

A Shortcut to Progress?

Commentary on Wippler's View of Development in Theoretical Sociology

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Recently, Wippler (1975) stated in this journal that the development in theoretical sociology went from preoccupation with descriptive vocabulary to a preoccupation with theoretical orientations, and he proposed that it was time to move on from theoretical orientations to explanatory theories. He sees progress in both moves: 1. the goal of empirical-theoretical science is to formulate true and informative theories; 2. preoccupation with concepts can only be a preparatory stage towards any theory formation (including theoretical orientations); therefore, 3. the move from preoccupation with concepts to a preoccupation with theoretical orientations is at least a step in the right direction; 4. theoretical orientations are themselves only preludes to the formation of testable, informative theories; therefore 5. direct attention to testable, true and informative (in short, explanatory) theories brings us closer to the chosen goal and hence represents progress.

In three respects, I could not agree more with Wippler: the chosen aim of empirical-theoretical science, the interest in bringing theoretical sociology closer to this aim, and the therefore unavoidable nausea over the current compulsion to equate theoretical sociology with the art of classifying 'approaches', 'perspectives', 'paradigms', 'presuppositions', 'orientations' etc. All three points deserve attention, and I am grateful that Wippler affords them so much attention. However, I am afraid that the way he has done so will unwittingly play into the hands of busy classifiers and presupposition-diggers in our field. For this reason, it seems important to me to analyze Wippler's article in some detail, beginning with a reconstruction of his argumentation.

What are 'theoretical orientations'? Wippler (1975, p. 5) defines them as 'perspectives or points of view on the basis of which people work in the empirical-theoretical sociology. They draw attention to a particular class of entities and characteristics that can be considered as problems to be explained or as explanatory factors in theory formation. They are work-traditions and examples of scholarly pursuit for which particular methods and techniques are better fitted than others.'¹ How does he propose to move from theoretical orientations to explanatory theories? At this point, his argumentation becomes somewhat elliptical. He suddenly changes from 'theoretical orientations' to 'orienting statements', and the relationship between the two remains unclear. Following Homans, he calls statements that establish a connection between phenomena but are insufficiently specified to allow falsification 'orienting statements'. An example for such a statement is the following: 'social inequity in a society depends on the technological development.' It is not clear whether a theoretical orientation is a collection of these orienting statements, or whether it just contains orienting statements in addition to other kinds of statements, or whether it consists of statements that generate orienting statements, etc. Wippler's own use of language allows different possibilities. For example: theoretical orientations 'are beginnings of theory formation which have to be further elaborated' (p. 20). This suggests that a theoretical orientation is a collection of orienting statements that have to be further specified. But then Wippler also talks about orienting statements as 'ideas which can be found *within* a theoretical orientation' (p. 20, my emphasis). This suggests, as does his definition above, that a theoretical orientation is something in addition to, or generates, orienting statements.² Why this point is important will be discussed shortly.

A sociologist can, according to Wippler, treat orienting statements in one of two ways. He/she can ask on what presuppositions the statement is based, or he/she can elaborate the statement so that it becomes a falsifiable hypothesis. The first strategy is said to result in an often highly arbitrary construction of, say, the 'model of man' or the 'societal vision' of an author, protecting the statement itself from criticism. The second strategy may lead not to one but many specifications, but each has the advantage of increasing the criticizability of the original orienting statement because the reconstructions are hypotheses open to falsification. Wippler recommends the second strategy because it is less arbitrary and because it increases criticizability.

This strategy can be applied in two different ways: either one elaborates different orienting statements within one theoretical orientation, irrespective of how diverse the subject matter is; or one elaborates orienting statements on the same subject matter from different theoretical orientations (p. 10ff.). In the first case, we get a kind of inventory of general hypotheses within a theoretical orientation (Wippler mentions Meleuski's (1959) reconstruction of Marxian ideas as an example); in the second case, we get competing hypotheses on the same subject matter (Wippler's examples are hypotheses drawn from authors that are identified with symbolic interactionism (Scheff), behavioral sociology (Festinger), functionalism (Merton), and historical materialism (Maleuski), respectively; each hypothesis dealing with change of beliefs). Criteria on truth value, empirical content, and applicability allow the sociologist to choose among these competing hypotheses, again increasing the criticizability of the original orienting statements (p. 17ff.).

Critique

My main point of criticism is not that Wippler's account is wrong but that it is misleading. While he claims to talk about theoretical orientations and explanatory theories and the move from one to the other, he actually *only deals with the specification of vague statements in a (less vague) propositional style*. In other words, most problems connected with theoretical orientations and explanatory theories and their relationship are simply ignored. This can have the undesirable effect of reinforcing preoccupation with theoretical orientations, against Wippler's own intentions. I will attempt to elaborate this criticism, beginning with theoretical orientations.

Theoretical orientations

Although Wippler mentions four criteria for the selection of theoretical orientations, viz. level of analysis, class of variables under consideration, preferred methods and techniques, and actual school-formation in contemporary empirical-theoretical sociology (p. 5), he does not tell us how to identify a theoretical orientation as opposed to, say, the works of a particular author and those who often quote him/her approvingly. This point does not become important to Wippler because he does not really distinguish between orienting statements and theoretical orientations, as

can be seen from his definition of the problem he set for himself: 'how can theoretical orientations be reconstructed *in the form* of explanatory theories' (p. 10, my emphasis). After having carefully defined theoretical orientations as 'work-traditions and examples of scholarly pursuit for which particular methods and techniques are better fitted than others,' he does not talk any more about 'work-traditions' or particular methods and techniques.

A great number of questions thus arise. For example, what influence can or should these work-traditions and methods and techniques have on the reconstruction of orienting statements, on their operationalization, on their testability and testing?³ Also: given a theoretical orientation 'draws attention to a particular class of objects and characteristics that can be considered as problems', as Wippler claimed in the beginning, do the reconstructed hypotheses take over this function of delineating problems? If so, he does not show us how, nor did he even consider the question. Malewski, whose reconstruction of many Marxian ideas Wippler cites a number of times as examples of what he has in mind, also neglects the question how a theoretical orientation can or should influence the reconstruction of orienting statements; but he is at least explicit in this point that his reconstruction leaves a lot unconsidered: 'This (theoretical orientation) will not concern me as collection of directives about the selection of problems or the selection of heuristic elements (i.e. aspects and factors deserving attention). Rather, it will concern me as a summary of general hypotheses' (Malewski, 1959, p. 282).

It is also important to see whether there are, in addition to orienting statements, other aspects of theoretical orientations that need careful reconstruction. Imagine, for example, that there was some theoretical orientation called 'Weberism,' an orientation as clearly or equivocally identified as 'Marxism'. No doubt, there could be a great number of propositions extracted from Weber, but my guess is that much of Weber's work would slip through the net because much of his 'scholarly pursuit' was directed towards what we may call today 'systems of initial conditions' or 'models' (cf. also Albert, 1973, esp. p. 155 ff.). We may, for instance, 'extract' from Weber's many orienting statements the following twin hypotheses: that people tend to attribute success to themselves and that they attempt to keep the advantages linked to success. It is only through Weber's own work that we know these two hypotheses are powerful, because the great effort he put into working out systems of initial conditions for them to demonstrate the dynamics of legitimation. Maybe these systems can be much improved,⁴ but first they need to be reconstructed.

The same holds true for the works of many other authors, although the effort needed for reconstruction may greatly differ. Theoretical orientations (if that's what they are) such as certain forms of rationalism (cf. Olson, 1965), structuralism (cf. White et al., 1975, behavioral sociology (cf. Schütte, 1972) which concentrate on finding systems of initial conditions or — a related effort — find methods of aggregation, these theoretical orientations would be rather useless to Wippler because the harvest of hypotheses to be expected from such works is rather meager. Yet, this kind of work is certainly important for theoretical sociology, and to some it even constitutes the central task of sociology. Exaggerating this point, Homans (1967, p. 106) says: '... the central problem is not analysis but synthesis, not the discovery of fundamental principles, for they are already known, but the demonstration of how the general principles ... combine over time to generate, maintain, and eventually change more enduring social phenomena.'

Another angle of this same point is the problem of *trivialization*. One does not have to be an ardent marxist to have some misgivings about Malewski's (1959) reconstruction of 'the empirical content of historical materialism' *because* it divorces reconstruction of hypotheses from systems of initial conditions. Many salient works in sociology (including those considered constitutive for theoretical orientations) consist of a package containing systems of (implicit) hypotheses interrelated with systems of initial conditions, and I have attempted to illustrate this in some detail with the works of Durkheim (Lindenberg, 1975).⁵ Piecemeal reconstruction of hypotheses, for which theoretical orientations are treated as grab-bags of ideas in need of specification, is certainly useful but it will not help to move away from preoccupation with theoretical orientations.⁶ To the contrary, it will heighten the importance of theoretical orientations as views that give at least some coherence to amassed collections of hypotheses (cf. Berelson and Steiner, 1964, as an example of such a collection). Wippler's belief that problems at hand, such as the question: how do beliefs change, function as rally-points around which we reconstruct and select otherwise disjointed hypotheses will not bear out, especially because social processes, i.e. combined systems of hypotheses and initial conditions, are not considered. In addition, Wippler's justified interest in increasing criticizability is little served by piecemeal reconstruction and neglected systems of initial conditions because it is frequently the latter that need improvement (and thus criticism) most.

Next, Wippler's reduction of theoretical orientations to orienting statements and his simple dichotomization of scientific work in either looking

for presuppositions or reconstructing testable hypotheses avoid the question of interpretative principles possibly contained in and constitutive for theoretical orientations. Sociology is nowhere near a solution to questions that bear directly on such interpretative principles. Take for example the problem of 'individualism' (cf. Lukes, 1973) or the problem of 'rationality' (cf. for instance Gellner, 1970) and their impact on the establishment and interpretation of data (cf. also Barnes, 1971). An effort to deal with these principles may not be seen as simply digging for presuppositions, and the result need not be vague or arbitrary or limited to a particular author.⁷ In many cases in the social sciences, reconstruction of these principles may even be related to the reconstruction of important general hypotheses. Under the cloak of 'model of man', including discussions of an author's view of individualism and rationality, we often find implicit but rather elaborate psychological theories.

Finally, there is one other problem which Wippler neglects and I find important. He says nothing about the possibility that different theoretical orientations are especially useful for certain aspects in the forming of *one* theory. It is for instance possible that symbolic interactionism is especially (not exclusively) useful for formulating low level hypotheses relevant for testing higher level hypotheses (a point made, again with a good deal of exaggeration, by Glaser and Strauss, 1967). It is also possible that behavioral sociology is especially (but not exclusively) useful for the formulation of high level hypotheses, that structuralism is especially (but not exclusively) useful for problems of cultural and structural aggregation, and that historical materialism and functionalism are especially (but not exclusively) useful for formulating systems of initial conditions.⁸ Different interpretative principles, if any, may thus be interrelated into sequences of theory formation, testing, and application. It could also mean that we should not judge all reconstructions from these orientations with the same criteria (cf. Wippler, 1975, p. 17ff.) but with criteria adequate to different phases of a sequence. I don't say that interrelation of interpretative principles is actually possible, but it seems plausible considering the claims of theorists who identify themselves with these different orientations.

Another possibility is the interlinking not of interpretative principles, but of hypotheses from different orientations. For example, one can assume that certain hypotheses about the 'self' (symbolic interactionism) actually specify what a person finds rewarding under certain conditions, which may then be linked with hypotheses about the relationship between reward and action frequency (behavioral sociology). Something like this has been tried by Singelmann (1972), and it may indeed be most relevant only for

the two orientations mentioned. Nevertheless, Wippler considers neither interrelation of interpretative principles nor interlinking of hypotheses. Instead, he has in mind only the extraction of *competing* hypotheses from different theoretical orientations (p. 12ff.), underlining his view of theoretical orientations as different collections of vague statements, and thereby again reinforcing those who take theoretical orientations serious to the exclusion of everything else.

In sum, Wippler's pragmatic identification of theoretical orientations with orienting statements neglects at least the following: the problem of interdependence of reconstruction, operationalization, testability and testing on the one hand with work-traditions on the other hand; the formulation and source of problems and heuristics; the reconstruction of systems of initial conditions; the importance of orientations chiefly concerned with systems of initial conditions and methods of aggregation; the problem of trivialization of theoretical orientations; the reconstruction and interrelation of interpretative principles; and the interlinking of hypotheses from different theoretical orientations. This rather massive neglect may well have the consequence that many may not be motivated to follow Wippler into giving up their preoccupation with theoretical orientations, and it has the consequence that anyone who does follow Wippler's suggestion of piecemeal reconstruction of hypotheses will find him/herself more dependent on a preoccupation with theoretical orientations than ever. More about this at the end of the commentary.

Explanatory theories

While Wippler talks about explanatory theories and testable hypotheses, he demonstrates only a propositional style. For example, he uses the following statement as illustration for an explanatory, testable hypothesis:

'The higher the level of co-orientation (i.e. the degree to which consensus is perceived) in interaction situations, the smaller the likelihood that a person's beliefs change in a direction that deviates from the beliefs of the interaction partner(s)' (p. 13).

This hypothesis is certainly interesting, but is it testable and is it explanatory?

Many things may make the testing of this hypothesis — as is — problematic. For instance, we have to add additional assumptions to make it testable, assumptions which, in turn, lead us back to the author on whose work the hypothesis is based (Scheff, 1967). Scheff suggests the Laing-Phillipson-Lee technique for measuring co-orientation.⁹ In this technique co-orientation is possible about agreement and disagreement. Which one

does Wippler intend? In all likelihood, he thinks only of agreement because he has 'low likelihood of disagreement' as dependent variable. Next, it is not clear whether the hypothesis is meant to hold in the dependent variable for all beliefs, or just those covered by agreement, or just those covered by awareness of agreement etc. Without this specification, we don't know when we should consider the hypothesis falsified. Judging, maybe somewhat arbitrarily, from what Scheff says about the power of co-orientation, I would guess Wippler does not think of 'all beliefs.' And if he would think of only those beliefs covered by awareness of agreement then the hypothesis is hardly testable at all because he does not leave any variation in the independent variable. So let us assume, Wippler meant those beliefs covered by agreement. Now we are still not out of trouble because the hypothesis does not say anything about time. Simplified, the hypothesis reads now: the higher the level of awareness of agreement on x the lower the likelihood of disagreement on x . Without time-specification this is trivially true (a tautology) and thus not falsifiable. Let us therefore rephrase the hypothesis (simplified): the higher the level of awareness of agreement on x at time t_1 , the lower the likelihood of disagreement at time t_2 . Much better, but we have still difficulties. When should we consider the hypothesis falsified or corroborated? If the time interval t_2-t_1 is one hour, one day, one year etc.? Or do we mean the hypothesis to hold for all time intervals? I could not even guess, but it may depend on what Wippler means by 'interaction partner'. Does he require that the persons involved are interaction partners during the time interval t_2-t_1 ? Or just at t_1 ? What interaction frequency would allow us to speak of 'interaction partners'? Again we would have to specify this to make the hypothesis falsifiable; and again we would go back to Scheff and/or other symbolic interactionists to seek for hints towards these further specifications. Wippler's hypothesis is itself a kind of 'orienting statement' that is in need of elaboration, and this elaboration is not independent of the theoretical orientation from which we took the statement in the beginning (symbolic interactionism).

But hypotheses from symbolic interactionism are not the only ones creating problems of testing. Wippler also mentions hypotheses from behavioral orientations. The testability of Homans' propositions and Festinger's dissonance theory, for example, did not remain uncontested.¹⁰ However, none of these problems are even mentioned by Wippler when he calls his example-hypotheses (including the co-orientation hypothesis and one based on Festinger) 'falsifiable' (p. 16).

In short, when Wippler calls the hypotheses in his article 'falsifiable', he seemingly does so solely on the basis that they are formulated in propositional style.

It is also not self-evident that the hypotheses are explanatory, even assuming they were testable. Costner and Leik (1964) and also Blalock (1969) have convincingly argued that simple deductive (explanatory) systems break down unless the correlations are very high or certain assumptions about the behavior of other (uncontrolled) variables can be introduced. Hummell (1972) has elaborated this point in some detail. I take it that Wippler would go along with this, but nothing in his article points to these problems. Again, I must assume that he takes the hypotheses to be explanatory because they are couched in propositional style.

In order to prevent a misunderstanding, I should add that I do not argue against the propositional style. It works wonders, but not all wonders. It is essential for economy of description; and it is quite obvious that a hypothesis in propositional style is less vague than — and therefore preferable to — a circumstantial description of an intended relationship. But I beg to consider that a. although propositions are an improvement, they are not automatically falsifiable and explanatory, and b. although propositions are useful, they are also limited in what they can do. Additional forms of construction and reconstruction, such as computer simulation and mathematical models, are also needed to deal with the varying complexity of theories and the problems of deduction (cf. Lindenberg, 1971, p. 101ff.). Both points also imply a greater direct involvement of methodological advancements in theoretical sociology.¹¹

Conclusion

Given we agree with Wippler on three important points: the chosen aim of empirical-theoretical science; the interest in bringing theoretical sociology closer to this aim, and the belief that the current preoccupation with classifications and presuppositions of 'approaches' or 'theoretical orientations' ought to be redirected in order to move theoretical sociology closer to the chosen aim. Can we say that Wippler has shown us how to redirect the current preoccupation? Yes, in a way he did, by suggesting that we attempt to talk more in propositional style and dig less in presuppositions; and he also suggested that we should judge and compare hypotheses with criteria that are geared to scientific progress rather than, say, the societal vision or certainty (cf. also Ultee, 1975). These are definitely suggestions

for advancement. But Wippler's suggestions are decidedly more modest than his claims. This can have negative effects on his own efforts. People who like to hunt for presuppositions, who like to classify approaches, or who prefer to simply preach fundamentals of their own theoretical orientation will be given reason by Wippler to keep doing what they do. They can say: if Wippler thinks that theoretical orientations are nothing but collections of vague hypotheses we have obviously not yet worked hard enough explicating, classifying, reflecting and preaching theoretical orientations. Further, if 'explanatory theories' are what Wippler shows us, viz. disjointed propositions, why should we try to 'advance' sociology in this direction? The field is fragmented enough, why cease the attempts of delineating coherence where we can find it, namely within theoretical orientations?

Wippler's article can thus backfire. There is seemingly no shortcut to progress. The problem of theoretical orientations has to be attacked on a broad front. The question: to what degree is it useful to distinguish theoretical orientations at all (i.e. to what degree are they not simply different problem areas, see Wippler, 1975, p. 17), has still to be treated as unanswered. But we will have to find some answer if we want to move on. Efforts to find this answer include reconstruction of interpretative principles and the question of interrelation of these principles. Another task necessary to answer the question are non-trivializing methods of reconstruction.¹² This kind of reconstruction should deal with both systems of hypotheses and systems of initial conditions and aggregation rather than with isolated hypotheses. At the same time, we should allow for both different modes of expression (say, propositional style, mathematical models, and computer simulation) and various approximations to explanatory theories. Different modes of expression are especially relevant with differing degrees of complexity of theories, and various approximations to explanatory theories allow us the advantage of certain modes of expression, say, the propositional style (mainly economy of description) without blocking increasing involvement of methodological advancements in subsequent formulations.

If this reasoning bears out, future textbooks of theoretical sociology would look somewhat different from what Wippler (1975, p. 20) suggests. They would deal with reconstruction and interrelation of interpretative principles, and they would contain nontrivial reconstructions of systems of hypotheses and systems of initial conditions and aggregation in various (and identified) approximations to explanatory theories expressed in different modes. Then we may still don't know whether we have left theore-

tical approaches behind us nor not, but we are reasonably assured that preoccupation with theoretical orientations is at least not in the way of moving towards explanatory theories.

Notes

1. Explicitly excluded from this consideration are scholarly pursuits 'complementary' to the empirical-theoretical one, such as 'philosophical-critical' and 'praxeological' pursuits (Wippler, 1975, p. 21). It will not be discussed to what degree these 'complementary' pursuits influence empirical-theoretical orientations. Thus, the question whether Wippler excluded these pursuits rightly or not will we left unanswered. However, it should be added that Wippler's attempt to differentiate between different goals of pursuits (see Wippler, 1973a) seems to me an important contribution to the goal he sets for himself in the current (1975) article: to reduce an overbearing preoccupation with approaches and to go on with the job. He has distilled nicely what three important jobs are (empirical-theoretical, philosophical-critical, praxeological), and he has argued convincingly that a discussion about which one of these jobs is the 'right' one is fruitless since these tasks are not in competition but complement each other.
2. Maybe it is not sufficient to interpret Wippler without looking at his other publications relevant to this point. However, in his main essay on development in theoretical sociology (Wippler, 1973b) we find the same equivocal view on theoretical orientations (p. 13). Unfortunately, he has dropped his earlier discussion of 'models' (Wippler, 1969, p. 278ff.) in favor of this rather unclear use of 'theoretical orientations'.
3. This point is further elaborated below under the heading 'explanatory theories', see also Feyerabend (1970, p. 221n) and Glymour (1970).
4. For Weber (as for most classical sociologists and economists), these systems are highly idealized. In improving them we cannot only criticize the idealized version but we can concretize them to various degrees using a 'method of decreasing abstraction' (see Albert, 1973, p. 158) a method which needs elaboration and improvement itself.
5. When I talk of a 'package' I do not mean that no distinction between systems of hypotheses and systems of initial conditions should be made. This distinction is very necessary to allow the use of separate criteria for the two and to thus increase criticizability. This is contrary to Blalock's (1969) approach to theory construction in which he blurs this distinction. For this reason, Blalock's modelling seems to be difficult to integrate with a deductive view of explanation and with relevant criteria of scientific progress (cf. Ultee, 1975). Separate criticism of hypotheses and initial conditions is thus also not easily achieved with Blalock's approach, although it is quite essential even for reconstructions (see Lindenberg, 1975).
6. Compared to Wippler's earliest article on the matter (1969), both his recent articles (1973a and 1975) must be seen as unfortunate simplifications in this respect. In 1969 he stated: 'The two strategies outlined above result only in a *list* of theoretically relevant propositions... A theory, however, should be understood to be a *system* of these propositions' (p. 285). As earlier mentioned,

- he still talks about 'models' in this article, not of 'theoretical orientations,' and he adds: 'We are of the opinion that models in the narrow sense of the word (formalized theoretical constructions) are useful whenever one attempts to combine a set of propositions into a system' (p. 285).
7. Cf. Lindenberg and Oppenheim (1974) for a detailed reconstruction and generalization of an important interpretative principle in micro-physics.
 8. Homans (1964) said that functional analysis led to very good research, although he rejects functional theory as untestable. Unfortunately, he does not elaborate this point. It could mean, however, that Homans has in mind that functional analysis is useful for setting up systems of initial conditions (e.g. descriptions of institutions). When Homans (1961, p. 378ff) talks about 'the institutional and the subinstitutional,' he also seems to take for granted that 'the institutional' can be described, for he can certainly not expect these descriptions from an exchange theory vocabulary.
 9. The technique is quite ingenious and simple. Two people who know (of) each other are asked to respond to an issue x. This will establish whether they agree or disagree. Next, they are asked to predict the response to x of the other person. If both predictions are right then there is an awareness of agreement or disagreement (first level co-orientation). Next, they are asked to predict the prediction of the other (second level co-orientation, if rightly predicted), etc. Actually, a number of problems arise here, too, such as specifications of statistical cut-off points (number of right predictions on a battery of issues needed to establish a level of co-orientation) and decisions on how to handle asymmetric co-orientation (one person is right in predicting, the other wrong) etc.
 10. Cf. Deutsch and Krauss, 1965; Abrahamsson, 1970; Brown, 1965, p. 602ff. One author (Liska, 1969) even makes unfalsifiability a possible virtue, while still others (for example Crosbie, 1972) again sharply defend testability.
 11. As we can see from other publications by Wippler (1969, pp. 275, 285; 1973b, p. 10), he is fully aware of this without drawing explicit consequences for the development of theoretical sociology.
 12. It is interesting (but saddening) to note that there is still no good methodology of reconstruction. To many, reconstruction is either an art or just an application of methods of theory construction.

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