959.
20. D. Friedman, "Why Workers Strike: Individual Decisions and Structural Constraints", in M. Hechter (ed.), *The Micro-foundations of Macrosociology*, op.cit.