Comment

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Frequency Encoding Is Not Inevitable and Is Not Automatic: A Reply to Hasher and Zacks

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Hasher and Zacks (December 1984) recently reviewed research suggesting, to them, that frequency of occurrence ("fundamental" information) is automatically processed. Hasher and Zacks did provide six defining criteria for determining whether a process is automatic or nonautomatic. To be met, those defining criteria demand a pattern of null results. I question whether the pattern of null results required by Hasher and Zacks for defining automatic frequency encoding is supported throughout the literature.

Hasher and Zacks (1984) reviewed their basic considerations of automaticity and stated that "these considerations have led to the development of six criteria, *all of which must be jointly satisfied,* for us to conclude that an aspect or attribute of experience is automatically encoded" (p. 1373, emphasis added). Two of their required criteria are: (a) people are sensitive to this (automatically encoded) information without intending to be (Hasher and Zacks include the effects of instruction and strategy under this criteria, p. 1374); (b) disruptions due to arousal, stress, or additional processing demands will have no impact on the processing of such (automatically encoded) information. Presumably, if these two criteria are not satisfied or do not show the required pattern of null results, we should question Hasher and Zacks's position concerning the automatic inevitable encoding of frequency information.

It appears that Hasher and Zacks should modify their position. I will review data showing that intention (or instruction) has a clear effect on subjects' ability to estimate frequency of occurrence and that situations that withdraw attentional resources from the frequency estimation task can have a severe impact on subjects' subsequent frequency estimation ability.

Instructional Effects

Hasher and Zacks implied that instructions have no influence on tests of memory for frequency of occurrence. This is clearly not substantiated by existing data. Fisk and Schneider (1984) presented data showing that instructions such as those provided by orthographic orienting tasks (i.e., subjects searched for and detected words containing the letter *g*) have a profound effect on frequency estimation. In addition, Fisk and Schneider (1984) showed that subjects could not estimate the frequency of word presentation when they performed a digit detection task while simultaneously "looking" at the words. For subjects to perform the digit detection task, the words they were "looking" at had to remain in foveal vision. Data presented by Rose and Rowe (1976) also demonstrated that instructions that orient subjects to emphasize processing the shallow orthographic features of words will disrupt frequency estimation during incidental learning conditions. In both the Fisk and Schneider (1984) and Rose and Rowe (1976) experiments, semantic orienting conditions resulted in relatively good frequency estimation, as did the intentional learning conditions.

Other evidence of the sensitivity of frequency estimation to instruction and subject strategy can be found in the literature. As Hasher and Zacks pointed out in a footnote (p. 1380), strategies or instructions that increase covert rehearsals will affect frequency estimation (e.g., see Postman & Kruesi, 1977). Begg (1974) provided data showing that task instructions (as well as the concreteness versus abstractness of nouns) interact with frequency estimation. In Begg's experiment, subjects who actively made frequency estimations each time a word occurred were better at a delayed frequency test than subjects who did not attempt to count each word's occurrence. Howell (1973) reviewed data suggesting that frequency estimation is a complex interaction between speed of stimulus presentation, massed versus distributed presentation, and type of stimulus (see Howell, 1973, pp. 46, 47, & 49 for the review of the relevant data).

It seems odd that Hasher and Zacks did not address this body of literature (Begg, 1974; Fisk & Schneider, 1984; Howell, 1973; Postman & Kruesi, 1977; Rose & Rowe, 1976) when suggesting that instructions or strategies do not influence frequency estimation (see Hasher & Zacks, 1984, p. 1374). Subjects' "intentions" or strategies can affect their ability to estimate frequency of word occurrences.

How is it possible to account for the data presented by Hasher and Zacks (1979)? Hasher and Zacks used a relatively weak manipulation of intention or instructions. For example, Hasher and Zacks's (i.e., 1979, p. 371) instructions to their "instructed" versus "uninstructed" subjects were not much different. Instructed subjects were told about the upcoming frequency estimation test prior to participating in the learning task whereas the uninstructed subjects were not explicitly told about that test. However, *both* groups of subjects were told, "After you see the list, your memory will be tested." Those instructions do not easily qualify either group as an incidental learning group. All of their experimental groups, in that study, appear to be encoding the stimuli under intentional learning conditions.

The picture coming from the research cited above does not provide the joint satisfaction of the six criteria called for by Hasher and Zacks (1984, p. 1379) as being necessary for defining automaticity of frequency encoding. Hasher and

Zacks have presented interesting data suggesting that frequency encoding can be controlled by variables somewhat different from those of other stimulus attributes; however, they have not demonstrated—by their own criteria—the automaticity of frequency encoding.

Substantial Disruption From Reduction in Capacity

Hasher and Zacks argued that automatic processes should not be disrupted by reduced capacity. Indeed, there is substantial evidence that automatic processes are resource (capacity) insensitive (e.g., see Fisk & Schneider, 1983, 1984; Schneider & Fisk, 1982, 1984). By the "joint satisfaction" rule of Hasher and Zacks, frequency estimation cannot be automatic if reduction in capacity disrupts frequency of occurrence estimations. Hasher and Zacks stated that "reductions in capacity over the ranges so far explored do not affect performance on frequency tests" (p. 1378). Available data clearly contradict this statement.

Fisk and Schneider (1984) carried out an experiment requiring subjects to perform a digit detection visual search task while simultaneously detecting words from a semantic category (e.g., types of vehicles). After substantial practice, untrained exemplars of the trained category were introduced as well as new distractors from other categories. The distractors were presented either 1, 5, 10, or 20 times. In a dual task, subjects were able to detect the untrained exemplars from the trained category with a high degree of accuracy without disrupting the primary digit search task performance. (Those results indicate that subjects were processing the words at least up to the semantic category level in an automatic mode.) However, subjects' estimated frequency of occurrence of the test distractors was independent of the actual presentation frequency. Frequency estimation was relatively good when the subjects' resources were not allocated to the digit search task (Experiment 2 vs. Experiment 1 semantic orienting condition). These data demonstrate that withdrawal of resources from the frequency estimation task disrupts the ability to judge frequency of occurrence. These data do not fit the pattern of results that Hasher and Zacks would need to argue for automaticity of frequency encoding.

Need for Substantial Methodological Care

Fisk and Schneider (1984) have clearly illustrated the requirement for substantial methodological care in attempting to assess relatively pure automatic and nonautomatic (controlled) processes. In light of the publication by Hasher and Zacks, it appears important to reiterate Fisk and Schneider's (1984) requirements. To assess relatively pure automatic processes, researchers must (a) provide evidence as to how well the stimuli actually are processed; (b) provide evidence of the sensitivity of the memory test; (c) provide an excellent cover story; (d) require subjects to perform a highly demanding controlled processing (attention-demanding) task as the primary task; (e) test for frequency estimation only after subjects have learned to allocate attention to the nonautomatic task; and (f) design the task to control for "drift" of attentional resources away from the controlled processing task to the automatic task. The latter three requirements may be met if (a) automatic processes are well developed (e.g., over 2,000 trials of successful executions of the automatized task); (b) subjects are trained to devote full processing capacity to the controlled processing (primary) task; (c) buffer words are presented after automatically processed targets; (d) buffer words are presented at the beginning of each trial to allow time to refocus attention; and (e) highly emotional words (such as rape or murder) are not used.

Summary of Automaticity of Frequency Encoding

Hasher and Zacks (1984) argued that the pattern of null results they reported is critical because their "definition of automatic frequency encoding hinges on the joint satisfaction of six criteria" (p. 1379). (Their criteria predict a pattern of null results.) This comment has pointed to data that indicate that two of their criteria are not supported in the literature. The references cited in this brief comment indicate clear patterns of instruction or strategy effects in the estimation of frequency. Data also clearly show the disruption of frequency encoding when resources are withdrawn from the frequency estimation task. Contrary to the assertion of Hasher and Zacks, it does not appear that frequency information is inevitably encoded into memory.

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Inevitability and Automaticity: A Response to Fisk

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In the contemporary cognitive literature, the term *automatic* has a number of definitions (e.g., Shiffrin, in press). For the most part, these definitions are not contradictory but complementary to one another. That is, there is significant commonality among them, with the differences arising mainly from the association of the various definitions with research in different cognitive domains. However, mismatches of concerns can occur when different views of automaticity are juxtaposed. Fisk's comment (this issue, pp. 215–216) presents such a mismatch on the topic of methodological problems in the study of automaticity.

As we have used the term automaticity, it refers to a process by which some attributes of an *attended to* stimulus are encoded into memory. We have studied this process mainly through the use of list memory procedures (Hasher & Zacks, 1984, p. 1373). By contrast, the context of Fisk's comment is a view of automaticity derived from the study of automatic search mechanisms, particularly as they slowly develop in multiple frame visual search tasks (e.g., Fisk & Schneider, 1984). Because of the different foci of the two positions, and especially because of the associated difference in research paradigms, several of the methodological problems that Fisk addresses either do not fit our concerns or are irrelevant to them. The latter is most dramatically illustrated by requirement (a) on page 216: that there be something "over 2,000 trials of successful executions" of a task before it can be assumed that automaticity has been established. In our view, frequency information is encoded into memory (assuming attention to stimuli) on all exposures, including the 1st and the 2,000th.

It is important to note here that we have specified one boundary condition for the obligatory encoding of such fundamental attributes as frequency of occurrence: that the stimuli, although not necessarily the attribute, be attended to (Hasher & Zacks, 1979, p. 359). It may be useful to elaborate on what we mean by the phrase "attended to." We interpret this phrase in a manner consistent with late selection views of attention such as that of Duncan (1980). He argued that stimuli are fully analyzed preattentively, including extraction of their form and meaning. The limited capacity attentional system comes into play to determine which products of preattentive processing will be attended to and thereby brought into consciousness. That is, evidence of some degree of processing of stimuli, even of semantic processing, is not sufficient to demonstrate that the stimuli have been attended to in a manner that meets our boundary condition.

These considerations form the basis for our response to Fisk's claim that reduction in capacity does (contrary to our view) disrupt encoding of frequency information. The data to support this claim come entirely from a study by Fisk and Schneider (1984). In that study it was demonstrated that, given extensive practice, subjects are able to automatically perform even semantically based category searches on words; that is, such searches can be performed on words that are not consciously attended to. If so, Fisk and Schneider's finding that subjects have no memory for the frequency of occurrence of distractors in this paradigm is not contrary to our position: According to our explicit boundary condition, stimuli that are processed in a nonattended way are not expected to leave a record in memory that supports reliable judgments of frequency of occurrence. In fact, Fisk and Schneider (1984, p. 189) explicitly acknowledged this boundary condition in discussing the relevance of their data to our view of frequency encoding. Thus, it is somewhat surprising that Fisk included this line of argument in his commentary.

We turn now to Fisk's remaining criticism, concerning the impact of instructions on the encoding of frequency of occurrence information. Our instructional criterion states that warning subjects about a forthcoming attributes test will not improve their ability to encode fundamental attributes. This is so because of the presumption that automatic encoding processes function optimally and continuously. Before addressing the issue of whether the data agree with this criterion, we need to clarify a distinction between test instructions and cover task instructions that is honored in the memory literature but is blurred over in Fisk's commentary. There is a difference between instructions about whether to expect a forthcoming memory test (and if so, of what specific type) and instructions about how to process each item as it appears (typically called "orienting" or "cover" tasks). This clarification is necessary to show that the existing data (a) largely conform to our instructional criterion or (b) can be explained by an assumption about subjects' covert rehearsal processes as they proceed through a list of items. We turn first to the impact of test instructions.

Intentional test instructions inform subjects about the nature of the target information that will be tested (e.g., the words themselves, the frequency with which each occurs, their temporal duration or order). Intentional instructions range in the degree to which they go on to specify the actual nature of the forthcoming test from ones that are detailed (e.g., four-alternative, forced-choice item recognition frequency estimation or discrimination, position judgments) to ones that are rather vague, as when subjects are simply told of a "test" without any further information. For our purposes of assessing the impact of instructions on the encoding of fundamental information into memory, intentionally instructed subjects must know that their knowledge of a particular attribute (e.g., frequency) is what is going to be tested.

Incidental instructions are of two types. The first warns subjects only of some unspecified type of test, without any specific information about the target information to be tested. For example, subjects might expect a memory test without knowing that frequency memory will be tested. In the second type of incidental instructions, subjects are totally uninformed about a memory test. In this circumstance (sometimes referred to as "truly" incidental) subjects are typically given some task that "orients" them to the items to ensure that the items are actually attended to and that subjects do not guess at the existence of a memory test.

In conformity with the instructional criterion of our framework, encoding of frequency of occurrence information occurs with both intentional and incidental instructions (see Hasher & Zacks, 1984, pp. 1373–1375). Furthermore, recent research of ours shows that encoding of frequency under a number of truly incidental conditions with compelling cover tasks (e.g., a Stroop task, a sentence completion task) is as good as that under incidental and/or intentional instructional conditions (Zacks, Doren, Hamm, Hasher, & Hock, 1985). Two recent articles have also addressed this issue, but they have yielded contradictory conclusions that make their impact unclear. On the one hand, Greene (1984) found that a truly incidental cover task yielded poorer frequency knowledge than the same cover task combined with either vague or explicit memory test instructions. On the other hand, using procedures very similar to Greene's, Kausler, Lichty, and Hakami (1984) obtained a pattern of frequency knowledge in keeping with the automaticity criterion of no instructional differences. Our current conclusion is that, in the main, the results on this variable confirm the automaticity view.

We turn now to the second type of instructional manipulation, which involves varying the type of orienting or cover tasks given to subjects. These instructions are sometimes, though not always, combined with the various types of intentional and/or incidental test instructions, a fact that no doubt contributes to the blurring of the distinction between what are actually two very different sorts of instructional manipulations. A variety of orienting tasks have been used. As examples, subjects may be asked to rate each item as it appears for pleasantness or to indicate the number of syllables each has. Such tasks ensure that subjects pay attention to each stimulus item, but they may also (depending on the particular tasks chosen) result in different amounts of covert rehearsals of the items in the list. For example, subjects who are rating items for pleasantness will try to keep their scale constant across the list and in so doing will rehearse previously presented list items ("Let's see, I think this word is a 6; it's as pleasant as word X, which I also called a 6"). When the cover task directs attention to individual items, as counting syllables would seem to, fewer rehearsals of prior items occur (see Postman & Kruesi, 1977). Typically (e.g., Fisk & Schneider, 1984, Experiment 1; Rose & Rowe, 1976) the judgments are higher for tasks that encourage rehearsals than for tasks that do not.

We agree that cover tasks differing in the degree to which subjects engage in rehearsals will result in different frequency judgments. We do not see this as a contradiction to our framework because of the following two empirical observations (see e.g., Johnson, Taylor, & Raye, 1977): (a) Subjects are able to judge the frequency of both actual occurrences of items and imagined (or rehearsed) occurrences; and (b) imagined occurrences inflate judgments of actual occurrences (apparently because people sometimes confuse memory traces from the two different sources; see Johnson & Raye, 1981). Thus from our point of view, any variable such as cover task instructions that allows for differential rehearsal rates will set the stage for differential frequency judgments. We have not, as Fisk alleges, ignored this issue, nor have we ignored the relevant data (see Hasher & Zacks, 1984, p. 1380); we seem, however, not to have made ourselves clear. In any event, Fisk's commentary does not attempt to criticize our explanation of the impact of orienting tasks on frequency judgments, and in the absence of such criticism there is no compelling reason to abandon it. His remarks do not present a clear case against our speculations about the special way in which frequency information is encoded.

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Anonymous Reviewing and the Peer-Review Process

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It is refreshing indeed to see that the topic of the peer-review process, specifically the matter of anonymous review of manuscripts submitted for publication, is moving from the realm of speculation to the laboratory. Ceci and Peters's comment (December, 1984) is a case in point. They reported an investigation in which reviewers for psychological journals routinely using so-called anonymous reviewing were asked to try to guess the author(s)' identity. Results of this study showed that overall, 35.6% of the 146 participating reviewers were correct in their identification of the author (or one of the authors) of the papers reviewed. These findings were taken as evidence that anonymous reviewing is "fairly blind" and that proponents of anonymous reviews should have confidence in its feasibility.

But does knowledge of authorship ultimately affect publication? Is there a negative bias against unknown authors affiliated with low-prestige institutions and a positive bias in favor of known authors affiliated with high-prestige institutions? These are the critical questions over which the peer-review process has come under attack.

It is regrettable that Ceci and Peters did not carry their study a step further and address this important question. As a start, it would be nice to know what proportion of the 35.6% of papers whose authors were identified by the reviewers was ultimately published. Is this significantly different from the proportion of the remaining papers (whose authors were not identified correctly) published? It is to be hoped that future studies will wrestle with these questions.

It appears that there are three possible approaches to the peer-review process. The two that have been most frequently employed involve either single-blind review, in which the reviewer knows the identity of the author but the author does not know the identity of the reviewer, and double-blind review, in which neither author nor reviewer knows the other's identity. Because both approaches have elicited so much heated controversy, is it not time to try the remaining alternative, namely, peer review in which the identities of author and reviewer are made known to each other? At the risk of igniting another controversy, I would like to suggest that this may indeed be the fairest and most effective solution to the problem. Why should a reviewer hide behind a cloak of anonymity? If a critique has merit and is really fair, surely the critic ought to have sufficient courage of his or her own convictions to be willing to sign it. Or is anonymous review simply a license for some reviewers to hit below the belt? Certainly open review would be in the tradition of Anglo-Saxon justice, where accuser and accused are able to face each other on equal footing and where openness and the weight of evidence take precedence over rhetoric and reputation.

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The Behavioral Effects of Sugar: A Comment on Buchanan

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Buchanan (November, 1984) labeled refined sugar a "toxin" and called for investigations of the effects of sugar on behavior. He appeared unaware that during the last several years studies have been undertaken to systematically examine the effects of sugar ingestion on the behavior of both hyperactive and normal children. The results of these studies, however, have failed to document any consistent adverse effects associated with this "most ubiquitous toxin." By presenting a brief review of these studies, we hope to clarify several erroneous conclusions drawn by Buchanan.

Contrary to Buchanan's assertion, there is not "a significant body of literature suggesting a relationship between refined sugar consumption and behavior change" (p. 1328). The only study he cited (Prinz, Roberts, & Hantman, 1980) simply demonstrated a significant correlation between dietary records of sugar intake and behavior observed in a playroom. In this study it was just as likely that restless or aggressive children sought out greater amounts of high-sugar foods as that sugar caused the inappropriate behavior. The interpretation of the Prinz et al. study is qualified further because the calculation of sugar intake was based on total food weight rather than on nutrient weight. The latter is the generally accepted method for determining sugar intake (Morgan & Zabik, 1981; Woteki, Welsh, Raper, & Marston, 1982).

Recently, several challenge studies have systematically investigated the effects of sugar on the behavior of both normal and behaviorally disordered children. In such studies, children were usually maintained on a restricted diet and then given, in a blinded and counterbalanced manner, drinks containing either sugar or a placebo of equivalent sweetness (e.g., aspartame). Behavioral measures were then collected to determine whether the two challenge drinks differentially affected the behavior of the children. Although somewhat limited in scope, such studies have allowed for a controlled and systematic examination of the effects of the challenge substance.

Although the results of these studies have not been entirely consistent, the overriding trend has been that sugar, when compared to a placebo, does not significantly impair the learning or behavior of the children (see Milich, Wolraich, & Lindgren, 1985). For example, Behar, Rapoport, Adams, Berg, and Cornblath (1984) examined 21 boys reported by their parents to respond adversely to sugar. The sample consisted of both behaviorally disordered and normal children. The results revealed a slight but significant *decrease* in observed motor activity at hour three following the sugar challenge drinks as compared to placebo.

Wolraich, Milich, Stumbo, and Schulz (1985) undertook two challenge studies, each employing 16 hyperactive children. A wide variety of observational, learning, and laboratory measures were collected following both challenge drinks. In neither study were there any significant effects associated with the sugar challenge. Further, when the results for the two studies were combined to make a more powerful analysis, only one of the 37 dependent variables reached significance, with children performing *better* on the sugar day.

Similar negative findings have been reported by Mahan et al. (1984) among 16 children who were reported to become aggressive, loud, and noncompliant following sugar ingestion. However, the investigators did eventually identify two of the children as responding adversely to both honey and sucrose. Ferguson (1984) also failed to find significant effects associated with sugar intake in two different studies. The first investigation involved 7- to 14-year-old children whose parents felt their behavior was adversely effected by sugar, and the second involved 17 preschoolers. Only three of the preschoolers exhibited even a suggestion of sensitivity to sugar, and none of the children showed a consistent pattern of adverse effects.

Only two studies apparently have found significant negative effects associated with sugar ingestion. Conners, Wingler, Schwab, Leong, and Blouin (1984) challenged 12 children who were inpatients on a psychiatric ward and did find a significant increase in total motor activity when the children were challenged with either fructose or sucrose. However, in a larger and better controlled study involving 37 inpatient children, Conners et al. found that sucrose significantly reduced fine-motor activity whereas fructose significantly reduced gross-motor activity. A study by Goldman, Lerman, Contois, and Udall (1984), employing eight normal preschoolers and a relatively high concentration of sugar (2 grams/kg), found significantly more errors on a continuous performance test following sugar ingestion.

Taken together, the results of these studies do not demonstrate a consistent pattern of adverse effects associated with sugar ingestion. There are more studies finding improvements following sugar intake than there are demonstrating adverse effects. Admittedly, further investigations are needed, especially ones employing challenges in more naturally occurring environments and examining interactions of sugar ingestion with diet and age. It is also possible that small numbers of children may respond adversely to sugar, just as there are small numbers of individuals who respond adversely to many different foods (King, Margen, Ogar, & Durkin, 1984). However, the evidence available to date in no way supports Buchanan's claim that sugar qualifies as the most ubiquitous toxin.

Buchanan further speculated that one reason for the lack of research examining the effects of sugar is "the political influence of sugar manufacturers, who are concerned with protecting their investments" (p. 1328). In fact, the Sugar Association, Inc., a representative for the sugar industry, has provided partial funding for at least two of the research projects cited above that investigated possible behavioral effects of sugar. In supporting this research, the association imposed no constraints in terms of publishing the results, regardless of how the studies might have turned out.

Buchanan also argued that adults, due to their greater cognitive sophistication, eat more balanced diets than children. The latter, responding to their basic drives, are presumed to consume large amounts of sugar and thereby upset their homeostatic balance. However, the limited evidence available does not suggest that children, either normal or hyperactive, eat disproportionately more sugar or carbohydrates than do adults. Wolraich, Stumbo, Milich, Chenard, and Schulz (1984), consistent with Prinz et al. (1980), found no differences in the dietary records of normal and hyperactive boys. Further, the proportions of energy nutrients eaten were similar to patterns found for normative adult samples (Woteki et al., 1982). It may well be that there are some children who upset their homeostatic balance through overconsumption of sugar, just as there are probably adults who do the same thing. It appears to be a gross oversimplification, however, to attribute this problem to all children.

One final point needs to be stressed. Using the term *toxin* to describe sugar appears to be a dramatic misrepresentation rather than a scientific fact. Buchanan was correct that the effects of sugar need to be systematically examined. However, the empirical evidence to date has not established any consistent adverse behavioral effects of sugar, let alone any justification for calling sugar a toxin. Unsupported statements such as Buchanan's do much to reinforce the fears of both parents and professionals, at the expense of reason.

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Defining "Impaired Psychologist"

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In Laliotis and Grayson's (January 1985) article, "Psychologist Heal Thyself: What Is Available for the Impaired Psychologist?" the term *impaired* lacks the definition and consistency needed to translate this concern into rational policies that serve the best interests of the public and the psychological community. Without precision and delineation of issues, miscommunication and continued difficulty in establishing clear standards for practice and the enforcement of such is likely to result.

Although the authors recognized definition as "a crucial issue," their own definition of impairment as "interference in professional functioning due to chemical dependency, mental illness, or personal conflict" does not clarify substantive issues and blurs some important distinctions. Impairment usually is defined (Gove, 1968) as "injury . . . deterioration . . . lessen" (p. 1131) and clearly implies a diminishment from a previously higher level of functioning. The more general issue of incompetence (Gove, 1968) is invoked when one is "lacking the qualities necessary to effective independent action" (p. 1144) and pertains to a lack of ability that may or may not be the result of an impairment. Given these guidelines, substance abuse generally would be viewed as an impairment. The broad categories of "mental illness" and "personal conflict" are difficult to evaluate as "impairments," because they are associated with numerous behavioral referents that may or may not imply diminished functioning.

The mixing of diagnostic categories and specific problem behaviors and the substitution of "impairment" for "incompetence" are responsible for the possible inclusion of such diverse areas as physical handicaps, substance abuse, sexual misconduct, psychosis, depression, and poor judgment under the concern for impaired psychologists. This medley makes it difficult to formulate reasonable policies and leads to an oversimplification of important issues. For example, persons guilty of sexual misconduct may or may not have a mental disorder defined by the DSM-III (American Psychiatric Association, 1980), which may or may not be related to their problem behavior, which may be related to a chronic difficulty in this area (e.g., personality disorder) or may be associated with a decline in functioning (e.g., adjustment disorder). Although diminished functioning may be difficult to establish in some cases, and although it may be desirable to offer assistance and to consider mitigating circumstances for some who are not "impaired" in the strict sense, careful analysis of relevant issues, specification of criteria, and consideration of individual differences would result in more effective decision making. For each situation, the appropriateness of rehabilitation (psychological and/or medical treatment), monitoring (close supervision), discipline (suspension or revocation of license), or any combination thereof should be based on a diagnostic assessment of the incompetent behavior.

It is also important to distinguish between persons who have provided inadequate professional service and those who have not but who are at great risk for doing so. The latter group consists of providers who are impaired but who are not guilty of misconduct. Self-help groups and awareness programs sponsored by local, state, and national psychological societies seem most appropriate in this regard. Those who are the targets of specific complaints must be handled by agencies that are responsible for protecting the public and who, it is to be hoped, also will help the psychologist to return to effective practice when possible.

The self-interest of psychological associations and the concern for the public welfare that is the charge of the state board of examiners can be satisfied and balanced only if key concepts are defined and important distinctions made. Sound policies are needed that will help the psychologist to maintain his or her practice in the midst of performance problems without diminishing the sense of individual responsibility for misconduct or providing any escape clause for incompetent behavior.

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Linguistic Constraints on Participation in Psychology

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Russell's (September 1984) article on psychology in its world context provides several examples of how linguistic and semantic factors, particularly the dominance of English as a universal language in psychology, limits psychology's potential development as an international discipline.

I would like to elaborate further on this important issue, using data that describe the linguistic characteristics of four cross-cultural psychology journals. The study is based on 338 articles published between 1978 and 1982 in the *Journal of Cross-Cultural Psychology (JCCP)*, the *International Journal of Psychology (IJP)*, the *International Journal of Intercultural Relations (IJIR)*, and the *Interamerican Journal of Psychology (IAJP)*. Three hundred and twenty seven, or 97%, of the studies were published in English. As indicated in Table 1, all but one of these articles had an English language abstract (the exception contained no abstract); 50% of the articles had French abstracts; and 20% had Spanish abstracts. The abstracts in languages other than English and for the 11 non-English articles were provided by three of the four journals as a matter of policy, perhaps as an attempt to make at least the broad outlines of cross-cultural research studies more widely available. It would be interesting to know whether nonnative English-speaking psychologists found that these abstracts helped them to participate in the cross-cultural psychology communication network represented by these journals.

Table 1
Language of Articles and Abstracts

Journal	No. of articles	English articles		No. of abstracts	English abstracts	
		N	%		*N*	%
Journal of Cross-Cultural Psychology	157	157	100	157	157	100
International Journal of Psychology	112	105	94	224	112	50[a]
International Journal of Intercultural Relations	56	56	100	168	56	33[b]
Interamerican Journal of Psychology	13	8	62	25	12	48[c]
Total	338	326	96	574	337	59[d]

[a] Abstracts in French or English are prepared by the *International Journal of Psychology*.
[b] Abstracts in French and Spanish are prepared by the *International Journal of Intercultural Relations*.
[c] Abstracts in Spanish and sometimes Portuguese are prepared by the *Interamerican Journal of Psychology*.
[d] All but one article had an English abstract; 168 or 50% had a French abstract; 68 or 20% had a Spanish abstract, and one article had a Portuguese abstract.

Language of Citations

In Table 2, the language of the 8,489 citations provided for the 338 articles is given. Ninety-seven percent of the citations were in English. Although citations were found in 16 languages, an English speaker with French, German, Spanish, and Hebrew as second languages could read 99.5% of the cross-cultural psychology literature cited in these four journals. The fact that about 92.5% of the literature cited in *Psychological Abstracts* is written in English, along with the variation by journals in percentage of English-language citations (i.e., 88.6%, 95.7%, 97.7%, and 98.3%) in the journals, suggests that some cross-cultural psychologists may be missing relevant research due to the language barrier. Although this failure to communicate is undoubtedly less of a problem than in the physical sciences (see Lewin, 1981), it certainly does not contribute to the advancement of cross-cultural psychology and puts a particularly difficult burden on speakers of languages other than English who want to have their work read and cited.

Summary of Cross-Cultural Journals Data

The cross-cultural psychology communication network, as represented by journal articles, was found to have diverse national inputs but was dominated by authors from the United States, Canada, Israel, Australia, the United Kingdom, and other English-speaking nations. Most studies (45%) were undertaken by single authors, and only 15% of all studies involved cross-national collaboration at an authorship level of credit. Single-culture studies (.37%) were most prevalent. Ninety-seven percent of the studies published and cited were in English.

The typical cross-cultural article described by this study was written in English about a single cultural group that was then compared to previous research or generalized theory. It was written by one or perhaps two English-speaking, probably North American, psychologists working in the same country. The typical study would cite on average 25 references and would have on average 1 reference to an article in a language other than English.

For the vast majority of cross-cultural psychologists, this stereotypic description provides an effective communication network because of the near universality of the English language within the system. However, the question that has to be asked is, Does the closed linguistic nature of this communication system pose serious disadvantages for native English speakers, and does it reduce the participation of or exclude non-English-speaking participants (Lewin & Jordan, 1981)? Do these limitations in the communication network then undermine the generalizability of the psychological laws under study?

Problems Faced by Non-English-Speaking Psychologists

The second part of this study examines the consequences of being a member of an international communication network dominated by the English language. Lonner noted that "one of *JCCP*'s shortcomings during the past decade has been its insufficient opportunity to publish more material from uncommon but probably important non-Western sources" (1980, pp. 14–15). He went on to argue that a better balance is needed to ensure the representativeness and generality of psychological laws.

Despite the attempts that have been made by cross-cultural psychologists to train and collaborate with the third-world psychologists, only a relative small number of studies are being published from this area. Some of this failure must be attributed to the language barrier that a 97% English-language field of study creates for nonnative English speakers.

Lewin and Jordan (1981) pointed out that nonnative English speakers must take additional years out of their academic or scientific careers to learn and maintain their English-language skills. These are skills that cross-cultural psychologists must have, as proficiency in English is essential to their profession. Native English speakers can use this same time for productive research. Native English speakers

Table 2
Language of Articles Cited in Articles Published in Leading Cross-Cultural Psychology Journals

Journal	Years	No of articles	Total no. of citations	Percentage of citations in English	French	German	Spanish	Hebrew	Other[b]
Journal of Cross-Cultural Psychology	1978–1982	157	3,294	97.7	0.4	0.4	0.2	0.7	0.7
International Journal of Psychology	1978–1982	112	3,044	95.7	2.9	0.6	0.5	0.1	0.2
International Journal of Intercultural Relations	1978–1982	56	1,914	98.3	0.2	0.1	0.2	0.7	0.5
Interamerican Journal of Psychology	1978–1981[a]	13	237	88.6	0.8	0.0	9.3	0.0	1.2
Total		338	8,489	97.0	1.2	0.4	0.45	0.45	0.5

[a] 1982 was not available at the time the data were collected.
[b] Other languages with one or more citations include: Chinese, Danish, Dutch, Hungarian, Indonesian, Italian, Japanese, Portuguese, Russian, Swedish, and Turkish.

can rely on English-language abstracts and when necessary can employ translation services to gain access to the few important scientific documents in their field that are not readily available in English. This option is impractical for nonnative English speakers, except on a very limited scale, because of the size of the literature that would have to be translated.

As Swales (in press) has indicated, very little evidence is available to document the nature and extent of the disadvantage that nonnative English speakers suffer. Although some general work has been completed (Baldauf & Jernudd, 1983), the degree to which language is problematic as a barrier to communication is unknown, nor are the consequences of this barrier known for the discipline or for its nonnative and native English-speaking members. Without this basic statistical and sociolinguistic data, it is difficult to suggest ways in which language patterns within the communication network might be altered or training improved to bypass the language barrier.

Characteristics of French-Language Users in *IJP*

To help answer these questions, let us first look at individuals who have made an available alternate language choice; that is, to publish in the *IJP* in French. Between 1966 and 1982, *IJP* published 393 research articles, excluding comments and discussion. Twenty-six of these articles were in French. For this part of the study, the 26 articles were examined along with 26 English-language controls, those being the article that followed (or preceded, to keep within the same volume year and number) the French-language article in the journal.

The pairs of French- and English-language articles were compared on a number of variables (Table 3). In terms of numbers of authors, the pattern was similar for both groups, with over half the studies having only one author. However, there the similarity ends. As might be expected, location of the authors was a good predictor of language selected. Whereas 38 of the 42 authors who contributed to studies in the French language came from France, Canada, Belgium, and Switzerland, only 6 of the 45 authors of studies in the English language did. On the other hand, there were 29 authors of studies in the English language from the United States, the United Kingdom, and Australia, but only 6 were from France, Canada, Belgium, or Switzerland. Several of the "cross-overs" in these groups can be explained by the cross-national authorship patterns in the studies (i.e., an American first author with a French co-author).

When type of study was examined, there appeared to be a difference of emphasis. Half of the studies in the French language were of the single cultural variety, whereas nearly half of the studies in the English language were of a cross-cultural nature. These apparent differences may reflect the small sample size, but they may also reflect the relative opportunities to conduct truly cross-cultural research in Francophone countries. Could it be that language is providing a limiting factor for the French-language authors?

The next section of the table examined differences between the languages of the literature cited by French- and English-language studies. English-language studies cited 96% English-language materials. By comparison, French-language studies cited only 66%. Although some of this difference may be explainable by availability of translated materials in both languages, it is fairly clear that the two language groups are operating from somewhat different databases within the same discipline. If this is the case, the communication of information within the larger cross-cultural psychology network is not based on a truly shared knowledge base.

A closer look at the articles cited reveals that French-language authors cited themselves 61 times for 13% of their citations; the English-language authors cited themselves 67 times for 11% of their citations. However, French-language authors' self-citations in French accounted for 31% of their French-language citations. Thus, if self-citations are removed, English-language authors still cite 97% English-language materials, whereas the percentage for French-language authors increases from .66 to .72.

In the final section of the table, citation rates are given for the 2 sets of 26 studies using data from the *Social Science Citation Index* for 1977–1982. Again, French-language authors are disadvantaged. Although self-citations are nearly equal, 10 and 9 respectively for French- and English-language authors, citations by external authors favor English-language authors over French-language authors by more than two to one. In addition, 13 studies authored in the French language received no citations, compared to 10 for the English controls. Because the English-language control authors were selected randomly, and the French-language authors included studies by Jean Piaget and by the journal's editors, it is unlikely that the quality of the content of the two groups of articles differs substantially. It seems probable, therefore, that the choice to publish in French means that an author's work will be less frequently read and cited. If this is the case, it increases the pressures on French-speaking authors to write in English, which then allows the English language to further dominate the field.

Table 3
Characteristics of 26 French-Language Studies and Their English-Language Controls from the International Journal of Psychology

Nationality	Characteristics				
	Number of authors				
	1	2	3	4	Cross-national
French	14	9	2	1	4 of 12
English	13	8	4	1	2 of 13
	Author location				
	France	Canada	Belgium	Switzerland	Czechoslovakia
French	15	12	6	5	1
English	1	2	1	2	0
	USA	UK	Australia	Africa	Other
French	1	0	0	2	0
English	19	5	5	5	5
	Type of study				
	Single	Cross-	Intra-	Review	
French	13	6	1	6	
English	9	12	1	4	
	Articles cited				
	Total	English	French	German	Other
French[a]					
N	459	303	137	9	10
%		66	30	2	2
English[b]					
N	618	596	16	6	0
%		96	3	1	0
	Citation of articles[c]				
	Total	Self	External	Article not cited	
French	40	10	30	13	
English	96	9	87	10	

[a] Authors whose articles were in French made 61 self-citations: 15 of these were in English, 42 in French, and 4 in Czechoslovakian.
[b] Authors whose articles were in English made 67 self-citations; 63 of these were in English, 3 in French, and 1 in German.
[c] Based on data from *Social Science Citation Abstracts*, 1977–1982.

How Authors Residing in Non-English-Speaking Countries Cope

JCCP has a policy of publishing only in English and does not publish abstracts in other languages. Where do authors residing in non-English-speaking countries get the English-language skills to publish in this journal? Some partial answers to this question can be found in the bibliographical information published at the end of each article.

Between 1978 and 1982, *JCCP* published 157 articles, which contained 325 author credits, and whose authors gave credits to institutions in 30 countries. If we now remove from consideration those authors located in the United States (174), the United Kingdom (13), Australia (18), Canada (14), and New Zealand (4) as probable native speakers of English, we have a sample of 102 possible nonnative English-speaking authors for analysis.

Table 4 indicates that 7 of these 102 authors were native speakers from the aforementioned countries living overseas. Of the 95 remaining authors, a number of overlapping criteria were found to apply. Forty authors had a co-author who resided in one of the five English-language speaking countries. Twenty-two authors—those from Nigeria, Hong Kong, India, South Africa, and the West Indies—came from countries where English is one of the official languages and (one of) the language(s) of higher education. Thirty-seven authors had studied, mainly for PhDs, in the United States, the United Kingdom, or Australia. In addition, four authors had taught or had held fellowships in the United States or United Kingdom.

The 27 Israeli authors were grouped together because of arguments by Lonner (1980) that they have close historical and political ties to the United States. Of the 27, however, only 10 acknowledged U.S. degrees, only one had a U.S. co-author, and only one indicated a teaching visit to the United States. Fifteen appeared to have no direct English-language connections. Ten authors from other countries were also identified as falling in this category. These figures suggest that only about 25% of nonnative English-speaking authors may be coping with the language barrier using primarily their own resources.

If we look at how this affects the citation pattern, we find that about three quarters of the non-English citations can be credited to the 60 articles that involved the 102 authors. These data are summarized in Table 5. Furthermore, this summary indicates that only 29 of the 157 articles in *JCCP* had even one non-English

Table 4
English-Language Coping Strategies for 102 Authors in 25 Nonnative-English Countries

Strategies	Total
Co-author resides in an English-language country	40
English is an/the official language	22
The author is an Israeli resident	27
The author has studied for a degree from U.S., U.K., or Australia	37
The author has taught/had a fellowship in U.S., or U.K.	4
The author is a native speaker living overseas	7
None of the above	10

Table 5
Journal of Cross-Cultural Psychology/*Citation Patterns for Articles Authored Only by Residents of English-Speaking Countries Versus All Others*

Country of residence	Number of articles	Articles non-English citations	Number of citations	Percentage in English
Native English[a]	97	12	2002	99.1
All others	60	17	1292	95.5
Total	157	29	3294	97.7

[a] U.S., U.K., Canada, Australia, and New Zealand.

citation. This suggests that most authors, even those from nonnative English-speaking backgrounds, do not read or use non-English-language material.

Directions for the Future

This study has examined descriptive data about journals that form the basis of the international communication network in cross-cultural psychology. The network, as expected, is dominated by the English language, to the extent that authors writing in and articles written in other languages are virtually excluded from the international communication process, and the impact of that work is relegated to the national communication network level. It is important, therefore, that native English-speaking authors develop their own language skills, thereby contributing to the communication process by more actively seeking, reading, and, where appropriate, citing non-English-language sources, to draw these authors into the communication process.

The dominance of Western psychologists, especially Americans, in the field, is increased by the English-only policy, which tends to exclude nonnative speakers of English who have no direct connections with American or British psychologists. Entry into the field for these "outsiders" is through degree studies or by teaming up with American or British co-authors.

Relatively few nonnative English-speaking authors without such contacts seem to have developed the bilingual skills to work independently in the field. This situation could be aided in several ways. Cross-cultural journals should publish abstracts in languages other than English. This would at least help nonnative English speakers to locate relevant materials.

Second, it may be that cross-cultural journals need an affirmative action program—not at the expense of quality but by providing a language editor and more in-depth, in-process guidance. These suggestions will take additional time and will go beyond the traditional impartial role that journals have played in promoting scientific knowledge. However, they should be seen as "in-service" training activities for which cross-cultural psychologists have an important responsibility.

Finally, the study shows that study programs obviously contribute to the ability of nonnative English speakers to participate in the cross-cultural psychology communication network. Cross-cultural trainers should not only worry about the psychological side of the learning process but should consider, in conjunction with colleagues skilled in English for special purposes (cf. Swales, in press), programs for the provision of specialist English-language skills for in-training and existing third world psychologists.

The suggestions in this comment are based on a study of one specialty within psychology. They examine only the formal, written aspects of psychological communication. I believe, however, that they are more generally applicable to the discipline as a whole. Psychologists need to consider ways in which best to proceed, to gain wider participation in their discipline so as to build a more generalizable science.

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Mislabeling the Black Client: A Reply to Ridley

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I find Charles R. Ridley's (November 1984) article on the nondisclosing black client a very welcome and helpful article. His investigative and clinical sensitivity to the dangers of blaming the nondisclosing black client are to be applauded. Likewise, his insightful perception of the two kinds of distrust among black clients is a tremendous contribution to both the theoretical and clinical understanding of the black client population. There is one major flaw in Ridley's article, however, that detracts from its contribution. In some respects this flaw tends to negate the argument being made in the article.

The flaw is one of nomenclature. Specifically, Ridley's careless use of the diagnostic term *paranoia* presented the black client as more disturbed than is actually the case. Ridley argued that the socialization process of blacks in the U.S. has resulted in the development of a "healthy cultural paranoia" (p. 1235), which acts as a protection against an oppressive environment that has been hostile to the interests of black persons. The use of the word paranoia to describe this phenomenon is not original to Ridley. In the article he quoted Grier and Cobbs (1968): "For his own survival, then, he must develop a cultural paranoia in which every white man is a potential enemy unless proved otherwise and every social system is set against him unless he personally finds out differently" (p. 149). Ridley was correct in identifying a mode of black client disclosing that includes a healthy mistrust of whites and white society. This mistrust is based in reality. For indeed there are white persons functioning within a society whose institutional racism has cheated, slandered, humiliated, and mistreated blacks. But should we call this response healthy cultural paranoia? Paranoia is not based in reality. It is a distorted mistrust of other persons and institutions, a form of mental illness (see DSM-III, American Psychiatric Association, 1980, pp. 195–198). Healthy paranoia is thus a self-contradictory term. The term is ambiguous at best and is another instance of "blaming the victim" at worst, just the outcome Ridley was trying to avoid. The appropriate mistrust that blacks have for those white persons and institutions that oppress, demean, and discriminate against blacks is not paranoia. It is a healthy mistrust of those who seek to demean and oppress.

Although Ridley's article attempted to counter the Type I error of diagnosing pathology where none exists, he inadvertently made such an error by means of the language he used. It would have been much more consistent with the focus of his article and constructive of its arguments if he had used the term *healthy cultural mistrust.*

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Self-Disclosure, Paranoia, and Unaware Racism: Another Look at the Black Client and the White Therapist

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Charles R. Ridley's (November 1984) article, "Clinical Treatment of the Nondisclosing Black Client," raised important issues about the need for white therapists to be sensitive to social, cultural, and political factors in assessing and treating black clients. He proposed (correctly, I believe), that a low level of self-disclosure, when found in black clients in mental health settings, may often be due to "the long experience of learning to survive as a minority person in a hostile culture" (p. 1239), rather than to a pathological process within the individual. However, although the overall message of the article is toward greater interracial understanding, in its very attempt to dissuade the reader from maintaining stereotypic (and perhaps unconsciously racist) views of black clients, it may, in fact, be unintentionally perpetuating some of those views.

In the first part of the article, Ridley reviewed data from psychology, sociology, and folk music to show blacks' general "reluctance to expose their inner psychological world" (p. 1235). He cited research findings that blacks disclose less than whites, with black males being the lowest disclosers of either sex or race. What is missing here is any mention of the *target* of the self-disclosure—the person(s) one is disclosing to—as a factor in differing levels of disclosure for blacks and whites. Do whites disclose more than blacks to *anybody,* regardless of race? Do whites in fact disclose more to other whites than blacks do to other blacks? Or is it that whites disclose to whites more than blacks disclose to whites—or that members of *both* races disclose more to racially similar than to racially different others? These are important questions that need to be answered. Ridley's review leaves readers to conclude that blacks are less open than whites in *all* interpersonal interaction, whereas the studies he mentioned were, in fact, not so definitive. The findings he cited of lower black self-disclosure were all based on the Jourard and Lasakow self-disclosure questionnaire (Dimond & Hellkamp, 1969; Jourard & Lasakow, 1958; Littlefield, 1974; Wolkon, Moriwaki & Williams, 1973), in which respondents were asked to rate their self-disclosure on different topics to parents and friends. The instrument measures *self-report* of self-disclosure, rather than self-disclosure itself. And the findings were not consistent; in the Littlefield (1974) study, the overall higher white mean was entirely due to high white female scores, with black males and black females reporting *higher* self-disclosure than white males. Further on in the article, Ridley did cite research showing that black clients have been found to disclose more to black than to white therapists, and that greater client self-exploration has been found in racially similar than in racially dissimilar counselor–client dyads. But the implications of these findings as part of a total schema of intra- and interracial self-disclosing are not addressed. Thus, the message conveyed throughout the article seems to be that whites are relatively uninhibited self-disclosers (and therefore, according to Ridley's discussion, emotionally healthy), whereas blacks are not.

In his analysis of the self-disclosing process, Ridley described two dimensions of black client interpersonal functioning. One he labeled "cultural paranoia," which he defined as "a healthy psychological reaction to racism"; the other he labeled "functional paranoia," which he defined as "an unhealthy condition that itself is an illness" (p. 1238). He then presented a four-mode typology, categorizing all black clients according to those two dimensions. Mode 1, the intercultural nonparanoiac discloser, includes black clients who are low on both functional and cultural paranoia, and who can be expected to disclose to either black or white therapists. Mode 2, the functional paranoiac, includes black clients for whom "the problem of nondisclosure lies primarily in . . . personal pathology" (p. 1238), and who are nondisclosing to both black and white therapists. Mode 3, the healthy cultural paranoiac, includes clients who are seen as showing a healthy psychological reaction to racism; for fear of being hurt or misunderstood, they are nondisclosing to white therapists, but they are likely to disclose to black therapists. Mode 4, the confluent paranoiac, includes clients whose problem is both a reaction to racism *and* personal pathology; like functional paranoiacs, they are nondisclosing to both black and white therapists.

This is a simple and easily understood typology. However, there are certain assumptions embedded within the definitions of these modes that warrant closer examination. The intercultural nonparanoiac discloser is, according to Ridley, very rare, with the great majority of black clients falling into mode three, the healthy cultural paranoiac. Thus the great majority of black clients are described by the author as suffering from some form of *paranoia,* a term that, even when preceded by the qualifier *healthy,* still has strong overtones of mental dysfunction. Ironically then, even as Ridley argued that the healthy cultural paranoiac is often incorrectly evaluated as pathological, his choice of labels perpetuates the very problem he hoped to eliminate. The unintended message of psychopathology is further conveyed by the use of a medical model in discussing the healthy cultural paranoiac mode—Ridley referred to it as a syndrome that, if misdiagnosed, could lead to further symptom development. The definition of Mode 2, the functional paranoiac, is also problematic, in that it attributes nondisclosure to the client's personal pathology, *as distinct from a reaction to racism.* In other words, black clients may suffer from a variety of paranoiac reactions, such as "unusual fears of persecution" (p. 1238), that are unrelated to their having grown up as a member of an oppressed minority group. This is a questionable assumption. In a society in which the oppression of blacks has been a historical fact for centuries, it can easily be argued that racism is *always* a direct or indirect etiological factor in the psychopathology of black clients. In any case, given the lack of data in this area, Ridley's distinction between paranoia as a reaction to racism and paranoia as personal pathology is unconvincing. Mode four, the confluent paranoiac, is similarly problematic, in that it too is based on the assumption of two distinct kinds of paranoia. The author pointed out the necessity on the part of the therapist for "balancing support for that portion of the client's experience that is truly the result of being victimized and that portion that must be confronted as resulting from pathological processes" (p. 1240). However, he did not tell us how to determine where one portion ends and the other begins.

There are other, more subtle ways in which the article inadvertently perpetuates negative images of black clients, mainly through the language and terminology used. The choice of the term "healthy cultural paranoiac" to describe "the great majority of black clients" (p. 1239) is puzzling, in that Ridley described these clients as showing a healthy reaction to past encounters with racism and/or racist cues given by a white therapist. Why not label the category "aware reality perceiver," or "healthy precautionary"—or some other nonpejorative title that acknowledges the positive aspects of such behavior? In contrast to this negative labeling of what he described as a healthy behavior pattern in black clients, the author's brief discussion of the behavior of white therapists seems tactful to the point of being overprotective. When considering the possibility that their behavior may cause *appropriate* negative reactions in black clients, he wrote only of the need for such therapists to modify their "deviant" behavior—thus both avoiding the use of the term *racist*, and implying that such behavior is certainly not the norm. Ridley used a number of different phrases and examples to describe such behavior on the part of white therapists; however, only twice in the entire article did he use the word racist or racism in those descriptions. Although it is understandable that he might choose to avoid phrases that might alienate the very readers he wishes to educate, it is at least as important to consider the effects of what might be called "cultural racism" on the behavior of white therapists, as it is to consider the effects of "cultural paranoia" on the behavior of black clients. By "cultural racism," I am referring to the unavoidable absorption of some racist atitudes, as a result of growing up as a member of the white majority in a society in which the oppression of blacks has been a cultural norm.

A final example is in the domain of emotion. The author presented interpretations of black clients' and white therapists' negative emotional reactions to their therapeutic encounters in ways that seem to "pathologize" the reactions of the former while glossing over those of the latter. He interpreted a black client's hostile behavior that is not accompanied by classical paranoiac symptoms as an attempt "to intimidate the therapist, thus exercising a personal sense of power—power otherwise unachievable over majority group individuals" (p. 1239). Although this is certainly a possibility, the interpretation may speak more to a white therapist's *experiencing* of a black client's hostility (i.e., as intimidating), than to the actual sources of the behavior, which may be legitimate, long-endured feelings of frustration, humiliation, and rage, or perhaps the client's immediate reaction to the therapist's unaware racism. White therapists' difficulties in dealing with black clients' strong negative feelings, on the other hand, were described only as "the inability of many white therapists to cope with the anxieties of their black clients" (p. 1240). Near the end of the article, in a brief paragraph on the personal psychology of the white therapist, Ridley mentioned in a general way the importance of sensitivity around issues of race, and awareness that the therapist's attitudes and behaviors may affect black clients. Nowhere in the article, however, did he deal with negative feelings that may be aroused in a white therapist who is treating a black client—such as fear, mistrust, guilt, or defensiveness—let alone discuss how to recognize, understand, and eventually work through those feelings.

In conclusion, Ridley addressed an important issue, and pointed out some of the obstacles to the successful diagnosing and treating of black clients by white therapists. His examples show a sensitivity to the kind of unaware racism of therapists from what he called the "old 'bleeding heart' liberal school of the 1960s" (p. 1240). However, the 1960s are long gone, and unaware racism is still with us in equally damaging but much subtler forms. It is important that nonminority therapists work to heighten their awareness of the social, economic, and political factors affecting the lives of minorities in this country, so that they may offer meaningful support on an individual level, as well as on the level of societal change. I am sure that Ridley has no quarrel with this, and that his article, which makes a persuasive case for greater sensitivity in the treatment of black clients, may move readers in that direction. However, it is also important that nonminority psychologists come to understand how living and working in a racist society may have shaped their own perceptions and behaviors, and how those perceptions and behaviors may affect the minority group members they are studying or treating. In this area, Ridley neither examined the issues nor provided the kind of forthright information and discussion that is needed.

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Optimum Service Delivery to the Black Client

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The comments by Ashby and by Bronstein in this issue are provocative and deserve to be read by the professional audience of the *American Psychologist*. I am delighted that my earlier article (Ridley, 1984) has merited such critical review.

The most formidable challenge is to my use of the term "healthy cultural paranoia" to describe a black person (or client) who is psychologically healthy but culturally guarded against racism. Both authors have converged upon this criticism, albeit through independent observations, and this convergence underscores the seriousness of the criticism they make. I concur with them that a danger exists in the use of this term, but the imminence of that danger lies in the misapplication of the term and not in its content as they contend. Professionals, at all times, must exercise clinical prudence in formulating diagnostic impressions.

Aside from the fact that I am not the originator of the term, I assert that Ashby and Bronstein have precluded the more fundamental issue. By focusing their arguments on terminology, they have failed to consider the impact of my proposal on the quality of care services to black clients. In my discussion, I operationalized the term with sufficient clarity; these authors had no difficulty in ascertaining its meaning. Why should they expect other competent professionals to have difficulty?

Labeling is a tool employed in the

service of treatment. Diagnostic labels are not therapeutic outcomes. Outcomes are the actual changes that occur in clients as a result of treatment. The real question is whether the proposed nomenclature can be beneficially used in the interest of treatment. Failure to ask this question will lead to erroneous conclusions about the labeling process. Certainly, accurate labeling is essential; however, the a priori concern is the capacity of treatment to yield constructive changes in the behavior and mental health status of the clients being served. Labeling becomes problematic only if use of the term "healthy cultural paranoia" or any other term militates against treatment effectiveness. I suggest that this is an improbable occurrence.

The following questions are relevant. Does the use of the diagnostic nomenclature contribute to more beneficial treatment planning? Are the differential diagnoses more valid (useful) than traditional diagnoses assigned to black clients? Does treatment result in more favorable outcomes? Is there a reduction in premature termination? Do patients develop more favorable attitudes toward the treatment endeavor?

These are empirical questions that, of course, are open to scientific inquiry. Admittedly, definitive answers are as yet unavailable. However, my prediction is that the data will support the alternate hypotheses: There are significant treatment effects. Preliminary evidence is encouraging. Feedback from numerous clinicians since the publication of my article has indicated its usefulness in helping to unravel complex etiological dynamics. Such evidence also disclaims the criticisms of carelessness and negative labeling by Ashby and Bronstein respectively.

Bronstein raises other critical concerns. Her requests for clarification, though, are beyond reasonable expectations for a journal-length article. Experienced writers and readers of major journal publications understand the need for content selectivity to avoid diffusion. She criticizes my brief discussions of the behavior of white therapists and the process of differentiating functional paranoia from cultural paranoia as being too brief. Each of these topics, to be given fair explication, requires an article-length discussion. Regarding the former, I refer Bronstein and the readership to my recent publication (Ridley, 1985) that details her concern about unaware racism.

My limited use of the term *racism* was deliberate. Racism is a multiordinal term that is loaded with multiple connotations. The connotative meaning resides in the user. Bronstein is absolutely correct in suggesting that I did not want to alienate my audience. However, she is equally incorrect in her assumption about the rationale behind that decision. To be sure, I did not want to alienate white readers because of their inadvertent reactions. There are many forms of racism, such as individual and institutional, overt and covert, and intentional and unintentional. Many people use the term to mean overt, blatant forms of bigotry as opposed to the unaware racism that Bronstein intimates.

Regarding the criticism of intergroup differences on the construct of self-disclosure, the data are not conclusive. More research needs to be conducted in this area. However, the literature does suggest an important trend. I stated clearly that the majority of blacks function in the mode 2 category. Thus, the message conveyed is not that whites are uninhibited disclosers whereas blacks are not; rather, blacks tend to be inhibited in the presence of white therapists.

In conclusion, the critiques by these authors raised challenging questions. Unfortunately, the major thrust of their arguments demonstrate such a preoccupation with the trees that they have overlooked the forest. If my treatment proposal and diagnostic nomenclature are used, I reassert that black clients will be better served. With the resulting more favorable outcomes, the profession will have fulfilled one of its most valued ethical principles: the welfare of the consumer. Certainly, Ashby and Bronstein could not endorse a more worthy clinical endeavor.

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Utopia or Myopia? A Reply to Fox

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As experimental researchers concerned with social dilemmas, we read with interest Dennis R. Fox's (January 1985) article, "Psychology, Ideology, Utopia, and the Commons." The central thesis was that psychological researchers have generally ignored "radical decentralization" as a possible solution to the global commons problem confronting society today. Fox argued that only by the establishment of a "decentralized, federated society of smaller, autonomous communities" will we be able to "avert major global crises while we simultaneously expand human dignity and meet human needs" (p. 48), and he suggested that psychologists have focused exclusively on a "centralized-state approach" to solving commons dilemmas. He found this type of solution objectionable on the grounds that it reduces an individual's autonomy and psychological sense of community.

We believe that there are a number of inaccurate statements and logical flaws in Fox's article that seriously weaken the strength of his argument. The first is the conclusion that most social psychological researchers favor the coercive, centralized authority solution suggested by Hardin (1968) and Heilbroner (1980). In fact, based on the comprehensive literature reviews on social dilemmas provided by Dawes (1980), Dawes and Orbell (1981), and Messick and Brewer (1983), we would argue that the opposite is true. A careful reading of these sources reveals that the majority of empirical studies have focused on *individual* approaches to solving commons dilemmas. That is, these researchers implicitly assumed that people faced with a commons dilemma could solve it through independent, voluntary changes in individual behavior, *without* resorting to centralized decision-making structures. The single experimental study cited in these three reviews that addressed the centralized authority solution was one reported by Messick et al. (1983). Fox, however, presented the Messick et al. experiment as the sole evidence for his assertion that most social psychologists agree with the centralized-state approach to solving commons problems. Clearly, this is a strange conclusion to draw from a thorough review of the literature. It is especially puzzling because Fox cited the Dawes and Orbell (1981) review (although, surprisingly, not those of Dawes, 1980, or Messick & Brewer, 1983).

In short, it is not true that social psychologists have ignored decentralist approaches to solving commons dilemmas. This assertion is a rhetorical "strawman" that Fox erected in order to create an issue for debate.

We must also respond to Fox's criticism of the Messick et al. (1983) study. He stated that "their research goal was to discover only 'when or under what circumstances individuals will voluntarily relinquish their freedom of access to a commons by turning the management of the commons over to a centralized authority' " (pp. 52–53). This statement reflects a highly selective reading of this article. Messick et al., as witnessed by the title of their article, "Individual Adaptations and Structural Change as Solutions to Social Dilemmas," were interested in *both* individual solutions (i.e., voluntary self-restraint among group members) and structural solutions (i.e., establishment of superordinate authority) to social dilemmas. Both modes of response were available to subjects as a means of solving the commons dilemma with which they were faced. Moreover, the authors showed no bias in favoring one solution over the other; rather, they simply described the effects exerted by the independent variables on each type of solution.

Fox also questioned the ecological validity of the Messick et al. (1983) experiment because (a) the subjects had no face-to-face communication, and (b) the groups were given only two options: free access or election of leader. These objections are unjustified and unfair to the research program as a whole. First, we would argue that in the "real world," face-to-face communication is likely to be the exception rather than the norm, especially in social dilemmas involving large numbers of strangers (see Messick & Brewer, 1983, p. 23).

Fox's second criticism is a classic example of what can happen when a single experiment is interpreted without reference to the overall research program from which it derives. The fact that only two alternatives (free access vs. superordinate authority) were presented to subjects does *not* imply that these are the only two solutions possible. It reflects the fact of experimental life that one can answer only a few theoretical questions with a single experiment. Subsequent experiments conducted in the Messick–Brewer research program have indeed explored alternative, noncentralist solutions to social dilemmas (e.g., Allison & Messick, in press; Kramer & Brewer, 1984; Messick & McClelland, 1983; Samuelson & Messick, in press; Samuelson, Messick, Rutte, & Wilke, 1984).

The utopian solution proposed by Fox, radical decentralization, appears to solve some problems posed by the commons, but it also presents several new problems of its own. For example, how would decentralized communities deal with increases in population size? As Edney (1980) pointed out, overconsumption of common resources can be caused by too many people using the same pool, by consumers using the common pool too fast, or by a combination of the two factors. It seems clear that successful management of the commons will require some mechanism for limiting population size within that community. Indeed, the long-term survival of decentralized communities in the past has been shown to depend on this crucial feature (see Bullock & Baden, 1977, for an analysis of the determinants of success and failure in two communes). Fox failed to explain how these decentralized communities would cope with this problem without some form of centralized decision making to control population size.

Another potential difficulty with Fox's solution concerns the issue of intercommunity cooperation. He made an implicit assumption that independent, autonomous communities would cooperate in coordinating resource use from the commons. Is this assumption justified? Will separate communities cooperate with each other voluntarily? When common resources are scarce, this type of decentralized arrangement could produce intercommunity conflict over the distribution of those scarce resources. Some recent psychological research on ingroup–outgroup biases and intergroup conflict (see Brewer, 1979; Komorita & Lapworth, 1982) suggests that dividing groups into smaller units may enhance *intragroup* cooperation but with the undesirable side effect of decreased intergroup cooperation. In the absence of some structural method of coordination *between* communities, the decentralized solution proposed by Fox may simply generate a commons dilemma at the next higher level of social organization: the community.

A third objection to Fox's solution focuses on his assumption that *most* people would prefer to live in small, autonomous communities (e.g., kibbutz, commune). Is this true? Even if it could be shown that decentralized communities do satisfy psychological needs and values most effectively, does it necessarily follow that most people would prefer to live in such environments? Small, highly interdependent communities (such as the kibbutz in Israel) require large personal sacrifices from their members, sacrifices many people may be unwilling to make. Paradoxically, Fox's utopian solution appears to reflect a set of values that would deny people the right to live in large, anonymous communities where few demands are placed on them.

Even if we assume that Fox's decentralized solution is a viable and effective one for solving the commons problem, we are still left with serious practical problems concerning implementation. Perhaps most important is the issue of time. Fox admitted that "working out such details [of a decentralized world] will take many years of speculation, imaginative investigation, and actual attempts to bring such a society about" (p. 49). Given the current state of common resources around the world, we may not have the luxury of waiting for Fox's utopian communities to come to pass. If steps are not taken soon to prevent the destruction of the global commons, we will not need to concern ourselves with saving the commons in the future; there may be no commons left. We believe that the global commons problem demands an immediate response in the short run to allow sufficient time to develop long-range solutions.

No solutions to the commons problem are perfect. Both centralized authority and decentralized approaches have strengths and weaknesses. We feel that long-run, permanent solutions to social dilemmas such as the commons problem will require both individual and structural approaches. It would be premature at this point to reject either type of solution as unacceptable because it conflicts with one's prejudgments about desirable social arrangements. Although coercive, centralized-state solutions may seem objectionable to many, they should be judged within the broader context of the potential ecological disasters that we face today. Hardin's (1977) concept of "situational ethics" is relevant here: "The morality of an act is determined by the state of the system at the time the act is performed" (p. 114).

More authoritarian solutions may be necessary in the short run to preserve the existing global commons for future generations. Although such systems may be inconsistent with individual autonomy and human dignity, perhaps even unfair, we would agree with Hardin (1968) that "injustice is preferable to total ruin" (p. 1247). Perhaps allocation systems that preserve *both* human dignity and the commons can be devised in the future. In the meantime, however, we should not forget Hardin's (1968) warning regarding the dangers of accepting the status quo when proposed reforms are imperfect:

We can never do nothing. That which we have done for thousands of years is also action. It also produces evils. Once we are aware that the status quo is action, we can then compare its discoverable advantages and disadvantages with the predicted advantages and disadvantages of the proposed reform. . . . On the basis of such a comparison, we can make a rational decision which will not involve the unworkable assumption that only perfect systems are tolerable. (pp. 1247–1248)

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Toward a Social Psychology of Solidarity

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In his article, "Psychology, Ideology, Utopia, and the Commons" (January 1985), Dennis Fox made two important points. First, he connected the problems of "global ecology" and of "individual needs and values," tying together in a meaningful way two major social problems that have received some attention from different groups of psychologists. Fox suggested that the connection between the psychology of resource conservation and the psychology of alienation is through the "anarchist insight" that centralized solutions to social problems create different social problems by weakening personal autonomy and the sense of community. In this connection Fox made his other point, that most of the solutions offered for the "tragedy of the commons" have been centralist in nature. This recognition has been obscured in the literature by proposals for solutions that appear individualistic, such as offering "selective incentives" (Olson, 1965) or changing reinforcement schedules (Platt, 1973) to promote group-oriented behavior. Fox correctly perceived that although these approaches act through individual self-interest, they must be imposed by some central authority.

However, Fox did not carry his call for decentralist solutions to the point of empirical test. After arguing the need to take anarchist thinking seriously, he should have noted the well-known practical problems of anarchist and decentralist experiments and called on social scientists to begin research to address them. There are empirical questions to answer, and some of the major ones are psychological.

Anarchist and decentralist communities have two major problems of external affairs: defense and intergroup coordination. Even when anarchist communities establish strong feelings of solidarity, they may not be organized tightly enough for defense against major external threats. This was the downfall of the anarchist communities in Spain when faced by Franco's armies. Coordination between groups is particularly important in preventing the tragedy of the commons because some important commons cross community boundaries. What is to prevent one anarchist community from exporting its wastes to another community downstream or downwind?

A call for "a decentralized society of federated autonomous communities" is not a sufficient answer. Such a society is of course necessary to an anarchist solution to the commons problem, but it is by no means clear how to create one. Good intentions and good feelings are not enough to resolve real conflicts of interest that arise both within and between communities, and utopians who fail to plan for conflict are headed for disappointment (Sarason, 1972). Social scientists could begin to think about how a decentralized society of federated autonomous communities could arise and about what would keep the process from resulting in the sort of centralized system spawned by the federalists who framed the U.S. Constitution.

The most critical barrier to a decentralized society, of course, is creating viable communities. The empirical questions here are largely social-psychological: They involve creating and maintaining the solidarity needed to hold a community together. As background for answering these questions, it is important to note that the tragedy of the commons, as well as the thinking that suggests centralist solutions to it, is rooted in a psychological assumption that humans are by nature egoists. This assumption underlies the work of neoclassical economists, who call it utility maximization; of behaviorists, who call it the laws of reinforcement; and of some political scientists and social psychologists, who see society as the sum of social exchanges among individuals. The theory that people take only themselves into account and the corollary that they limit their concerns to their own lifetimes logically imply the tragedy of the commons when resources are scarce and depletable. And if humans in fact always behave as short-sighted egoists, tragedy must inexorably follow from that fatal character flaw.

Hardin (1968) and others did not seem to fully appreciate the depth of the tragedy they wrote of. Given the fatal flaw of short-sighted egoism, the tragic protagonist cannot escape destiny. As I have noted elsewhere (Stern & Kirkpatrick, 1977), social-psychological research gives an empirical basis to doubts about the value of individualistic solutions based on coercion or incentives: Coercion provokes psychological reactance (Brehm & Brehm, 1981), and extrinsic rewards weaken intrinsic motivation (e.g., Condry, 1977). By prescribing or rewarding civic virtue, society can create vicious cycles that undermine the psychological basis of that virtue and increase the need for penalties and incentives. If the metaphor of tragedy is accurate, there is no solution without changing the protagonist's character.

To put this positively, solution is possible only if humans are social, rather than isolated, animals. Of course, people sometimes are social animals, as Fox (1985) recognized with the distinction between communal and exchange relationships. The potential for communal bonds raises a set of important social-psychological questions critical to building a decentralized society: When is behavior governed by self-interest and when by social interest? What social conditions bring about a psychological sense of community? How can group solidarity survive tensions and conflicts of interest between individuals and groups divided along race, class, sex, and other social fault lines? What noncoercive social forces can bring people to preserve scarce resources for the common good? What social structures can bring people to internalize concern for the general welfare and for the distant future? These are broad questions for social science, but they also form an agenda for a social psychology of solidarity that could help uncover ways to address some of the grand social and ecological problems. When the problems are truly global, as with many of the world's major environmental and resource problems, these questions have an urgency that justifies an effort to promote solidarity even across historical chasms of conflict.

Where does one look for knowledge about the social psychology of solidarity? Many literatures that might be relevant are not helpful because their independent variables operate at the individual level. Research on altruism that focuses on the qualities of people who help others in distress, or research on cooperation that focuses on leadership styles that promote cooperative behavior, will offer few insights into how to build community. It is unrealistic to expect a transition to a communal society to flow from the personalities of individuals raised in an exchange-based society, unless they have extensive social support.

The social psychology of solidarity is more likely to be built from research in which the independent variables are social structures or processes and the dependent variables are individuals' attitudes, feelings, and behaviors. Thinking can usefully follow the lines of what Watson and Johnson (1972) call S-P-A theory. According to this framework, social structures set in motion social processes, and individuals' attitudes are shaped by participation in these processes. In this view, the psychological entities such as "sense of community," "cohesiveness," and "solidarity" come from joining with others in interactions that engender such feelings. The research agenda for a social psychology of solidarity begins with identifying those types of interactions and the social structures that promote them.

One set of relevant concepts comes from research on the prisoners' dilemma and related social dilemmas, which shows that cooperation increases with increased probability of continued contact. As Axelrod (1984) has conclusively shown, structures that keep people interacting engender cooperation even among individuals whose motives are egoistic: It pays to cooperate when your partner can get even. Laboratory games show that such social structures can also foster the formation of cooperative group norms. This requires a group of more than two and free communication among group members over a period of time, qualities absent in the standard prisoners' dilemma but present in true commons dilemma simulations (e.g., Stern, 1976). Laboratory groups playing commons dilemma games have evolved norms, such as for taking turns, that preserve resources for the common good (Stern & Kirkpatrick, 1977). Political game-players evolve similar norms (Axelrod, 1984).

The research in social psychology of clearest relevance to building community concerns intergroup conflict and cooperation. Researchers have illuminated the roles of common enemies, equal-status contact, and superordinate goals in strengthening solidarity in small groups, and have also shown how intergroup tensions affect processes within groups. The classic reference in this field is, of course, the Robbers Cave Experiment (Sherif, Harvey, White, Hood, & Sherif, 1961), which made the idea of superordinate goals operational and showed how, over time and repetition, structures that elicit cooperative behavior can increase feelings of solidarity, break down enmity between groups, and change intragroup processes and individual attitudes.

Sherif's success in building solidarity should not be taken as strong support for the feasibility of the decentralist program. Even in groups like Sherif's that are carefully manipulated, very small, and circumscribed in purpose and time, positive results are not easy to achieve. The failures of Sherif's two earlier experiments attest to this. Building solidarity will be difficult, especially in the large and complex groups, riddled with divisions, that Fox wishes to form into a decentralized society.

There are, however, a number of opportunities for learning. In what amount to natural experiments, community groups are restructuring problems that people might consider personal to provide superordinate goals—for example, by organizing neighborhood watch programs to prevent crime or by creating community-based home weatherization projects. Such experiments could be evaluated not only for their effects on their primary goals, such as cutting crime rates or energy consumption, but in terms of their effects on community. Evaluation studies could ask how a program changes social processes and attitudes or, more important for questions about building community, whether a program leads to creation of permanent local institutions that continue to promote cooperative processes. Evaluation studies would also ask which program structures are most likely to have effects that strengthen community, and what happens to preexisting social divisions in the communities at the same time. In short, the evaluation of social programs can yield useful knowledge for a social psychology of solidarity.

A focus on building community also has policy implications, especially at a time when the federal Administration espouses a desire to find local solutions to problems. Public policies could be structured to create superordinate goals for communities in the hope that the process of working toward those goals will strengthen the communities themselves. Such experiments will often fail, but Fox's argument suggested that they are worth trying. In the true spirit of experimentation, such projects should be carefully evaluated to learn from the experience. And in the spirit of solidarity, communities should be intimately involved in designing the experiments on themselves and in evaluating their results.

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Beyond Individualism and Centralization

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One of the goals of my original article (Fox, 1985b) was "to participate in the crucial process of exposing our own basic assumptions to constructive peer criticism" (p. 49). I appreciate the many thoughtful responses sent directly to me, and I am glad to have stimulated a brief dialogue in this journal. I do regret, however, that Samuelson, Messick, Allison, and Beggan (this issue, pp. 227–229) do not straightforwardly discuss the nature and impact of their own ideological perspective. Their comment—particularly when viewed in comparison with that of Stern (this issue, pp. 229–231)—strengthens my argument concerning the degree to which unacknowledged assumptions affect psychological interpretation.

Samuelson et al. (this issue) apparently object to my view that "researchers often agree, explicitly or implicitly, with the conclusions of Hardin and Heilbroner: In our modern, technological, complicated world, a tragedy of monumental scope is inevitable unless we resort to increased centralized governmental power" (Fox, 1985b, p. 49). I went on to ask "why do most social scientists not even discuss the conclusions drawn by those dissenting from the centralized-state approach?" (p. 52), and I cited the study of Messick et al. (1983) as one in which "researchers generally do not even take decentralist autonomous-community solutions into account" (p. 52). In short, I was accusing mainstream researchers not so much of consciously focusing "exclusively" on centralized approaches, as Samuelson et al. (p. 227) put it, but of generally ignoring the whole issue, thereby contributing to the perception that decentralized solutions are not even worth investigating.

Stern, whose suggestions for empirical research are a welcome complement to my argument, points out that the commons literature's generally centralist tone is "obscured . . . by proposals for solutions that *appear* individualistic" (p. 1, italics added). Part of the obscurity, I think, comes from confusion over the difference between individualistic, centralized, and decentralized approaches. Thus, Samuelson et al. repeatedly refer to individualism and decentralization as if they are the same thing, ignoring, for example, the vast differences between an individualistic, self-oriented free-for-all in which isolated individuals take into account only what is best for themselves, and a cooperative federation of autonomous communities in which individuals are rooted in the mundane mutuality of everyday interdependence. The important point here is the crucial role played by the existence of a real community, without which a psychological sense of community is impossible and the tragedy of the commons inevitable (in a related context, see Levine, 1983; in press).

In claiming that most psychologists oppose centralization, Samuelson et al. argue that "the majority of empirical studies have focused on *individual* approaches to solving commons dilemmas" (p. 227). They are apparently satisfied with the assumption that "people faced with a commons dilemma can solve it through independent, voluntary changes in individual behavior, *without* resorting to centralized decision-making structures" (p. 227), and they conclude that "long-run, permanent solutions to social dilemmas . . . will require both individual and structural approaches" (p. 228). Unfortunately, they identify these approaches simply as "voluntary self-restraint among group members" and "establishment of superordinate authority" (p. 228), neither of which includes the option of a decentralized, ongoing, small-group community.

This simple individualistic-centralized dichotomy, with its appeal to those steeped in the American value system, is exactly what I was objecting to in the study by Messick et al. (1983). It would be more useful, and less ideologically confining, for researchers to provide decentralized alternatives to both rampant individualism and centralized authoritarianism, alternatives based on small groups in which there is "free communication among group members over a period of time" (Stern, this issue, p. 230). Such free communication was noticeably absent not only in Messick et al. (1983); it was absent as well in all three of the published studies Samuelson et al. cite as examples of a research program that has "explored alternative, noncentralist solutions to social dilemmas" (p. 228). Furthermore, Samuelson, Messick, Rutte, and Wilke (1984), although identifying important cultural differences between American and Dutch subjects, presented those subjects with the same dichotomized free-access–authoritarian-leadership choice as in Messick et al. (1983); the studies by Messick and McClelland (1983) and Kramer and Brewer (1984), although very interesting, did not directly deal with the centralization issue and thus are not particularly relevant to this discussion.

Samuelson et al.'s objection to my characterization of much social psychological research as implicitly centralist might be more convincing if they did not go on to explicitly conclude that "More authoritarian solutions may be necessary in the short run in order to preserve the existing global commons for future generations" (p. 228). They note in a matter-of-fact manner that, "Although coercive, centralized-state solutions may seem objectionable to many, they should be judged within the broader context of the potential ecological disasters that we face today" (p. 228). These and similar statements—such as Samuelson et al.'s (1984) observation that "our subjects seem to make the sensible choice that regulation is preferable to depletion" (p. 102)—seem to me to place Samuelson et al. firmly in the Hardin-Heilbroner camp, despite their apparent discomfort with that position.

Observers can reasonably disagree about the relative dangers of growing social dilemmas on the one hand and growing centralized authoritarianism on the other. As I indicated in my original article, my own view is that our desire to resolve environmental crises should not panic us into prematurely and unnecessarily resorting to solutions that will do greater damage to the lives of individuals, particularly because so much of what we take to be "inevitable" social dilemmas is in fact *created* by a variety of changeable cultural factors (see also Edney, 1981; Roberts, 1979). Samuelson et al. would have been more reassuring if they had discussed exactly who would impose the authoritarian solutions they see as necessary and exactly how those "short-term" solutions can be prevented from becoming the long-term status quo. There are important value differences here, and the priorities of those "who are committed to maintaining the social system essentially in its current form" (Fox, 1985b, p. 48) may not match the priorities of those who would welcome radical change in order to better meet their social, psychological, and physical needs.

Turning to the kind of decentralized society I see as necessary to attain a better balance between individual autonomy and psychological sense of community, Samuelson et al. say that I assume "*most* people would prefer to live in small, autonomous communities" such as kibbutzim and communes, and that at the same time I am ready to "deny people the right to live in large, anonymous communities where few demands are placed on them" (p. 228). I did not say most people would now prefer life in communes; in fact, I discussed reasons that a decentralized society would be "unappealing" (p. 53) to many. The point is that psychologists, who are well suited to assess the mental health outcomes of different settings, should be in the forefront of those who insist that our current society is detrimental to psychological well-being. The fact that many people would choose to retain their anonymity and isolation is a symptom of a society gone wrong, and psychologists should be wary of considering that choice to be a healthy one. We must become more willing to proclaim clearly that an alternative society would better suit our needs, and then move on to investigate noncoercive methods of bringing that alternative about. (For recent discussions of problems associated with individualism, isolation, and materialism in American life, see Bellah, Madsen, Sullivan, Swidler, & Tipton, 1985; Wachtel, 1983.)

The problems of population increases and intercommunity cooperation pointed out by Samuelson et al. are indeed serious ones, requiring much research at least partly along the lines suggested by Stern. As I noted, there is "no guarantee that even a significantly decentralized society would be able to resolve the entire multidimensional complex of global and individual problems, because the obstacles are immense, and no single approach will be totally successful" (p. 49). All solutions have accompanying problems. The decentralist approach at least has the virtue of moving beyond a single-minded focus on saving the environment—which it does have the chance of doing at least as well as alternative approaches—to allow for the equally important possibility of fulfilling human needs and values.

Samuelson et al. note that "In the 'real world,' face-to-face communication is likely to be the exception rather than the norm, especially in social dilemmas involving large numbers of strangers" (p. 228). Their conclusion seems to be that face-to-face communication is irrelevant to their research program. My conclusion, in contrast, is that research must explore how face-to-face communication in the real world can be increased, and how the problem of "large numbers of strangers" can become a thing of the past. Such a conclusion is in keeping with the benefits of long-term, comprehensive, even "utopian," change, along the lines suggested by the many anarchist and decentralist psychologists and political theorists I cited (see also Fox, 1984b). Consequently, Stern's advocacy of a "social psychology of solidarity" and of evaluating change efforts "in terms of their effects on community" (p. 230), his suggestions for empirical research to resolve expected problems, and his earlier focus on small-community management of commons dilemmas (Stern, 1978; Stern & Gardner, 1981) strike me as more optimistic and potentially liberating than does a narrow, defeatist focus on the supposed inevitability of the here-and-now. The real promise of social psychology is to be found in the creation of new possibilities of social life, not in the technical manipulation of people in the current cultural context.

Perhaps the most crucial point is that psychological debate cannot be divorced from political ideology. Those who take comfort in the safety of established "objective" procedures are often the least aware of how their own underlying assumptions affect their research problems, their methods, and their conclusions. It is important to make explicit our own political perspectives and value priorities and to take clear stands on the range of controversial issues that affect our work, as I have tried to do in my original article and elsewhere (Fox, 1983, 1984a, 1984b, 1985a, 1985b, in press–a, in press–b). Inevitably, psychologists who want society to move in a direction that is positive for people as well as for the planet must consider more closely the intertwined complexity of their politics, their theories and methods, and their professional and personal lives.

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