ANALOGS, THEORIES, AND DECISION MAKING
Comments on Walter Isard's Paper

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Thinking of Walter Isard, I always remember the lectures he gave a few years ago when I was a visiting student at the University of Pennsylvania. What impressed me most then—and certainly what I appreciate most now—was not necessarily what he said, but rather why and how he said it. In particular, I remember how fluid his lectures were and how they always illustrated the constant reevaluation to which he subjected his own research.

This constant reevaluation approach characterizes also his most recent writings on the interrelations between the concepts of time and distance. Thus, in comparing the Santa Monica paper from November, 1969 [5], with the London paper from August, 1970 [6], some major changes can be detected. The same holds, although naturally to a smaller extent, if one compares the current November, 1970 [7], version with that prepared three months earlier for the London meeting. Because of this open-mindedness on Walter Isard's part, it is almost certain that practically anything a discussant can say the author already has either rejected or saved for a future conference. Nevertheless, it may be useful to summarize and perhaps rephrase what I take to be the goal of the Isardian trilogy. Once this has been done, it may be easier to understand why even an experienced explorer sometimes can be tempted to chart his course through risky and wreckage-torn methodological waters.

To put Walter Isard's dilemma in perspective, one must recall that the current series of papers is concerned with the important problem of how social scientists can develop theoretical constructs that are both intellectually pleasing to the academic community and practically useful to decision-makers and policy-formulators. Given the traditions of regional science, it is not surprising that he wishes the imagined theory to treat time and space concomitantly. For this reason, he focused the first part of the trilogy on a reexamination of the interrelations of time and distance. The hope was that such a reexamination would lead to a dynamic spatial theory capable of assigning probability values to projected future events. Initially, this theory was conceived in terms of a standardized metric equally valid at all times and at all places. Even though Isard's increased awareness of cultural differences subsequently forced him to abandon this hope for universality, this must, of course, not keep us from acknowledging the boldness and urgency of his attempt.

The following comments stem from my conference discussion of Walter Isard's paper [7].

But it is not enough to have good intentions and applaudable reasons. Instead, the researcher must prove the strength and validity of his arguments by carefully detailing his procedures of reasoning. Perhaps the most important requirement in this regard is that the employed procedures of reasoning are so consistent that the final answer is certain to be the answer to the original question. As I interpret the Isardian papers, this original question concerns the formulation of a general dynamic spatial theory in terms that are acceptable both to ivory tower academicians and to policy-formulating decision-makers. More precisely, it was stated in the Santa Monica paper [5] that if the proposed concepts "were found acceptable by the academic and decision making community, the associated magnitudes ... could be held in common by all scholars, decision makers, and policy formulators."

If this interpretation of Isard's goal is correct, then I must admit that the selected procedure of reasoning bothers me. More precisely, I am bothered because of the heavy reliance on notions borrowed from the general relativity and field theories. Although reasoning by analogy can be very illuminating in the early intuitive and inductive phases of theory formulation, I had hoped the author now could have proceeded into distinctions between the negative and the positive parts of the analogy. Such distinctions would have been particularly timely, as he has tended to extend the formal similarities into substantive analogies.

But it is not the failure to provide distinctions between the positive and the negative parts of the analogy that concerns me most. Rather, I am uncertain whether reasoning by analogy is likely to furnish meaningful answers to the initial question—how to design a theory with straightforward implications for planning and decision making. This is a crucial reservation based on my view that the relations between decision making and theory formulation are best established by specification of target variables and causation flows. Thus, I think Isard's problem can be reformulated so it becomes a matter of determining how one set of variables can be influenced through the intentional manipulation of another set of variables. The solution of the problem thus defined requires the theory to be provided with clear correspondence rules. Since it is doubtful whether Isard in fact has isolated those variables and causal flows, it may be argued that he has not answered his initial and applaudable question. To substantiate this rather blunt remark about the role of correspondence rules in planning theory, it is necessary to draw attention to some classical issues in the philosophy of science.

It is well known that positivists like Carnap [2] tend to equate correspondence rules with operational definitions or, in another terminology, with the "operational conveyance of meaning." In short, strict positivists use correspondence rules as a means for linking the terms in the theory with those observables via which their theory is made falsifiable or confirmable. In accordance with his instrumentalist view of what a theory is, Hempel [4] is somewhat more permissive, suggesting that correspondence rules be defined as interpretative statements that characteristically contain a combination of theoretical and observational terms. Others speak of

the "dictionary of a theory" through which the experimental hypotheses of the theoretical language are tied to their observational implications. Logicians, finally, prefer to think of correspondence rules as semantical rules which specify what is denoted by a syntactical term.

The mentioned definitions of correspondence rules are all rather closely connected with the dichotomous view of theoretical and observational languages that characterizes the works of the positivists. As recalled, the proponents of this view frequently argue that perhaps the most important phase in scientific discourse is the bridging of the gap that separates these two languages. Traditionally, this has been done by providing the theory with a model, which then in turn has been connected with observables. In case discrepancies between the language of the theory and the language of the empirical observations were revealed, then the positivists argued a priori that it was the theory that was wrong and needed to be reformulated.

Drawing on the recent debate in philosophy, I think few people nowadays would argue for the extreme dichotomy approach of the traditional positivists. At least partly this is so because a strong empiricist emphasis seems irreconcilable with the idea that social science theories be used as guidelines for action and change; to argue otherwise would be a contradiction in terms, implying that we are so satisfied with the current state of society that we would do our best not to change it. As a consequence, I think planners and social scientists will become increasingly concerned with fitting reality and observational statements to a priori preference premises and less engaged in formulating theories which attempt to mirror empirical occurrences as they are presently observed. This is a rather radical view of objective science, because it suggests that the social scientist *cum* social engineer ought to be concerned not only with altering his theories so they conform with current empirical observations but also with rebuilding the empirical basis of his observations so that it conforms with the moral and ideological premises of his theories. It follows that the appeal of a particular theory would not be independent of our anticipation of the effects the theoretical predictions would have if actually implemented through practical planning.

In accordance with the metatheoretic preferences just outlined, I think it necessary to define the concept of a correspondence rule somewhat more broadly than the positivists would have us do [8, 10]. In particular, I prefer to think of correspondence rules as providing linkages in collapsed causal sequences, where these sequences typically belong to different levels in the hierarchy of statements. On the highest level of abstraction, this would require the use of correspondence rules for establishing the connection between moral preferences on the one hand and the choice of a particular logical language on the other. On the next level, the chosen logic would be allowed to influence the intermediary translations into theories and models. In turn, these correspondence rules can influence the low-level reasoning that occurs when theoretical model predictions and empirical observations are confronted with one another. Finally, there is a need for well-

specified translation functions for comparing imaginary model predictions with the pragmatic utilities and moral consequences that would arise if a plan based on an imaginary model were to be implemented in the real world.

Returning now to Walter Isard's series of papers, I think it is fair to say that he has not provided much detail on his correspondence rules. Moreover, I think this ambiguity prevails regardless of the level in the hierarchy of statements that we decide to examine. More exactly, I suspect a politician would feel quite lost if he tried to figure out the social costs and benefits that would be attached to the implementation of this rudimentary theory. In addition, I think the planner would feel equally lost if he tried to isolate target variables and flows of causation that would allow him to manipulate society in a reasonably safe way. I tend to share this confusion and I believe it stems primarily from the heavy use of reasoning by analogy. One may hope, therefore, that when the present trilogy eventually grows into a quartet, the emphasis will be on the specification of broadly defined correspondence rules and on the separation of positive and negative parts of the analogy.

Once this has been done, I am confident Walter Isard will proceed to the reevaluation of the entire quartet. Having performed that reevaluation, I would not be surprised if he still were dissatisfied. In particular, he is likely to be dissatisfied because I suspect he would find that some sentences in his theoretical language are semantically indeterminate. If this is the case, then he may eventually wish to search for new approaches in the literature on the logic of empirical theories as this logic has been developed during the 1960's by Polish logicians like Suszko and Prełęcki [9]. I am suggesting this approach beacuse the model-theoretic or semantical treatment of logic has provided a natural setting for discussions of empirical meaningfulness, that is, of the problem that lies at the root of all the above comments. Another attractive feature of these logical structures is that the law of the excluded middle fails and that they consequently do not assign any truth status to indeterminate sentences.

At this point, I should perhaps follow Walter Isard's good example and undertake a reevaluation of my own evaluation. For instance, there are several technical points in the three papers that I have not even mentioned, much less discussed. Despite this neglect, I hope not to be criticized for committing my omission. Thus, to speak with Bentham [1, Chap. 8; 3], I feel I have not "failed to warn the King of the impending danger and done so in order to bring it about that the King would be killed." Instead, I have tried to point out some of the hidden methodological rocks he may run into if he insists on pursuing his current course. However, the intuitive feeling that these and many other unrecognized dangers still may imperil the voyage must not keep us from acknowledging what an achievement it would be if the voyager could stay clear and safely reach his goal on the other shore.

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