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THE PERSISTENCE OF CATEGORY MISTAKES IN PSYCHOLOGY

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ABSTRACT: Gilbert Ryle's book *The Concept of Mind* was published in 1949. According to Ryle, his "destructive purpose" was to show that "a family of radical category mistakes" is the source of the "official doctrine," that is, a "double-life theory," according to which "with the doubtful exception of idiots and infants in arms every human being has both a body and a mind." By numerous examples, Ryle showed quite forcefully how psychology and philosophy at the time were misled into asking the wrong kinds of questions. More than 50 years have elapsed since the original publication of Gilbert Ryle's book and, as Ullin T. Place wrote shortly before passing away, Ryle's conceptual analysis is now due, if not overdue, for a comeback. The purpose of this paper is to discuss the persistent relevance of category mistakes to current problems in the analysis of behavior.
Key words: conceptual analysis, category mistakes, materialism, dualism, inference tickets, explanations.

It has been said about Gilbert Ryle that "the man was a philosopher, not a scientist . . . in particular, he was not concerned with advancing empirical knowledge about behavior and its controlling circumstances" (Schnaitter, 1985). In contrast, although Ullin T. Place learned from Ryle and his contemporary British philosophers, he also kept an enduring interest in the empirical science of behavior. Thus, through his frequent participation in behavioral conventions, meetings, books, and journals, many behavior analysts learned to appreciate Ullin's contributions, his enthusiasm for the subject matter of behavior and philosophy, and his gentleness.

An early version of the present article was presented in a Symposium on *Category Mistakes in Psychology* at the *Fourth International Congress on Behaviorism and the Behavioral Sciences* in Seville, Spain, in November 1998 (Holth, 1998). After the session, Ullin came up to continue the discussion and offered to send over copies of some of his recent works that he felt were relevant. About six weeks later, I received a letter from Ullin with the promised articles along with an apology for the delay of his post: "The reason for the delay is that on my return from Seville the cough from which, as you may have noticed, I was then

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suffering was diagnosed as lung cancer for which I am now undergoing chemotherapy in the hope of increasing my life expectancy from 6-9 months to 12-18 months" (U. T. Place, personal communication, January 18, 1999).

One of the reprints that Ullin had sent over was written very recently for O'Donohue's and Kitchener's (1998) *Handbook of Behaviorism*. The title of Ullin T. Place's chapter was "Ryle's Behaviorism" (Place, 1998). He argued in favor of a comeback for Rylean conceptual analyses but added, "Whether a new generation will carry the torch forward remains to be seen" (p. 396). I hope that what follows will be considered in just that vein—as carrying the torch forward.

Why is Not Psychology More Effective as a Basic Science?

Although psychology may have its merits in distinguished areas, it does not seem to constitute a very effective basic science of behavior. As Sidman (1989) noted, "we find experts on the psychology of everything from chess to sex, from computer programming to mental illness, and we can make no generalizations across the boundaries of each area . . . The students, unfortunately, get no systematic account of human conduct that they could then apply to many of their concerns, whether they are faced with the problem of managing a horse or of managing themselves" (p. 52). Standard psychology textbooks continue to start somewhat apologetically, by offering at least two standard excuses for what they have to offer:

- Excuse #1: Psychology is concerned with an exceptionally complex subject matter.
- Excuse #2: Psychology is still a relatively young science.

Regarding the first "excuse," however, few would seriously contend that current research questions in psychology are more complex than research questions now asked in physics, chemistry, and medicine, for instance. More to the point here seems to be the fact that prominent representatives of different research traditions in psychology do not agree on what constitutes progress nor even on what has been found out. (In *Psychology Today*, May 1982, for instance, 11 leading American psychologists were asked to identify the most important examples of progress in psychology during the last 15 years. The only achievement mentioned by more than one of the 11 was the discovery of endorphins—which can hardly be ascribed to the science of psychology at all.)

The second excuse may be even more acclaimed, pointing to, possibly, one of the most popular quotes in all of psychology, namely Ebbinghaus's (1911) comment that "psychology has a long past, but a short history." However, as Wittgenstein pointed out in his *Philosophical Investigations*:

> The confusion and barrenness of psychology is not to be explained by calling it a "young science"; its state is not comparable with that of physics, for instance, in its beginnings. . . . For in psychology there are experimental methods and *conceptual confusion.* (1953, p. 232[e])

Thus, neither the profound complexity of the matters of psychology nor its short age, seem like appropriate excuses for the lack of effectiveness of psychology as a basic science of behavior. Instead, I will argue with Ryle that the reason is ". . . one big mistake and a mistake of a special kind. It is namely a category-mistake. It represents the facts of mental life as if they belonged to one logical type or category (or range of types or categories), when they actually belong to another" (Ryle, 1949, p. 17).

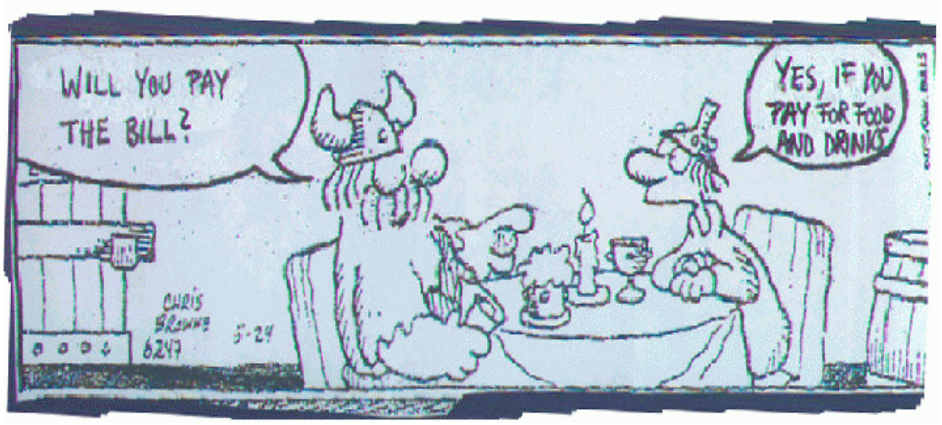

Figure 1: A simple version of a category mistake.

The cartoon (shown in Figure 1) is a very simple demonstration of a category mistake. The bill is conceived of as a member of the same logical category as food and drink, that is, as an additional thing to be paid for. One of Ryle's most well-known examples is that of the first-time visitor to Oxford who is shown colleges, libraries, playing fields, museums, scientific departments, and administrative offices and, then, asks, "But where is the University. I have seen where the members of the Colleges live, where the Registrar works, where the scientists experiment and the rest. But I have not yet seen the University in which reside and work the members of your University" (Ryle, 1949, p. 18). The mistake was to allocate the University to the same category as that to which the other institutions belong. Now, obviously, such mistakes with respect to very discrete events are quite rare. Any person that we would call language-able will have little if any difficulty seeing the logical problems involved in a whole range of similar examples. However, as Ryle pointed out:

> The theoretically interesting category-mistakes are those made by people who are perfectly competent to apply concepts, at least in the situations with which they are familiar, but are still liable in their abstract thinking to allocate those concepts to logical types to which they do not belong. (1949, p. 19)

Furthermore, according to Ryle, a family of such category-mistakes is the source of what he called "the *Official Doctrine*," "the dogma of the *Ghost in the*

Machine," or the *Double-life theory* according to which "[w]ith the doubtful exception of idiots and infants in arms every human being has both a body and a mind" (p. 13). In spite of Ryle's (1949) thorough treatment of the problem, it persists in hampering present-day psychology. Ullin T. Place may have identified one reason why Ryle's writings have not been more influential, namely "Ryle's failure to sharpen up the notion of a category" (Place, 1998, p. 371). As also pointed out by Schnaitter (1985), "although the idea of a mistake is clear enough, Ryle never explicitly defined the notion of a 'category'" (p. 560). On the other hand, Ryle's (1949) series of illustrative examples indicate that the basic problem should not be dismissed that easily. In psychology—including behavior analysis—the error, basically, consists of treating patterns of behavior that can only be identified through repeated observations as if they were singular events. Let us examine some common types of examples.

Some Prototypical Examples

Identity—The Same Word Twice

Logical problems are fairly obvious when the same word is used in the description of a "cause" and its "effect." For example, a picture of the actress Rita Hayworth in a weekly magazine was accompanied by the following text: "Rita Hayworth suffers from a pre-senility that has made her senile and unable to communicate normally with her surroundings" (*Norsk Ukeblad*, 1985, p. 18).

The "senility" in this case was inferred from behavioral regularities over time and did not designate an isolated occurrence of any kind. It is what Ryle called a law-like proposition.

> They apply to, or they are satisfied by, the actions, reactions and states of the object; they are inference-tickets, which license us to predict, retrodict, explain, and modify these actions, reactions, and states. (1949, p. 119)

I will argue later that there is a sense in which such law-like propositions can be said to explain behavioral events to which they apply. However, there is of course no way in which "senility" or anything else reflexively explains itself, and occasional instances of this sort are easily detected by everyone.

Identity—Synonyms

More obscurity arises when two different words are used to designate a "cause" and its "effect," even if the observational basis for those two words are one and the same event. Hence, as Skinner (1969, p. 238) has noted, "The speed and facility with which the mental life of a pigeon or person is reported are suspicious." However, at this level of complexity, psychologists too, including behavior analysts, make category-mistakes. For instance:

- Nonreinforcement decreases rule use **due to** extinction.
- A relatively low rate of responding initially during the session was **due**

to warm-up . . .
- Responses spread to new stimuli **because** of generalization.
- . . . children acquire central conceptual structures, networks of concepts and relations that **permit** them to think about a wide range of situations in more advanced ways.

Dispositions and Occurrences

The more common "causes" in psychology are not themselves events or occurrences at all. Semi-episodic summary labels, that include as their observational basis a number of different kinds of behavior in many different situations, are taken to refer to discrete events or processes that can, logically, fulfill the role of causes for occurrences that the label summarizes. Thus, an "intelligence test" is sometimes used to measure intelligence, and the level of intelligence thus measured is, next, used to "explain" that person's performance on a particular item of the test. Yet, as pointed out by Wessels (1981), although "the proposal that cognitive theories are logically circular is cogent in many instances . . . cognitivists can avoid the problem of circularity by anchoring constructs to logically independent operations" (p. 159).

Obvious problems with circularity are sometimes completely avoided when the application of a summary label or construct is based entirely on the observation of phenomena other than those it is afterwards used to explain. Thus, it has been suggested that ". . . the items on an intelligence test, at the same time that they measure specific content, are also measuring general [basic behavioral repertoires] that apply to various learning tasks" (Staats, 1996, p. 211). The objection may not be that such constructs do not count as explanations but that they do not constitute explanations in the same sense that accounts in terms of historical event variables do. I will return to this point in a later section.

Concealing Category Mistakes by Avoiding "Duplication" Phrases

The laxity, with respect to logical problems in the psychological literature, seems so vast that the only sound strategy may seem to be to consider every occurrence of terms that seem to imply some sort of causal or functional relation as a warning signal. Such terms include "because," "therefore," "resulting from," "hence," "due to," and so on. However, it is possible to avoid these terms and yet preserve the basic mistake. There is a story about a seven-year-old boy and his younger brother who had just agreed that it was about time to start swearing. First, the seven-year-old was asked by his mother what he wanted for breakfast. "Aw, hell Mom, I guess I'll have some Cheerios," said the boy, and was, of course, duly punished. The mother then looked at the four-year-old and asked with a stern voice, "And what do YOU want for breakfast, young man?!" "I don't know," he blubbered, "but you can bet your ass it won't be Cheerios." After becoming aware of descriptions a la Ryle of the logical fallacies that are often involved in the use of

"causal" terms, authors may simply avoid terms, but there is no automatic guarantee against category mistakes. For instance:

- **As** sustained attention improves, children become better at deliberately focusing on just those aspects of a situation that are relevant to their task goals, ignoring other information.
- **With** generalized imitation, the child will also produce novel imitations . . .
- Having applied the set theory definition of the equivalence relation to our behavioral observations . . . , we now find that the emergent conditional discriminations in our original experiment are **no longer mysterious**. All those performances are to be expected if the baseline contingencies have established an equivalence relation.

Although direct "causal" terms are avoided in these examples, "sustained attention," "generalized imitation," and "equivalence relation" all seem to put on some independent existence, apparently with an explanatory role in the excerpts.

Some Examples of (Partial) Awareness of the Problem Through History

The illusion that psychological phenomena can be explained by hypothetical event-duplicates of the phenomena to be explained has been noted occasionally by authors in philosophy and psychology long before Ryle's (1949) systematic treatment. Here are just a few notable examples:

Theophrastus (ca. 300 BC):

> . . . with regard to hearing, it is strange of him [Empedocles] to imagine that he has really explained how creatures hear, when he has ascribed the process to internal sounds and assumed that the ear produces a sound within, like a bell. By means of this internal sound we might hear sounds without, but how should we hear this internal sound itself? The old problem would still confront us. (as cited in Stratton, 1917, p. 85)

Moliere (in his 1673 play Le Malade Imaginaire):

> I am asked by the learned doctor for the cause and the reason why opium induces sleep. To which I reply, because there is in it a soporific virtue whose nature is to lull the senses.

Spinoza (1677/1883):

> There is in the mind no absolute faculty of understanding, desiring, loving, etc.—these and similar faculties are either entirely fictitious, or are merely abstract or general terms, such as we are accustomed to put together from particular things. (Note following proposition XLVIII)

Darwin (1859/1958):

> It is so easy to hide our ignorance under such expressions as "the plan of creation," "unity of design" & c., and to think that we give an explanation when we only restate a fact. (p. 444)

Watson (1930):

> This ancient view led to the philosophical platform called "**dualism**." This dogma has been present in human psychology from earliest antiquity. (p. 3)

> If "*mind" acts on body, then all physical laws are invalid.* This physical and metaphysical naïvité of the psychopathologist and the analyst comes out in such expressions as "This conscious process inhibited this or that form of behavior"; "the unconscious desire keeps him from doing so and so." (p. 296)

However, an occasional awareness of the basic problem of category mistakes is clearly not sufficient to prevent authors from being trapped by the same kind of mistake over and over again. Here are some remarkable examples:

Freud (1916/1978):

> A joke of Lichtenberg's takes a quite special place among these "stupid" jokes: "He wondered how it is that cats have two holes cut in their skin precisely at the place where their eyes are." To wonder about something that is in fact only the statement of an identity is undoubtedly a piece of stupidity. (p. 97)

Although Freud was obviously aware of the duplication fallacy, this did not prevent him from spending much of his scientific career engaged in constructing internal, causal duplicates.

E. B. Holt (1931):

> Yet so great is the reifying power of words that even in so flagrant a case as this the mere name of the phenomenon is accepted by many persons as the *vera causa*. One could have hoped that Molière, in the seventeenth century, had given the *coup de grâce* to such verbal tomfoolery . . . . (p. 4)

After an excellent introduction, and with occasional reminders throughout the book, his aim remained, as Skinner (1938) pointed out, to explain behavior with a conceptual nervous system.

Whimbey and Lochhead (1991):

> One way to improve your analytical skills is to see the types of errors that people frequently make in solving problems, and then guard against making those same errors yourself. (p. 11)

There is no direct allusion here to the problem of category mistakes but, more generally to "failing to approach a problem in a step-by-step manner," "making leaps in logic," and "failing to spelling out relationships fully." Thereafter, the authors proceed with a checklist of errors typically made during problem solving, for instance:

> Student missed one or more words (or misread one or more words) because the material was not read carefully enough. (p. 18)

"Solutions"

Ignoring the Problem

It has become fashionable for psychologists to describe themselves as "eclectic," that is, they ". . . combine what they regard as the most valid doctrines," usually failing to add that these doctrines may even be mutually contradictory. (Eclecticism ~ Greek *eklegein*—to "pick out;" cf. Henle, 1957). This eclecticism is sometimes called pragmatism: "As a pragmatist, I will take as my explanatory schemes whatever will work" (Moravcsik, 1988, p. 119). Thus, many psychologists simply ignore the problem of category mistakes by treating both behavior and mental "events" *concurrently* as dependent variables or explananda and both historical and mental "events" *concurrently* as causes, with no embarrassment whatsoever. Introductory textbooks demonstrate this position when they define psychology as the scientific study of behavior *and* mental processes, and proceed with mixtures of so-called behavioristic *and* cognitive (i.e., cognitivistic) principles. Modern textbooks still describe a double-world target of psychology, as illustrated by the following passages:

> My definition of psychology is a fairly conventional one: The study of the behavior and experience of organisms . . . By experience, I mean such processes as perception, learning, thinking, believing, and feeling, all of which take place within the organism, hidden from the direct scrutiny of an outside observer. (Walker, 1996, p. 1)

> Psychology can be defined as the scientific study of behavior and mental processes. (Atkinson, Atkinson, Smith, & Bem, 1993, p. 4)

Thus, behavior on the one hand, and "experience" or "mental processes" on the other, are treated as members of the same logical category. It follows, as T. R. Miles (1994) has suggested, that one might perfectly well study behavior on Monday, cognition or mental processes on Tuesday, etc.

Materialism and Idealism

Sometimes, embarrassing implications of a psychophysical interactionism are avoided by some form of materialism, idealism, or parallelism. A popular version, of course, is to substitute brain for mind. However, as Ryle pointed out, "The

belief that there is a polar opposition between Mind and Matter is the belief that they are terms of the same logical type. It will . . . follow [if my argument is successful] that both Idealism and Materialism are answers to an improper question" (Ryle, 1949, p. 23). A similar point was made by Skinner (1969), suggesting that "it is a little too simple to paraphrase the behavioristic alternative by saying that there is indeed only one world and that is the world of matter, for the word 'matter' is then no longer useful" (p. 248).

U. T. Place (2000) was impressed with how Ryle's (1949) analysis of mental disposition or states came close to "getting rid once and for all, of the private world of mental events." Yet, Place felt that some "aspects" of mental life (i.e., the purely covert aspects of mediational processes) had proved impermeable to Ryle's dispositional analyses. Hence, Place (1956) developed a mind-brain identity thesis, contending that certain mental processes (consciousness) are identical with certain brain processes. Thus, Place (1999) suggested that, as a result of recently discovered techniques of brain imaging, "perfect correlations" may be identified between "consciousness" and "brain processes"—corresponding to that of water and H_2O. However, investigating such correlations between "consciousness" and specific brain processes, presupposes brain-independent criteria for determining whether "consciousness" is taking place. The premise that there are such criteria implies that the purported remainders of a Cartesian mental world beyond Ryle's analyses must already be shown to consist of phenomena susceptible to a behavioral description, before their correlation with certain brain processes can be investigated. (Cf. Catania, 1998, p. 2, for a corresponding argument with respect to brain changes and the definition of "learning.") Avoiding this trap, Skinner (1974) suggested that "after substituting brain for mind, we can then move on to substituting person for brain and recasting the analysis in line with the observed facts" (p. 216).

Operationism/Methodological Behaviorism

Problems resulting from multiple "meanings" of psychological terms have been sought remedied by extensive application of operational definitions. In psychology, terms from ordinary language are given operational definitions in order to avoid the confusion that results from multiple meanings. The basic problem persists, however, when the operational definition does not remove other usages of the term and researchers do not restrict their conclusions accordingly. For instance, a researcher may be interested in "relations between mood and problem solving capabilities." Operational definitions seem necessary of "mood" as well as of "problem solving capabilities." An innovative psychological researcher may figure out that different mood states may be induced in people by exposing them to different sorts of video clips. Whereas, watching Mr. Bean for 5 min may bring on a good mood, watching a close-up of a hunger catastrophe in Africa may very well evoke a bad mood, and a documentary on the dykes of Holland may have little effect in either direction. As a measure of problem solving capabilities, some sections of standard intelligence tests seem appropriate enough.

Let's suppose that, following careful operational definitions, experimental design, recordings, quantifying, and statistical procedures, the researcher finds a positive correlation between "bad mood" and "good problem solving." Naturally, people get interested in the reported finding. The actual operational definitions no longer seem so important. They seem like some minor nuisances that were only necessary at some stage in order to establish the more interesting "psychological" relations. However, the observational basis for the stated relation between "mood" and "problem solving" remains the fact that those people who watched Mr. Bean for 5 min produced significantly lower scores on the particular problem solving tasks than those who watched the hunger catastrophe video. Yet, the reported general relation between mood and problem solving implies that the same relation holds for every imaginable operational definition of mood and every imaginable operational definition of problem solving capabilities. Hence, as pointed out by Peter Harzem (1986), traditional psychological terms—however well they are operationally defined—serve as smuggling-in vehicles of lawful relations for which there is little or no empirical support. As Wittgenstein (1953) also pointed out:

> The existence of the experimental method makes us think we have the means of solving the problems which trouble us; though problem and method pass one another by. (p. 232[e])

Psychologic

Similar problems have also been repeatedly noted by Smedslund, for instance in a paper titled: "Ebbinghaus, the illusionist: How psychology came to look like an experimental science" (Smedslund, 1987). Smedslund (1991) suggested that a focus on research methodology at the expense of conceptual analyses leads to the "ignoring [of] the possibility of a large-scale waste of time, effort, and money, which characterizes pseudoempirical work" (p. 329). His proposed solution, "psychologic," is "an explication and formalization of the basic conceptual structure of psychology" in terms of ordinary language. This can be seen as an important contribution to the refinement of what might be called the psychology of "inference tickets." As such, it can be considered as a discipline separated from a psychology concerned with historical behavior-initiating events.

Avoiding Reification

A transformation of verbs and adverbs into nouns seems frequently to be involved in the category mistakes of psychology (cf. Chiesa, 1994; Hineline, 1980; Skinner, 1974, 1980; Woodworth, 1934). For instance, Woodworth wrote:

> But, like other learned branches, psychology finds it rather convenient to transform its verbs into nouns. Then what happens? We forget that our nouns are merely substitutes for verbs, and go hunting for the things denoted by the nouns—for substances, forces, faculties—but no such things exist; there are only activities with which we started. (p. 29)

At the extreme, two world wars are explained by "misperception" (White, 1976). However, even verbs may mislead us to "go hunting" for classes of events that do not correspond meaningfully to any particular behavioral class. Verbs such as to "learn," "perceive," "understand," "think," "develop," "feel," "remember," "generalize," "discriminate," etc. do not refer to unitary response classes. These terms summarize a number of observations of different behavior under different circumstances. Although they are verbs and, thus, sound behavioral, they do not correspond to any meaningful behavioral unit. Furthermore, just like the nouns, they almost automatically become causal explanations for all the kinds of behavior that they "summarize." Occasional slips occur even in behavior-analytic texts with suggestions that a particular behavior occurs upon a given setting because the subject has learned, perceived, remembered, generalized or discriminated something. As Skinner (1938) pointed out:

> "The existence of a popular term does create some presumption in favor of the existence of a corresponding experimentally real concept, but this does not free us from the necessity of defining the class and of demonstrating the reality if the term is to be used for scientific purposes" (p. 42).

Although the transformation of verbs into nouns often seems to be involved in category mistakes, the crux of the problem remains even with verbs and adverbs, as soon and as long as they summarize events that do not covary in an orderly fashion. Since the basic problem of world duplication thus persists, just avoiding the transformation of verbs or adverbs into nouns obviously does not go far enough in splintering the basic category mistake.

The Duplication of Explanations in Contemporary Psychology

In addition to the duplication of phenomena that are to be explained in psychology, there is also a duplication of phenomena on the explanatory side. Thus, a second very basic category mistake in psychology arises from two very different types of explanations that are both common in the vernacular. To illustrate these different kinds of explanations, I will elaborate somewhat on Ryle's (1949) example of a glass that breaks when hit by a stone:

(a) Why did the glass break?

—A stone hit it.

(b) Why did the glass break when the stone hit it?

—The glass was brittle.

This is the kind of explanation with which we have massive practice in everyday life. Now, we could also be interested in how to make a brittle object, or how to make an object brittle. Thus, we might ask:

(c) How did the glass become brittle?

The answer would include specifications of the history through which the brittle glass came about—in such detail as would allow us to produce another object that is brittle or to avoid making it brittle.

If we have specified the procedures sufficiently to produce another glass that is brittle in the sense that it will break when hit by a stone, there is no place for "brittleness as an additional event or process to be taken into account in producing a new glass that will break when hit by a stone." "Brittleness," however, includes more than "breaking when hit by a stone." It is not an internal characteristic of the glass. Rather, it describes how the object will be affected by certain kinds of external events. To the extent that those different effects of different kinds of external events covary as a unitary phenomenon, "brittle" characterizes a useful unit. "Brittle" explains what happened to the glass when hit by the stone in the sense that it constitutes what Ryle called an "inference-ticket," (e.g., 1949, p. 119) which licenses us to predict and to retrodict members of the class of events that the semi-episodic term "brittle" applies to. It is relevant to what we can do to smash or to preserve similar objects in the future. Furthermore, it explains the breaking of the glass in the sense of attenuating potentially competing "explanations," such as that it was already broken or fractured.

While the explanation in terms of brittleness will be of common usefulness to most people, only those with some interest in the production of objects with various degrees of brittleness, a glass blower, perhaps, will find interest in the series of events by which an object comes to be brittle. Imagine that a glass blower has found a procedure by which he produces unbreakable glasses for children. Watching the glass remaining intact after falling to the floor, etc., we might ask, "Why does this glass not break?" Whereas most of us would now be content with the answer that it is "unbreakable" or "not brittle," a rival glass blower would like to know "why" in the sense of being able to conduct the operations through which an "unbreakable" glass is generated.

Suppose that the second blower now is successful in persuading his friendly rival to reveal his production secrets. Is it likely that our second blower would then complain that his rival was not so friendly after all—because his description of the procedure did not say a word about "unbreakability"? There is simply no way "unbreakability" would add to the answer to the "why" question asked by the glass blower. On the other hand, no specifications of the production procedure adds to "unbreakability" as an inference ticket. The two "explanations" are answers to two different "why" questions and cannot be conjoined in an answer to either one of those "why" questions. With little effort, the analysis can be extended to psychological concepts. A cognitivist sees them from the position of daily life. A behavior analyst sees them from the position of the glass blower.

It has often been pointed out that the conceptual confusion in psychology results from the fact that our "special vocabularies" both overlap with and, to a large extent, are derived from thousands of years of prescientific accounts of behavior (e.g., Gilbert, 1972; Hineline, 1980; Skinner, 1938, 1945). Behavior analysis may have gone farther than most other research traditions in constructing a technical vocabulary that is stripped of multiple connotations that are typically attached to terms in the vernacular. It has even been suggested that the "special dialect" of behavior analysts has been responsible for a lack of communication between them and the rest of the psychological community (e.g., Krantz, 1971). In

fact, there have been numerous discussions and debates between behavior analysts and cognitivists (cf., Catania & Harnad, 1988; Modgil & Modgil, 1987; Special issue of *Journal of Behavior Therapy and Experimental Psychiatry*, September 1995). Of all those discussions, however, not a single one seems to have ended with any significant agreement or consensus between representatives of the two traditions. The "special dialect" of behavior analysis can hardly be responsible for all those failures to reach some sort of consensus, since not all cognitivists have been that remarkably devoid of understanding of what they criticize, and neither are behavior analysts generally devoid of understanding of the concerns of so-called cognitive psychology or of traditional psychology in general (cf. Special issue of *Journal of Behavior Therapy and Experimental Psychiatry*, September, 1995). A common feature of most of these discussions has been a concern with "What counts as an explanation?" "Explanation," is obviously not itself a purely technical term and, as we have just seen, the term is regularly applied in at least two very different senses in the vernacular. These two different kinds of explanations seem to correspond to the two different kinds of explanations with which behavior analysts on the one hand, and the majority of psychologists on the other, are primarily or exclusively occupied.

First, behavior analysts have claimed that dispositional, mental or cognitive explanations are not explanations at all but, simply, "explanatory fictions." This view is clearly at odds with all dominating conceptions within the field of psychology, as well as with the predilections and interests of the sophomores in psychology and those of journalists and other laymen public commentators. Hence, it is no big wonder that behavior analysis has "not yet become psychology" (cf. Skinner, 1989, p. 64).

On the other hand, cognitivists have treated the two kinds of explanations as belonging to the same logical category. They continue to refer to behavior analysis or "behavioristic psychology" as S-R psychology (e.g., Atkinson et al., 1993), which was outdated approximately 60 years ago, and argue that since most behavior does not enter into a lawful relation with any previous stimulus, the discrepancy must be repaired by *adding* "cognitive" events or processes:

> Theories that place the cause of behavior in the environment stimuli cannot account for variation in performance accompanying the same external input. (Bandura, 1995, p. 181)

> The "concept of an internal representation is useful because it allows us to explain the occurrence of responses that are not entirely governed by external stimuli." (Terry, 1988, p. 366)

> Cognitivism was the position that complex mental processes played an important role in shaping human behavior, and much of the field shifted to studying these mental processes. (Anderson, 1995, pp. 3-4)

> The contemporary analysis of learning includes cognitive factors as well as behavioristic [i.e., S-R] principles. (Atkinson et al., 1993, p. 285)

While the systematic study of behavioral regularities, dispositions, or tendencies, including the summarizing of those patterns in cognitive terms, may be considered as a perfectly legitimate scientific activity, it should not be confused with the practice of studying the historical event variables of which such regularities are functions. "Cognitivistic" accounts on the one hand, and functional accounts in terms of historical events on the other, constitute completely different kinds of explanation, as they are answers to different kinds of "why" questions. The category mistake here is to convert the regularities summarized by cognitive terms into "events" that can properly be embodied, by conjunctive propositions, with historical events as causes of behavior. Whereas it is moot to claim to study behavior on Monday, cognitions on Tuesday, etc., it would be possible to work on dispositional or "cognitive" explanations on Monday, historical event-causes on Tuesday but not at any moment on any day to conjoin the two kinds of explanations in an answer to a single "why" question. There is simply no way in which behavior can properly be considered to emerge as a joint product of historical *and* cognitive "events."

A Rylean type of solution, then, is to recognize the separate psychologies of inference tickets and of historical behavior-initiating events. The distinction made here between a science of "inference tickets" and that of behavior-initiating events parallels Catania's (1973) distinction between the psychologies of structure and function. Catania argued that both perspectives (or their key terms) are descriptive rather than explanatory and that structural and functional concerns complement rather than conflict with each other. The present argument is that, although key terms are descriptive in both perspectives, both are also explanatory in the simple, ordinary sense that they are aimed at producing answers to questions regarding why organisms behave as they do, and that, as explanatory, the two perspectives are not commensurate. As long as none of them are usually even nearly successful enough to appear completely compelling or deterministic, the practice of combining them in an effort to complete the picture is likely to continue. When this approximates a 200% explanation of behavior, the mistake might be more obvious.

Concluding Remarks: Ryle's Lasting Contribution

Recently, it has been argued that Gilbert Ryle's "linguistic behaviorism" is "out of fashion these days" because after having read his book, *The Concept of Mind*, ". . .one knows little more about mind than before" (Bem & deJong, 1997). However, this should not be allowed to subtract from the appreciation of Ryle's explicitly "destructive purpose" of demonstrating the category mistakes that, unfortunately, persist in their relevance to current problems in the analysis of behavior.

Psychological concepts are summary labels that require repeated occurrences of certain discrete events for their proper use. As far as they are applied to discrete events, psychological concepts are, inevitably, inferences. Controlling relations can never be revealed by our observations of single instances of what we may

nevertheless refer to as examples of perceiving, thinking, or meaning something. The category mistake is to convert those summary labels into descriptions of hypothetical discrete-event duplicates of the events that they summarize, regardless of the alleged physical, mental, or purely conceptual status of those hypothetical constructs.

Behavior-analytic concepts are no less inferential than other psychological concepts when applied to single instances. We may point to a discrete event and call it a discriminative stimulus, a response, or a reinforcer but, in fact, several manipulations and observations of instances and non-instances may be required for the identification of the implied controlling relations. Controlling relations in single instances remain inferences.

Inferred properties of relations are, of course, not necessarily explanatory fictions. Nevertheless, it may be important to realize how easily they may become so when we classify them as causal events belonging to the same logical category as any other events that are, at least in principle, manipulable in an experimental analysis. Furthermore, the use of behavior-analytic words is no guarantee against the category mistakes involved when the inferred properties are treated as if they exist apart from the series of observations upon which they are based.

In sum, then, the lesson to be learned from Ryle is not, of course, that psychological, mental, or cognitive phenomena do not exist, or that they are inaccessible. They are just not events to be considered as members of the same logical categories as the behavioral events they are sometimes said to explain. "Indeed," as Ryle (1949) said, "if we are asked whether imagining is a cognitive or a noncognitive activity, our proper policy is to ignore the question. 'Cognitive' belongs to the vocabulary of examination papers" (p. 244).

Excluding such terms from a technical vocabulary does not, of course, imply that the relevant psychological phenomena are excluded from consideration.

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