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Continuing Commentary

Commentary on **Daniel Holender (1986) Semantic activation without conscious identification in dichotic listening, parafoveal vision, and visual masking: A survey and appraisal. BBS 9:1–66.**

Abstract of the original article: When the stored representation of the meaning of a stimulus is accessed through the processing of a sensory input it is maintained in an activated state for a certain amount of time that allows for further processing. This semantic activation is generally accompanied by conscious identification, which can be demonstrated by the ability of a person to perform discriminations on the basis of the meaning of the stimulus. The idea that a sensory input can give rise to semantic activation without concomitant conscious identification was the central thesis of the controversial research in subliminal perception. Recently, new claims for the existence of such phenomena have arisen from studies in dichotic listening, parafoveal vision, and visual pattern masking. Because of the fundamental role played by these types of experiments in cognitive psychology, the new assertions have raised widespread interest.

The purpose of this paper is to show that this enthusiasm may be premature. Analysis of the three new lines of evidence for semantic activation without conscious identification leads to the following conclusions. (1) Dichotic listening cannot provide the conditions needed to demonstrate the phenomenon. These conditions are better fulfilled in parafoveal vision and are realized ideally in pattern masking. (2) Evidence for the phenomenon is very scanty for parafoveal vision, but several tentative demonstrations have been reported for pattern masking. It can be shown, however, that none of these studies has included the requisite controls to ensure that semantic activation was not accompanied by conscious identification of the stimulus at the time of presentation. (3) On the basis of current evidence it is most likely that these stimuli were indeed consciously identified.

High-level factors alter signal detectability

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This commentary concentrates on Holender's (1986) analysis of backward pattern masking experiments involving the post-cued judgements task. He claims that these experiments provide no evidence of semantic activation at short SOAs (stimulus onset asynchronies); this conclusion is challenged here.

We can distinguish four hypotheses concerning the relationship between high-level (semantic) activation and low-level (detection/graphic) activation in their capacity to support appropriate responses (abbreviated here as H and L, respectively). They are:

(1) $H > L$	[2]
(2) $H = L$	[1]
(3) $H < L$, but $H > 0$	[1]
(4) $H = 0$ and $L > 0$	[0]

where "0" indicates chance responding (the figures in square brackets are explained below). A fifth possibility, the null experiment, is that

(5) $H = 0$ and $L = 0$ [1].

Marcel (1983) claimed (1) and Holender claimed (4). Both seem to have assumed that if (1) is not true, then (4) must apply; hypotheses (2) and (3) were never tested. Doyle and Leach (forthcoming) reanalysed the experiments that Holender thought showed no evidence of semantic activation (Fowler et al. 1981, experiments 1, 2, and 3; Marcel 1983, experiments 1 and 2; Nolan & Caramazza 1982). The number of experiments favouring each hypothesis is given in square brackets beside each hypothesis, above. Taken as a whole, these experiments suggest that there is evidence of high-level activation at short SOAs which is at least as useful in making responses as low-level activation.

The hypothesis that low-level judgements were assisted by high-level activation is consistent with this pattern of results. The reversal of the assumed flow of causality implied by this hypothesis was tested directly by using a two-alternative forced-choice detection task. Words were found to be more detectable than orthographically regular nonwords. Thus, levels of signal detectability can be affected by high-level activation. In order to remove low-level discrimination it may first be necessary to remove high-level activation. Cheesman and Merikle (1984) have inadvertently demonstrated this very point by showing that the conditions which yield zero priming are those which yield zero discrimination.

Other experiments (Doyle 1985) reinforce the contribution of high-level activation in signal detection. When stimuli were balanced on simple sensory qualities such as pixel count or the number of pixels in the target not overlapped by the mask, a letter frequency effect was found. When stimuli were balanced on sensory qualities and on letter frequency, an orthographic regularity effect was found. Word superiority and word category effects which could not be reduced to lower levels of activation were also found. Thus stimuli which are hard to detect leave demonstrable differences at a number of increasingly abstract levels of representation.

Interestingly, not all effects appeared in a straightforward manner; some (e.g., the orthographic effect) had to be teased out using a special sort of mask which produced apparent movement in a left–right direction (assumed to affect scanning of the target). Further evidence of the extent to which stimuli have undergone processing is the finding that which hand was used to respond (button-press) not only had an overall effect on

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signal detectability, but also interacted with some of the variables previously found important.

Signal detection is apparently one of the lowest-level tasks for which a voluntary response is possible, yet it is affected by high-level factors. Furthermore, the picture that emerges is not simple; an exceedingly complex set of operations is hidden within this simplest of tasks. For this reason alone forced-choice discrimination is not, as Holender and others have assumed, a suitable proxy for subjects' awareness, though it promises to be a useful tool in its own right to investigate the processing of stimuli at short SOAs.

Editorial note

Professor Holender has read this commentary and has declined to respond.

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Commentary on **Nicholas P. Spanos (1986) Hypnotic behavior: A social-psychological interpretation of amnesia, analgesia, and "trance logic." BBS 9:449–502.**

Abstract of the original article: This paper examines research on three hypnotic phenomena: suggested amnesia, suggested analgesia, and "trance logic." For each case a social-psychological interpretation of hypnotic behavior as a voluntary response strategy is compared with the traditional special-process view that "good" hypnotic subjects have lost conscious control over suggestion-induced behavior. I conclude that it is inaccurate to describe hypnotically amnesic subjects as unable to recall the material they have been instructed to forget. Although amnesics present themselves as unable to remember, they in fact retain control over retrieval processes and accommodate their recall (or lack of it) to the social demands of the test situation. Hypnotic suggestions of analgesia do not produce a dissociaion of pain from phenomenal awareness. Nonhypnotic suggestions of analgesia and distractor tasks that deflect attention from the noxious stimuli are as effective as hypnotic suggestions in producing reductions in reported pain. Moreover, when appropriately motivated, subjects low in hypnotic suggestibility report pain reductions as large as those reported by highly suggestible hypnotically analgesic subjects. Finally, the data fail to support the view that a tolerance for logical incongruity (i.e., trance logic) uniquely characterizes hypnotic responding. So-called trance-logic-governed responding appears to reflect the attempts of "good" subjects to meet implicit demands to report accurately what they experience.

Toward a new paradigm of hypnosis: A model combining the social-psychological and special-processes paradigms

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In a previous issue of this journal, Spanos (1986a) surveyed contemporary experiments on three hypnotic phenomena: suggested amnesia, suggested analgesia, and "trance logic." Although the experiments he reported are of major interest and constitute an important contribution to the study of the conditions and mechanisms of suggestibility, we do not agree with his general interpretations of the data. Our present comments and criticisms will focus on two main points: (1) misconceptions about the relationship between hypnosis and suggestibility, and (2) misconceptions about what is known as the "state theory."

1. Misconceptions about the relationship between hypnosis and suggestibility. In order to be considered a state, hypnosis must be characterized by an increase in suggestibility. However, in our view, this requirement is in no way necessary: The word "hypnosis" simply refers to a change in consciousness; besides, most advocates of the state theory have stressed that the relationship between hypnosis and suggestibility is weak (Hilgard 1965, p. 392). In 1889, Janet showed that in what was for him the deepest form of hypnosis, namely, somnambulism, suggestibility could totally disappear. Fifty years later, White (1937), who analyzed the differences between active and passive hypnosis, argued against classifying subjects on the basis of suggestibility criteria alone. More recently, in a study focused on behavioral and subjective changes during hypnosis, we have ourselves found four classes of hypnotic behavior, two of them positively related to suggestibility (one strongly and the other moderately) and two negatively related to suggestibility (Michaux 1982).

Spanos, like other investigators, uses waking suggestions as a control, as though the administration of suggestions could be considered a neutral procedure. The main arguments for this position are based on subjective reports. However, why should we trust the subject's report that he was not hypnotized when the opposite report is disregarded? It seems that a skeptical view should be adopted in both cases.

Subjective testimony has structural limits and one cannot be sure that any kind of change in mental functioning is automatically identified as such by the subject. In fact – and this applies not only to hypnotic mental changes – the subject does not generally seem aware of its occurrence. To be aware of a change in mental functioning, the subject must focus attention on its detection or must deduce it from his unusual perceptions and behavior. In most cases these changes are only inferred by observers or deduced afterwards by the subject himself; either he is unable to remember his feelings and behaviors precisely (amnesia), or he feels unable to integrate his behavior during this situation into his usual behavior. To be effective, this last mechanism implies accurate retrieval by the memory of types of behavior incongruous with normal waking behavior. As a consequence of this incongruity, the subject's consciousness often does not allow such retrieval and incongruous behavior is

rejected either by memory alterations (e.g., denial and distortion) or, as already suggested, by amnesia (i.e., repression).

Unawareness of mental changes is illustrated in Milgram's experiment (1974a) relating to authority. In this experiment, the behavioral changes are so manifest that the author finds it necessary to introduce a new concept, that of the "agentic state," even though the subjects involved in the experiment did not themselves allude to such a special "state." Perhaps there are so many social situations in which this kind of mental change may occur (such as panic states, love, group reactions, and so forth) that the subject cannot identify state changes as such.

As for the rejection from the subject's consciousness, a process very closely related to the thought mechanisms described by Freudian analysts, this is well illustrated by the behavior of some subjects who, although watching their video-taped behavior, still cannot remember precisely the content of their hypnotic behavior.

To speculate about the possibility of mental change occurring during "waking" control is not just a rhetorical exercise. Hypnotic induction procedures vary from motor to verbal techniques and from long to very short verbal inductions (Barber & Calverley 1965). In the waking condition, the subject is given suggestions whose content and form – including loss of control, automatism, and delusion – have been historically linked to hypnosis. The suggestions in themselves might therefore be perceived as a specific kind of induction whose manifestations (i.e., activity, suggestibility, apparent awareness, and so forth) would be largely a consequence of what Orne (1959a) defined as "demand characteristics." One may even wonder to what extent, in an appropriate context, "waking" suggestions are not the more appropriate way to elicit hypersuggestibility. In this context, the feelings of automatism and nonvolition which generally differentiate hypnotic from waking suggestibility would not represent major differences between the mechanisms producing suggestion, but would rather discriminate between the different mental contexts in which suggestion occurs. During hypnosis, general alterations in mental functioning produce feelings of automatism and nonvolition; during the waking state, suggestion behaviors are decoded in terms characterizing the usual waking interpretation of behavior, i.e., voluntary control, volition, and so forth.

2. *Misconceptions about the state theory.* Coe (1983) complains about the "reification" of the role-playing metaphor by proponents of the state theory. However, it seems that there is a similar "reification" of the state paradigm by opponents of the theory, particularly Spanos. To believe, like the "state theorists," in the occurrence of special mental processes during hypnosis does not automatically mean to believe that these processes are exclusive to the hypnotic setting.

The problem lies in defining the concept of the waking state. There is a tendency to identify waking mental functioning with a single type of mental activity. However, many alterations in mental processes can be observed during the waking state, including alterations in reality testing, time perception, attention, and so forth. It is difficult to reduce these functions to a common denominator; they can only be roughly subdivided into sleeping and waking behaviors. If Spanos and other opponents of the state theory had such a truly broad concept of the waking state, the controversy would no longer persist, but that is not the case. Thus, when nonstate theorists attempt to demonstrate the normality of mental processes during hypnosis, they do so with an implicit unidimensional concept of the waking state. This concept includes being fully conscious, having goal-directed behavior, and being actively involved in roles. From this point of view, to reduce hypnosis to the waking state implies that will, consciousness, and motivation are the ultimate determinants of hypnotic behavior. It also implies that the subject cannot be opaque to himself and that everything is willed, with the subject attempting to fulfill the hypnotist's demands and endeavoring to perform well. In such a context the subject's claims to act involuntarily or unconsciously must all be interpreted as attempts to perform the hypnotic role to the best of his ability. Subjective experience is consequently a delusion, and whether it is called "role playing" or "role enactment," it refers to an interpretation in terms of lying and simulation.

The implicit philosophical opposition between the subject's free will and submission to influence therefore constitutes one of the major underlying causes of the persistent conflict between opponents and supporters of the state theory. This opposition, which can be found in most disciplines, is particularly obvious in hypnosis and social psychology, both of which deal with influence. One side, which we could call group-centered, claims that the influence is real and the subject is passive. The other side, which adopts a voluntarist, subject-centered position, stresses the importance of subject intervention in influence mechanisms. The Freudian concept of identification (1921/1981) and Sarbin and Allen's concept of role enactment (1968) both illustrate this second position.

Insofar as they concern the problems of individual freedom and responsibility, these two antagonistic positions have major moral implications; hence, perhaps, the persistent conflict about the reality of hypnosis. In this regard we recall that the condemnation of animal magnetism by the Royal Commissioners in France was mainly based on moral arguments; they thought Animal Magnetism was socially dangerous because it was contrary to contemporary standards of good behavior.

To come back to Spanos's proposition: One cannot help being surprised by the reference he makes to social psychology, because the role-playing theory, with its subject's centered position, is not the whole of social psychology. Studies of conformity, influenceability, and submission to authority rest on quite a different basis and clearly show the limits of the subject's self-control and sovereignty.

Another source of the conflict between free will and submission to influence lies in the fact that, historically, the alternatives to prevalent conceptions about hypnosis were always formulated in a negative way: For example, the Royal Commissioners concluded that Animal Magnetism was nothing but imagination, Bernheim (1916/1975) asserted that hypnotism was no more than suggestion, and Spanos (1986a) states that "hypnotic phenomena are similar to other forms of social behavior and explicable without recourse to special processes" (p. 449). As regards this last assertion we would like to point out that the negation of special processes, which constitutes an implicit negation of hypnosis, is based on the unsubstantiated assertion that social behavior can be satisfactorily explained without reference to possible changes in mental functioning and therefore without considering the existence of special processes. Such an affirmation tends to restrict researchers to demonstrating the reality of hypnosis by showing that during this state there are some behavioral or mental changes that cannot be observed under any other conditions. However, we rather believe that hypnosis is just the artificial reproduction of naturally occurring mental changes and that it is therefore all the more likely that the same changes will occur in certain other situations. The proximity of hypnosis to other phenomena such as imagination, suggestion, or role-playing does not necessarily mean that hypnosis is merely produced by these mental mechanisms. Hypnosis can also have specific characteristics, particularly mental ones, that explain the manifestations observed in these fields. Instead of allowing fruitful reflection, the kind of negative alternative proposed by Spanos generates misunderstanding and misleading conflicts because it conceals the basis on which it rests.

In conclusion, we believe that there is no real contradiction between the cognitive social-psychological view and the "special processes" position. Although hypnosis must be explained from a cognitive psychological standpoint, it remains true that there is as yet no reason other than faith for claiming a priori that hypnosis cannot involve any special mental processes.

The presence in animals and in all human societies of "hyp-

noticlike" behaviors, the need for drugs, the radical change associated with the behaviors somewhat arbitrarily classified by Sarbin and Allen (1968) as deep involvement roles, seem to argue more strongly for a "cognitive-social-special-processes paradigm" than for a restricted "cognitive social-psychological paradigm," with all its limitations and misunderstandings.

Author's Response

Misconceptions about influenceability research and about sociocognitive approaches to hypnosis

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Michaux's commentary contains a number of misconceptions concerning the views expressed in my target article (Spanos 1986a). I will accordingly begin by summarizing my views about the theoretical controversies in the hypnosis research area.

Special process and sociocognitive views. Responsive hypnotic subjects (i.e., high suggestibles or hypnotizables) behave in seemingly unusual ways. In response to hypnotic induction procedures they close their eyes and appear to fall asleep. When later given test suggestions they appear to respond in a robotlike manner, as if they no longer controlled their own behavior. For instance, following suggestions for arm rigidity they appear to struggle unsuccessfully to bend their arm, following suggestions for amnesia they respond as if they were unable to remember the material covered by the suggestion, and so on.

Usually, "hypnotic state" or "special process" theories begin, either tacitly or explicitly, by accepting hypnotic behavior at face value. For example, on the basis of the apparent struggles of subjects to bend their arms or to recall "forgotten" memories, special-process theorists have often inferred that these subjects have indeed lost volitional control over motor movement and memory (e.g., Hilgard 1977a). Having accepted hypnotic behavior at face value, special-process theorists have gone on to posit unusual mental processes that they believe are required to account for the seemingly unusual behaviors (e.g., "trance logic" dissociation).

The social-psychological (or sociocognitive) account does *not* begin by taking hypnotic behavior at face value (Barber 1969; Sarbin & Coe 1972; Spanos 1986; Wagstaff 1981). On the contrary, the target article (Spanos 1986a) indicated how a great deal of empirical work conducted within the sociocognitive tradition has repeatedly demonstrated that hypnotic responses are not as they appear. Despite appearances, hypnotic subjects remain awake, actively process environmental inputs, and do *not* lose control over their motor systems or their memories. In a related way, hypnotically age-regressed subjects do *not* behave like real children but like adults who are attempting to behave like children (Nash 1987), and hypnotically deaf subjects do not respond to delayed authority feedback in the same way that organically deaf people do, but instead like people with unimpaired hearing who are acting as if they were deaf (Barber 1969).

Such examples can easily be multiplied. Taken together they indicate the following: (a) Hypnotic behavior is usefully conceptualized as the goal-directed activity of aware agents who remain attuned to their social context and who are motivated to present themselves in a way that is consistent with the social demands of the hypnotic test situation. (b) The important antecedents to hypnotic behavior are frequently similar to the antecedents that account for behavior in other social influence situations (e.g., the wording and source of communications, subjects' preconceptions, attitudes, and motivations, the way that ambiguous communications are interpreted). (c) Claims that there are processes or states such as "dissociation" and "trance," implying that subjects have been transformed from agents who control their own responses to recipients who passively observe their own automatically occurring responses, are misleading and counterproductive. I can find nothing in Michaux's commentary that offers a serious challenge to any of these contentions. Instead, Michaux has presented a number of misconceptions about sociocognitive views of hypnosis.

The hypnotic state construct. According to Michaux, I have the following misconception: "In order to be considered a state, hypnosis must be characterized by an increase in suggestibility." I made no such statement in the target article or elsewhere. On the contrary, what I and other sociocognitive theorists (Barber 1969; Sarbin & Coe 1972) have repeatedly argued is that the hypnosis construct is vague, misleading, and is not a useful conceptual tool for organizing and understanding the findings in this research area.

Michaux makes many statements about hypnosis based on the presupposition that this construct is relatively unambiguous, and that objective, specifiable criteria for its assessment are available and agreed upon. For example, Michaux speaks about the proximity of hypnosis to other phenomena such as imagination; he contends that hypnosis can have specific mental characteristics and that "the relationship between hypnosis and suggestibility is weak." Unfortunately, Michaux nowhere tells us how hypnosis is supposed to differ from imagination, what the specific mental characteristics of hypnosis are, or what validated criteria of hypnosis he is referring to that are only weakly related to suggestibility. Contentions like those proffered by Michaux have proven extremely difficult to evaluate empirically precisely because state theorists – Michaux included – have been unable to agree upon specifiable criteria for inferring the presence or absence of a hypnotic state.

Orne (e.g., 1959b) attempted to use "trance logic" responding as a criterion of hypnosis that could be assessed independently of responsiveness to test suggestions. As discussed at length in the target article, however, the research conducted on this topic has yielded data that are more consistent with a sociocognitive view than with a special-process view of hypnotic responding.

A number of "state" theorists (Conn & Conn 1967; Hilgard & Tart 1966) have argued that subjects' reports of having been hypnotized (depth ratings) should be used as

an independent criterion of the hypnotic state. Here again, the empirical data concerning hypnotic depth reports can be accounted for more parsimoniously by sociocognitive interpretations that emphasize the role of contextual factors in guiding subjects' attributions, than by special-process theories (Radtke & Spanos 1981). For example, highly hypnotizable subjects who report large reductions in pain following analgesia suggestions tend to rate themselves *either* as deeply hypnotized or as not at all hypnotized depending upon the expectations transmitted to them in the pain-testing situation (Spanos et al. 1988). Because Hilgard (1977b) accepts depth reports as valid indexes of hypnosis, these findings are inconsistent with his contention that high hypnotizables respond maximally to analgesia suggestions only by first "drifting into hypnosis" (p. 50). On the other hand, these findings are easily explained by sociocognitive explanations that emphasize the importance of contextual factors and expectations in determining subjects's self-descriptions as hypnotized or not.

One can of course argue, as Michaux appears to do in portions of his commentary, that subjects do not and perhaps cannot know whether they are "really hypnotized." Contrary to Michaux's contentions, sociocognitive theorists have no objection whatever to such an argument. It is, after all, special-process theorists, *not* sociocognitive theorists who make inferences about "hypnotic states" on the basis of subjects' verbal reports (Radtke & Spanos 1981). If Michaux wishes to retain the "hypnotic state" construct while rejecting verbal reports as in valid indexes of that state, he is of course quite entitled to do so. However, statements such as "the relationship between hypnosis and suggestibility is weak," will carry little useful meaning until he provides some objective and valid criterion of "hypnosis" that is independent of suggestibility.

Influenceability research and hypnosis. According to Michaux, the results of studies on influenceability and authority cannot be accounted for in terms of social role and require the positing of special mental states. He singles out Milgram's (1974b) well-known research on obedience to demonstrate the necessity of positing "special state" explanations of response to authority.

Michaux's use of influenceability research to argue against social role theory is particularly curious because such research is almost invariably cited to support the importance of contextual factors such as role legitimation, status differences between participants in interaction, and the adoption of the "experimental subject role" in determining the degree of conformity exhibited by subjects. This was certainly the view of Milgram (1974b), and it is the view of the sociocognitive theorists who cite Milgram's (1974b) work in relation to hypnosis research (e.g., Sarbin & Coe 1972; Wagstaff 1981). Michaux, however, appears to have developed an unusual view of Milgram's work as well as an unusual view of sociocognitive explanations.

When speaking of an "agentic state" or "agentic shift," Milgram (1974b) was simply referring to a shift in attitude that people develop when they enter an authority system. He defined agentic state as "the condition the person is in when he sees himself as an agent for carrying out another person's wishes" (Milgram 1974b, p. 133). According to Michaux, Milgram introduced the notion of agentic state even though his subjects were unaware of undergoing any mental changes. On the contrary, the above quoted definition makes it quite clear that Milgram explicitly grounded the notion of agentic state in the subjects' self-perceptions and self-attributions. Unlike the hypothetical processes posited by special-state theorists, the notion of agentic state carries no implication that the obedient person's behavior is no longer goal-directed, or that purposeful behavior has been transformed into automatic responding, or that obedient behavior is a function of unusual cognitive structures or processes such as amnesic barriers, trance logic, hidden selves, and dissociations.

Milgram's (1974b) experiments were aimed at identifying the contextual variables that led subjects to define themselves and to act in accordance with the view that they were agents for carrying out another's wishes. Similarly, sociocognitive theorists' work on hypnotic subjects' reports of involuntariness is based on the contention that the subjects' behavior remains goal-directed, but that they come to interpret their goal-directed behavior as occurring involuntarily (Gorassini 1987b; Lynn et al., in press; Spanos 1986). One of the important concerns in this research has been to delineate the contextual variables that lead subjects to shift from viewing themselves as goal-directed agents to viewing themselves as passive observers of their own automatically occurring responses. In short, the constructs used by Milgram (1974b) to explain obedience are in no way inconsistent either with social role theory or with the constructs used by sociocognitive theorists to account for hypnotic behavior. On the contrary, both Milgram's (1974b) work on obedience and the empirical work on hypnosis generated by the sociocognitive approach show how useful it is to view human social responding as the contextually bounded, goal-directed behavior of active agents.

At one point, Michaux implies that suggestions given to hypnotic subjects (those undergoing a hypnotic induction procedure) produce more frequent and/or more intense experiences of responding involuntarily than do the same suggestions given to waking subjects (those not given a prior hypnotic induction). A number of studies have explicitly addressed this issue; all results contradict Michaux's hypothesis (cf. Spanos 1986 for a review), indicating that the wording of the test suggestions is a much more potent determinant of experienced involuntariness than is the presence or absence of a hypnotic induction procedure. Both hypnotic and nonhypnotic subjects who were administered passively worded communications (e.g., "your arm is rising") were likely to rate their subsequent response as feeling involuntary, whereas those who received actively worded communications (e.g., "please raise your arm") were likely to rate their responses as voluntary (Spanos & Gorassini 1984). These findings are consistent with the emphasis placed by sociocognitive theory on the contextual determinants of subjects' self-attributions. They are less consistent with the notion that hypnotic procedures facilitate "dissociations" that underlie involuntary responding.

Misconceptions of sociocognitive theory. According to Michaux, sociocognitive accounts of hypnotic responding tend to associate wakeful mental functioning "with a

single type of mental activity," and fail to take into account such activities as changes in reality testing and attention. Sociocognitive theories are also supposed to be based on the "unsubstantiated assertion that social behavior can be satisfactorily explained without reference to possible changes in mental functioning." These theories also supposedly imply that "the subject cannot be opaque to himself," and that the terms "role playing" and "role enactment" refer "to an interpretation in terms of lying and simulation."

Not only are these claims about the sociocognitive perspective wrong, but they are wrong about issues that were specifically addressed in the target article and in my response to the original commentators as well as in the first-round commentary by Wagstaff (1986) and in the second round of commentary by Gorassini (1987a). For example, the target article reviewed a number of studies that described how implicit and explicit demands can lead subjects to engage in imaginal and other cognitive strategies for pain reduction. Also reviewed were a series of studies examining the relationship between different types of amnesia tasks, shifts in attention, and disorganized recall. The section in the target article on "trance logic" responding dealt in detail with the differences in cognitive functioning that characterize highly suggestible hypnotic subjects and subjects instructed to fake hypnosis. These differences in cognitive functioning were in turn related to differences in the interpretations of the hypnotic test situation by hypnotic subjects and simulators.

Obviously, sociocognitive approaches do not deny the existence of cognitive activity or contextually induced changes in cognitive functioning. On the contrary, the sociocognitive research has focused extensively on the interrelations of context, cognitive change, and behavior in hypnotic settings; this focus was clearly reflected in the target article.

The idea that subjects can be opaque to themselves usually refers to the notion that subjects frequently make incorrect inferences about the causes of their own behavior and frequently base self-attributions on these incorrect inferences. Contrary to Michaux's thinking, these notions are not incompatible with the sociocognitive view. In fact, I used the idea of misattribution in accounting for the reports of involuntary responding from hypnotic subjects (e.g., Spanos 1986); Gorassini (1987a) outlined a sociocognitive account of such misattributions in his second-round BBS commentary. Sarbin (e.g., 1984) has written extensively on this topic; he explicitly described hypnotic subjects as self-deceived and developed a sociocognitive account for such self-deception.

Again in contradiction to Michaux's view, the notion of changes in reality testing has also been addressed at some length in sociocognitive accounts of hypnotic responding. For example, a central theoretical concern in my own work (e.g., Spanos & Radtke 1981) and also in the work of Sarbin and Coe (e.g., 1972) has been to account for how subjects (both hypnotic and nonhypnotic) come to assign credibility or "reality status" to their imaginings.

Equating role playing or role enactment with lying or faking was explicitly rejected by Sarbin (1950) when he first applied role theory to hypnotic responding and has been repeatedly rejected by him ever since (e.g., Sarbin & Coe 1972). I have also repeatedly rejected this in my own work (e.g., Spanos 1986) and did so at some length in my response to the original commentaries. Despite all of this, Michaux states that notions like role enactment imply lying and simulation. This, and the other inaccuracies specified above, suggest that Michaux has seriously misinterpreted the social-psychological position that he attempts to criticize.

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Commentary on **M. B. Berkinblit, A. G. Feldman, and O. I. Fukson (1986) Adaptability of innate motor patterns and motor control mechanisms. BBS 9: 585–638.**

Abstract of the original article: The following factors underlying behavioral plasticity are discussed: (1) reflex adaptability and its role in the voluntary control of movement, (2) degrees of freedom and motor equivalence, and (3) the problem of the discrete organization of motor behavior. Our discussion concerns a variety of innate motor patterns, with emphasis on the wiping reflex in the frog.

It is proposed that central regulation of stretch reflex thresholds governs voluntary control over muscle force and length. This suggestion is an integral part of the equilibrium-point hypothesis, two versions of which are compared.

Kinematic analysis of the wiping reflex in the spinal frog has shown that each stimulated skin site is associated with a group of different but equally effective trajectories directed to the target site. Such phenomena reflect the principle of motor equivalence – the capacity of the neuronal structures responsible for movement to select one or another of a set of possible trajectories leading to the goal. Redundancy of degrees of freedom at the neuronal level as well as at the mechanical level of the body's joints makes motor equivalence possible. This sort of equivalence accommodates the overall flexibility of motor behavior.

An integrated behavioral act or a single movement consists of dynamic components. We distinguish six components for the wiping reflex, each associated with a certain functional goal, specific body positions, and motor-equivalent movement patterns. The nervous system can combine the available components in various ways in forming integrated behavioral sequences. The significance of command neuronal organization is discussed with respect to (1) the combinatory strategy of the nervous system and (2) the relation between continuous and discrete forms of motor control. We conclude that voluntary movements are effected by the central nervous system with the help of the mechanisms that underlie the variability and modifiability of innate motor patterns.

Motor control as adaptational biology: Relevance to education and rehabilitation

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Berkinblit et al.'s (1986) target article can be divided into two main topics: the concept of motor constancy and adaptation as it relates to the observation of modifiable protective reflexes in the spinal frog, and the problem of modeling the stretch reflex in man. We will restrict our comments to the former.

Berkinblit et al. make a powerful and provocative statement in their abstract: "Redundancy of degrees of freedom at the neuronal level as well as at the mechanical level of the body's joints makes motor equivalence possible." Unfortunately, this statement does not appear to be either adequately supported by direct evidence or explicitly elaborated. It is not clear how one should interpret "degrees of freedom at the neuronal level" and how these dimensions are to be distinguished from the biomechanical degrees of freedom of the body mechanism. Nevertheless, this is an intriguing explanation for the principle of motor equivalence and motor constancy as manifest in the goal-directed adaptive actions that have been grouped together and labeled the "wiping reflex" of the spinal frog. The word "reflex," however, carries some unfortunate connotations that are antithetical to the concept of motor constancy or functional specificity. A reflex has often been defined as stereotyped or machinelike reactions elicited by specific stimuli (e.g., Carew 1985), a notion that derives from a simplistic input–output mechanistic analogy. The behaviors that Berkinblit et al. describe in the spinal frog do not fit with this idea and suggest that we must either redefine what we mean by the "reflex" concept, or create new terminology. The wiping reflex of the spinal frog is not simply a stereotyped response to a specific input, just as the vestibulo-ocular "reflex" is not simply a fixed stereotyped movement of the eyes linked inflexibly to movement of the head.

These behaviors can be viewed as purposive, adaptive actions with readily defined goals that have biologic significance. The wiping reflex is a specific self-contained action that is clearly goal-oriented and adaptive. That it offered a solution to the "motor problem" of preserving the integrity of the integument by physically displacing a noxious contact probably led to its evolutionary selection and reinforcement. Evolution selects for functional specificity by virtue of the fact that survival is outcome dependent. Furthermore, the wiping reflex is not simply a single movement in a particular context but an *ensemble of action forms* from which a particular action is selected as a result of the conjunction of the eliciting condition (e.g., applying a noxious stimulus to a particular point on the skin) and the menu of available actions and actuators (behavioral forms), which appears to have a hierarchical structure (e.g., amputation of the hind limb ipsilateral to the site of stimulus application reveals the "hidden" ability to use the contralateral hind limb to achieve the same outcome).

What holds the ensemble together is the common adaptive function served. There is no *normative* element in the ensemble of potential actions; rather, the key characteristic is the adaptive function subserved by the behavior. The forms in the ensemble vary because the same adaptive function must be performed under a myriad of potentially constraining circumstances stemming from factors intrinsic to the organism as well as from variation in the environment. Moreover, this variety and thus the flexibility with which an action can be performed are made possible by the redundant biomechanical degrees of freedom available to perform the act and to attain the goal.

These biomechanical degrees of freedom can be viewed as defining a multidimensional body position space (or, equivalently, joint angle), B, into which all possible biomechanical trajectories (time-dependent changes in body configuration), T, can be mapped. In preparation for the performance of each act, a particular trajectory is isolated and selected from this space through an emergent neural process. During the selection process, the eliciting condition and the context of performance progressively constrain the boundaries of B from which a satis-