Inducing Change and Stability in Belief Systems and Personality Structures

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Because I needed to get as far away from Brooklyn as I could when I graduated from college, I chose to go to Berkeley rather than Iowa; I was thus not privileged to meet or work with Kurt Lewin. Nonetheless, I have been influenced by his writings throughout most of my career. My formulation about the open and closed mind (Rokeach, 1960), and the Dogmatism Scale I had constructed to measure general authoritarianism as an alternative to the measurement of Fascist authoritarianism (Adorno, Frenkel-Brunswik, Levinson, & Sanford, 1950), were heavily influenced by Lewin's emphasis on the ahistorical and contemporaneous, and by his concepts about the structure of the person and the life space—such concepts as cognitive structure, the central-peripheral dimension, time perspective, differentiation, and rigidity. Similarly, my work on the organization of values, attitudes, and belief systems was influenced by Lewin's structural way of thinking, even though I preferred the verbal to Lewin's topological style of conceptualization. Later, my work benefited from Kurt Lewin's emphasis on change experiments and on his insistence that in order to understand a phenomenon we must study the conditions under which it will undergo change (Marrow, 1969).

I was also deeply influenced by what he had to say about the importance of integrating psychological theory and research with social action. But in deciding what problems were worth my time and energy, I more or less consciously employed three criteria rather than Lewin's two: theoretical relevance, social relevance, and personal relevance. I also saw a difference between trying to be socially relevant and trying to be socially useful. It seemed far easier for me to decide in advance of whatever research I embarked upon whether it might be relevant to a deeper understanding of society and the self than whether it would be socially useful. So I was content to proceed on the assumption that the more a
work passes the test of being theoretically relevant and socially relevant and personally relevant, the greater the probability that it would also turn out to be socially useful.

The third criterion, that the results of research should contribute to a deeper understanding of the self—that is, personal relevance—played an especially crucial role in determining not only the direction of my current research interests but also in determining the kind of social action that I found most congenial. I felt uncomfortable about the idea that social change is the province of the social psychologist and sociologist, whereas personal change is the province of the psychotherapist. Such discomfort led me to carry out research on the cognitive and behavioral effects of telling people something extremely important about themselves; more specifically, the effects of telling people what their value priorities are, how their value priorities are shared or not shared by significant others, and thus needling them to confront the question as to whether value priorities and related attitudes and behaviors are compatible with their ideal conceptions of themselves. I have elsewhere identified this as a value-therapy approach to behavior modification (Rokeach & Regan, 1980). I suspected that feedback of such personally relevant information would not only deeply affect the person to become a more mature or integrated person, but would also affect his or her social action. Finding a way to influence a person to become a somewhat different person and finding a way to influence the person’s social action—for instance, to join or contribute money to a political, anti-racist, or anti-sexist organization—is, I believe, one form of action research. It is also research guided and informed by theory.

But I am getting a bit ahead of my story. So let me back up a bit to first explain the title of this paper—“Inducing Change and Stability in Belief Systems and Personality Structures”—and thus to state its purpose.

For the past decade and a half, my work (along with that of my collaborators) has been concerned with the problem of the conditions for inducing long-term changes in values and in value-related attitudes and behavior (Ball-Rokeach, Rokeach, & Grube, 1984; Rokeach, 1973, 1979). This work has progressed far enough now to lead me to see more clearly, on the one hand, some major shortcomings in the way personality and social psychologists have typically gone about defining their subject matter. On the other, it has encouraged me to go beyond earlier formulations about the conditions affecting change in the organization of belief systems to look as well into the conditions affecting the stability and integration of belief systems. Equally important, it encourages me now in this paper to take another step—a very cautious step—to consider the

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1One reason, in my opinion, many have perceived that contemporary social psychology is in a crisis is that it has emphasized too much in its experimental research the criterion of theoretical relevance and has downplayed too much the criteria of social and personal relevance.
possibility that our research procedures, and the theoretical approach guiding them, might have enduringly affected the personality structure of the person exposed to them. My purpose in this paper is thus, first, to discuss some major shortcomings that I see in personality and social psychology and, second, to explore the conditions under which it might be reasonable to argue that significant effects on values and on related attitudes and behaviors are manifestations of significant effects on personality structure.

Three Considerations

Three sets of considerations thus lead me to focus upon stability as well as change and, moreover, to proceed from stability and change in belief structures to stability and change in personality structures. The first concerns certain limitations that became evident in the work on the authoritarian personality which, notwithstanding its having provided us with a profound insight into the psychodynamics of the authoritarian personality, suggested little about what could be done about it—except possibly through the costly, time-consuming, and arduous route of psychoanalysis (Ackerman & Jahoda, 1950). Surely, I felt, we should be able to arrive at better suggestions about affecting the authoritarian's proclivity for bigotry and conservatism. The work on the authoritarian personality also left us in the dark about how to go about strengthening and integrating the anti-authoritarian, democratic personality structure.

The second consideration concerns limitations that I felt were inherent in the group dynamics approach in particular and in the experimental approach of contemporary social psychology in general: attitude change and behavioral change are typically induced by manipulations of group context or situational conditions. As Marrow (1969) puts it in describing the life work of Kurt Lewin: "... groups can be managed so as to bring about desired changes in the attitudes of their members" (p. 167). And again: "The group modifies the behavior of its individual members . . . to some more socially desirable ends" (p. 168). Lewin's (1947) notions about unfreezing and then moving to a higher level and then refreezing are really notions about inducing enduring behavior or attitude change by re-arranging environmental forces acting upon a person in a more or less permanent way by "freezing group life." The reason why I saw this as a limitation is because unfreezing and refreezing are more easily said than done, because the permanency of behavior or attitude change would depend upon the permanency of "freezing group life." Achieving such permanency is problematic because it requires political action by the citizenry as well as social action by social psychologists, and beyond such political action, social legislation that enforces social control. Put another way, attitude or behavioral change would persist only so long as the group or situational conditions continued to be present; they would decay or dissipate soon after the group or situational supports were removed. Or, as
Newcomb, Koenig, Flacks, and Warwick (1967) have shown, the changes would persist as long as the person selects to remain in a supportive group, social climate, or environment. But so conceived, the person continues to be the same person, that is, his or her personality structure will remain unchanged.

The real challenge thus shifts, it seems to me, from a theoretical concern with the conditions leading to demonstrable effects on a person's behavior and cognition—which typically decay soon after the situational conditions are removed—to the conditions leading to demonstrable effects that will persist within a person even after the situational conditions are removed, that is, because the personality structure has been affected. In addition to the question of unfreezing and refreezing attitudes and behavior by freezing group life, there is then the question of unfreezing and then refreezing value priorities, and thus possibly the question of unfreezing and then refreezing personality structure.

The third consideration stems from more general concerns about what all the sciences are about, what they all share in common. Whenever social scientists have occasion to interact with scientists from other disciplines, say, with those in the field of genetics, physics, chemistry, or biology, they will readily perceive, notwithstanding their different substantive interests, that they at least have a common commitment to the same method—the scientific method—a common interest that enables them to talk the same language with one another and to exchange at least methodological ideas.

But what we will often overlook is that scientists also have something else in common, something at least as important as our shared interest in scientific method—a common interest in structure, in the organization or architecture of whatever it is that is our subject matter. The geneticist, for example, specializes in trying to understand more deeply the structure of the gene or DNA, the nuclear physicist specializes in trying to understand more deeply the structure of the atom, and the astrophysicist the structure of the universe. The sociologist is interested in the structure of society and its institutions, and the psychologist in the structure of the person or the person's cognitive structure.

Two questions that I believe we all ask concern the stability and change of structure. Whether we are interested in understanding the universe or galaxy, the molecule or atom, the gene or DNA, we may note a common pursuit of answers to the same two fundamental questions: "How does this structure manage to maintain its sameness in a more or less stable state?" and "How and under what conditions will it undergo change?" In this common search for answers to these same two questions there is, I believe, a unity in all the sciences.

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2An appreciation of the primary importance of structure, at both the institutional and cognitive levels of analysis, has recently been displayed by the French social psychologist, Jean-Pierre Deconchy (1984).
The famous geneticist, G. B. S. Haldane, best articulated the twin preoccupation with stability and change when he wrote: "Genes exhibit a good deal of stability in their reproduction, otherwise heredity would be impossible. They do not exhibit complete stability, or evolution would be impossible." Haldane thus saw genetic material as having some sort of structure enabling it to stably reproduce itself, yet constructed in such a way that it also allows it to undergo change. Going beyond this particular example, the conceptual analogue of the notions of heredity and evolution can surely be found in the subject matter of every scientific discipline, from the astrophysicist’s universe and galaxy to the physicist’s atom, from the sociologist’s social system to the psychologist’s personality structure. All such subject matters focus upon some kind of structure begging to be more deeply understood, and they all are assumed to have properties enabling them to remain stable yet capable of undergoing change.

There is an intimate dialectical relation between the constancy and the capacity for change in any structure. They are really the opposite sides of the same coin. The reason why we will undertake to study the conditions that will facilitate constancy is that in doing so we will end up with a deeper understanding of how the particular object of our scrutiny is constructed, how it works. And the reason why we will undertake to study the conditions that will facilitate change is the same: to gain a deeper understanding of structure, and again, how it works. We carry out what Kurt Lewin called "change experiments," in the first place, because we cannot directly see the structure we are interested in through our microscopes or telescopes—it is too small, too far away, too complicated, or too abstract to be directly perceived. So we are prepared to do the next best thing, to make inferences about such structures from the changes we undertake to observe under carefully controlled experimental conditions. A concern with the conditions under which a structure will undergo change is thus seen to be a means to the end of understanding structure rather than an end in itself.

If one really understands how any given structure stably maintains itself, one should also understand how, when, and why it will undergo growth or change. Conversely, an understanding of structural change implies an understanding of structural stability. A comprehensive theory of structure—say, the double helix theory of the DNA structure—is a theory of stability yet also a theory of change. And with all living structures, to say that we understand a structure is also to say that we understand the functions of a structure, which implies that stability will be maintained so long as the structure functions, and will undergo change as soon as it malfunctions.

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3I first cited Haldane's aphorism in a somewhat different context in Rokeach (1960) and it has guided my thinking ever since. But regretfully, I have been unable to locate the reference to it.
Compartmentalization of Change and Stability in Social Science

With these general remarks about what I think any science is about, I would now like to address myself to the first purpose of this paper—to offer some observations about what I see to be some major shortcomings in the fields of personality and social psychology. The theoretical treatment of stability and change in these two areas of psychology seems rather compartmentalized. Experimental social psychologists in particular are typically change rather than stability oriented, and they proceed on the assumption that conditions leading to social change, group change, and cognitive and behavioral changes are more worthy of research than conditions leading to the maintenance of social and group stability, or cognitive and behavioral constancy. We tend too much, I believe, to associate scientific advance with scientific research on change. The field of experimental social psychology is especially concerned with the manipulation of variables and group conditions under which people will undergo change, but is usually silent about the conditions leading to stability, and it is also silent about structure. Our focus upon change is usually seen to be relevant to the testing of the validity of some "change theory" we are interested in rather than a means of making inferences about the structure being affected. Our theories of attitude change, for example, are not so much designed to provide us with a deeper understanding of the organization of attitude structures as to inform us about the validity of some change theory that would enable us to better construct messages, contrive situations to persuade, or to change what people normally say, believe, or do. But such changes are not typically seen to be changes in the person.4 In fact, such changes usually decay soon after the experimental conditions are removed (Cook & Flay, 1978).

In contrast, personality theorists typically have a lot to say about structure. But they talk about structures that are so stable they are downright rigid. This is because personality theorists tend to conceive of personality structures primarily in terms of traits, factors, or latent structures. So conceived, traits and related concepts are, in my opinion, inherently "reactionary" concepts, not because the empirical evidence for their cross-situational consistency is so meager (Mischel, 1968), but because it is conceptually difficult to envision situational or social conditions that could modify or alter them. Life-span research employing measures of traits show that "stability is a primary characteristic of adult personality while personality change is negligible" (Moss & Sussman, 1980, p. 581). There is virtually no experimental literature on the modification of traits. About the

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4Recent social psychological interest in schema theories are also overly change oriented and insufficiently stability oriented. "As a consequence of the fact that schemas are responsive to experience, they are revised continually and clearly cannot be static" (Fiske & Linville, 1980, p. 553).
only thing we can do with them is to analyze them statistically: intercorrelate them, factor analyze them, or measure how much of the total variance they might account for. But, notwithstanding the aims of clinical psychology, we do not attempt to change traits because they are thought to be inherently unchangeable. Even psychoanalysts do not attempt or claim to change traits.

Division 8 of the American Psychological Association, the Division of Personality and Social Psychology, has recently split into two sections, one on Personality and the other on Social Psychology. Members of Division 8 are given the option of affiliating themselves with one or the other, or both, and the two sections are governed separately. Such a split serves only to institutionalize the compartmentalization and isolation of change-oriented social psychologists who typically focus upon the effects of situations upon changing attitudes and behavior, from stability-oriented personality psychologists, who typically focus upon showing cross-situational stability of traits. And this is further reinforced by the main journal of Division 8, the *Journal of Personality and Social Psychology*. This journal is presently organized into several sections, social and personality sections that are, respectively, change and stability oriented, having different editors who make their day-to-day decisions more or less independently of one another. It is a division not only structurally and conceptually divided from other divisions but also within and against itself. It might not be altogether inaccurate to think of it as the Division *between* Personality and Social Psychology, and for this reason I must confess to having difficulty in identifying myself with either section, even though I am a member of both.

I believe it would not be out of place in this discussion to offer some brief observations about a somewhat parallel compartmentalization between stability and change apparent within our sister discipline of sociology. I am thinking mainly here about structural-functionalism on the one hand and more radical theories in sociology on the other. Notwithstanding the fact that structural-functionalism can account for change, as Merton has shown (1949), it in fact de-emphasizes change and over-emphasizes integration, stability, social order, and consensus. As Van den Berghe (1969) writes: "... functionalism regards consensus as a major focus of stability and integration, and the dialectic views conflict as the source of disintegration and revolutionary change." (pp. 209–210). And as Dahrendorf (1969) puts it, it is "erroneous to assume that a description of how the elements of structure are put together into a stable whole offers, as such, a point of departure for a structural analysis of conflict and change" (p. 216). There is, moreover, little room in functionalism for the idea that socialization and value consensus can also foster independence, creativity,

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5I would thus like to see Division 8 restructured under some such new name as the Society for the Psychosocial Study of Stability and Change (SPSSC).
dissent, disobedience, and rebellion against the social order (Wrong, 1961). It is a theory that seems mainly focused upon but one of the two main questions that can be asked about social systems: “How is social order maintained?” I thus see at least a rough parallel between structural-functionalism in sociology and trait theories in psychology.

The second question: “How is social order maintained, changed, or destroyed?” seems, in contrast, to be a main concern of radical theorists like Gouldner (1970), Mills (1956), and many others (Flacks & Turkel, 1978). Here, too, I see a rough parallel between the change-oriented experimental social psychologists and the change-oriented radical theorists.

I would now like to return to Haldane’s aphorism about how genes are constructed in order to modify this aphorism so that it will apply more directly to sociology and psychology, and thus provide us with a yardstick against which we might be able to assess the comprehensiveness of our theories of structure. First, to modify it for sociology: “Societies exhibit a good deal of stability in their social structure, otherwise social order would be impossible. They do not exhibit complete stability, or social evolution and revolution would be impossible.” As sociologist Dahrendorf (1969) puts it: “Stability and change, integration and conflict, function and ‘dysfunction,’ consensus and constraint are . . . two equally valid aspects of every imaginable society” (p. 217). A similar view is expressed by Van den Berghe (1969): “Value consensus constitutes the most basic focus of social integration, but it is also true that societies . . . often exhibit considerable dissension about basic values. . . . Consensus, then, is a major dimension of social reality, but so are dissension and conflict” (p. 203).

A parallel perspective on stability and change will also be found in the interactionist approach to group dynamics taken by Moscovici and his co-workers (Moscovici & Mugny, 1983; Mugny, 1984) on majority and minority influences: “… the function of social influence is also to guarantee social change . . . not just to assure [its] uniformity, stability, and conformity” (Moscovici & Mugny, 1983, p. 43). More specifically, I believe Moscovici and his colleagues are saying that whereas the function of social influence by the majority is social stability, the function of social influence by the minority is social change.

Analogously, Haldane’s aphorism can be applied to individual psychology. “Humans exhibit a good deal of stability in their daily lives, otherwise continuity of personality would be impossible. They do not exhibit complete stability, or development and change in personality would be impossible.”

**Change and Stability in Personality Structures**

If trait theories are lattice works through which we are able to see only the stabilities in personality structures, how are we to proceed if we are to also see
the changes that personality structures, like any other structure, must surely undergo? One way we might proceed is to replace trait conceptions of personality structures with conceptions about enduring belief systems or cognitive structures (Feather, 1971). More specifically, theories about the structure of belief systems, especially if they incorporate conceptions about the organization of beliefs, attitudes, and values concerning self and others, are lattice works through which we are allowed to better see not only the stabilities but also the changes that personality structures might be undergoing. Figure 1 shows the correlation between the terminal and instrumental value priorities of a national sample, on the one hand, of Americans in their twenties and, on the other, of those in their forties, fifties, sixties, seventies, and beyond. Even though these are cross-sectional rather than longitudinal data, they strongly indicate that while value priorities are relatively stable over the whole life span they are nonetheless undergoing gradual change among Americans proceeding from young adulthood to old age. The value priorities of older Americans become increasingly dissimilar from those of Americans in their twenties. Such data thus confirm

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6 These data are calculated from data reported in Rokeach, 1973, pp. 76–77.
7 I am puzzled about and cannot explain the consistent small increase in correlations between respondents in their 20s and respondents in their 40s, for both terminal and instrumental values.
Feather and Newton’s (1982) assumption that “values are relatively stable but not unchanging across the life-span” (p. 220). A more stable and unchanging pattern of results is obtained when traits are plotted across the life span (Costa & McCrae, 1980), thus giving us a misleading picture of stability over the total adult life span. A similar point is made by Ryff (1982) when she points out that more stability is found when objective measures (of traits) are employed than when clinical methods are employed as, for example, in the research by Levinson, Darrow, Klein, Levinson, and McKee (1978).

I thus agree with Moss and Sussman (1980) that while stylistic, expressive, or temperamental traits such as extraversion—intraversion and energy level might indeed show stability over the life span, this does not necessarily mean that personality structure remains stable over the life span. They also draw our attention to the fact that findings about change and stability depend on the method employed: “. . . studies using an ipsative strategy are effective in demonstrating stability and change” (p. 591). My colleagues and I, in our own work, have always used an ipsative strategy because the very notion of stability and change in value priority forces us necessarily into an ipsative strategy of measurement—that is, ranking two or more values for relative importance—and it is thus sensitive to the presence of both stability and change across the total life span. In contrast, rating methods are typically used when assessing traits, which means each trait is measured in isolation, independent of every other trait. It is thus easier to demonstrate stability than change across the total life span.8 Ispative measurement, notwithstanding that statisticians and psychometricians frown upon it, is, I believe, often more sensitive than ipsative-free, independent measurements because life is ipsative, because decisions in everyday life are inherently and phenomenologically ipsative decisions. Moreover, ipsative measurement is more compatible with a conception of personality as a structure composed of interrelated parts. This suggests that trait theorists might be better off concentrating on the perceived relative importance of one’s own or other’s traits with respect to one another rather than the absolute amount of each trait possessed. If they do so, it becomes somewhat equivalent to measuring perceived instrumental value priorities. And over and above such methodological considerations there is evidence suggesting that persons who describe themselves as having certain traits also perceive themselves as having certain value priorities. Put another way, saying that persons have such-and-such traits often can be readily translated into an hypothesis predicting that they will also exhibit certain values. Feather (1984) makes this point better than anyone else:

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8A similar point is made by Kagan (1980) when he describes the following two kinds of stability and continuity: First: “The persistence of a psychological quality as reflected in minimal rate of change over time;” and second: “The persistence of a hierarchical relation between complementary dispositions within an individual (ipsative stability).” (pp. 31–32). The second kind of stability (ipsative stability) will show less persistence than the first kind, thus making it more possible to show personality change.
In the present study, for example, those subjects who described themselves as loving and helpful were more likely to report that these were relatively important values within their own value systems. Thus the items that are involved in commonly used inventories such as the BSRI, the PAQ, and the EPAQ may be seen as tapping not only descriptive beliefs about a person's characteristics ways of behaving but also prescriptive beliefs about preferred modes of conduct. (p. 616)

Thus, shifting from traits to values allows us to see the stability effects of personality across situations yet at the same time, the change effects of situations that activate different behaviors, depending upon different expectancies of consequences of behavior in different situations (Feather, 1982).

There is, moreover, a close connection between stability and change in value priorities, and stability and change in personality. Carol Ryff has shown (Ryff, 1982; Ryff & Baltes, 1976) that there is a self-perceived transition among adult women from a preference for instrumental values to a preference for terminal values as they proceed from middle to old age. She calls this the "instrumentality–terminality sequence hypothesis" and interprets her findings as representing a "self-perceived personality change." Similarly, Feather and Newton (1982) observe that values are "central aspects of personality and are closely bound up with one's sense of self" (p. 220). And again, the close connection between values and personality is drawn in Allport, Vernon, and Lindzey's *A Study of Values* (1970), which they consider to measure the dominant interests in personality.

So if we find evidence of persisting stability yet also of gradual change in value priorities over the adult life span or in the transition from middle to old age, are we not also finding developmental evidence for change as well as stability in personality structures?

Veroff (1983), in an interesting paper on "contextual determinants of personality," also finds personality change across the life span in national survey data. But he gathers his data not from the measurement of traits or values but from the measurement of motives. He, however, equates contextual changes in motives with contextual changes in values and thus with contextual changes in personality: "Characteristics related to values, such as motives, are more susceptible to changing cultural prescriptions and hence show less stability over time. The contextual influences on personality . . . most clearly apply to personality characteristics directly relevant to values. . . " (p. 339). And such a line of reasoning leads Veroff to conclude: " . . . with the contextual viewpoint we have been able to cover many diverse deficiencies in a classical model of person-

Similar views are expressed by Rokeach (1973) who sees values as cognitive representations and transformations of needs, by French and Kahn (1962) who see both needs and values as sharing the conceptual property of motivating goal directed behavior, by Feather and Newton (1982) who see values as a particular class of motives, those motives that have a normative or oughtness quality about them, and by Feather (1975) who sees " . . . no reason why values should not be treated as basic personality characteristics similar and perhaps identical to motives" (p. 300).
ality that assumes both consistency and stability . . . our belief in the constancy of personality has to be reconsidered” (p. 341).

**Inducing Change and Stability**

The discussion thus far has focused upon naturally occurring stabilities and changes; they are observed to be functions of aging or functions of stability and change in the context of the social environment (Dannefer, 1984; Veroff, 1983). I come now to the second purpose of this paper—to consider the question as to whether it is now also possible to conceive of experimentally inducing personality change and stability. In doing so, I am advocating an experimental approach that is also a person-centered approach.

I believe that personality structure can be said to be experimentally affected if the following five stringent criteria are met: (1) if that which has been affected can be conceptualized to be some central or deep-lying feature or disposition of personality structure; (2) if the disposition that has been affected can be shown to affect, in turn, other, less-central yet related dispositions; (3) if there are demonstrable behavioral consequences conceptually related to the affected central and less-central dispositions; (4) if the behavioral consequences matter to self and others; and (5) if each of the affected central and less-central dispositions and their behavioral consequences persist over time.

On all five counts, the research program on the effects of telling people about their own value priorities my colleagues and I are involved in can be considered as dealing with effects on personality structure. As I have tried to show, value priorities are indeed central components of personality; we find in our research that effects on value priorities persist for a long time and lead to effects on related social attitudes—effects on central and less-central dispositions persisting, at least as long as 15 to 17 months afterward; and such value and attitude effects are found to have important behavioral consequences, important to self and others, at least as long as 21 months afterward.10

Closely related, we have found that significant changes in value priorities and related social attitudes are induced regardless of individual differences in many personality traits and, even, regardless of individual differences in authoritarian personality structure, whether measured by the California F Scale or the Dogmatism Scale (Rokeach, 1973, pp. 300 and 307). Such findings have implications that remain largely unrecognized and suggest changes in personality structure, even among those having authoritarian personality structures.

If it is possible to induce an increasing instability and thus change in a structure, it should also be possible to induce an increasing stability in structure.

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10 The most updated review of the experimental findings will be found in Ball-Rokeach, Rokeach, and Grube (1984).
Belief Systems and Personality Structures

What sort of data can be taken as evidence for inducing stability rather than change in belief structures? In our research not all persons assigned to experimental groups were found to undergo change in value priorities. In those experiments wherein, for example, egalitarian values were the target values focused upon, some respondents were already high in these values (on the pretest). There is no theoretical reason why those persons should undergo change in their value priorities. Nonetheless, we do expect the experimental treatment to affect such persons in a very important way. One manifestation of an increasing stability or integration within such persons’ belief systems is that their social action becomes more consistent with their value priorities as a result of the experimental treatment, more consistent with their value priorities than is the case with other persons matched in values but not exposed to the experimental treatment, that is, not exposed to information about their own and others’ value priorities. We have therefore ascertained not only whether the experimental treatment affected the behaviors of those initially low on the target values, but also whether it induced persons already high in such target values to act in a manner more congruent with their pre-existing value priorities. The full details of such analyses are presented in Ball-Rokeach, Rokeach, and Grube (1984; see especially the data presented on pages 62 and 126). These data provide us, I believe, with reasonably strong evidence suggesting not only that persons initially low in target values change their behavior as a result of the experimental treatment, but so also are persons already high in such target values affected; they behave in a manner more consistent with their pre-existing values, thus suggesting that stability or integration of personality has been experimentally induced.

I must admit now to a considerable amount of ambivalence about accepting my own argument that inducing persisting value, attitude, and behavioral effects is a manifestation of induced stability or change in personality structure. On the one hand, such a view is exciting because it goes considerably beyond traditional views and claims of contemporary personality and social psychology, namely, that it is not possible to enduringly affect deep-lying and socially important values, attitudes, and behaviors by any single experimental intervention. On the other, it is an admittedly extreme view that, if accepted, would force us to accept even more extremist interpretations and claims. Consider the following.

1. Our most recently completed work concerns our use of television to affect social cognition and behavior (Ball-Rokeach et al., 1984). This work, published ironically enough in 1984, reports how we employed television to affect the value priorities, and related social attitudes and actions, of large numbers of adult viewers watching our program voluntarily in the privacy of their homes. We had produced a 30-minute television program, co-hosted by Ed Asner and Sandy Hill, entitled “The Great American Values Test,” and aired it on all three commercial channels simultaneously as a public service program in the Tri-cities area of eastern Washington. Two to three months after this program
was shown we found significant effects on socially important actions, on monetary contributions to anti-racist, anti-sexist, and pro-environmental organizations, on deeply held egalitarian and environmental values, and related social attitudes.

In line with the considerations developed in this paper, such findings now lead me to raise the question: Is it thus possible to induce persisting effects on value priorities and related attitudes and behaviors, and thus on personality structure, not only in the laboratory, but also by showing certain kinds of television programs to people in their natural environment?

2. A few years ago I reported on long-term changes in value priorities induced when a person interacts with a computer (Rokeach, 1975). Students filled out my Value Survey (Rokeach, 1967) and then obtained comparative information from a computer about the value priorities of racists and anti-racists, the educated and uneducated, the young and old, and men and women. They were thus able to compare their own value priorities with those obtained previously by these positive and negative reference groups. A control group also filled out the Value Survey but was not provided with such comparative information. The experimental group, but not the control group, showed significant changes in their value priorities two months afterward. It is thus again possible to ask: do such changes in value priorities therefore suggest that changes in personality structure may be induced by having a person interact in a certain way with a computer?

We have already suggested (Rokeach, 1973, 1979; Ball-Rokeach et al., 1984) that all such persisting effects on social cognition and behavior, whether obtained in the laboratory, by television viewed in the home, or by the computer, are induced by the method of self-confrontation. Most people do not know, and are unable to articulate, their value priorities. But when they find out with our assistance what their value priorities are by the method of self-confrontation, the information they thus obtain in private largely short-circuits ego-defensive rationalizations typically brought about by self-presentation to significant others. They are thus in a better position to ponder and to discover for themselves whether their value priorities, and the social attitudes and behaviors they perceive to follow from their value priorities, are compatible with their attempts to maintain and to enhance conceptions they have of themselves as competent and moral persons. Obtaining such indisputably important information about themselves leads them to experience satisfaction or dissatisfaction with self. To the extent that self-satisfaction is experienced, the self is affirmed and the stability and integration of belief systems and behavior is increased. And to the extent that self-dissatisfaction is experienced, a process of change in belief systems and behavior is activated.

Put another way, the theoretical approach we are advocating is a theory of self-education (Ball-Rokeach et al., 1984), or a theory about what it is that we
social psychologists could possibly tell a person that would conceivably make an 
enduring difference in stabilizing or changing what he or she might persistently 
believe, say, and do. Such a theory of self-education implies that what is crucial 
is that the information is personally relevant. Also implied is that it makes no 
difference whether such information is conveyed face-to-face, by television, or 
by computer. The method of self-confrontation can be employed to provide 
personally relevant information by all such channels.

Providing personally relevant information by the method of self-confronta-
tion also suggests a way to teach social psychology that could conceivably affect 
the personality structure of our students. We are ordinarily willing to tell our 
students more than they ever wanted to know about what we have found out from 
our research investigations. We are willing to tell them about what the re-
spondents in our research investigations believe, say, or do, but we are extremely 
reluctant to let them find out, or to help them discover for themselves, what they 
_themselves_ believe, say, or do. For instance, we are willing to tell them about the 
authoritarianism, the prejudices, the stereotypes, the attributions, the values, the 
altruism, the conformity, or the internal–external control of the participants in 
our studies, but not about their own. Have we unreflectively internalized the 
norms of clinical psychology that forbids revealing the results of one’s responses 
to the Rorschach, the intelligence test, or the MMPI to include the personality 
and social psychologist’s arsenal of tests about racism, sexism, authoritarianism, 
and the like? We have forbidden ourselves to tell our students where they stand 
on such variables and, thus we have thrown away our best weapon—making the 
subject matter personally relevant—a weapon that physicists and astronomers do 
not possess when teaching their subject matters.

Lundy and Rokeach (1979) have reported the results of a study wherein an 
experimental group of students in social psychology were exposed to a given set 
of lectures, reading materials, and examinations. and were also given the oppor-
tunity at the beginning of the semester to fill out various tests covered subse-
quently during the semester—the F Scale, the Dogmatism Scale, the Value 
Survey, the Edwards Personal Preference Schedule, and the Sixteen Personality 
Factor Test. These tests were returned several days later, scored, and the scales 
were identified and discussed in order to give students comparative information 
about their own standing on these tests, compared to norms obtained previously 
for other students. All such feedback was given by another instructor (not the 
regular one), and the students were assured that records of individual scores were 
known only by the individual student. Control students took the identical course 
in social psychology but were not given the opportunity to take these tests—or 
they took the tests, which were returned to them unscored and unexplained. 
Compared with control groups, the experimental group ‘‘(1) reported the . . . 
course to be more personally relevant than students not so exposed; (2) asked 
more questions during class discussion; (3) achieved higher scores on their final
examination; and (4) achieved higher scores on an examination six months afterward. The difference in academic performance between the experimental and control group was roughly one letter grade . . . ’’ (p. 285).

The preceding summarizes a research study showing significant effects on academic performance following feedback of information that is personally relevant. It did not attempt to ascertain whether such an improvement in academic performance was accompanied by changes in value priorities or by changes on the other personality tests taken. This is a problem worth investigating in the future. But I will allow myself to conjecture—in the light of findings from many other experimental studies and the line of argument developed in this paper—that such a personally relevant method of teaching social psychology might also have affected personality structure.

Extending all the preceding, we thus come to envision the possibility, in this day and age of microcomputers, of a “Socrates machine.” A Socrates machine would be a machine that invites you to ask it such questions as: “Compared to other people, what are my value priorities? And to what degree am I: a racist or anti-racist? a sexist or anti-sexist? a liberal or conservative? closed- or open-minded? authoritarian or anti-authoritarian? internally or externally controlled?” and the like. It would also be programmed to allow you to find out by inviting you to take various tests, instruct you on how to score them, provide you with norms, offer you some interpretations, and caution you about limitations (Green, 1983; Matarazzo, 1983). Such a “know thyself” machine is nothing more than an extension of previously published work I have discussed in this paper concerning the induction of effects by using television and the computer and of teaching social psychology in a personally relevant way. And again, in line with the arguments developed in this paper, could we say that such a machine might be able to induce stability and change effects—if you will, genuine enlightenment effects (Gergen, 1973)—on value priorities, related personality variables, related attitudes, related behaviors, and thus on personality structure? And could we say that the development of such a Socrates machine would also qualify as another potential form of social action?

All this, about the possibility of persistently affecting personality structures, may be scientifically exciting, but it is not without its social dangers. I end this discussion on an uneasy Orwellian note intended to remind us that our theories and techniques have a potential for destructive as well as constructive use, a potential we must continue to be on guard against, as we are about to say “goodbye” to 1984 and “hello” to 1985. The full flavor of our apprehension is conveyed in the closing chapter and in the closing paragraph of our 1984 book on how we had affected the political values and behaviors of television viewers (Ball-Rokeach et al., 1984).
Concluding Comments

Many problems surely remain to be investigated. But I will content myself here to mention only two that I consider to be among the most challenging. First, while we have found that people high in authoritarianism are just as likely as those low in authoritarianism to exhibit long-term effects on their value priorities and thus on their personality structures, it has not yet been ascertained whether they also undergo changes in authoritarianism, as it might be directly measured by the F Scale or Dogmatism Scale, however insensitive such trait-like measures might be for detecting change. Second, I do not really believe that the only way to induce persisting stability or change in cognitions and behavior is by inducing changes in personality structure. Surely the persistence of stability and change is also induced by various kinds of social support and social pressure present in primary groups, social institutions, and society. In short, I see the persistence of stability and change in belief systems and behavior to be joint and interacting manifestations, on the one hand, of psychologically originating processes of satisfaction and dissatisfaction with self and personality structure, and on the other, of group-based processes of social control present within the many contexts of social structure. My proposals about unfreezing, refreezing, and stabilizing belief systems and even personality structures thus hopefully supplement Kurt Lewin’s optimistic view that it may be possible to induce persisting socially desirable effects by applying principles of group structure and dynamics.

References


