Why Do We Still Call Them "Paradoxes"?*

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The fact that therapists label some events "paradoxical" may suggest that our current beliefs or theories are limited in their ability to adequately account for those phenomena. It is argued that our underlying belief in objectivity surrounds therapeutic "paradoxes" with a persistently paradoxical aura, and leads to confusion in our understanding of a variety of phenomena. Maturana's ideas regarding structure determinism, instructive interaction, and phenomenal domains are used to suggest an answer to these difficulties. It is claimed that the problematic status of many theoretical concepts (for example, communication, information, resistance, homeostasis, and pathology) is revealing of something quite important—that the experiential validity of instructive interaction repeatedly leads us into implicitly or explicitly employing instructive interaction in a domain where it can never be valid: the domain of theory and explanation.

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THE THINKING AND BEHAVIOR of a therapist is guided by a set of assumptions or beliefs about the nature of human beings, the nature of human problems, and the nature of the mechanisms whereby therapeutic change occurs. These beliefs generate a set of interventions that "make sense" (to that therapist). In other words, these "sensible" interventions are those that—given the therapist's assumptions about human beings, human problems, and the nature of therapeutic change—seem likely to be successful. Conversely, the therapist's assumptions also implicitly specify a (much larger) set of interventions that do *not* "make sense." This second set of interventions are those that, because they are inconsistent with the therapist's assumptions, seem likely to fail (or even to make matters worse). It is in this latter set of interventions that so-called paradoxes would be expected to be found. Why? *Because we call them "paradoxes."* It hardly seems likely that a therapist would call an intervention "paradoxical" if that intervention is consistent with his or her theoretical beliefs (4).

Now, obviously, many therapists who regularly use paradoxes would say, "But I *don't* find these interventions to be paradoxical or nonsensical. They may be paradoxical to my clients, but not to me. Why would I use them in the first place if they did not make sense to me?" For the moment, my response to this quite justifiable reply is in the form of another question: If these interventions *do* make sense, then why call them *"paradoxes"*?

My intention, at this point, is to focus our attention on our use of this word, "paradox." Perhaps, as some therapists would claim, we use the label, "paradox," because these interventions are paradoxi-

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cal to our *clients* (but not to us). This may be the case, but I am unconvinced: First, the term "paradox" is used by many therapists who pay no attention whatsoever to the question of whether their "paradoxical" interventions might be paradoxical to their clients. Second, many so-called paradoxical interventions are explicitly framed so that the paradoxes *make sense to the client.* And third, the term, "paradox," is frequently used in the professional literature (and colloquially in therapists' discussions) to refer to interventions that clearly are not paradoxical to the client. Why, then, do we continue to use the term, "paradox"?

Belief and Paradox

We human beings seem to be powerfully attached to our beliefs and expectations about the world in which we live. When faced with having to *change* a belief—that is, when events fail to substantiate our expectations—we often seem to be more willing to invalidate the data before us than to invalidate our belief. Thus, we may deem the data to be spurious; we may judge the situation before us to be defective or malfunctioning (7, 8); we may label the observed phenomenon "a paradox."

When we call something a "paradox," we may or may not be willing to confront it. On the one hand, we may choose to confront it by wrestling with the difference between (a) what our belief predicted would happen and (b) what we actually observed to happen (that is, the paradox). On the other hand, we may use the label "paradox" to *avoid* confronting this discrepancy. When we take this latter course and avoid coming to terms with paradox, the paradox becomes embedded within our theory or belief like a dormant cyst. When that happens, the belief or theory can remain impervious to the paradox almost indefinitely.

Science is generally willing to confront its paradoxes. When a paradox is identified by the scientific community, it rapidly becomes the target of considerable effort to reconcile it with the currently accepted theory. Physics, for example, frequently uses the label, "paradox," to identify an as yet unexplained phenomenon. As a consequence, the paradox is consensually understood to be the next major puzzle that this particular branch of physics must solve. In contrast to these physicists, however, psychotherapists may be somewhat less willing to confront their paradoxes.

By and large, therapists are (understandably) more attracted by the practical question of how to use paradoxes than by the theoretical question of how paradoxes are to be explained. Accordingly, therapists have generated a large body of knowledge regarding the clinical use of these interventions. In this applied, clinical sense, "paradoxical" therapists unquestionably understand what they are doing and, indeed, find these interventions to be eminently sensible. In no way do I mean to contest this.

The point of this essay (as it pertains to paradoxes) is that we lack an adequate and comprehensive explanation of these interventions. *What is needed is a theory that will integrate paradoxes so well with our larger superstructure of theory that these interventions will no longer appear to us to be even vaguely paradoxical.* With the possible exception of the MRI explanation, that the solution maintains the problem (25), such a theory has not yet been developed. Instead, the psychotherapy literature now contains a fair number of *post hoc* mini-theories about paradox that are, at best, only loosely integrated with any comprehensive theory of people, families, and therapy.[1]

[1] Paradox and its mini-theories are probably a good example of what Bateson (1) called "imperfectly defined explanatory notions" (p. xviii). Bateson contended that the behavioral sciences have been enor-

From all of this, I draw two conclusions. First, in contrast to my earlier thinking (4), and in spite of our mini-theories and clinical ability to apply these interventions, I am now convinced that there is *still* an important sense in which paradoxes do not quite make sense to us. That is, having found no convincing reason for our continuing to call them paradoxes, I conclude (despite our clinical competence with such interventions) that we still—in some sense—find them to be paradoxical. Second, our failure to completely explain these interventions (so that their paradoxicalness absolutely vanishes) may mean that we are protecting (or, at least, acting in accordance with) some important assumptions. If this is the case, then the concept of "paradox"—and its accompanying mini-theories—may actually be serving to safeguard these assumptions by confining paradoxes to a relatively sterile cyst within the corpus of our larger theory. Berenson (3), in fact, has argued that labeling and mini-theories are currently protecting our conventional Western epistemological assumptions from a variety of theoretically disquieting phenomena (for example, hypnosis, religious conversion, spontaneous remission, placebo effect, paradox, and so on).

It is only for very compelling reasons that paradoxes (or other phenomena) are encysted via this process of labeling and mini-theories. Typically, the compelling reason lies in (a) the convincing nature of our past experiences, and (b) the extent of the disruption that these paradoxes would bring if our basic assumptions were required to accomodate to them.

The Copernican Paradox

The shift in world view that Copernicus necessitated was enormous—both cognitively and emotionally. Its *emotional* impact has often been emphasized: "Copernicus' heliocentrism brutally and insultingly removed man from the centre of things and placed him at the periphery" (13, p. 29). The enormity of the *cognitive* shift that heliocentrism required became apparent as soon as *De Revolutionibus Orbium Coelestium* (1543) was published. The world divided into two camps: those who believed Copernicus and those who continued to believe that the sun revolves around the earth. That, of course, is not surprising. The surprising fact is that, *for years afterward, even the scientists who believed Copernicus referred to his heliocentric model as "The Copernican Paradox"!* To see the earth revolving around the sun as being "natural" rather than "paradoxical" was just too much to achieve quickly. Today, we who have grown up with the Copernican model see it as natural that the earth revolves around the sun; despite our experience of the sun rising in the east and setting in the west, we find it almost impossible to see the Copernican model as a paradox.

At present, therapists seem to be divided into two camps with regard to the issue of paradoxical interventions: (a) those who believe in (and advocate the use of) paradoxes, and (b) those who do not believe in them (and who, perhaps ironically, advise against using them). Like the followers of Copernicus, who achieved an initial understanding of a heliocentric solar system, so, too, have the paradoxical therapists achieved a certain clinical understanding of paradoxical interventions. Morever, it is my hunch that paradoxical therapists find these interventions to be just as much a

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mously hampered by their penchant for generating "imperfectly defined explanatory notions" (for example, ego, intelligence, maturity, locus of control, introversion, and so on), each of which have their own body of theory and research but are *minimally related to one another*. As such, therapeutic paradox can become more perfectly defined only by being adequately related to a more general or comprehensive theory.

paradox as did the early believers of Copernicus find the heliocentric world view to be a paradox. Why? Because, like the early followers of Copernicus, paradoxical therapists continue to use the word "paradox."[2]

I believe that when our theories have completely accomodated to these paradoxical interventions (to the same degree that we have all accomodated to the heliocentric world view), then we will stop calling these interventions "paradoxes." In addition, I believe that we therapists will no longer see the outcome of such interventions as being in *any* sense paradoxical. Instead, we will see the world operating according to what we have come to accept and understand as the "natural" order of things.

"Symptomatic" Behavior and Natural Selection

The principle of natural selection is a simple tautology: Those organisms that can survive in a particular environment do survive (and reproduce); those that cannot survive in that environment die. The same principle can be applied to behavior. A behavior that continues to be exhibited by an organism is obviously compatible with the environment in which it is being manifested. Conversely, a behavior will cease whenever the environment is no longer compatible with it. Compatibility is here defined in the same empirical, tautological manner as is survival and natural selection. Those behaviors that are compatible with their environment survive; those that are not compatible cease.

The symptomatic behavior of a client is compatible with that client's environment—by definition. If the symptomatic behavior were not compatible, then it would cease. Therefore, when a client comes to see a therapist, the therapist can know that there has been nothing in that patient's environment that is incompatible with the continued existence of that symptom. This is necessarily so. If the person's environment *were* incompatible with the existence of that symptom, then the symptom would no longer exist and the person would not be in the therapist's office. This tautological conclusion is obvious, but it is not trivial. It is not trivial because one can assume (unless one is given to excesses of arrogance) that the patient and his or her significant others have tried their best to solve the client's problem. They have tried their best to make the environment incompatible with the continued existence of the client's symptomatic behavior. And they have failed: spouse, friends, children, parents, employers, police, schoolteachers, previous therapists, and the client him- or herself.

Each of these people has his or her own beliefs about the nature of human beings, the nature of human problems, and about how one solves these problems. Given the number of people who are likely to have been involved, it is probable that they constitute a fair cross section of our culture's beliefs regarding people and the nature of their problems. This collective set of beliefs generates a set of interventions that "make sense" or are "logical." If one is willing to credit the client and his or her significant others with a reasonable degree of effort and persistence, then two interrelated things can be said. First, the general set of cultural beliefs (about how to change that client) have been proven to be at least inadequate if not flatly incorrect. The more chronic the problem, the more strongly we can make the latter assertion. Second, almost everything that "makes sense" to try or that is "logical" has already been tried with that client—and has been proven not to work. Again, the more chronic the problem, the more strongly this

[2] This statement is not completely accurate. An increasing number of unconventional therapists are beginning to avoid using the word "paradox." I think that these therapists constitute the leading edge of a shift in paradigm that is currently underway in several fields (see, for example, 3, 11, 14, 15, 19, 22, 23, 24).

second assertion can be made. Finally, it is perhaps humbling to consider that the more chronic the problem, the more strongly the second assertion can be made about *therapists:* The interventions that "make sense" to therapists have been unsuccessful too.

If everything logical has already been tried and has been proven to be ineffective with a client, then what remains to be tried? What remains, of course, are interventions that do not make sense, interventions that are paradoxical.

Now, admittedly, one can dispute this contention—that only paradoxical interventions are left—in at least two ways. First, one might claim that interventions, which previously did not seem to make sense, are now seen in a new light and are found to make a great deal of sense. This is certainly true and even quite likely. In fact, a growing number of therapists (a) are successfully treating such chronic problems with interventions that may not make sense to the previous helpers who were involved in the case; (b) are finding these interventions to be eminently sensible to themselves; and (c) do not call these interventions "paradoxes." Nevertheless, what are we to make of the fact that successful interventions are often called "paradoxes"—even by the therapists who use them?

Second, one might argue that the interventions that remain to be tried are not paradoxical, but are, instead, simply *unknown.* One could just say, "I don't know how to solve this problem." This argument is quite defensible, but its applicability is severely circumscribed. Specifically, one can make this claim (that the interventions which remain to be tried are not paradoxical, but unknown) *only* if one admits that one does not know what to do, *only* if one admits that one's present theory is not working. I think that we very seldom concede the defeat of our theories. We may concede defeat in solving the problem, but we tend to hold firmly to our theoretical beliefs about how to solve that problem (25). This is as true of us therapists as it is of our clients and their significant others (who are trying to solve the client's problem). As long as we refuse to allow failure in therapy to call our theory into question, then successful interventions, which are inconsistent with that theory, must be seen as paradoxical.

What, then, is the bottom line with regard to paradox in psychotherapy? I suggest that today's therapists are being confronted by anomalies (paradoxes) that herald the need for a theoretical revolution (16). In particular, I think that certain phenomena are implicitly experienced as paradoxical because they conflict with important basic assumptions that we hold. Thus, although this paper is ostensibly about paradoxes, it is actually about something much larger. I take our preoccupation with paradoxes to be both a symptom and a signpost that points the way to that larger issue.

Structure Determinism

In the following account, one assumption is made: All living systems behave according to their structures. That is, when an organism interacts with some aspect of its environment, the structure of that organism determines how it will respond. This, in fact, is the fundamental assumption of science. Science assumes that, when probed in a particular way, an object will reveal its own reliable, characteristic way of behaving (when probed in that particular way). Thus, it is not the probe that unilaterally determines the object's reaction, but the object that unilaterally determines its reaction to the probe. Maturana (18) calls this assumption "structure determinism."

The primary correlate of structure determinism is that objects (including living systems) do not undergo "instructive interaction" (18). That is, a professor's lecture

does not determine what the students come to understand; instead, it is the structure of each student that determines the fate of the professor's lecture. Although we speak of classroom lectures in terms of "instruction," such instruction is not lineal. The professor cannot unilaterally cause the students to understand the lecture. Similarly, a robber with a gun does not unilaterally cause the victim to surrender his or her money; some victims faint, some scream, some run, some counterattack. It is the structure of the potential victim that determines what happens when the robber says, "Your money or your life." From this, we can see the following: Even if the victim gives his or her money to the robber, the victim is still not undergoing instructive interaction; the victim is simply behaving according to his or her structure. The apparent illusion of instructive interaction is due to our success in correctly calculating the structure-determined response of an object (or organism) to a particular event in its environment. Thus, because we know that the vast majority of humans will give their money to a robber, we (and the robber) may experience the illusion that instructive interaction is possible. It is not.[3]

If clients were able to undergo instructive interaction, then therapy would be an effortless endeavor: The therapist would make his or her intervention and every client would respond identically to that intervention. Unfortunately (perhaps fortunately), this does not occur. Instead, the therapist intervenes and the client's structure determines the fate of the therapist's intervention.

In observing the fate of that intervention, the therapist has the opportunity to learn something about the client. If the intervention succeeds, then the therapist learns that his or her view of the client was correct: The client *is* changed by that sort of intervention. If the intervention fails, then the therapist may learn that his or her view of the client was incorrect: The client is *not* changed by that intervention. This latter lesson, however, is often not learned. Despite the visible evidence that the intervention did not change the client, we therapists often continue to believe that the intervention in question will or should change the client.

As therapists, we typically respond to the failure of our interventions in one of three ways. We decide that our theory and view of the client is correct, *but* that (a) "I did the intervention incorrectly," (b) "I didn't do it long enough," or (c) "It should have worked, but there is something wrong with this client" (that is, the client is uncooperative, resistant, pathological, not ready for therapy, and so on). In other words, we are able to interpret or disqualify the data so that our theoretical assumptions are preserved. (See, especially, the work of Shaw, 22). And, because our theories remain intact, we then continue to make interventions that are specified by those theories. Watzlawick, Weakland, and Fisch (25) call this the "more of the same" phenomenon.

This "more of the same" phenomenon, of course, is not limited to our functioning as therapists. We are *always* reluctant to surrender our beliefs, particularly those beliefs that are most important to us. Max Planck knew whereof he spoke when he said that "a new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents gradually die, and a new generation grows up that is familiar with it" (20, pp. 33–34). It is inevitable that this general tendency to conserve our beliefs plays a role in insulating therapists

[3] In his last book, Bateson (2) adapted his concept of the "dormitive principle" and used it as a label for those explanations that gave credence to the possibility of lineal, unilateral, or instructive interaction.

from the (potentially) theoretically corruptive impact of paradoxes. Still, there must be more than Planckian conservatism involved in the puzzle of why we still call certain interventions "paradoxical." This essay will now argue (a) that our slowness to normalize (to de-paradox) our therapeutic "paradoxes" is due to our deep-seated belief in *objectivity,* and (b) that our conceptual difficulties with paradox and objectivity are, in turn, due to our tendency to act as if instructive interaction is possible.

The Myth of Instructive Interaction

Maturana (18, 19) and many constructivists (24) have provided the ultimate epistemological insult: We are fundamentally incapable of perceiving the world objectively. Worse, we can have no valid grounds for contending that there is an objective reality that exists independently of us (14, 15, 18, 19, 23). As noted above, structure determinism directly implies that we are incapable of instructive interaction. And, because we are incapable of undergoing instructive interaction, it is impossible for us to receive objective information about the world around us. The world does not determine how we perceive it; instead, our structure determines how we perceive or bring forth a world. Thus, we are not the passive, innocent recipients of "the" world's information. In one sense, we all know this. Yet, there is evidence that *we repeatedly seem to forget that instructive interaction is impossible.*

Objectivity

The world we see is not a world which objectively is; the world we see is the one that our structure-determined existence in the medium brings forth (19). Although humans can never see an objective world, we frequently argue about what "really" happened and who saw things "correctly." The point of structure determinism is that each person sees things "correctly"—according to his or her own structure—but that there can be no absolutely correct view. Any view that is agreed upon as being the correct (or objective) view is just that—*agreed upon.* It is a consensus.

This matter of the impossibility of objectivity is, in itself, certainly nothing new. The history of this position begins at least as early as the Pre-Socratics (if not earlier) and continues to the present day in the work of the constructivists and others (see, for example, Watzlawick's *The Invented Reality,* 24). Given this earlier work, it is certainly not my intent to assert that the impossibility of objectivity is a new idea. The essential message of this article resides in its twofold claim *about* the impossibility of objectivity: (1) that we have not yet realized the breadth of the ramifications attached to this impossibility of objectivity[4]; and (2) that a fundamental grasp of the problem of objectivity—one that readily reveals the extent and pervasiveness of the

[4] Von Glasersfeld (24, 25) refers to those who fail to take seriously the problem with objectivity as "trivial constructivists." Trivial constructivists acknowledge that our perceptions and knowledge of the world may not be objective, but—failing to take seriously the problem with objectivity—they continue to try to discover the "objective truth" about (what they believe is) a real world which exists independently of any observer. In contrast, radical constructivists contend that it makes no sense to speak of any reality apart from the operations of the observer (see also 12). Von Glasersfeld (14, 15) and Stolzenberg (23) provide particularly clear accounts of this latter view of reality.

Maturana, however, says that he is *not* a constructivist for the following reasons: (a) he claims that an observer considers cognition to occur in a living system whenever he or she accepts the system's behavior as adequate behavior in a domain that he or she specifies; (b) that we bring forth at any instant the world that we bring forth in social domains as the only one that we can bring forth, and that all alternative worlds exist only in our descriptions; and (c) that his explanation of cognition is not a model—which is what von Glasersfeld considers radical constructivism to be (15)—but an ontological explanation.

problem—requires a knowledge of its underlying mechanisms: (a) the structure-determined functioning of human observers who operate in structural coupling with the medium in which they exist, and (b) the impossibility of instructive interaction. Knowledge of these latter mechanisms has only recently become available with the radical, biological ontology of Humberto R. Maturana (see 11).

Communication

Inasmuch as what we see or hear can only be what we each uniquely bring forth in our structure-determined interaction with the medium in which we exist, then disagreements about what was objectively communicated (for example, "You didn't hear what I said"; "You aren't listening to what I am saying"; "You are deliberately misunderstanding what I am saying") reveal our underlying reliance on an objective world that actually does not exist. Such a disagreement may eventually result in the party of the second part understanding what the party of the first part wants him or her to understand, but that understanding does *not* establish the objective nature of what was communicated. Successful communication is consensual, not objective.

Reinforcement

Naive students of learning theory often have trouble grasping the fact that reinforcers have no objective existence. Instead, each organism responds to certain environmental contingencies by increasing (or decreasing) the frequency of a particular behavior. The reinforcement that occurs is, of course, the organism's *response* to the contingency; the contingency is a reinforcer only *relative* to the structure of that organism. A behavior therapist cannot go to the store and buy a box of reinforcers; there are no objective reinforcers. The everyday pervasiveness of believing in objective reinforcers is revealed by the frequency with which parents and teachers stubbornly continue to use unrewarding "rewards" and nonpunishing "punishments."

Information

Structure determinism directly implies that our conventional understanding of information is in error (18). We speak of teachers as having information that they impart to their students, but that information can have no objective existence. The teachers may perceive the information as being objective, but such apparent objectivity is actually a manifestation of their consensus regarding that information. "Information," when given to the students, does not determine what they will learn; instead, their structures determine how they will respond (what they will make of) the information. For the same reasons, there can be no such thing as an objective *stimulus*. In an experiment that uses a standard stimulus, each organism in the experiment defines the nature of that stimulus by how it responds to it. As Ruesch and Bateson (21) have noted with regard to communication, the meaning of a communication is the response that it produces. Similarly, the nature of a stimulus is defined by the response that it elicits—and it is different for every organism, including the experimenter and each reader of his or her article. There is, however, (usually) sufficient consensus in the scientific community regarding the operational definition of the stimulus, that (a) it may appear to be objective and (b) research is able to proceed.

Paradoxical Responses and Paradoxical Interventions

Responses are perceived to be paradoxical because the observer (who considers the response to be paradoxical) believes that a particular stimulus is objective. For exam-

ple, the "paradoxical," calming effect of "stimulants" on a hyperactive child is paradoxical *only* because the observer has slipped into believing that amphetamines are objectively stimulating.[5] They are not. The effect of an amphetamine is determined by the structure of the organism, not by the amphetamine. Similarly, paradoxical interventions are "paradoxical" because the observer (who considers those particular interventions to be paradoxical) is, at some level, believing that the interventions in question are objective. They are not. The effect, for example, of telling a person to practice his or her symptom, is determined by that person's structure, not by the supposedly objective or instructive nature of the intervention (which might lead us to expect that practicing the symptom would make it worse).

Resistance

The concept of resistance, as it is colloquially used by most therapists, is only intelligible if one believes that instructive interaction can or should take place. That is, the notion of resistance assumes that the therapeutic intervention (which is supposedly being "resisted") *ought* to have elicited a certain response (or a certain general class of responses). When the client fails to manifest that certain response (that is, when the client fails to be correctly "instructed" by the therapist's intervention), then the client is considered to be "resisting." On the other hand, if this situation is conceptualized in terms of structure determinism, there is no resistance: The patient is responding to the intervention in the only way that he or she can—according to his or her structure. To call that response "resistance" is (a) to refuse to accept that the patient's structure is the way it is, and/or (b) to insist that instructive interaction should have taken place but that the patient is willfully or unconsciously preventing instructive interaction from occurring, and (c) to fail to see that one has used an ineffective or inappropriate intervention (see also 6).

Homeostasis

A system that remains unchanged in the face of certain interventions is often said to be "homeostatic." Such so-called homeostatic systems may appear to be maintaining themselves the same, or, said differently, may appear to be resisting change. To formulate homeostasis in this fashion, however, is to see homeostasis as being a form of resistance. To speak of the system as maintaining itself the same (or as resisting change) is to imply that change should have taken place—but that it was prevented. It is to imply that instructive interaction could have or should have occurred.

Homeostasis is not an objective property of the system; it is a relational phenomenon (10). *Relative* to a particular set of interventions, a system—which is behaving according to its structure—remains unchanged. Relative to a *different* set of interventions, the system will change (10). To describe a system as "homeostatic" is

[5] One might claim that physicians do not really believe that amphetamines are objectively stimulating. That is, perhaps they call the hyperactive child's reaction to methamphetamine a "paradoxical response" *only* because that response differs from the typical or expected one. It is certainly true that the response is not what one would expect from an amphetamine, but the term "stimulant" clearly reveals that we have objectified and made unilateral the supposed stimulating ability of the "stimulant." In addition—and more importantly—the use of the term "stimulant" reveals our manner of dealing with the everyday, expectable world: *We habitually reduce the relational interactions of everyday situations (e.g., the amphetamine-organism interaction) to the action of lineal agents ("stimulants") on passive recipients.* Such reductions may, indeed, be a shorthand convenience, but the habit of these reductions repeatedly leads us to speak and act as if instructive interaction is possible, and generates "paradox" whenever the expected, instructive interaction does not occur.

actually to say that interventions, which one had believed *would* change the system, do not change the system.

Pathology

Like resistance and homeostasis, pathology is often understood in ways which imply that instructive interaction could or should have taken place. The claim that an organism is pathological—that something is wrong with it—implies that the organism ought to be responding differently than it is responding. It implies that the present set of environmental circumstances *should* be eliciting different behavior from the organism. But why should it be behaving differently? When we think that the organism should behave differently, and when we label it as "sick" for not behaving differently, it is clear that (1) we are unwilling to accept the structure of the organism as it is (that is, we claim that there is something wrong with it), and/or (2) we think that instructive interaction should have taken place but that the organism failed to be correctly instructed because it is defective. This kind of reasoning is quite evident in what is probably the prototype for understanding pathology in terms of instructive interaction: psychiatry's concept of *reality testing*. A person who does not see the world "correctly" (one who fails to be "correctly" instructed by "reality") is considered to be defective or pathological. Reality, however, is not objective. Reality is consensual.

The Underlying Question

We now see that the question with which we began—"Why do we still call them 'paradoxes'?"—has developed a deeper refrain: Why do we believe (or at least act as if we believe) that instructive interaction is possible? I think the answer is twofold: (a) our lineal experience tells us otherwise and (b) we are reluctant to pay the cognitive and emotional costs which such a shift in our world view would exact from us.

The Experiential Validity of Instructive Interaction

In addressing ourselves to the questions about the nature of paradox, objectivity, structure determinism, instructive interaction, and so on, it is crucial to draw a distinction between the domain of *experience* and the domain of *explanation* (see, for example, 19). All concepts—such as paradox, objectivity, and so on—are *representations* or *explanations* of the experience of observers. In no case, however, can experience be replaced by concepts, representations, or explanations. These are nonintersecting phenomenal domains that cannot be reduced to one another. Yet, theories and hypotheses (which reside solely in the domain of explanation) can be empirically validated only in the domain of experience (19)! That is, a hypothesis or theory specifies (at least implicitly) the operations that an observer must carry out in order to obtain (in order to experience) empirical validation of the hypothesis or theory in question (see also Stolzenberg, 23). So, experience validates explanation.

Why are we so persistent in believing in objectivity and instructive interaction? I think part of the answer is that *we experience an objective world.* Said differently, our experience of the world we perceive is instructive—and necessarily so, for experience can *only* be experienced instructively.

Experience is constitutively instructive or lineal. When someone hits me with a rock, my experience is that one aspect of my experience—the rock—unilaterally (instructively) causes another aspect of my experience—my pain.[6] Similarly, if I throw

[6] The assertion that experience is constitutively lineal or instructive remains valid even if it can be shown that (a) causality is a genetic construction of the child (as Piaget contended), and (b) causality can never be logically justified by the sequence of experience that leads us to make *post hoc ergo propter hoc* causal assumptions (as Hume contended). Both Piaget and Hume were referring to explanation rather than to experience.

a rock across the street, my experience is that my throwing (which is one aspect of my experience) causes the rock-to-fly-across-the-street (another aspect of my experience). In the domain of *explanation,* however, I will insist that these incidents were *not* examples of instructive interaction or lineal causality, but rather were structure-determined interactions among my body, the rock, the earth's gravitational field, and so on. Thus, although we necessarily experience events instructively, our use of the concept of instructive interaction (or lineal causality) to *explain* those same events is inadequate.

Still, our lineal (instructive) experience of an objective world is obviously quite adequate for our existence as living creatures. That is, this instructive experience is the only experience we have; it allows us to live, function, and do all the things we do. Only when it is employed in the domain of explanation is our instructive experience inadequate (in the domain of explanation). And yet, our instructive experience of an objective world is absolutely compelling. As a result, we often confuse domains by using our compelling instructive experience to characterize or explain the world we perceive. Whenever we do this, however, (1) we will believe in an independent reality, (2) which will lead us to believe in strange notions of communication, resistance, homeostasis, and objective pathology; and (3) we will consign ourselves to struggling with an increasingly convoluted set of explanations that are trying (unsuccessfully) to explain those strange objective notions of communication, resistance, homeostasis, and so on.

The Costs of Surrendering Our Belief in Objectivity

The emotional costs of abandoning our belief in objectivity (and, hence, our "paradoxes") are that we lose certainty, and that we must assume responsibility for "the" (our own) world. If we endorse the thesis of structure determinism, then we must accept that objectivity is impossible. Having done that, we could no longer be certain; we could no longer claim that we know how the world really is. In compensation for this loss of certainty, we would be given the burdensome gift of knowing that we are responsible for our own perceptions and reactions (see also 9). We are responsible for our perceptions and reactions because *we*—not a supposedly objective world—generated them. We could no longer claim that objective circumstances instructed us to behave this way: "Our cozy expedient of attributing blame to others or to circumstances would no longer be available" to us (24, p. 327).[7]

The cognitive costs of embracing structure determinism are myriad, but they can be summed up in a phrase: Our world view is called into question. We could no longer see resistance, homeostasis, paradox, and pathology in the same old way. We could have no certainty about what to fix (that is, pathology) in therapy and could no longer use resistance and homeostasis to explain why we sometimes fail to produce change. We would normalize our paradoxes. We would be required to reconsider seriously the nature of information, communication, and even our existence on the planet. The human condition would necessarily be subject to re-evaluation. And, most important of all, consensus would be seen to be a fundamental aspect of existence rather than merely a negotiated, social phenomenon (see 11, 18, 19).

A Final Remark

The reader may or may not be convinced by my thesis that we call certain interven-

[7] To my knowledge, such an ethics of responsibility was first presented at a Wenner-Gren conference on cognition in Chicago, March 1969, by Maturana (see 17). Since then, this ethic has been articulated by von Foerster, von Glasersfeld, Varela, Watzlawick, and others (see 24).

tions "paradoxical" because we harbor an underlying belief in objectivity (that is, instructive interaction). Whichever, I hope that at least I have infected the reader with my question, "Why do we still call them 'paradoxes'?" I think that we owe ourselves an answer to that question.

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