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CONCLUSION

by PATRICK SUPPES

I think the reasons we have not had more questions to address to M. Jonckheere are also the reasons why I should attempt to make this summary relatively brief. According to my count we have heard 19 papers plus the opening address of M. Fraisse. Many of us now have that onset of relief as the last remarks are about to be made even though we have enjoyed and learned a great deal; the saturation point has been reached. The papers we have heard were of course in the very program itself organized in a progression from more general to more specific topics. Secondly, it has been characteristic of the theme of this colloquium that methodological and general issues rather than detailed substantive experimental discussions of particular models have been emphasized. In terms of the models that have been discussed it seems to me that the papers we have heard can in a rough way be classified into the following threefold division. We began with the consideration of measurement and statistical models with the paper by Luce; the same theme, with variation of course, was to be found in the paper by Broadbent; similar questions were raised by Matalon and Reuchlin concerning classical statistical models.

The second main division contains stochastic learning models. Audley has discussed models of choice, particularly in relation to reaction time. Falmagne has discussed some questions about parameters in reaction-time models. In the case of Faverge, I am not so certain he should necessarily be put in this classification, but the bulk of the models he mentioned might be classified as stochastic learning models of the general type relevant to applications in industrial settings. Jonckheere has just given us a perusal of some of the problems of identifiability and interpretation. Rapoport discussed the possibility of using stochastic models in the context of non-zero-sum games for broader psychological studies. Restle has discussed the application of stochastic models to relatively complex conceptual tasks. Rouanet has raised methodological issues concerning the general problem of identifiability and the analysis of data for stochastic models.

The third main division contains structural models. Bresson emphasized the desirability of going beyond the process models of stochastic learning theories and the necessity of imposing structure on the stimuli and possibly also on the responses. Flament has discussed algebraic models for social psychological questions, in particular, problems concerning the analysis of questionnaires. Frey has outlined an application of combinatorial logic to structural models of the sort particularly discussed in Geneva. Klix could perhaps have been placed among the stochastic learning theorists, but it seems to me that the main thrust of his paper was in terms of structural analysis. Piaget has given us a summary of some of his current interests, particularly structural models that are also process models. Simon gave us a survey and discussion of programming models.

Altogether this is a considerable array of models, and I am not going to attempt to make detailed or subtle comparisons and evaluations. It does seem to me that certain issues, three or four in number, have been prominent in our discussions. The most general issue is simply the one of models versus no models, the issue of formality of a general mathematical sort versus informality and intuition. In this case I feel confident enough of the historical direction of scientific research in these areas to be generous and not to attempt a detailed argument. The weight of evidence in these discussions and elsewhere shows clearly enough that models will continue to be of considerable importance in psychological research; it seems doubtful that they are simply a passing phase characteristic of the 1950's and 1960's and will soon depart to leave us once again in the land of intuition. A more specific issue, but still of a very general sort, has been the somewhat surprising denigration of statistics in many of the discussions. The issues involved in the application of statistics to models are complex and subtle, but again I find it hard to believe that statistics will not continue to occupy an important part, or to continue to be an important aspect, of psychological research, both for those who use models and those who do not.

Within the domain of models the issue that I would identify as most significant for scientific discussion would be the comparative advantages and disadvantages of the stochastic process models on one side and the structural or programming models on the other. Certainly our discussion did not permit us to come to any definitive conclusions and in view of the entrenchment on all sides it is perhaps doubtful that completely detailed and at the same time uniformly-agreed-to conclusions could be obtained, given still another week to talk just about this single issue. On the other hand, it does appear that we have in this instance a very interesting and a very genuine issue. I do not mean to suggest that it would not be desirable and interesting to continue both lines of research,

but it is characteristic of the history of science that not every direction can be correct. It cannot all be love and kisses. There have to be disagreements, somebody has to be wrong. But in the case at hand it is not yet possible to see who is right and who is wrong.

About the problems of identifiability and problems of isomorphism that Jonckheere has just discussed, I shall not say much except that it is obvious, not only from our own discussions here, but from much current research, that these issues will be of considerable importance in the near future. One of the reasons is perhaps to be found in the very proliferation of models. There are now so many models on the market that increasingly subtle analysis is required in order to separate the chaff from the wheat. An outsider listening to the discussion might well think that the scientific study of human behavior is in considerable disarray. The many differences of opinion expressed, the many different viewpoints, the many different kinds of values obviously underlying personal pursuit of research might suggest that the tower of Babylon and not the tower of Hanoi should be the symbol of our colloquium, but I think this would be a mistake and would represent a misunderstanding of the nature of scientific research. The only place where one finds tidiness, utter organization, total agreement, complete clarity as to what concepts are important, what methods matter, are in those areas of science that are well described in the libraries and have been completed for some time. It seems to me characteristic of every area of science, and not simply psychological research, that the tower-of-Babylon effect that we have witnessed and that one always witnesses in a colloquium is a superficial effect. Once we listen a little more carefully to the talk there are various kinds of agreement that can be found in the main drift of the conversation and discussion. It seems to me that there were many agreements on the kind of problems that are central : the discussion of structural models, the emphasis on all sides of the importance of understanding structure more satisfactorily; the admission by those concerned with stochastic models that clearly they are not adequate as they stand to complex problems, and the admission on the part of others that the evidence is not in front of us in any complete detail as to what will be successful in solving these complex problems of structure. A second point of agreement has been in the offhand, casual and natural use of mathematical language. Increasingly the mathematical discussions of psychologists are becoming like the mathematical discussions of physicists. The mathematics per se is not of interest, but mathematics is a tool that can be helpful and that can be useful and even necessary. Questions of rigor from a mathematical standpoint are not as important as the richness and power of the application.

A very hopeful note, as far as I am concerned is that there are no

really sharp natural boundaries in our discussion of models; there are not French models on the one hand and American models on the other, in the sense at least in which we are talking about models ! I have looked at the distribution of the papers according to the classification I gave you. The statistical-and-measurement-model papers were split evenly between the continental speakers and the Anglo-Saxon, 2 and 2, the papers on stochastic learning models were split as nearly equally as possible, 4 by continental speakers and 5 by Anglo-Saxon speakers. The only place in which there is some discrepancy is the case of the structural models as I have classified them. Five of the papers by continental speakers and one by an Anglo-Saxon (the present speaker). The evidence on the whole, and I present this as a piece of empirical evidence generated by the colloquium itself, is that the conception of what models are interesting is not very nationalistically oriented. The kind of representation we have had in this colloquium is typical of the increasingly large body of work on models throughout the scientific world. Consistent with the aims of the colloquium to have a methodological discussion of the general issues involved in the application of models to human behavior, it seems to me we have had a good sample of the work currently being done in various parts of the world on models.

Finally, I want to say that I as well as others here have been more than impressed with the efficiency of the organization and always of course there is the pleasure of being in Paris. On behalf on the participants I would like to thank M. Fraisse and his collaborators, M. Bresson and M. Faverge, for a very delightful and very successful colloquium.

DISCUSSION

Bresson. — Je remercie très vivement M. Suppes de cette synthèse si éclairante pour nous tous. Je souhaiterais qu'une discussion générale puisse s'ouvrir entre nous tous. M. Suppes a montré les points importants sur lesquels cette discussion pourrait porter. Je pense qu'après l'exposé de M. Jonckheere qui a soulevé le problème fondamental de l'interprétation des paramètres, il doit y avoir des questions générales auxquelles chacun de nous pourrait répondre.

Fraisse. — Il semble que la distinction qui a été relevée par M. Suppes entre les modèles de type stochastique et les modèles structuraux a été en effet un des apports essentiels de ce Colloque. Je voudrais souligner que ceux qui font des modèles stochastiques ont peut-être moins l'illusion que ceux qui font des modèles structuraux de rendre compte des processus sous-jacents.

Matalon. — Je crois que je suis entièrement d'accord sur le fond de la distinction que vient de faire M. Fraisse. Je me demande si le fait que les

stochasticiens, si je peux employer ce barbarisme, ont moins d'intentions explicatives, n'est pas lié au statut de la probabilité dans ces modèles. En fait, une probabilité est très difficile à interpréter; on ne sait pas très bien à quoi ça correspond, ce que devient la probabilité pour un individu isolé; il y a peut-être là une limite à l'interprétation des modèles stochastiques. Il est possible que je me trompe et que je sous-estime les capacités d'interprétation, mais je pense que les modèles probabilistes sont nécessaires si l'on veut passer d'un modèle à la réalité, mais qu'en même temps on est obligé de faire intervenir la probabilité qui est une variable qui peut-être n'a pas de correspondant psychologique véritable.

KREWERAS. — Contrairement aux orateurs précédents, je ne pense pas que la distinction entre modèles stochastiques et modèles structuraux soit aussi fondamentale. En tout cas, elle me semble difficile à mettre en œuvre à l'égard de certains modèles très simples et très généraux; par exemple, les modèles de la très vieille école qui recherche les facteurs au travers des nuages statistiques en essayant de découvrir dans ces nuages des directions privilégiées, lesquelles ne sont autres que les facteurs dégagés par la théorie. Je ne sais pas si, dans la distinction qui a été proposée entre modèles structuraux et modèles stochastiques, ce type de modèle doit être considéré comme structural ou comme stochastique. Probablement comme stochastique, mais certainement en tout cas comme un type de modèle qui vise, avec plus ou moins de succès à découvrir des structures au travers de données probabilisées.

FRAISSE. — Sur cette opposition qu'il ne faut pas en effet exagérer, je voudrais faire une autre remarque. Peut-on opposer deux types de modèles et conclure en faveur des uns plutôt que des autres ? Je crois, pour ma part, que ces différents modèles cherchent à rendre compte de types de conduites tout à fait différents et je les considère comme complémentaires. Il y a des situations où stimuli et réponses sont aléatoires, particulièrement dans les conduites de tâtonnement où les erreurs peuvent nous apprendre quelque chose. Il y a d'autres conduites qui sont des conduites plus cognitives et c'est dans ce second domaine qu'on nous a surtout donné des exemples de modèles structuraux. Je crois d'ailleurs qu'il ne faut pas confondre modèles structuraux et recherches de structures.

RESTLE. — I should like to attempt a very homely analogy between these two possibilities and two feet where it seems that in order to represent the structure of a process we will often need a structural model and to represent the detailed data results we shall often need a statistical version. Of course, at each moment one must decide which to put first.

RAPOPORT. — With regard to some phenomena it is useful to view them from entirely different points of view. When one does so, it brings out entirely different aspects of phenomena. These aspects may very well be connected but we may simply not be able to perceive the connection. I have two examples that I like to offer always when discussion starts going about the relative merits of stochastic and structural models. One example has to do with the game of chess. I picture a Martian coming to study people playing chess. Being interested in the quantitative aspects of this phenomenon, he counts the number of pieces on the board, he draws curves representing the depletion of pieces as the moves go on, he then develops differential equations in order to predict the depletion of the pieces. Statistically he does very well indeed, because he has tens of thousands of games at his disposal. At the end he has a very complete

theory as to what happens to the pieces; he may also distinguish between pieces of various shapes, he gets correlations between the way the black and white pieces are depleted, etc. He has a complete theory and predicts everything perfectly. But the chess player tells him that it is not chess that he has described. The chess player is interested in entirely different aspects of the problem, namely the structural aspects. He will describe the game from an entirely different point of view that has nothing whatsoever to do with all these statistics. Now to my way of thinking it is important to recognize whether one wants to view the game of chess from the point of view of the chess player or the point of view of the pieces, and one may have entirely different interests depending on from which point of view one views the game of chess. I am not saying which is the more legitimate. I, for example, in certain situations feel much more sympathy for the pieces than I do for the players. The other example which I have in mind has to do with mathematical linguistics. The older branch of mathematical linguistics, statistical linguistics, was concerned with the distribution of phonemes, morphemes, word counts, lengths of words, etc. A great deal of work has been done in that direction and I think some very valuable work. Along come structural linguists, semanticists and poets, if you like, and declare that all of this has absolutely nothing to do with language, its character, its function, its role in human affairs. That is also correct. I am inclined to say that everybody is correct, but it only is that people look at phenomena from different points of view. All of these may be equally legitimate depending on what one has singled out for attention. This is what determines which methods are of greatest use and significance.

Simon. — I can't resist a comment on the chess example that Dr. Rapoport mentioned because in fact the Martian has been here and the statistical analysis has been tried, but I think with a very different result from the one he suggests. In de Groot's work on *Thought and choice in chess* (Amsterdam, Mouton, 1965) he did in fact make detailed comparisons between the statistics of very high grade players, grand master level, in choosing a move and the statistics in the same positions of ordinary players. To summarize a vast mass of data, his conclusion was that you cannot distinguish statistically between these two classes of players. The only datum that discriminated between them was the move they chose in the position. And this discriminated very well indeed : namely, the grand masters chose the right move, and, since the position was a difficult one, no one else did.

The conclusion I would draw is that there may be whole classes of situations where by grouping the data in particular ways we lose such a large part of the information that there is no longer any possibility of answering the significant questions. I think I could develop the history of statistical linguistics, in which I have been involved a little bit, in the same direction. There are ways of treating language — by chopping it up and counting units after destroying the interactions — which are guaranteed to lose almost all the information in the original data. People who are interested in stochastic or probabilistic approaches generally feel a little more comfortable in chopping data up and losing interactions than people who prefer structural approaches.

Bresson. — Je crois que nous serions tous d'accord pour dire que le codage des données et leur traitement dépendent de la problématique que l'on s'est fixée au départ. La question est que souvent, n'ayant pas la

même problématique, nous discutons de codages différents ou de traitements diférents. Il faudrait peut-être remonter au niveau de la problématique.

ROUANET. — Je voudrais demander à M. Suppes dans laquelle des trois catégories qu'il a mentionnées il se place.

SUPPES. — I would say that I have been in the stochastic county trying to find how to get into the structural one. But in answer to your question I would not perhaps want to say that I've had categorical changes of opinion. This colloquium has reinforced a change of opinion that had already begun before I arrived; it has had an increasing effect in pushing me to a critical awareness of the problems of the stochastic models and the necessity of bringing in the kind of structural considerations we have talked about. It seems clearly necessary to bring in this kind of complexity of structure in order to make the stochastic models adequate to the diversity of learning we would all like to be able to account for.

LUCE. — I fear that I cannot bear so much good will within a scientific group : one must inject some argument when the atmosphere begins to become so warm and friendly. Perhaps some of our apparent agreement stems from a particular dichotomy that has loomed large in these discussions, namely, structural versus stochastic models. (Incidently, I happen to think that these are both poor terms to describe the distinction we have been attempting to make, but that is another issue). Whatever we may call it, this dichotomy exists and is important, but it is not the only one and in other symposia different ones might come to the fore.

One issue that has been buried in, or masked by, our discussions is whether, at the present stage of development, all or almost all of us should head off as rapidly as possible toward the construction of very complex models for very complex processes, or whether, indeed, there are not a number of fundamental processes — not as exciting, perhaps, nor as socially relevant as the complex ones — that are still in need of a good deal of investigation. The development of physics and the resulting technological growth seems to have followed a somewhat different pattern than is being proposed for the behavioral sciences. When we, as human beings, want to understand other human beings, the challenge, in many cases, is really an engineering one, and we are attempting to solve such engineering problems without any thorough understanding of the fundamental variables that underlie them. Do not misunderstand me : I certainly am not opposing the development of complex models for complex processes, but rather I am urging that the other avenue not be completely abandoned.

This other avenue, it should be noted, is not synonymous with stochastic models — in particular, not with the type that has received a certain amount of criticism at this symposium. There are a variety of probabilistic and non-probabilistic models that are concerned with relatively simple processes and that can properly be called structural, but that are quite different from the simulation models that we have tended here to equate with structural models.

SIMON. — I don't know whether I am agreeing with Duncan Luce, and therefore defeating his purpose of raising some issues, or whether I am indeed disagreeing with him. I think we need to consider, on the one hand, the simplicity or complexity of the models, and, on the other, the intrinsic simplicity or complexity of the tasks given to our subjects; and these may indeed be two independent variables. One thing people with structural

inclinations have been arguing for in recent years (and I would include a lot of people who would not use simulation techniques) is that many of the phenomena that have been regarded as extremely simple, and therefore perhaps susceptible to treatment with models having two or three equations, are in fact exceedingly complex. The human organism, when it's doing something that looks to be extremely simple, like learning paired-associate syllables or going through a maze, exhibits behaviors that makes it almost irresistible to talk about strategy. Because of this, some of us feel obliged to move in the direction of very complex models even if we were to limit ourselves to the kinds of simple behavioral situations that have been preferred by American psychologists during the last 40 years.

Now once you take that step down the primrose path, you can ask yourself a second and even more dangerous question : Assuming that we're going to use rather elaborate models of even "simple"behavior, is it in fact easier to find out the nature of the underlying mechanisms by limiting ourselves to the simple behavioral situations ? Might it not be a more powerful strategy of research, and a simpler one, to examine these more complex models in the context of more complex behavior. American behaviorism for 40 years has assumed that, of course, to understand a very complex system you should find some areas where it doesn't reveal all of its complexity and study those. This assumption is no longer intuitively obvious, and we have had opened up to us in the last few years important alternative research strategies.

Luce. — There is some region of agreement, and some of disagreement. I feel that we must be careful about citing particular examples, such as the probability learning experiments, which have been alleged to be simple, and then damn a whole direction of research because they are not simple. Some of us never thought they were and never performed any. Although they have consumed a lot of energy over the past 10 years, I don't think that the only alternative is to turn to complex models. One can still attempt to discover truly simple experimental situations — not isolated ones, but a series of inter-related experiments whose behavioral inter-relations may provide some understanding of certain basic processes.

I agree with the point that you made about behaviorism — that this tack has, in a sense, been too simple to lead anywhere —, but again I don't feel that the only alternative is to construct highly complex models.

Let me now raise some, to my mind, critical questions about the attempts to understand complicated processes by means of complicated models. Clearly, the processes had better be, in fact, complicated if we are going to apply complicated models to them — otherwise, the whole activity does not make much sense —, but then we face difficulties of interpretation which stem from at least two sources. One of these, which we did not explore adequately following your talk, is how to evaluate a complex programming model in terms of data. It seems to me that it is questionable whether, at the present time, the data used to test these models are in any real sense in the public domain, and it is not evident how they are to be made public since full reports of subjects' protocols would require an enormous amount of printed paper. But even if this problem begins to be solved, the question remains as to the inferences that should be drawn from a failure of the model. What changes should be made in a model (actually, a program) that is composed of a whole series of fairly complicated, interacting subdecisions ? One can try this or that change in the program, but unless you have some direct evidence about the component processes,

I would think that this would prove to be a rather hopeless approach. It reminds me of the problem of providing an adequate description of the behavior of a digital computer, about which nothing is known, without being allowed to cut its "skin" in any fashion. This is a monumentally difficult problem, and yet in a certain sense this must be what we are trying to do.

AUDLEY. — When this discussion started I was willing to feel with Dr. Rapoport that there was a basis for general agreement and eclecticism. But hearing the expression of so much concord has driven me to remark that there are possibly two forms of eclecticism, one in which there is some general agreement and another which is now growing in me which comprises a general disagreement with the generalities of this late discussion. I do think, for example, that there remain national differences which cloak attitudes towards Psychology in general if not perhaps towards mathematical models (after all, everyone here is interested in mathematical models and if one brought together anarchists from different countries I fancy there would be a relatively high level of agreement among them). Also, this apparently accepted differentiation between the "process" models and the "structural" models I find hard to grasp, or at least it seems to mean different things to different people. For example, when it is suggested that Restle's paper is concerned with structural models this appears to be with reference to the fact that he paid attention to the nature of the stimulus in his situation. Instead of calling it an unknown S connected to an R, in the manner of the stochastic learning theorists, he chose to investigate what this S was in some detail. This does not seem to me to lead to a more complex or different kind of model, it is adopting a more thorough psychological attitude to his problem; in fact, this is a traditionally fruitful framework of the psychologist tackling a new situation.

The cultural differences are useful, because the important thing in science is to keep on with research and the different emphases of the nations all have their own virtues. In America, the use of stochastic models has been particularly fruitful in the hands of the representatives of the approach which we have at this meeting. In France, it is clear that a great deal of work has been carried out in developing what I understand as structural models. More emphasis has been placed by our hosts on the cognitive aspects of psychological phenomena. As for the British, I do not believe we ever fully develop a national line. This is to some extent our loss, because extremism has the virtue of thoroughly exploring the consequences of a theoretical position. Perhaps our line is scepticism; and my remarks are a cultural response to the cordial agreement developing in our discussion to-day.