Traits, Consistency and Conceptual Alternatives for Personality Theory

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The trait approach to personality has been under heavy attack of late. Palpable hits have been scored. Traits would seem to be on the run. However, even if we accept the critics' assessment of the results, it is not sufficiently clear exactly what has been shown, nor indeed exactly what they take themselves to have shown. In particular the literature leaves relatively indeterminate:

A. The exact target of the attack. In just what sense of 'trait' are traits being criticized? What variables are to be put under this rubric?

B. Just what defects are traits claimed to have? What *desiderata* is it they are said not to satisfy?

Until we have rendered the position sufficiently determinate on these points we will not be in a position to determine whether any of the arguments deployed by the critics give adequate support to their contentions. Nor, assuming that the arguments do the job, will it be sufficiently clear just *what* has been shown defective in *what* way. As a result we will be exposed to the twin dangers of dismissing concepts that do not fall under the ban, and of reintroducing concepts that do. In scientific, as in political matters, it is all too easy to win the war and lose the peace.

This paper is an attempt to provide the needed clarification by invoking some fundamental conceptual and methodological distinctions. I should make it explicit at the outset that my interest is by no means confined to removing ambiguities in the existing literature, or to determining exactly what certain authors 'really mean' (or ought to mean). My primary interest is in making a contribution of my own to the enterprise of choosing between basic conceptual alternatives for personality description. However, that contribution being of a conceptual and theoretical rather than an empirical sort, a useful springboard will be provided by the identification

and resolution of indeterminacies in the recent controversies over traits,[1] and an assessment of the argumentation in that literature.[2]

By way of preview the most important conclusions of this essay will be:

1. Although the attack on traits has primarily focused on their generality (lack of situational specificity), there is a more basic issue of conceptual type that has been masked by the former concern.

2. The 'utility' of trait-attributions, another focus of attention, is both multi-faceted and complexly determined, and it is important to separate out the simpler components and consider them separately.

3. Empirical consistency data have little bearing on the choice of a basic conceptual framework.

4. The crucial considerations for that choice lie in the theory of motivation.

I shall direct my remarks specifically to the leading opponent of traits on the contemporary scene, Professor Walter Mischel. I choose Mischel for this purpose not because his writings are especially rich in ambiguities. On the contrary, just because he is the most careful and incisive of the critics, any clarification called for by his writings is genuinely needed to advance the discussion.

I

What is the target of the criticism? Traits and other personality characteristics

In the most recent statement of his position (Mischel 1973) Mischel opposes the 'fundamental assumption' of 'traditional trait approaches', that 'personality comprises broad underlying dispositions which pervasively influence the individual's behaviour across many situations and lead to consistency in his behaviour' (p. 253). In other passages he specifies his target in similar terms: 'global traits that manifest themselves pervasively' (p. 253), 'global personality disposition' (p. 253), 'widely generalized dispositions' (p. 256), 'global underlying traits and dispositions' (p. 262).

[1] In some instances where the critic is precise and unambiguous my point will be rather that there are other possibilities that should also be considered and that are sometimes much more important.

[2] I should also counter any impression I might already have created that I will be arriving at a single resolution of the indeterminacies mentioned, e.g., a single specification of the target of the attack. Rather I will often display a spread of alternatives, between which it may or may not be necessary to choose.

Thus it is clear that Mischel conceives the object of his criticism as 'broad dispositions'. But in order to pinpoint the target more narrowly, we need to know whether 'broad dispositions' are opposed just because of their breadth, just because of their dispositionality, or because of both. If because of both, is one or the other also objectionable by itself? And if being a disposition at least contributes to the deficiency, is *any* kind of disposition objectionable, or only some sub-type thereof?

A full budget of questions. Fortunately one of them is quickly answered. Clearly Mischel objects to the *breadth* of traditional trait concepts. Most of his critical arguments concern that point. The only difficult questions are whether he objects to other features as well, and, if so, just what they are.

I fear we will not get much help on these points from Mischel's explicit statements, which leave it quite unclear whether he opposes dispositions generally or only the 'broad' kind. Indeed we are given no guidance as to just what Mischel means by 'disposition' and so to what does and does not fall under this term. On the whole one gets the impression that he is using the term to cover all personality characteristics, including those he himself advocates,[1] in which case he could not be objecting to dispositions as such. But there are also hints that he means to be using the term in some narrower sense in which it would not range over all personality charac-teristics.[2] If we are to determine whether the attack on traits is (or could be or should be) at least in part an attack on 'dispositions' in some sense of that term, we shall have to give explicit consideration to the concept of a disposition and the varieties thereof.

A. T-*concepts:* S–R *frequency dispositions*

To say that x has a certain *disposition* is to assert a certain hypothetical proposition, a proposition that if x is in a certain type of situation (S),[3] x will emit a certain type of response (R). It is better to speak of dispositional

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[1] Thus he seems to take the disposition–situation relation as equivalent to the personal variable–situation relation (1973, pp. 253, 256). Again almost all his negative remarks about dispositions (e.g. that inferences to them have little pre-dictive utility) use a qualification like 'broad' or 'global'; he never explicitly objects to dispositions without qualification.

[2] For example, in introducing his own positive proposals he says, 'The focus shifts from attempting to compare and generalize about what different individuals "are like" to an assessment of what they *do*—behaviorally and cognitively—in relation to the psychological conditions in which they do it' (1973, p. 265). This makes it sound as if on his approach we are not dealing with dispositions of any sort.

[3] Note that I am *not* using S to abbreviate *stimulus*.

concepts (alternatively, 'dispositional *terms*'), rather than of dispositions as a kind of state or attribute; for one and the same state or condition can often (perhaps always in principle) be conceptualized both in dispositional and non-dispositional terms. Thus fragility is construed dispositionally when we think of x's being fragile simply as the truth of the hypothetical proposition, 'If x is struck sharply x will break.' But if we can discover what micro-structure of x is responsible for the truth of that hypothetical proposition, we can think of the same attribute in non-dispositional terms. This ambiguity does not affect concepts, which are unequivocally either dispositional or not, depending on whether in attributing the concept to x we are committing ourselves to some proposition of the form 'If x is in S, then x will R'. The content of a particular dispositional concept is given by such a hypothetical proposition: a dispositional concept is made up of an S-category and an R-category put together in an if–then structure. Thus a dispositional concept 'embodies' an S–R regularity, differing from a mere report of that regularity only by way of also embodying the claim that there is something more or less stable in x's constitution that is responsible for the regularity (but without specifying what that is).

Dispositional concepts are familiar in personality description. Thus in attributing sociability to x, we are saying that if (when) x is (S) presented with an opportunity for social contact, x will (R) take advantage of it. Some other familiar concepts of this sort, each with its distinctive S- and R-categories are:

Cooperativeness —(R) complies with (S) (reasonable) requests.
Persistence   —(R) continues an activity (S) in the face of difficulties.
Domineeringness—(R) takes advantage of (S) opportunities to exercise
                            control over others.

To be sure, in attributing, e.g. sociability, to x we are not claiming that x takes advantage of *every* opportunity for social contact; we are in no position to attribute *absolute* or *unqualified* dispositions to persons. The S- and R- categories with which we work are not invariably connected. Rather we work with *frequency* dispositions: sociability is a matter of *frequently* (relative to some norm for the S–R pair) taking advantage of opportunities for social contact. To be somewhat more precise, we think of different persons having different degrees of the disposition, where the degree is a function of the frequency of R's in a representative set of S's, along with he average magnitude of the R's. The term 'trait' has often been defined along these lines.[1] But since 'trait' is often in fact used more widely I shall

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[1] See Allport (1937), p. 295 and (1961), p. 337; Vernon (1933), p. 542; Cattell (1965), p. 28; Cronbach (1960), p. 499.

use the more schematic term '*T*-concept' for concepts that fit the *S–R* frequency disposition model.

If one is to work out a wholly explicit and unambiguous model for *T*-concepts[1] he will have to deal with some rather thorny issues. Here I shall mention only two.

(A) *S*- and *R*-categories of various sorts can be put into the *T*-form. The above examples all involve overt, publicly observable *R*'s: but there are familiar *T*-concepts with 'private' *R*-categories that range over cognitive or affective responses. Consider, e.g., 'introspective' where the *R*-category is something like 'paying attention to or dwelling on one's own thoughts, feelings, and characteristics'; or 'analytical' where the *R*-category is something like 'thinking about problems in an analytical way'. Again the above examples feature *S*-categories that are stated in terms of objective features of the situation, e.g. whether there *is* an opportunity for social contact or whether a request *has* been made. But it may be more useful in personality description to construe the *S*-categories as having to do with how the person perceives the situation; a failure to comply with a request of which one was unaware should not count against one's cooperativeness.

(B) *T*-concepts differ in the extent to which distinctive *S*-categories are involved, i.e. in the extent to which *R*'s will count in favour of the trait attribution only in certain kinds of situations. Where there are such situational restrictions, this may be because the restriction is built into the very meaning of the *R*-category (it makes no sense to speak of 'complying' unless one is responding to something like a request) or it may be that although *R*'s of that category are possible in other situations they *do not* count as manifesting this trait unless the appropriate *S*-category is exemplified. (Thus 'continuing an activity' does not count as manifesting persistence unless it is 'in the presence of difficulties'.) However, there are traits which appear to involve no situational restrictions at all; this is most obviously true with respect to 'stylistic' traits, the *R*-categories of which have to do with the manner in which something is done—*methodical* or *energetic*. It would seem that one may proceed in a methodical or energetic manner in any situation in which one is doing *anything*; and that doing so will count towards possession of the trait, whatever the situation.

B. PC (*Purposive–Cognitive*) *Concepts*

Although many psychologists are given to using the term 'trait' (defined

[1] For a detailed discussion see Alston (1970).

as an *S–R* frequency disposition) to cover all concepts employed in personality description, the fact remains that not all of them fit the *T*-model. This can be shown by applying a basic test for the applicability of the model. When we attribute a high degree of a *T* to *x*, it is part of what we are asserting (what we mean) that, given a representative set of *S*'s, *x* will emit a large number of *R*'s (relative to the norm for that disposition). The concepts listed above, and many others, pass this test. To say that *x* is very cooperative is to *say* that if *x* is confronted with a representative spread of reasonable requests, he will comply in a large proportion of the cases. That is what we mean by 'cooperative': it is constitutive of our *concept* of cooperativeness. But concepts of needs, motives, interests, values, attitudes, and abilities, among other, do not pass the test. In these cases there is no empirically accessible *R*-category such that a high degree of the attribute necessarily involves a frequency of such *R*'s. To be sure, these attributes *do* often have typical *R*-manifestations. Thus an ability is typically manifested in its exercise, a need in efforts to satisfy it, a favourable attitude in actions directed to promoting or benefiting its object. Nevertheless it is not part of what we *mean* in attributing (even a strong degree) of some ability, need, or attitude, that such manifestations will frequently occur. A person may have abilities that he rarely exercises. Thus a man may be a crack pistol shot, but, because he doesn't have a pistol or because of lack of interest, rarely exercises this ability. A person may have a strong need for close relationships, but because of fear of rejection rarely or never seeks to satisfy it.

   The point I am making is a conceptual rather than an empirical or factual one. I am not saying that there really are factors that can prevent even highly developed abilities from being exercised and that can inhibit attempts to satisfy even very strong needs, although I believe this to be the case. My contention has to do with the structure of concepts. I am saying that it is part of what we *mean* when we attribute a *T* that *R*'s of the appropriate category will be frequently emitted; whereas nothing of the sort is part of what we *mean* when we attribute a need, ability, or attitude. It would be self-contradictory to say 'he is very cooperative but he rarely complies with (reasonable) requests'; but it would not be at all self-contradictory to say 'he has a strong need for close relationships, but he rarely does anything to foster them'. The contrast is most striking when we take *T* and *PC* concepts with the same 'filling'. If one agrees that *S* rarely takes advantage of opportunities for social contact, one is thereby debarred (conceptually) from going on to say that *S* is very sociable (a *T*-concept), but one *can* go on to say without contradiction that *S* has a strong *need*

for social contact. One would be contradicting himself if he said '*S* is very methodical (a *T*-concept), but rarely proceeds in a methodical fashion'; but it is intelligible to say '*S* is *very good at* methodical organization but he rarely takes the trouble to do things methodically'.

Thus needs, abilities, attitudes and the like cannot be construed as *S–R* frequency dispositions. How then? To put the matter in wider perspective, let us note with Mischel that the characteristics we attribute in personality description are not themselves observable but are inferred from observation.[1] One way in which a concept of something non-observable may be formed in a scientifically respectable fashion (so as to permit empirical testing of its applications) is to put observable characteristics into an if-then form, thereby yielding a dispositional concept. Here though the filling of the concept is observational, the hypothetical form prevents the attribute conceptualized from being itself observable. But most philosophers of science recognize another way in which non-observable concepts may be formed, a way which yields concepts that do not even have an observational filling but still preserve sufficient links to the observable. This other way involves constructing a theory of the unobservable 'fine structure' of an object or system with a view to explaining its observable behaviour. The various terms of that theory will then get their meaning from their place in the theory, understanding 'place in the theory' to include the functioning of the theory in the explanation of empirical data, as well as its internal structure. 'Theoretical' terms are thus related to observable data more indirectly than dispositional terms. An application of a dispositional term, as we have seen, has definite implications all by itself for patterns of observable occurrences. But an application of a theoretical term will have such implications only in conjunction with other premises. In applying the dispositional term 'magnetic' to *x* I *thereby* imply that certain observable movements will occur under certain observable circumstances. But an application of a theoretical term like 'has a free electron in the outer shell' to *x* does not, by itself, imply anything about observables. To get such implications we have to put that particular attribution in the context of a complicated theory as to how being in that state will be manifested in certain observable phenomena. Theoretical terms relate to observable phenomena only in systematic interrelations, while dispositional terms are hooked up to the data one-by-one.[2]

[1] Actually he makes this point explicitly (1973, p. 253) only for 'broad underlying dispositions', but it patently applies to any sort of personality characteristics, including those he favours (see below, pp. 31–2, and footnote p. 29).

[2] For a classic presentation of the theoretical–dispositional distinction see Carnap (1956).

My suggestion is that the non-T items among concepts of personality characteristics, at least the more interesting and prevalent of them, are theoretical concepts. Of course each theory constitutes a spawning ground for theoretical concepts; there are as many distinctive sets of such concepts as there are distinctively different theories. In principle concepts for personality description might be drawn from various neurophysiological theories, information processing theory, and so on. However I would suggest that virtually all the interesting theoretical concepts in personality theory spring from one or another form of a theory of motivation that I shall call Purposive–Cognitive Theory. According to this way of thinking, which in its gross outlines is familiar to all of us from early childhood, intentional action is undertaken in order to reach certain goals, the particular means employed being a function of the agent's beliefs as to what, in the current situation, is most likely to attain that goal. Sam is starting his car because the dominant goal for him at present is to arrive at his office at 9.00 a.m. and he believes that the best way of ensuring this is to drive his car to a certain parking lot, and that in order to do so he has to start the car. In stark outline this model features three basic types of inner psychological determinants, *desires*, which, so to speak, mark out certain states as 'to be striven for', beliefs, which provide bases for selecting lines of action as the most promising ways of reaching those goals, and *abilities*, which delineate the response repertoire from which the desire–belief combinations make their selection.

Typically more than one desire (including aversions under that heading) is activated at a given moment; and for any given goal, typically more than one means is envisaged. This means that usually there is conflict as to which of several alternative lines of action is most promising *vis-à-vis* a given goal; and, since the various desires and aversions activated at a given moment can rarely all be satisfied simultaneously there is conflict as to which goal is to be actively pursued. These facts force us to complicate the theory by inserting a 'field' of tendencies between the activated desires and beliefs on the one end, and the actual responses on the other. An appropriate desire–belief pair would, in the absence of contemporaneous competition, give rise to the actual deployment of the means in question (assuming they are within the power of the person). Where there is competition, we may think of *each* desire–belief pair as giving rise to a *tendency* to a certain response, where to say that P has a 'tendency' to R at a time k, is to say that in the absence of interference (and assuming that R is within his power) P emits R at k. Thus we get a field of such tendencies, and out of the interaction within that field, some particular actual R

emerges, its character being determined by such factors as the relative strength of the various tendencies and their compatibility or incompatibility. The desire to go to a party, plus knowledge concerning its location and available means of transportation, gives rise to a tendency to walk toward the garage, while the contemporaneous desire to finish a paper, plus relevant beliefs concerning location of materials and suitability of work spaces, gives rise to a tendency to walk to the study. Since these R's are clearly incompatible and since a compromise direction would have no chance of even partially satisfying either desire, the issue will be determined by the relative strength of the two tendencies. This theoretical approach has been elaborated in different ways by Freund, Lewin, Tolman, and more recently by e.g. Atkinson and Birch (1970) and by 'cognitive social learning' theorists, such as Bandura (1969) and Mischel (1968, 1973).

To return to our conceptual concerns, a desire or belief, I would suggest, is conceived as what occupies a certain position in motivational processes as depicted by the PC theory. A desire for a goal, G, is that psychological state which, together with a belief that doing A is likely to contribute to the realization of G, will give rise to a tendency to A: that state which, together with a belief that G has been attained, gives rise to a tendency to feel elated, and so on. A complementary statement can be given as to what a belief is. Let us use the term 'PC concept' for the concepts that are spawned by PC theory.[1]

We can now see *why* it is conceptually possible to have even a strong desire without frequent manifestations. Whether the tendencies to which a desire is a disposition are actually carried out depends not just on *their* character, but also on the competition they encounter in the current psychological field. A strong desire will (given suitable beliefs) necessarily give rise to one or more strong *tendencies*, and its strength *is* a function of the strength of those tendencies. But whether those tendencies will frequently, or even ever, be carried out is not a function of their strength alone. Thus even a strong desire may fail to be frequently manifested if frequently confronted by even stronger contrary desires.

However we are not yet at the level of personality characteristics. What I have just presented under the heading of 'desire' is what we may term an 'activated' desire, an internal state that exercises an active influence on thought, behaviour, and feeling. A person has an activated desire for food or for recognition during those periods in which there is some internal pressure on him to seek food or recognition, some tendency to feel dis-

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[1] Alston (1970), (1973) present a more detailed contrast of T- and PC-concepts (the latter called D's rather than PC's).

appointed if he fails to secure it, and so on. Activated desires are typically rather short-lived; they dissipate upon satiation, continued frustration, or the onset of more pressing concerns. Desire for food is notoriously cyclical, and even with a more 'psychogenic' desire like that for dominance or recognition, it is rare to find a person so single-minded as not to be sometimes wholly preoccupied with other matters. But when we set out to describe S's personality, we mean to be attributing to him relatively stable characteristics, each of which he possesses uninterruptedly for a considerable period of time.

Of course there is controversy as to how stable something needs to be to count as a component of personality. Mischel, along with other opponents of traits, is given to inveighing against the idea that personality should be described in terms of 'relatively stable, highly consistent attributes that exert widely *generalized* causal effects on behaviour'[1] (1973, p. 253). One gets the impression from such statements that Mischel wants personality theory to restrict itself to studying particular person–situation interactions, and to abjure any attempt to describe what it is a person carries around with him from one situation to another. However, this impression is countered both by the next paragraph in which Mischel indicates that personology should be concerned with 'the psychological products within the individual of cognitive development and social learning experiences', and by a scrutiny of his proposed variables. One of these is 'encoding strategies and person constructs,' which is explained as 'the perceiver's ways of encoding and grouping information from stimulus inputs' (1973, p. 267). By this is meant not the way a person does this on some particular occasion but the way he generally or habitually does it (when in the presence of certain kinds of inputs). Another of the variables is 'behaviour-outcome and stimulus–outcome expectancies'. Suppose we determine how Mr. A expects Mr. B to react to him in a particular situation. Surely no one would take that to be even part of a description of A's personality.

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[1] Consider also the following passages, in which he is working up to the introduction of his own favoured variables:

> ... it seems reasonable in the search for person variables to look more specifically at what the person *constructs* in particular conditions, rather than trying to infer what broad traits he generally *has* ... (1973, p. 265). ... The proposed cognitive social learning approach to personality shifts the unit of study from global traits inferred from behavioral signs to the individual's cognitive activities and behavior patterns, studied in relation to the specific conditions that evoke, maintain, and modify them and which they, in turn, change. The focus shifts from attempting to compare and generalize about what different individuals 'are like' to an assessment of what they *do*—behaviorally and cognitively—in relation to the psychological conditions in which they do it (*loc. cit.*).

For that expectation might be quite atypical, due to some constellation of circumstances that rarely if ever recurs. What we need for a characterization of *the person* is an account of what he usually expects to result from doing so-and-so in a certain kind of situation. However specific we make the so-and-so and the kind of situation, they will still be general categories that can have many particular instantiations. There is an ineluctable generality in personality characteristics, even in the hands of one who emphasizes specificity as much as Mischel.

Hence a report of the activated desires or beliefs that are operative in a person during a minute, an hour, or a day, would not count as part of a description of his personality. When we say, as part of a personality description, that *x* has a strong desire to dominate others (or in Mischel's lingo, that domination of others has a 'high subjective stimulus value' for *x*) what we are ascribing to him is rather a general liability to frequently acquire activated desires for domination; this liability is something he *can* possess uninterruptedly for considerable periods of time. Similarly when we say, in the context of personality description, that *x* expects to be rejected by new acquaintances, we are not reporting a particular activated expectancy he has at some particular time, but rather a general tendency for expectancies of that sort to be activated in situations of that sort. Let's call these higher level liabilities *latent* desires (expectancies, etc.).[1]

It follows from the preceding paragraph that latent desires are conceived as dispositions in a sense, i.e., dispositions to undergo the formation of activated desires for the object in question. For that matter activated desires themselves can be thought of as a sort of disposition. Recur to our characterization of an activated desire on page 25. One part of it reads: 'A desire for a goal, *G*, is that psychological state which, together with a belief that doing *A* is likely to contribute to the realization of *G*, will give rise to a tendency to *A*.' Another way of putting that is to say that a desire for *G* is a disposition to *R* if *S*, where *R* is the arousal of a tendency to do *A* and *S* is the activation of the belief that doing *A* is likely to contribute to the realization of *G*. But if both active and latent desires are construed as dispositions, how can we say they are distinguished from *T*'s in being theoretical rather than dispositional?

A way out of this dilemma is found by considering the fact that a great variety of materials can be put into the dispositional form. Put most

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[1] It is worthy of note that not all *PC*-concepts exhibit this two-level structure, with a distinction between the activated factor that is operative in motivation, and the general liability thereto that characterizes personality. With abilities, e.g., it seems to be the same ability to fly an airplane that characterizes the person and that is one necessary condition of flying behaviour in a particular situation.

generally, to attribute a disposition to x is to say that under conditions of a certain kind, C, x will do or undergo some kind of happening, H. In this most general sense dispositions are by no means limited to cases in which C and H are directly observable. But in characterizing T's as $S-R$ frequency dispositions, we were implicitly limiting our S- and R-categories to what is directly observable either by sense perception or by introspection,[1] and neither active nor latent PC's are dispositions of this kind. An activated desire is a disposition *to* a postulated, non-observable state (a tendency) and a latent desire, being a disposition to a disposition, also lacks an observational R-category. The basic contrast between dispositional and theoretical concepts concerns how they relate to the observational level; this difference remains firm even after we recognize the sense in which PC's are dispositional.

So far the discussion of PC-concepts has been restricted to desires, beliefs, and abilities, which I take to be basic because of the role of their active forms in motivation as depicted by PC theory. Other PC personality characteristics can be shown to derive from abilities, latent desires and latent beliefs. An expectation is a certain kind of belief; a need (in the psychological, not the biological sense) is a desire the non-satisfaction of which has particularly aversive consequences; an interest in music is (primarily) a complex of latent desires for goal-states connected with, e.g. music, an attitude is made up of such items as latent desires for the weal or woe of its object and evaluatively toned beliefs about that object. The detailed mapping of this conceptual field is, I believe, of the utmost importance for the psychology of personality, but I shall have to reserve that task for another occasion.

Personologists have not taken proper account of the $T-PC$ distinction.[2] They typically speak in a blanket fashion of the various 'indicators', 'behavioural referents', and 'measures' of an attribute without distin-

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[1] This restriction captures the positivistic, anti-theoretical orientation that has dominated the trait approach to personology. Much of the attractiveness of the T-model has stemmed from its promise of avoiding anything more occult than a disposition to observable responses, given observable conditions.

[2] Mischel is no exception. He does note that in psychodynamic theory 'diverse behavioural patterns serve the same enduring and generalized underlying dynamic or motivational dispositions' (1973, p. 253). That is tantamount to recognizing that, as we have put it, a given desire is not necessarily manifested in any one particular pattern of behaviour. But he seems to suppose that this is only because of the defensive distortions postulated by psychodynamic theory. He fails to see that the same is true of all PC-concepts, whether or not they are used within the version of PC theory that features defences. Hence he includes, e.g., attitudes and motives among the 'global traits' he is attacking. And he fails to note that his own variables may also be manifested in 'diverse behavioural patterns'.

guishing the crucially different ways in which an attribute may be related to its 'indicators'. Thus they miss the point that whereas it is part of the *meaning* of a *T*-term like 'domineering' (part of the concept of domineeringness) that if one is very domineering, 'indicators' like attempts to control others will happen frequently, it is not part of the meaning of a *PC* term like 'need for dominance' that if a person has a strong need of this sort, such attempts will frequently occur. Although we may have good (independent) reasons for supposing that they will, that does not follow just from what is meant by '*x* has a strong need for dominance'. The neglect of this point is only one example of a general lack of concern for the precise boundaries of one's concepts, a failing by no means restricted to personality theory but especially prominent there.[1]

Let's recall that we entered onto this extended conceptual investigation in the context of considering whether the criticism of 'traits' like aggressiveness is directed only at their generality, or also at their being 'dispositions' in some sense. What has emerged from our conceptual contrast is the point that the basic conceptual alternatives confronting the personality theorist include '*T* or *PC* (or both)' as well as 'more or less general'. Mischel, along with most other critics of traits, does not explicitly raise the former issue, but it is there under the surface none the less. This is apparent from the fact that Mischel's own variables differ from (most of) those he explicitly criticizes not only in being more specific, but also in being *PC*-rather than *T*-concepts.[2] Clearly the switch from very general *T*'s to Mischel-type variables is not sufficiently motivated just by a concern for specificity. If over-generality were the only defect of *T*'s like aggressiveness (construed as a disposition to behave equally aggressively in any

[1] For a similar complaint see Fiske (1971). The particular conceptual unclarity under discussion here is no doubt encouraged by operationalism, which encourages its devotees to lump all measures and indicators indiscriminately into the content of the concept. A more respectable reason for failure to notice the *T–PC* distinction is the fact that it cross-cuts some more salient distinctions. We find *PC* theorists, e.g., on both sides of the learning theory–psychodynamics distinction and the nomethetic–idiographic distinction, which have been so central in thinking about personality.

[2] A concise summary of these variables is on p. 275. Briefly, his 'construction competencies' are particular kinds of *abilities*; the 'encoding strategies and personal constructs' have to do with the conceptual resources available for *belief* formation, and predilections for using some of these rather than others in certain kinds of situations. 'Behaviour-outcome and stimulus-outcome expectancies' are particular kinds of *beliefs*. 'Subjective-stimulus values' represent an alternative way of talking about what I have been calling *desires*. 'Self-regulatory systems and plans' constitute a certain kind of *internalized rules*, which are found at a higher level of the *PC* motivation theory, discussion of which is prevented by lack of space.

interpersonal situation) the most obvious remedy would be to transform them into more specific *T*-concepts (like a disposition to act aggressively when in the presence of one's parents), rather than move to a radically different type of concept. Therefore in proposing that *his* list of variables be substituted for very general *T*'s, Mischel is implicitly raising the *T–PC* issue as well as the specificity issue. No doubt he is not plumping for *any* *PC* concepts but for a distinctive sub-type. Nevertheless the general *T–PC* contrast is very much involved in the contrast between what he rejects and what he favours.

And quite apart from what Mischel and other current trait-critics do and think, I am prepared to argue that the *T–PC* distinction is the most fundamental conceptual alternative for personality description. Which of these conceptual types we use (or which we stress if we use both) reflects, as we shall see pervasive theoretical allegiances in psychology. And many issues will take on quite different forms depending on which sort of concept is involved among them, choice and validation of measures, hypotheses relating personality to behaviour, and the structure of personality.[1] By comparison, questions concerning degree of generality are matters of empirical detail rather than of basic theoretical orientation.

II

What negative thesis about traits are the critics asserting?

Just what is Mischel's conclusion concerning traits? Again there is some uncertainty. In his most explicit statements he stresses that he is not denying that traits exist (in fact he is avoiding that question) but only that there is less utility than traditionally supposed in 'inferring broad dispositions from behavioural signs' (1973, p. 262). Utility for what purposes? The emphasis is on prediction and guidance of therapeutic intervention (the latter being based on prediction) (*loc. cit.*). Explanation is also mentioned but is not discussed as such. Mischel is also at pains to emphasize that trait attributions can still be of value in other connections, e.g., in everyday non-scientific dealings with people and even for (rough) predictions, especially provided further factors are taken into accounts (1973, pp. 262–3).

However, some of Mischel's arguments seem to point to the conclusion

[1] These points are discussed in Alston (1973).

that people are simply not generally characterized by very unspecific traits, like acting dependent in *any* interpersonal situation. The studies he cites in Mischel (1968) demonstrating a lack of correlation of R's across situational differences show, if they show anything, that people do not generally *have* a stable disposition to emit R with a certain frequency. And his theoretical considerations concerning discrimination learning (1973, pp. 258–9) point to the same conclusion.

Whatever the correct interpretation of Mischel, the issues can be clarified by becoming more explicit about (various) questions of existence and utility and their interrelations.

First, it seems that Mischel eschews the question of existence because he thinks of it as 'metaphysical' (1973, p. 263). Is it? The question whether a given characteristic 'exists' (at least the question that is of interest in the present connection) is the question of whether anything *has* that characteristic or perhaps whether every member, or a certain proportion of members, of a given class have it. Now why should we deny that it is an empirical question whether a given person has a given trait? Because traits (like other personality characteristics) are unobservable? But in order to use empirical evidence to settle the question of whether x has characteristic C it is not necessary that we be able to observe C (or observe that x has C), but only that some thing(s) we observe will confirm or disconfirm the hypothesis that x has C. So the question of whether the 'existence' of traits is an empirical issue boils down to the question of whether it is possible to use empirical data to confirm or disconfirm trait attributions. And, granting the adequacy of the above analysis of trait concepts, the answer to that is clearly in the affirmative. Ideally we would observe a person, P, in each of a considerable number of sets of S's and determine the frequency of R's in each set. We would then determine how stable this frequency is over the different sets of S's. If we get a reasonable stability we have strong empirical support for the hypothesis that S is characterized by a reasonably stable disposition to emit R in S with a frequency that falls within certain limits. If we find no such stability, we have strong support that S is not characterized by any such stable disposition.[1]

No doubt *T*-concepts are imprecise in various ways and this will introduce some indeterminacy in the outcome of empirical tests. For example, there is no definite answer to the question, 'How stable a frequency of R's

[1] No doubt we are rarely if ever in a position to gather empirical data of this maximally conclusive sort: we have to content ourselves with more indirect indications that such an ideal investigation would have one or another outcome. However the present point is only that trait concepts leave open the possibility of this sort of empirical test.

over different sets of S's must we have in order to be justified in attributing the corresponding T?'. And it would undoubtedly be unwise to tighten-up T-concepts by making a precise requirement. But if we were to rule out of science all questions infected with this kind of indeterminacy, what would be left standing? T-concepts are certainly precise enough to permit an empirical determination of their applicability that satisfies any reasonable requirements.

Of course psychologists are interested not just in whether some particular individual has a given trait but also in whether everyone has traits, and with whether there are certain traits that everyone, or most people, have. But if it is possible to determine empirically whether a given person has a given trait, it will thereby be possible to answer these further questions on empirical grounds.

Not only can the question of existence be raised in a scientifically legitimate fashion; it is more fundamental than questions of utility. The former must receive an affirmative answer before the latter can usefully be raised. We can hardly expect that trait attributions will be useful for prediction or explanation unless people really do have the traits in question. How could we expect to predict academic performance from degree of conscientiousness (except by luck) unless people *are* conscientious to various degrees. And a person's inability to maintain friendships cannot be explained by certain of his traits unless he does have those traits: it is intrinsic to the concept of explanation that a phenomenon can be explained only by what is really there.

These truisms may be obscured by talk of 'useful fictions' and 'models' that can be used without supposing them to literally represent the actual state of affairs. But we have recourse to such devices only when we are unable to determine empirically, in some more direct fashion, just what the state of affairs is.[1] When we are using concepts the applicability of which can be determined empirically, the only sensible course is to settle that question first and then, if the answer is affirmative, go on to use the concepts in prediction and explanation.

Though applicability of trait concepts is a necessary condition of their scientific utility it is hardly sufficient. A concept, e.g. nose length or liking for Dover sole, can be applicable to persons without being of any great utility in the prediction or explanation of behaviour. As the discussion progresses we shall see what else is required.

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[1] Indeed I should suppose that to the extent that a 'fiction' or a model does help us to predict or explain, we thereby have reason to suppose that it does, to some extent, accurately represent the subject matter.

One final point about utility. We should not assume in advance that different utilities are perfectly correlated. The trait of methodicalness may be very useful for predicting success in library work and other occupations, but not be of much value for basic theory or for explaining any wide range of behaviour. And unconscious conflicts may be efficacious in explaining neurotic compulsions, but, because of the difficulty of antecedent diagnosis and because of the complexity of other factors, not give us much leverage on the prediction of neurotic disorders.

III

The bearing of empirical evidence on the conceptual issue

Against the background of these distinctions let us consider what empirical evidence might conceivably show about the suitability of *T*- and *PC*-concepts for personality description.

The first point to make is that the most a particular study can show by itself is that the particular concept under investigation is deficient in some respect; any more general conclusions will be based on an induction from a number of such studies. Thus the first order of business is to determine what a certain kind of investigation can show about the particular concept investigated; this will set a ceiling on what we can hope for in the way of general conclusions.

The empirical studies most often cited by Mischel in his attack on traits are those indicating poor predictions from trait measures and those indicating a lack of 'consistency'. We shall consider them in turn.

Suppose we gather measures on one or more traits, for a certain population, and then try using this information to predict some outcome, such as academic success in college or diagnosis as neurotic. And suppose the results are disappointing. Does this show, or tend to show, either that the traits in question do not 'exist' generally in that population, or that they are not useful for prediction? No such conclusion can be drawn just from these data. The point is that there are at least three factors that would be responsible for the failure, alone or in combination. (To simplify the exposition I shall suppose that only one putative trait was involved.) (1) Most of the population simply does not have the trait. (2) The instruments used do not give accurate measures of that trait. (3) The trait is not tightly enough connected with that outcome to afford a basis for

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predicting *it*. If (2) could produce the results all by itself, then the results leave wide open the possibility that the trait does 'exist' and that it is closely enough connected with that outcome to be a good predictor if we could just find a way of getting at *it*. If (3) is the culprit, then though the existence of the trait is not cast in doubt, it does follow that the trait is not usable to predict that sort of event. But of course it remains possible that the trait can be used to predict other outcomes, or even, when combined with other variables, to predict this one.

Thus in order to use these results to show non-existence we have to be able to rule out the possibility that (2) or (3) could be responsible; to show lack of predictive utility, even for this particular dependent variable, we have to rule out (2). And the point is that we are rarely, if ever, in a position to do so. As for (2), with the exception of behaviour sampling all of the commonly employed trait measures are of dubious validity. As for (3), if we know anything about 'outcomes' like academic success and onset of neurosis, it is that they can be influenced by a great variety of factors. We are rarely, if ever, in a position to say that *if* a person does have a high position on trait dimension T_1, T_2 . . ., then (it is reasonably certain that) he will get high grades in college.[1] Hence lack of predictive success generally *could* be due just to the fact that the trait in question is not lawfully connected with what we are trying to predict.

Of course when a prediction from trait measures does consistently pan out, that is good evidence we have hold of something real and useful. But because of the above considerations negative results are highly ambiguous.

'Consistency studies' can usefully be divided into three types:

(1) Studies showing a low correlation between measures of the same trait.

(2) Studies showing a low correlation between different sub-classes of the appropriate *R*-category for a given trait.

(3) Studies showing a low correlation between responses of the appropriate *R*-category in different sub-classes of the appropriate *S*.

(1) would be exemplified by a study showing the absence of high correlations between measures of 'dependency' by self-report questionnaire ratings, direct observation of behaviour, Rorschach, and TAT. (2) would be exemplified by a study showing an absence of high-correlation between

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[1] Of course this itself constitutes a reason for regarding traits as low in predictive utility, a reason based on much more general considerations than those we are presently considering.

a number of sub-classes of 'dependency behaviour' in children, e.g. 'negative attention seeking', 'positive attention seeking', 'seeking reassurance', 'touching and holding', and 'being near'.[1] (3) would be exemplified by a study showing lack of high correlation between frequency of dependency behaviour in different kinds of situations, e.g. classroom, home, and playground, or in the presence of different persons or types of persons, e.g. parents, siblings, peers, teachers. Of course one and the same study may exemplify more than one type.[2]

All that we can conclude from the first kind of study is that not all the measures studied are (good) measures of the same thing. If there is no basis for picking any one measure as sufficiently reliable[3] we can further conclude that we are not presently in a position to determine whether or to what degree the concept applies to a given case and, *ipso facto*, are in no position to use the concept in prediction or explanation. But nothing follows from this concerning the place of the characteristic in the structure of personality or its potentialities for personality theory. Even if we are not *now* able to find a valid measure of T, it still may be that T actually characterizes people and that it is central enough to be important for the explanation and prediction of behaviour. No doubt failure after many and

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[1] See Sears, (1963).

[2] The difference between the first type and the other two is often slighted, for reasons similar to those responsible for the neglect of the T–PC distinction. If one lumps together all 'indicators' of an attribute, he will see no difference between those that are implied by the very concept of that attribute (the R's studied in the second and third types) and the 'measures' that are *believed* to be correlated with it. But they are fundamentally different. It is part of the concept of rigidity, as a T-concept, that the rigid person will relatively frequently cling to a solution, concept, or attitude, even in the face of reasons to think it no longer appropriate. Hence, data about the frequency of such responses have a direct and unambiguous bearing on the concept and its application. But it is *not* part of what is meant by 'rigidity' that the rigid person will give certain sorts of responses on the Rorschach or TAT or self-descriptive questionnaire, or that he will be rated as rigid by his acquaintances. (If one of these measures were to become firmly entrenched in scientific practice and theorizing, the concept of rigidity might undergo such an accretion. But clearly that is not the present state of affairs). Hence if a person gets a low rigidity score on one of these measures that does not decisively and unquestionably show that he is not rigid, and if these measures of rigidity are uncorrelated, that does not in itself discredit the concept.

[3] It would seem to follow from our analysis of T-concepts that there is always a reliable measure that is in principle available to us, *viz.*, behaviour sampling. If we have found a reasonably stable frequency of R's in a number of largish sets of S's, then by the very definition of the T-term, this constitutes good reason for ascribing a certain degree of that trait. Of course this procedure is rarely a practicable alternative. And PC concepts, as well as many other sorts of psychological concepts, do *not* carry with them any such built-in guarantee of the realiability of a particular measure.

varied attempts is some augury of continued failure. But it is always difficult to estimate how skilful and determined have been the attempts to date. And in any event there are well-known pitfalls in predicting future scientific developments.

What about the second kind of study? What further implications can be drawn from the lack of a high correlation between sub-classes of R? Before tackling that question, let us note that this kind of 'inconsistency' seems to be an ineradicable feature of any $S-R$ disposition, no matter how specific. The point is simply this. The R-category in an $S-R$ disposition is general, ranging over an indefinite number of instances. Indeed this is involved in the very notion of a *disposition* as a liability to emit an R *of a certain sort* whenever in an S *of a certain sort*. Furthermore, even if it is possible to construct an R-category so specific that it admits of no sub-categories, and this is debatable, it is clear that any interesting R will admit of sub-categorization. One can be aggressive either physically or verbally: one can be sociable by engaging someone in conversation, issuing invitations to a party, showing up for communal lunches, and so on. The same point holds at more specific levels. Consider one of the sub-categories of dependency behaviour distinguished in Sears (1963), 'seeking reassurance'. One can seek reassurance by explicitly asking someone what he thinks of one, or by indirectly steering the conversation in that direction; one can seek reassurance concerning one's appearance, one's competence, and so on. Even if we make the category much too specific to be of any interest for personality description, there will still be many alternative modes of realization. Consider the category, *asking someone whether I am attractive*. This can be done by using a variety of different sentences, even in the same language; it can be done in speech or in writing; in many different tones of voice, inflection patterns, and degrees of loudness. All these matters and many others would have to be precisely determined before we arrived at a maximally specific category that permits no sub-categorization.

Now it would be enormously surprising if the frequency with which a person exemplifies one sub-category of a given R were closely matched with his frequency on all the others. Among the various ways of exemplifying a category a given person will generally have a style that limits him to some sub-set of those ways most of the time. It would be a fantastic virtuoso who would use the various possible dynamic intensities, intonation patterns, and accents, in about the same proportions over the occasions on which he asked a certain question. Again it would be a veritable chameleon who would use equally often all the various modes of *seeking*

dominance. This is not to say that *some* sub-categories may not be highly correlated. For example, it could have been the case that 'positive attention seeking' and 'seeking reassurance' (as sub-categories of 'dependency behaviour') are highly correlated; it is a contribution of Sears to have shown otherwise. However I do believe that considerations of the above sort strongly support the thesis that for any *R*-category likely to be of interest to a psychologist, there will be some sub-categories that are not highly correlated with each other.

To return to the question at issue. Clearly a lack of high correlation between sub-classes of *R* can do nothing to show an inapplicability of the concept, for no such correlation is required by a *T*-concept. A *T*-concept is at a level of generality defined by its embedded *R*-concept. It does not 'purport' to encapsulate information about more specific response types; it is irrelevant to its applicability how those more specific types are distributed. So long as Jones' frequency of dependency behaviour is fairly stable over different sets of situations he can unambiguously be credited with a certain degree of the trait of dependency, whether or not he seeks succourance in some ways much more than others. It may well be interesting to determine the pattern of these sub-types, but that is a *further* question: it cannot affect the truth of the more general trait attribution.

Essentially the same point is to be made concerning the bearing of these studies on predictive or explanatory utility. The fact that one does not seek succourance in all possible ways to the same extent has no implications whatever as to the scientific utility of information about one's degree of dependency. We *may* be able to predict and explain many differences between Jones and Smith just by knowing that whereas the former frequently seeks succourance in some way or other, the latter infrequently seeks succourance in any way. Think of a trait like *creativity*, sub-categories of which no one would ever have expected to be highly correlated. It would be absurd to suppose that highly creative persons are equally creative in music, painting, literature, science, institutional innovation, and personal relationships. Obviously people specialize in their creative endeavours. Nevertheless it is useful to distinguish persons who are highly creative in some way or other from those who are not highly creative in any way. No doubt it is *also* useful to classify people in terms of more specific attributes, like 'creative in music', and 'creative in science'. But this by no means implies that it is *not* useful to employ the more general concept. The third kind of consistency study is more to the point. We have pointed out that in attributing a *T* to a person we imply that he will produce something like the same frequency of *R*'s in every large set of *S*'s. And the

results of this third sort of study seem to directly contradict that implica-
tion. If *S* frequently seeks succourance at home but infrequently at school,
that shows he does not have a stable disposition to seek succourance with
about the same frequency in any kind of interpersonal situation. But we
must proceed with caution. The implication of even approximately equal
frequencies of *R*'s in any large set of *S*'s is obviously false for any interest-
ing *R*- and *S*-categories. We can always devise variations on the appropriate
S-category so as to produce a marked deviation from the average *R*-
frequency. No matter how dependent a person is, so long as he is in control
of his behaviour he will seek succourance less often than usual where
severe penalities are attached to doing so. No matter how cooperative a
person, he will cooperate less than usual when under severe emotional
strain. No matter how creative, a person will act less creatively at some
times and in some moods than others: Mozart did not produce a master-
piece every day. But this is to make trait psychology too easily refutable.
A sober conception of traits will allow for variations in mood, for satiation
effects, and for effects of abnormal situations. The stability condition will
rather be that there is a fairly stable frequency over 'normal' situations in
which the subject is in a 'normal' condition. These are rough qualifications,
and so there will be no sharp line between satisfaction and non-satisfaction
of the condition. But in a typical 'consistency' study of the third sort we
take a large number of subjects and compute the average correlation
between frequency of aggressive behaviour over several different kinds of
interpersonal situations. Because of the number of subjects oscillation in
mood and other temporary conditions will presumably cancel each other
out, and we take care to make the situations 'normal'. Hence if the average
correlation turns out to be very low, insignificant, or negative, we *can*
conclude that people in general[1] simply do not have any such general trait
as aggressiveness in interpersonal situations. For the data do indicate that
even if we held the relevant emotional and motivational condition of the
subject constant and avoided unusual features in the situations, people
would not generally act aggressively to about the same extent in any
interpersonal situation. These data do show that the *R*- and *S*-categories
for that trait are not suitably matched, that *S*'s of that sort (at that level of
generality) cannot be depended on to consistently produce any particular
frequency of *R*'s of the specified category.

It is interesting that the only type of consistency study with direct

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[1] Clearly the fact that aggression at home and at school are not generally highly
correlated does nothing to show that *some* people may not have a stable disposition
to be aggressive to about the same degree in any interpersonal situation.

bearing on the status of trait concepts, should bear directly on their exist-
ence rather than their utility. No doubt it follows from a successful study
of this sort that the *T*-concept studied is not to be recommended for ex-
planation and prediction. But that is because what the study directly
shows is that the concept is not applicable in the population at large and
therefore is incapable of advancing our general understanding of behaviour.

At this point we may digress briefly to consider what bearing empirical
studies of these sorts might have on the status of *PC*-concepts. Predictive
studies and the first kind of consistency study (correlation between different
putative measures) can yield the same conclusions as with *T*-concepts.
The differences between *T*'s and *PC*'s are not relevant here. Where the
differences obtrude themselves is in the second and third types of con-
sistency studies. We cannot (properly) do those studies for *PC*-concepts
just because there is no *R*- and *S*-category for a *PC*. This is not to say that
psychologists have not tried to do this sort of thing for *PC*-concepts, but
in doing so they have betrayed a lack of understanding of the concepts with
which they are dealing. With respect to the third kind of study in particular,
since in attributing a *PC* we are not committing ourselves to a stable
frequency of *R*'s of some particular category in sets of *S*'s of a certain
category, correlations or lack of correlations of *R*'s over sub-types of some
S can have no direct bearing on the status of the concept.[1] While showing
that the incidence of aggressive acts differs sharply between home and
playground undermines the general *T*-concept of aggressiveness, it leaves
the concept of a need for aggression untouched, for that concept by itself
has no implications concerning the frequency of aggressive acts. In fact
there is good reason to suppose that a strong need for aggression would
not give rise to similar frequencies of aggressive acts in all situations, for
different situations will often present contrary motivation to different
degrees, which means the inhibitory forces will often be of different
strength in different situations.

This point is often missed for just the reasons we cited earlier in ex-
plaining the failure to notice the *T–PC* distinction.

Let us return to *T*'s. We have seen that of the sorts of empirical investi-
gation cited by Mischel, only the third type of consistency study has a
strong bearing on the viability of the *T*-concept under investigation. Can

[1] This is not to say that *PC* concepts are invulnerable to any empirical data (and
a good thing, too). But to show their inapplicability is a much more complicated
task. Because of their systemic embeddedness in a theory, what is required is to
show that the theory from which they spring does not (or, much more conclusively,
cannot) adequately perform its allotted task *vis-à-vis* empirical data. And that is a
notoriously difficult matter to settle for any interesting theory.

any conclusions be drawn from this for the status of *T*-concepts in general?
Clearly nothing can be concluded about *T*-concepts generally from the
deficiencies of a single concept. But suppose that all *T*-concepts investi-
gated have been shown defective. Can't more general conclusions be drawn
in that case? Well, if the concepts investigated all have quite general *R*-
and *S*-categories such results may show that we are unlikely[1] to find stable
S–R dispositions *at that level of generality*. Thus data like these can have a
bearing on one of our conceptual alternatives—degree of generality. But it
is not helpful with respect to the more basic *T–PC* alternative. More
specifically, it cannot show that *no T*-concepts can pass the test. In fact
the data suggest a way of removing *this* defect from any given *T*-concept:
viz. make the *S*-category more specific.[2] If there is low correlation between
aggression at home and aggression in play situations, separate the general
concept of aggressiveness into aggressiveness at home and aggressiveness
at play, and see whether we get stable frequencies of aggressive behaviour
across different home samplings and different play samplings. If degree of
yielding to social influence differs markedly with different role relation-
ships, try some new *T*-concepts with the same *R*, but where the *S* in each
case embodies some specification of role relationship.[3] These are still *T*-
concepts; they have the basic *T*-structure we outlined earlier. It is simply
a matter of the *degree* of specificity of the *S*-category. Of course there is no
guarantee that these or any other more specific *T*'s will turn out to be
consistent across sub-types of their *S*-categories. It may well turn out,
e.g. that frequency of aggressive behaviour is uncorrelated over different
kinds of home situations. It may even be that no matter how specific we
make the *S*- and *R*-categories we still fall short of consistency. But the
studies we are now considering, which all deal with very general *S*-
categories, are insufficient to yield that conclusion.[4] Indeed it is dubious
that any empirical data could show that no consistent *T*-concepts *could*

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[1] Whether this is just an unlikelihood, rather than an impossibility, will have to
be settled by a determined search for exceptions, or else by theoretical considera-
tions.

[2] Note that the *S*- and *R*-categories provide two independent dimensions along
which *T*-concepts can vary in generality. It seems plausible to suppose that con-
sistency will vary inversely with generality of *S*, but directly with generality of *R*.
If the *R*-category is general enough (doing something) its frequency will be guaran-
teed to be the same in all situations.

[3] Cf. Argyle & Little (1972).

[4] Of course it may be that in order to get cross-situational stability of *R*-fre-
quencies we have to make the *S* so specific as to render the concept useless for
personality description. But again further investigation would be required to show
that.

be constructed. For consistent T-concepts might be produced not only by making our categories more specific, but also by finding more felicitous ways of constructing S- and R-categories (more felicitous in that the S-concept captures more fully the determinants of the R). And one can hardly expect to have any real basis for supposing that a given kind of conceptual breakthrough will *not* occur in science.

IV

A theoretical basis for the choice of concepts

The preceding discussion has revealed the powers and limits of consistency data. A consistency study can show that a particular T-concept is not generally applicable; from a number of such results we can conclude, more generally, that T-concepts above a certain level of generality are unlikely to be applicable. But consistency data will do nothing to show that no T-concepts are applicable nor, assuming that is the case, that it is PC-concepts, either in general or of the sort favoured by Mischel, that should be put in their place. Consistency data will not support the claim that T-concepts should be replaced by concepts of a radically different type. For this more fundamental considerations are needed.

A. Methodological considerations

One may be tempted to exclude T's on purely methodological grounds. We have seen that the T-concepts actually in use are, at most, frequency dispositions; none of them specify S- and R-categories that are invariably associated. That being the case, we need invoke nothing more specific than the general principle of determinism to show that even if such concepts are applicable, they cannot represent *fundamental* features of persons. Consider sociability. Not even the most gregarious of men takes advantage of every opportunity for social contact. At best people differ only in the *average frequency* of doing so. But if all behaviour is causally determined there must be some thing(s) that makes the difference between the occasions on which the person does and those on which he does not act sociably. So long as that is unspecified we cannot claim to have given an adequate

account of (this aspect) of the person. Frequency dispositions are at best a first stab at describing a person.[1]

Unfortunately this argument has exactly the same limitations as the consistency argument. It shows a defect in the *T*-concepts currently employed, but it does nothing to show that *any* *T*-concept must display those defects, and hence it fails to show that *T*-concepts as such are defective. It leaves open the possibility that *S–R* dispositions can be found that are invariable and hence (on this count) are suitable for describing what a person is fundamentally like.[2]

B. *Theory of motivation*

We have not succeeded in determining the fate of *T*-concepts by appeal to general methodological principles. The next place, in the order of decreasing abstractness to look for 'fundamental considerations', is basic psychological theory. Here, I believe, we shall find the crux of the matter.

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[1] Note that this argument is quite independent of the consistency argument. Even if the relative frequency of sociable responses is quite stable across situations (so that the concept *does* apply), sociability would still fail to qualify as a fundamental feature of personality just by virtue of being a frequency disposition.

[2] It is worth noting that if we are sufficiently liberal with our *S*-categories, there is a strong argument for the conclusion that there *must* be invariable *S–R* dispositions. If we add to the general principle of determinism the assumption that only what exists now can produce an effect now, we get the result that for any actual response, there must be some set of present conditions that causally determine it to occur just as it does occur. But that means that a set of conditions of that sort will invariably be followed by a response of that sort. So that if we could embody those sorts in *S*- and *R*-categories, we would be in a position to formulate an invariable *S–R* disposition.

The hooker in this argument is that it depends on putting no restriction on what can be included in the set of antecedent conditions. In particular it allows that set to include unobservable states of the person; it does not follow just from the general principle of determinism that every *R* is causally determined by *observable* factors. Hence this argument does not show that persons possess absolute *S–R* dispositions, where the *S*- and *R*-categories are restricted to directly observable variables. And that restriction is largely responsible for the attractiveness of *T*-concepts. Moreover even if every *R* is causally determined by a set of observable factors, there is no guarantee that the associated *S–R* dispositions will be simple enough, or general enough, to be usable in personality description. Suppose that we can find observable causal determinants for an *R* only by giving descriptions of both *R* and determining factors which are so specific that the constellation will rarely occur, and so specific that an enormous number of such dispositions would be possessed by a given person. In that case, the existence of absolute *S–R* dispositions would be of no significance for psychology.

I would suggest that the question of what *sort* of description of personality is *theoretically* preferable is to be decided by determining the most adequate theoretical account of that range of phenomena to the explanation of which personality is supposed to contribute. Now it is generally agreed that personality, whatever else it is, is that which is contributed by the person to the determination of behaviour. But then the question of how to conceptualize personality *is* the question of how to conceptualize what contribution the person makes to the determination of behaviour. And how we answer that question will depend on our general theory of motivation.[1]

On what theory of motivation would one or the other of our basic conceptual types be preferred? That question has already been answered for *PC*-concepts. For we saw that those concepts derive their content from their place in a certain theory of behaviour. But what about *T*-concepts? My suggestion is that personality could be adequately described by *T*-concepts only if an *S–R* theory were adequate for the explanation of behaviour. This whole matter can be best set out by a parallel comparison of the two sorts of theories and the two sorts of concepts.

On both *S–R* and *PC* theories, stimuli from the environment and from the organism play a role in response determination.[2] But what is contributed by more stable features of the person, features that could properly be mentioned in a description of personality? Here too there is at least one common element—abilities. On both approaches it will be acknowledged that response evocation is limited by the response repertoire of the person, what he is capable of. But as for what determines the selection from that repertoire the accounts diverge. On an *S–R* approach the further contribution of the person is in the form of a (very large) number of '*S–R* bonds', each of which consists of a disposition to emit a response of a certain type upon the presentation of a stimulus of a certain type, these dispositions conceived as varying in strength. The stimuli presented to the organism at a given moment 'activate' all its dispositions that involve stimulus categories to which any of the current stimuli belong. As a result of this activation, instances of the response categories of each of these dispositions will be produced, except where (as is normally the case) two or more such categories are incompatible; in this latter case the response from the strongest of the competing tendencies will be emitted.[3]

[1] I am using the term 'theory of motivation' to cover any theory of the contemporaneous determinants of behaviour. It has to do with what Tolman called 'principles of performance' in contrast to 'principles of learning'.

[2] The current state of the organism may contribute in other ways too, as in a theory that gives a prominent place to current drive level.

[3] It goes without saying that each of our theory-types exists in many different

In *PC* theory, on the other hand, selection from the response repertoire is determined by which tendency is strongest in the current 'tendency field'; and this in turn is determined by the relative strength of various desires and beliefs, which combine in various ways to produce response-tendencies, as we saw in our earlier sketch of the theory. Here stimuli function both to produce new beliefs (concerning the present state of the environment and organism) and to activate various standing beliefs and desires. (The details of the latter process must be enormously complicated and are still very little explored.)

Thus on *S–R* theory the motivational resources deposited in the individual by past learning[1] consist of an enormous number of (more or less specific)[2] 'habits', dispositions to react to a certain kind of stimulus with a certain kind of response. And behaviour is generated through the 'automatic' elicitation of these by current stimulation. Whereas in *PC* theory motivation involves the individual's utilization of what he has 'learned' about the world in an ordinary sense of 'learn' (acquisition of information), in relation to his desires and other orientations towards goals. These, together with current information from the environment, enter into what is much more like a process of *computing* what responses are most likely to arrive at certain goals. Indeed in the more explicit and sophisticated cases of response-generation it can become a literal conscious computation.[3]

versions, which will differ among themselves on more or less crucial points, e.g. how competition between simultaneously activated tendencies is resolved. Furthermore my general rubrics, especially '*S–R* theory', are sometimes used with different boundaries from those I am drawing. Hence even my highly schematic characterization cannot claim to be completely neutral as between rival versions. However I feel sure that this partiality does not compromise the generality of the points I am concerned to make in this paper.

[1] Again it is controversial just what is learned and what is innate. And again the contentions of this paper would be unaffected by where that line is drawn.

[2] It is often assumed that the bonds postulated by *S–R* theory must involve highly specific *S*- and *R*-categories. But no matter how general the categories, an *S–R* approach is sharply differentiated from a *PC* approach in the ways we are emphasizing.

[3] The stark outlines of this contrast inevitably becomes softened as each approach tries to take account of the points emphasized by the other. In that process one contestant may take on features of the other to the extent that discrimination becomes problematic. As things have worked out, it is *S–R* theory that has increasingly taken on features of *PC* theory, rather than vice versa. To be sure in the work of Tolman *PC* theory was dressed out in a quasi-*SR* garb. But with unanticipated reinforcement from computer modelling and information theory *PC* theory has exhibited a new-found determination to stick to its guns. *S–R* theory, on the other hand, has been forced by the stubborn pressure of the facts to recognize that a wholly peripheralist version is untenable. Behaviour is not solely a function of the objective features of current physical stimulation; it makes all the

Thus on an *S-R* theory of motivation personality (what the person contributes to the determination of behaviour) consists of *S-R* dispositions:[1] while on a *PC* theory personality consists of such facts as latent desires and beliefs. Hence whether personality should be described in *T* or *PC* terms depends on what theory of motivation is most adequate. Whatever reasons we have for supposing that behaviour can be adequately explained without invoking anything more conceptually complicated than the activation of dispositions by current stimuli, are also reasons for choosing *T*-concepts for the description of personality. And whatever reasons we have for supposing that behaviour cannot be adequately explained without considering the beliefs and goal-orientations of the behaver, are *ipso facto* reasons for choosing *PC*-concepts for personality theory.

Recalling the distinctions of section II, let us make explicit just *what* negative conclusions about *T*'s (and positive conclusions about *PC*'s) could be established by an argument from the theory of motivation. Like the methodological argument described in section IV.A, this argument will *not* show that *T*'s do not exist. A *PC* theory of motivation is compatible with the existence in a given individual *and* in the population at large, of any number of *S-R* connections of any degree of consistency, provided these *S-R* regularities are themselves to be explained by the operation of desires and beliefs, rather than taken as ultimate features of the personality. Suppose that *x* tries to dominate others whenever he is in a social situation. Suppose further that each such attempt is due to a strong desire for reassurance of his own worth (and a belief that dominating others provides a good chance of securing that reassurance). The consistency of the domineering behaviour would then be explained by the fact that this desire–belief pair is consistently activated in sufficient strength in any interpersonal situation. Or it may be that some attempts at domination are to be given that *PC* explanation and others are due to some other desire–belief pair, e.g. one involving fear of what might happen if someone else

difference in the world how this is perceived by the person, and how this engages his goal-hierarchy. *S-R* theorists have attempted to take account of this, without using *PC* concepts, by inserting internal *S-R* links between the external stimulation and overt behaviour. They seek to preserve the basic conceptual shape of the approach, while complicating the details and abandoning the Puritanical restriction to publicly observable factors.

[1] Just what filling is allowed in these dispositions depends on how liberal the *S-R* theory is. If and only if it countenances internal 'responses' can the *S-R* theorist allow his *S*-categories to range over how the situation is perceived, as well as its objective character. But in either case he will want his description of personality to feature units that can be 'automatically' activated by current stimulation.

were in control. In that case the consistency of attempts at domination would be explained by the fact that in any interpersonal situation, *some* desire–belief pair that will yield domineering behaviour is activated in sufficient strength. Thus a *PC* theory of motivation does not imply that there are no generally shared consistent *T*'s. What it does imply is something about their lack of utility; not (primarily) predictive utility, but *theoretical* utility, usability in a theoretically fundamental description of personality.[1] On a *PC* theory of motivation, if there are consistent *S–R* connections they are explainable by features of the person's desire–belief structure. In that case the most fundamental description of personality would specify not the surface *S–R* consistencies, but the desire–belief structure that is responsible for it.[2] What the theoretical argument, if successful, will show is that *PC*'s rather than *T*'s are the basic constituents of personality.

We must keep in mind the possibility that both a pure *T* and a pure *PC* theory of motivation are inferior to a mixed theory, according to which some behaviour occurs in order to reach certain goals, whereas in other cases one acts 'just out of habit', regardless of one's current beliefs and desires. For example, even if I am typing at this moment (rather than, e.g. driving my car) because I want to finish this paper and believe that using the typewriter is the best way to do it, it may still be that various features of my current behaviour, e.g. stroking my beard between bursts of typing, results from the activation of *S–R* bonds rather than from any belief, even unconscious, that stroking my beard is likely to contribute to the attainment of some desired goal. On a mixed theory of motivation we would need both *T*- and *PC*-concepts for a theoretically basic description of personality.

The theoretical argument and the current controversy

Conflict over fundamental issues makes strange bedfellows. Despite the

[1] Of course if *PC*'s are theoretically fundamental, they are *in principle* more powerful predictors, for other features of personality are derivative from them. However, since actual success in prediction requires not only that we have identified the right type of variable, but also that we have accurate measures of particular instances, there is no guarantee that we can actually make correct predictions from *PC* descriptions, however fundamental they are.

[2] This means that the theoretical argument, like the methodological one, is independent of the consistency argument. Whether or not there are consistent *T*'s, theoretical considerations may show that they are not fundamental.

controversy between 'social learning' theorists like Mischel and 'psycho-dynamic' theorists, they are clearly on the same side of the *T–PC* alternative.[1] In insisting on the importance of the latter issue I do not in the least want to play down the importance of the issues separating different *PC* theorists. To justify his own 'social learning' brand of *PC* personality description, as against the psychodynamic brand, Mischel quite properly deploys different theoretical considerations from those I have brought to the *T–PC* alternative.[2] I would only insist that unless we keep the various issues straight one may not know who his opponents and his allies are in a given struggle. And since the considerations needed to support 'cognitive social learning theory' variables against *S–R* dispositions, and against other kinds of *PC* variables are so radically different, it is important to disentangle these different issues and appreciate the distinctive character of each.

I do not wish to make a secret of my conviction that a *PC* theory offers the only real hope of understanding the springs of motivation and hence the only hope of understanding personality. Even if there are *some* pure *S–R* connections (so that a mixed theory is required) I still believe that desire–belief structures are responsible for the most interesting aspects of human behaviour. However, I cannot defend that conviction in this paper, but must content myself with the humbler job of pointing to the field on which the decisive battle will be fought.

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[1] It would seem that Mischel, and other 'cognitive social learning' theorists, like Bandura, are not aware of this. They tend to present their theory as one form of 'liberalized' *S–R* theory. Of course, in view of the protean shapes and transformation assumed by *S–R* theory one cannot deny them the right to the label; and no doubt their sort of *PC* theory is one that has been tightened up empirically in ways heavily influenced by the *S–R* tradition. Nevertheless the fact remains that the issues would be considerably clarified if it were recognized on all hands that the variables stressed by Mischel are fundamentally *PC* in their conceptual type.

[2] It is also worthy of note that Mischel relies on theoretical considerations only to support his positive proposals, not (or hardly ever) to argue *against* traits.

REFERENCES

ALLPORT, G. W. *Personality: A Psychological Interpretation.* New York: Henry Holt, 1937.

ALLPORT, G. W. *Pattern and Growth in Personality*. New York: Holt, Rinehart, and Winston, 1961.

ALSTON, W. P. Toward a Logical Geography of Personality: Traits and Deeper Lying Personality Characteristics. In M. K. Munitz (ed.), *Mind, Science, and History*. Albany, New York: State University of New York Press, 1970.

ALSTON, W. P. A Conceptual Divide in Personality Description. Invited address to the Personality and Social Psychology Division of the American Psychological Association, September, 1973.

ARGYLE, MICHAEL & BRIAN R. LITTLE Do Personality Traits Apply to Social Behaviour?, *Journal for the Theory of Social Behaviour*, vol. 2, no. 1, April, 1972, pp. 1–33.

ATKINSON, J. W. & D. BIRCH *The Dynamics of Action*. New York: John Wiley & Sons, 1970.

BANDURA, A. *Principles of Behavior Modification*. New York: Holt, Rinehart & Winston, 1969.

CARNAP, R. The Methodological Character of Theoretical Concepts, in H. Feigl & M. Scriven (eds.), *Minnesota Studies in the Philosophy of Science*, vol. I. Minneapolis: University of Minnesota Press, 1956.

CATTELL, R. B. *The Scientific Analysis of Personality*. Chicago: Aldine Pub. Co., 1965.

CRONBACH, L. J. *Essentials of Psychological Testing*, 2nd ed. New York: Harper & Row, 1960.

FISKE, D. W. *Measuring the Concepts of Personality*. Chicago: Aldine Pub. Co., 1971.

MISCHEL, W. *Personality and Assessment*. New York: Wiley, 1968.

MISCHEL, W. Toward a Cognitive Social Learning Reconceptualization of Personality, *Psych. Rev.*, 1973, vol. 80, no. 4.

SEARS, R. R. Dependency Motivation, in M. R. Jones, ed., *Nebraska Symposium on Motivation*, Lincoln: University of Nebraska Press, 1963.

VERNON, P. E. The Biosocial Nature of the Personality Trait, *Psych. Rev.*, vol. 40, 1933.