8

Constructs Are Hypotheses

Finn Tschudi

How seriously do we take Kelly's (1955) paradigm "man—the-scientist [p. 4]"? Perhaps digging, we should substitute "personal scientist," thus following the injunction against sexist language. I suspect that lip service is sometimes being paid to the personal scientist paradigm. A prime reason for this is that a quite vicious attack on the Kellyan position by T. Mischel (1964) has never been fully answered, but rather has been quoted with approval by, for instance, Hinkle (1970) and Reid (1979). I wish to put Mischel's arguments to rest and to revitalize the personal scientist paradigm.

Criticizing Kelly (1955, p. 940), Mischel (1964) argues that constructs do not function as hypotheses for making predictions. He proclaims that there are "radical differences . . . between what I do when, on the basis of my constructs, I anticipate certain behavior from another and what a scientist does when, on the basis of a hypothesis, he makes a prediction [p. 181]." After dealing with these "radical differences" in the first section, we turn to Mischel's own view, that constructs function as rules for guiding and directing behavior or prescribing what should be done. Social sciences are taken to deal with rule-following behavior and thus to "require a model

'This chapter has been supported by a grant from the Norwegian Research Council for Sciences and the Humanities.
of explanation which differs from that of the physical sciences [Mischel, 1964, p. 192]." This point of view is critically considered in the second section, where we argue that Mischel's view of rules ignores the consequences of action. The third section then discusses cases where the consequences of rules are not obviously given, and finally we focus on some normative aspects of Kelly's position.

I. On Validation—Mischel's View as a Naive Theory of the Accomplished Scientist

Validation is a crucial notion in the personal scientist paradigm. Kelly (1955) states: "A person commits himself to anticipate a particular event. If it takes place, his anticipation is validated. If it fails to take place, his anticipation is invalidated. Validation represents the compatibility (subjectively construed) between one's predictions and the outcomes he observes [p. 158]." Consider now the three "radical differences" which Mischel claims distinguish scientific predictions from anticipations based on constructs. In support of Kelly's paradigm it will be pointed out that his view of the personal scientist has a gratifying similarity to Kuhn's (1970) view of scientists.

A. SELF-FULFILLING PROPHECIES

This is discussed by way of Mischel's (1964) main example: "the dominating boss [p. 181]." From the constructions "my boss is dominating, but I want to dominate and not live in fear," Mischel describes two contrasting anticipations: "I dominate the boss and makes him fear me" versus "the boss dominates me and I fear him [p. 181]." This further implies two contrasting actions: "I insult the boss" versus "I don't show him what is what," and he goes on to state:

My anticipation of the boss' behavior tends to be a self-fulfilling prophecy. Because I construe him as dominating I insult him, and this is likely to make him do just what I predicted he would do when I construed him as dominating. But scientific predictions have no tendency towards self-fulfillment, what I do in predicting an eclipse in no way tends to make it happen [p. 181].

It is obvious that such self-fulfilling prophecies bring to shame claims of having "validated" a construction. Mischel gives no indication of whether Kelly is aware of this dilemma or not. To dispel any doubt on this issue, consider an example from a subsection entitled "Constructs as Grounds for Prediction" (Kelly, 1955, pp. 12-13). Kelly describes a man who construes
his neighbor's behavior as hostile and tries out this construction by throwing rocks at his neighbor's dog. Believing that the neighbor's angry rebuke points out, of course a fallacy. We take the point to be that there are bad ways of designing and interpreting experiments. Put otherwise, a person may "mess up her or his experiments," as Don Bannister (personal communication) once expressed it. In the same subsection there is a rarely quoted qualification of Kelly's paradigm: "Man might be seen as an incipient scientist [p. 12; italics added]." Thus "personal scientist" does not necessarily imply an accomplished scientist, as Mischel seems to assume—neither in scientific laboratories nor in everyday life.

Kelly's basic concern is "the psychological reconstruction of life [1955, p. 23]." Surely there are pronounced differences between the predictions of an accomplished scientist and the man throwing rocks. However, a more appropriate comparison is to see this man as an incipient scientist, perhaps caught in self-defeating and crippling behavior. Kelly invites us to see him as similar to any scientist who messes up her or his experiments and thus needs training in how to conduct them.

B. SUBJECTIVE VALIDATION VERSUS OBJECTIVE VERIFICATION

Mischel (1964) claims that

subjective validation . . . differs sharply from the objective verification of predictions. Facts about which there is intersubjective (i.e., objective) agreement determine whether the eclipse I predicted did or not occur. But suppose I have construed the boss as hostile, he asks me to work overtime and I construe this as validation" [pp. 181–182].

Mischel further states: "even a conventional 'how are you' is something I could construe as having hostile intent. . . . That such 'validation' lacks objectivity is clear [p. 182]." Again Mischel gives no indication of whether Kelly is aware of the dilemmas involved in this type of validation. Kelly however, gives an example of another kind of fallacy which has a remarkable similarity to Mischel's "hostile boss". Kelly (1955) describes a man who interprets his neighbor's conventional "how are you" as "just what is predicted of a hostile person—he will be eager to know when I am in any way vulnerable [p. 13]." Once more we recognize the incipient scientist, badly in need of training as to how to interpret her or his experiments.

As for validation in science, Mischel has fallen prey to the illusion that there always exists such a thing as "objective verification." In Mischel's view facts can be sharply separated from theories, but for Kuhn (1970) "the distinction between fact and theory will prove to be exceedingly artificial
Space forbids considering in detail one of Kuhn's illustrations (e.g., the discovery of oxygen). Suffice it to say that the main point is that science is a constructive enterprise, and no single construction can be seen in isolation from the total construction system.

C. "UNFALSIFIABILITY" OF GENERAL CONSTRUCTS

Mischel next carries his argument a step further, from subjectivity of specific predictions to impossibility of falsification for the basis of predictions, that is general constructs. Mischel (1964) correctly points out that Kelly's theory provides concepts for describing how constructs may be retained although predictions following from them may be invalidated (e.g., by shifting pole or by constriction), and he concludes that "since constructs cannot be falsified (not even subjectively) by what in fact happens, they differ fundamentally from predictive hypotheses which can be falsified [p. 182]."

Constructs seem as general hypotheses may be more tenacious than specific predictions, but it does not follow that they cannot be invalidated. This can be taken to point to an asset of Kelly's personal scientist paradigm rather than to a weakness. For example, suppose that a fear-dominaion construct is of vital importance to me. I may experience invalidation and even a sense of crisis, but if there is no alternative construct to take its place (it functions as a paradigm for me), it will be retained for whatever predictive possibilities it still harbors. If my roof leaks, I do not tear it down unless I am able to construct an alternative roof. First I seek to patch it up, or I may simply ignore the leak. The leak corresponds to Kuhn's (1970) concept of anomaly, and the necessity for reconstruction of the whole roof corresponds his concept of crisis. Kuhn points out that "persistent and recognized anomaly does not always induce crisis [p. 81]." He states further:

Though they [the scientists] may begin to lose faith and then to consider alternatives, they do not renounce the paradigm that has led them into crisis . . . A scientific theory is declared invalid only if an alternative candidate is available to take its place. No process yet disclosed by the historical study of scientific development at all resembles the methodological stereotype of falsification by direct comparison with nature [p. 77].

So retaining a seemingly "invalidated" construct in no way implies any "fundamental difference" between me and, what according to Kuhn, is a scientist, and it appears that Mischel has fallen prey to what Kuhn calls a "methodological stereotype."

Summarizing this section we note that the examples that Mischel has used to illustrate his three "radical differences" share one feature: They
are ambiguous and seemingly far removed from "hard science." He thus
loses track of the basic point that there are borders, albeit fuzzy, separating
validating from invalidating experiences. A good construct needs two poles:
For the personal scientist we suggest "accomplished" versus "incipient."
Again the borders separating the poles are fuzzy. We all to varying degrees,
commit errors—what Kelly calls fallacies. The incipient scientist can, how-
ever, be trained. "Psychotherapy is a form of experimentation [p. 940],"
as Kelly (1955) puts it.

II. Everyday Life Behavior Has a Dual Aspect: Follow
Rules and Ask Questions

The major point to be made in this section is that Kelly's and Mischel's
views may be seen as complementary rather than competing. Unlike Reid
(1979), who argues that each view fits some episodes of behavior, we argue
for a dual aspect of all behavior.

In his view of constructs as rules to decide what should be done, Mischel
(1964) chooses to emphasize the prescriptive aspect of rules. Rules "pre-
scribe what should be done" and "such rules cannot logically be confirmed
or disconfirmed by facts [p. 184]." He ignores that, for several rules, there
is also a pronounced descriptive aspect and indeed that all rules may be
seen as having both prescriptive and descriptive aspects. The descriptive
aspect is brought into focus when we consider rules to be: descriptions of
the conditions under which specific kinds of behavior will have specific con-
sequences. [The present analysis is highly similar to Skinner's (1969) view
of rules.] Consider now Mischel's (1964) analysis of the following state-
ment: "I will take an aspirin because I have a headache [p. 181]." Mischel
is correct in saying that "I will take an aspirin" is not a prediction of what
I will do, but rather "expresses my intention (decision) [p. 181]." However,
Mischel fails to consider that, nevertheless, a prediction is involved—that
the headache will be relieved by taking the aspirin. Predictions are not usu-
ally geared toward my action, but toward events, the consequences of ac-
tion. This is already stated in the fundamental postulate: "A person's
processes are psychologically channelized by the ways in which he antici-
pates events [Kelly, 1955, p. 46]." In commenting upon events Kelly (1955)
writes: "Man ultimately seeks to anticipate real events. This is where we
see psychological processes as tied down to reality [p. 49]."

So in the aspirin example, we have a descriptive rule that may be con-
ceived of as having three terms: If you have a headache (condition), take
an aspirin (behavior), and the headache will be relieved (consequences). A
descriptive rule could also be described as a hypothesis or a question, and
we all know that there are conditions where the rule can be disconfirmed. By and large, however, action tends to fulfill anticipations (why else take an aspirin?), or as Mischel (1964) puts it: “In following the rule one makes things happen. . . . The rule . . . thus tends to bring about what it prescribes, indeed that is its function [p. 184].” This, however, is not, as Mischel claims, a sufficient basis for sharply separating human action from scientific predictions.

The fact that action tends to lead to anticipated events will here be called relatedness, and this indeed is a basic characteristic of human life. It may be convenient to think of relatedness as some sort of a variable—that is, relatedness can take on different values. One extreme case is perfect relatedness. In this case action would make anticipated outcomes perfectly equal to actual outcomes. The other extreme is zero relatedness, where the anticipated outcome would have no relation whatsoever to the actual outcome. Our main point now is simply that relatedness by and large is less than perfect. This establishes a similarity between testing hypotheses and following rules, since by the present view a rule can be disconfirmed or fail to bring about “what it prescribes.”

Mischel’s emphasis on rule-following can now be seen to be equivalent to emphasizing that relatedness is greater than zero. Kelly would in no way deny this, but we take his position to be that relatedness is less than perfect. The point is not so much that actions tend to “bring about” anticipations, but rather that they do not always do this. Predictions do not always come true. Invalidation may in some sense be more basic than validation, since invalidation is a spur to reconstruction and change. On the other hand another quotation from Kelly (1969) would seem to emphasize validation: “Man predicts what will happen. If it happens, his prediction is validated, the grounds he used in making it are strengthened, and he can venture further next time. If it does not happen, his prediction is invalidated. . . . The road ahead becomes less clear [p. 275].” An interesting point made by the Norwegian sociologist Aubert (1965) is that without invalidation we would never have come to be able to distinguish between ourselves and the external world. Instead of involving ourselves in the issue of whether invalidation or validation is the more basic, we take the Kellyan position that the construct validation/invalidation is basic.

This analysis rejects the either/or perspective that Mischel offers us. It is not necessary to choose between regarding constructs as rules to guide behavior versus as hypotheses used for making predictions. Rather, there is a dual aspect to all construing: Constructs serve both guiding and predictive (anticipatory) purposes. Insofar as one emphasizes the rule-guiding perspective, the focus is on relatedness being greater than zero. Conversely,
insofar as the predictive perspective is emphasized, the focus is on relatedness not always being perfect. This position subsumes Mischel’s as a special case. The basic weakness of Mischel’s position is that he has effectively sealed himself off from stating how rules may be changed. What if they do not bring about what one anticipates? Kelly, on the other hand, invites us to share the wisdom of hard science in grappling with these problems. For Mischel (1964) rules and action are not necessarily related since one can “disobey” (p. 184). Without, however, giving any context for “disobeying,” what else but capriciousness can induce such behavior?

What is here called “relatedness” corresponds to Kelly’s (1955) point of departure: “The organism is delivered fresh into the psychological world alive and struggling [p. 37].” Concerning “processes” in the fundamental postulate: “The subject of psychology is assumed at the outset to be a process. . . The organism is basically a behaving organism . . . not an object, . . . a form of motion [pp. 47–48].” Also, “the direction of this movement, hence his motivation, is toward making more and more of the world predictable [p. 157].” Mischel has failed to consider the anticipatory nature of action. As Kelly (1969) put it: “It is in this relationship between anticipation and realization [what we call ‘relatedness’] that the real fate of man lies [p. 275].”

It now suffices to give a very brief treatment to a further issue that Mischel makes into a “fundamental difference.” He states (1964), “rules [constructs] guide by evaluating certain behaviors as the right (appropriate) thing to do [p. 184]”; whereas “a prediction, as such, never evaluates what it predicts [p. 183].” Relatedness takes care of the evaluative aspect since it implies that movements differ in attractiveness. Some directions must be more valued than others. Without comparative values deliberate, nonrandom action would be pointless [cf. Kelly’s choice corollary (1955); also Miller, Galanter, & Pribram (1960, Chapter 4)].

III. What About Mores, Rituals, and Our Trivial Everyday Life?

It may be legitimate to ask whether Mischel’s views on rules really have gotten a fair hearing so far. Notice that the previously discussed “aspirin rule” could also have been stated as a prescriptive rule with just the first two terms: “If you have a headache, take an aspirin.” Here, however, the third consequence term is implicitly given. The question now is whether there are pure prescriptive rules where no third consequence term can reasonably be inferred. What about cases where one simply follows rules and
there do not seem to be any clearcut consequences? Consider a hypothetical case by Berger and Luckmann (1967) who conceive of a society where part of its wisdom is culled in the rule “Go when your wife cries, but run when your cousin calls for you [p. 94].” Our concern at present is with cases where one complies because it feels “the right thing to do,” and not because of specially constructed consequences such as, for example, “God will punish the transgressor.” As presented by Asch (1952, Chapter 12) social rules typically fulfill necessary functions and are usually different from constraints that we follow because of coercion. It is thus a highly incomplete analysis to treat rules as if they existed in a vacuum. Rules are part of a system, a social order. Berger and Luckmann’s description of “social order” may be seen as a sociological parallel to Kelly’s personal construct system on an individual level.

Following Berger and Luckmann we suggest that validation is implied by rule-following in the present example, although it will be of an “indirect,” consensual nature, unlike the more “direct” feedback one gets in the aspirin example. Furthermore what is validated goes far beyond brother/ cousin—it is the social system, the social order that is at stake. This point became clear to me when I recalled the rituals connected with oral exams at Oslo University some 15 years ago. Marking the occasion by wearing special clothes just seemed the right thing to do. Today in Norway there are absolutely no such rituals surrounding exams. This reflects a decreasing importance of exams; it may be seen as part of a deinstitutionalization as Berger, Berger, and Kellner (1973) would put it. So it would be an incomplete analysis just to say, “I did just what felt right to do”; rather, I participated in validating the prevailing social order. Not to do so would have been a minor rebellious action. This would of course not have succeeded in invalidating the social order; rather, it would have served to jeopardize my own position. However, concerted attacks on aspects of social structure may well contribute to invalidate aspects of the social order. So we suggest that in following rituals we may conceive of an implicit question, “Am I performing my part adequately?” This further entails the broader question: “Do I properly contribute to upholding the social order?” It should be born in mind that the social order is a construction, and a precarious one at that, and it must be maintained by continuous affirmations.

Let us now take a look at our everyday life, the seemingly trivial aspects like talking about the weather, reading the morning newspaper, and so forth. Do such activities fit the scientist paradigm? When Kuhn’s (1970) work is quoted, it is usually the more glamorous aspects that are focused upon, like “crisis” and “revolution”. I wish to suggest that we pay more attention to “normal science,” which constitutes the lion’s share of the
scientist's activities, since we may profitably construe a similarity between normal science and our trivial everyday activities.

Normal science is described as being aimed at articulation of a paradigm. The paradigm is an evolving achievement. It supplies a criterion for choosing problems, sets standards for solutions, and provides a source of methods—in short, determines what is "important." The paradigm must be taken for granted since it provides the background for the capacity to solve puzzles. Normal science is not directed at novelty but, rather, at its suppression. The aim is not to test or challenge, but to defend the current paradigm, to resolve puzzles for whose very existence the paradigm must be assumed. When doing normal science, solving puzzles will serve to increase faith in the paradigm in terms of which they were formulated.

The paradigm is introduced to students by exemplary experiments; this may be seen as similar to inducing us to take the reality of our social world [cf. Berger, 1977, Chapter 1] for granted. Normal science corresponds to the process of constructing a consistent and meaningful reality—an overarching organization of symbols for our everyday life. This socially constructed world is continually mediated to and actualized by the individual. The main point here is that this world is in need of validation, perhaps precisely because of an ever present glimmer of suspicion as to its social manufacture and relativity. In a sense all the other coinhabitants of this world serve a validating function. Thus for instance reading the morning newspaper, and watching the television news, etc., serve to validate "the widest coordinates of my world [Berger, 1977, p. 29]." Perhaps some of us may have felt discomfort if suddenly deprived of this sort of validation. (See Tschudi & Rommetveit, 1982.)

It is now possible to suggest an answer to a question raised by Steve Duck (Chapter 4, in this volume): "Why is it that what happens at the bar at a scientific conference is so often seen as the most meaningful activity at such places?" Perhaps participation at conferences, a Kellyan one being no exception, should not mainly (if at all) be described in glamorous terms like "being on the frontier," "participating in pushing forth a new paradigm" or whatever. Rather, it may with no less validity be described in more humble terms as "buttressing one's perhaps sometimes shaky identity as 'scientist.'" One may compare participation at conferences to Bateson's (1951, p. 213) description of conversations of leisure hours that serve to affirm contact with other persons. Similarly, one needs to know that one is in contact with a "scientific community"; this may well be far more important than the factual content. If one loses sight of this function of conferences, participation may be quite frustrating, but one may learn to "articulate the paradigm." Have I lost contact with the reader?
IV. Some Normative Aspects of the Personal Scientist Paradigm

By considering some possible objections to the preceding defense of the personal scientist paradigm, we may bring to light normative aspects, some moral values, in the personal scientist paradigm. Reid (1979) makes much of a possible conflict between understanding and control. Does this conflict with Kelly's (1955) view of the personal scientist who is "ever seeking to predict and control the course of events with which he is involved [p. 5]"? Reid (1979), however, is concerned with persons who "cook the books, make events more manageable at the expense of understanding [p. 237]." This is clearly Kellyan "hostility," tampering with the evidence to coercively bring forth "validation." We take this to epitomize bad science and have previously argued that hostility may well be involved in all disorders of construction (Tschudi, 1977). So, when Reid (1979) asserts, "Constructs predispose certain outcomes. . . . They cannot be described in terms of their predictive properties [p. 237]," this might hold for hostility or other shortcomings of the incipient scientist but will not generally hold good for the relatedness property of our construing. Another facet of bad science is to ask the same trite question over and over again—what Berne (1972) would call "follow scripts [p. 125]." The Kellyan ideal is clearly the contrasting "autonomy" (versus scripts)—that is, to ask new questions through one's behavior, to "venture further."

What kinds of child rearing and what types of political systems provide the best "laboratories" for encouraging people to ask questions rather than follow orders, whether in the form of one's own scripts or socially enforced behavior? This is not the place to pursue these questions, but we must bear in mind that the wish to extend the frontiers and to ask new questions must be coupled with the necessity for order and stability. It is both necessary to uphold a social system and to maintain one's identity. We should remember that "scientific revolutions" are possible only against the background of "normal science." How to further progress and development without sacrificing the required degree of stability, is a problem worth much further work.

Acknowledgments

I am grateful to Asle Nilsen and Andrew Jones for helpful comments on earlier drafts of this chapter.
8. Constructs Are Hypotheses

References

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