limited goal of our target article (Plomin 1994). It discusses how genetic and environmental theory are coming together in models that recognize the organism's active role in selecting, modifying, constructing, and reconstructing experience. Concerning **Grimshaw & Bryden**'s comment, what we can say empirically is that tests that are widely used as measures of the environment show genetic influence. We preferred this more limited operational conclusion because of semantic complications that are raised by the argument that the environment itself is heritable. The environment itself can show genetic influence only to the extent that it reflects genetic differences among individuals, because environments do not have DNA. The weather is not heritable. Nonetheless, as suggested by its title, a theme of the book is that genetic factors contribute to experience – the individual's interaction with the environment.

The book describes two key programmatic directions for research. First, we need to understand the developmental processes by which genetic factors come to play such an important role in ostensibly environmental measures. For example, does genetic influence on measures of parenting reflect genetic influence on children's temperament or cognitive ability? So far, research on this topic suggests that such obvious trait candidates provide only part of the answer. The rest of the answer might elucidate context-specific aspects of behavior at the interface between nature and nurture that are not tapped by our traditional transsituational traits. The second direction for research investigates the extent to which genetic influence on environmental measures spills over into genetic influence on outcome measures. That is, if genetic factors contribute to variance on environmental measures such as life events, and if genetic factors also contribute to outcome measures such as depression, is it possible that genetic factors contribute to the covariance between environmental measures and outcome measures? The first research on this topic suggests that the answer is yes, a conclusion with far-reaching implications. The book leads up to a theory of the genetics of experience based on the quantitative genetic concept of genotype-environment correlation.

Lamb asks how one can assess the relative importance of genetic and environmental influences on interindividual variation when measures of environment reflect genetic influences. This is not a problem for behavioral geneticists because their methods do not rely on measures of the environment. It is a problem for environmentalists who believe that theirs are pure measures of the environment. We agree, however, that finding genetic influence on environmental measures should stimulate interest in understanding the developmental interface between nature and nurture. We object to the shibboleth that genetic research is of no interest to developmentalists because genetic research supposedly tells us only about outcome, not about process. One person's process is another person's outcome. The issue is levels of analysis, not right and wrong ways of thinking or of conducting research. Genetic processes should be of fundamental concern to developmentalists.

In Anastasi's 1958 article there is confusion about the important distinction between an individual and individual differences in a population. It is difficult to investigate the extent to which genetic factors contribute to the height of a particular individual. Does anyone doubt, however, that genetics is largely responsible for height differences among individuals in the U.S. population? For the same reasons, **Lamb**'s statement that "heredity and environment are inextricably linked" is wrong. It is wrong when the focus of the research is on individual differences, as in behavioral genetics research.

We agree that more attention should be paid to the difficult topic of the developmental interface between nature and nurture, but this effort will not be made unless developmentalists are convinced that genetics is a force to be reckoned with in the origins of individual differences in psychological development.

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Commentary on Ilan Golani (1992) A mobility gradient in the organization of vertebrate movement: The perception of movement through symbolic language. BBS 15:249–308.

Abstract of the original article: Ordinary language can prevent us from seeing the organization of whole-animal movement. This may be why the search for behavioral homologies has not been as fruitful as the founders of ethology had hoped. The Eshkol-Wachman (EW) movement notational system can reveal shared movement patterns that are undetectable in the kinds of informal verbal descriptions of the same behaviors that are in current use. Rules of organization that are common to locomotor development, agonistic and exploratory behavior, scent marking, play, and dopaminergic drug-induced stereotypies in a variety of vertebrates suggest that behavior progresses along a "mobility gradient" from immobility to increasing complexity and unpredictability. A progression in the opposite direction, with decreasing spatial complexity and increased stereotypy, occurs under the influence of the nonselective dopaminergic drugs apomorphine and amphetamine and partly also the selective dopamine agonist quinpirole.

The behaviors associated with the mobility gradient appear to be mediated by a family of basal ganglia-thalamocortical circuits and their descending output stations. Because the small number of rules underlying the mobility gradient account for a large variety of behaviors, they may be related to the specific functional demands on these neurological systems. The EW system and the mobility gradient model should prove useful to ethologists and neurobiologists.

Implications of methodological rigor in movement analysis for the study of human communication

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Science, being a theoretically minded animal, wants to know that its facts not only accumulate but also make sense, so when a new method of data collection or data analysis is developed it needs to be shown to be theoretically relevant, that is, able to address controversies of the time, even if only to show them to be ill-formulated. Hence, a new empirical method cannot content itself with the presentation of new facts or patterns, it should also engage in the redefinition of existing concepts (in its own terms) and then proceed to resolve critical issues in theories that address the set of phenomena to which the new method applies.

Golani's (1992) research largely follows this idea in applying a new method, the EW (Eshkol-Wachman) movement notation, for the behavioral study of animate movement. The new method has afforded the description of some previously unnoticed behavioral phenomena (e.g., the mobility gradient); it has also offered a rigorous redefinition of various concepts in animal behavior, rendering them less ambiguous and more operational. An immediate theoretical gain has been the redefinition in geometrical terms of basic notions such as "homology" and "play," notions crucial to the ethological enterprise as a whole, thus facilitating their empirical investigation. The technique has also been applied in the field of neuroscience, especially with regard to the motor control of movement (e.g., locomotion), and in determining some functional connections among various anatomical structures of the CNS, especially those connected with the basal ganglia and the dopaminergic system. EW notation has not, however, been incorporated as a regular method in studies involving movement analysis, probably because of the demand it puts on human resources, coupled with its complexity. A contributing factor may have been a failure to perceive new ways in which the use of EW notation could resolve theoretical issues that could not be resolved using simpler (and less time-consuming) methods. This, I think, makes it worthwhile to suggest other fields that could benefit from the use of EW notation; I should like to propose this for the study of human movement in communication (henceforth, "kinesics"), where it has not been used so far.

The kinds of kinesic phenomena most closely related to the mobility gradient are those referred to as "posture changes" or "postural shifts" (PSs) (Bull 1983; Hadar et al. 1984; Scheflen 1964). These movements involve a large displacement of the trunk/head system relative to the ground or other body parts, or to the cointeractant (or all of these). The function of PSs in conversation is controversial. In one account, PSs convey communicative attitudes such as sympathy, antipathy, reservation, compliance, and so forth (Scheflen 1964). Thus, turning toward a participant in a conversation is said to communicate the seeking of contact, whereas turning away from a participant communicates the reverse and tends to influence negatively the maintenance of communication. According to an alternative scenario, PSs help regulate speaking turns; for example, turning toward listeners prior to a terminal juncture signals the readiness to transfer the floor to them whereas turning away from listeners signals continued occupation of the floor (Duncan 1972). In yet another conception, PSs mark the boundaries between linguistic units on the macro level (e.g., sentences and paragraphs) and are related to cognitive processes that organize linguistic structure (Kendon 1972). Hadar et al. (1984) suggest that PSs concern processes of motor control, enhancing the beginning of speech after pauses and silences by offering a reference signal, as well as raising the baseline level of activation in the articulatory system. These accounts make highly diverse claims concerning the nature of PSs, some of which may be mutually exclusive. For example, if PSs regulate floor time, they should occur primarily toward the beginning and end of speaking turns, whereas if they facilitate motor control they will tend to occur after long silences irrespective of their position inside a speaking turn, yet seldom toward terminal juncture. We have here contradictory claims about the timing of PSs, all of which have found some support in the related literature. This raises the possibility that the related phenomena are not identical, or that some of the above approaches have not been sufficiently precise in defining or measuring their variables. Analysis based on EW notation may be helpful here in a number of respects.

First, the use of EW notation may establish whether or not the variety of motor sequences referred to as PS represents a unitary behavioral phenomenon. Like the mobility gradient, PSs have distinct vertical and horizontal components, with the head usually leading the movement. Those PSs that occur after silences (often well inside speaking turns) seem to tend to start with a vertical component, the horizontal component being optional (Hadar et al. 1984). The critical component of a regulatory PS, however, must be relative to the coconversant, having its salient part in the horizontal plane (Hadar 1986). We may have here different types of PS, distinguished by the respective saliencies of the horizontal and vertical components, but until a rigorous analysis is performed we will not know this. Functional claims here must resort to descriptive specificity of the kind afforded by EW notation, a point Golani makes repeatedly in the target article.

Second, once descriptive rigor is achieved (and perhaps a subclassification of PS), functional claims may be based on the computation of invariance relative to different frames of reference. Since invariance forms the kinematic analogue of statistical correlation, its occurrence supports the existence of a functional connection (Golani 1981). Admittedly, invariance may emerge at different levels of analysis, but it is assumed that the strength of a (possible) functional link depends on the level at which it occurs. Roughly speaking, invariance occurring earlier in analysis reflects (potentially) stronger links. Thus, the mobility gradient is relevant to motor control because it reflects a first-order invariance. Now, one of the advantages of EW notation is that it readily allows description relative to different frames of reference, which may be selected to decide between different theoretical claims. Thus, the invariance of PS relative to articulatory gestures (gestures of the tongue, jaw, lips, etc.) will be suggestive of a motor connection and will support the motor facilitation hypothesis, whereas invariance relative to postures of the coconversant will suggest an interactional connection and possibly a regulatory function.

The case of PS was chosen for discussion here merely because of its structural affinity with the mobility gradient, but similar gains may be made regarding other aspects of kinesics. To mention a couple of these, Hadar (1989) has argued that movements that cooccur with phonetic stress (called "beats") are physically different from content-bearing ("symbolic") movements. Some circumstantial evidence has been summoned to support this claim, but no authoritative analysis has ever been performed. Similarly, it has been suggested that symbolic arm and hand movements may serve two different cognitive functions, one that is related to the conceptual processes that underlie speech, and one related to the retrieval of words (Kendon 1985). It may well be that these functions, if they exist, are subserved by different kinds of movement but, again, we will not know this until an appropriate analysis is performed. The efficacy of EW notation in resolving theoretical problems of the kind suggested above could contribute, I should think, not only to kinesics, but also to greater consideration by the scientific community for rigor in movement analysis.

Implications of Eshkol-Wachman movement notation for behavioural pharmacology

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The review by Golani (1992) of the Eshkol-Wachman (EW) movement notation shows that this method provides novel and important insights into pharmacological actions on specific movement subsystems. Specifically, the outlined differentiation between movement patterns induced by apomorphine and amphetamine are both informative and impressive. From the viewpoint of the behavioural pharmacologist, however, there are several issues, both practical and theoretical, which may discourage frequent utilisation of movement notation. As a preface, this commentary should perhaps be qualified by the admission that at least some of these issues derive from time/resource constraints, rather than any inherent problems in the analysis. In this context, the complexity of EW movement notation requires a lengthy learning process for adequate rater reliability and a comparatively complex and time-consuming procedure following this learning period. Clearly, such an investment is valid if movement notation, as Golani suggests, provides a considerably more cogent and informative interpretation than simpler alternatives, such as global rating scales.

Since the advent of movement notation in the early 1980s, however, there seems to be a dramatic disparity between the level of behavioural complexity essential to EW analysis and the somewhat limited application of this method to an increasingly complex pharmacological context. Golani does emphasise that the data from single-dose administration of apomorphine, amphetamine, and quinpirole are not intended to provide any firm conclusions in terms of "brain-behaviour relations" but were presented to illustrate aspects of the mobility gradient. However, there is clearly a need to superimpose a suitably complex pharmacological analysis on the behavioural framework provided by EW analysis. For example, the growing evidence for the existence of further ($D_3/D_4/D_5$) dopamine receptor subtypes (Sibley & Monsma 1992), the high affinity shown by quinpirole for the D_3 receptor (Sokoloff et al. 1990), and the knowledge that, at the dose used, amphetamine administration may elicit the 5-HT syndrome (Taylor et al. 1974) would indicate the need for a balance in complexity of approach to behaviour and pharmacology. This is to some extent stating the obvious, but the apparent paucity of pharmacological data derived from EW analysis may raise doubts as to the utility/practicality of the method.

As a more general issue it may prove prudent to aim for a balance between pharmacological and behavioural complexities in the formulation of any analytical approach. In global terms, movement analysis could be construed as one of the simpler components of behavioural pharmacology, in comparison with such enigmas as anxiety or social behaviour. In this context, it is difficult to believe that such detailed methodology as EW movement notation could be applied to more complex phenomena. For example, it does not seem feasible to advance from the level of complexity inherent to the ethological analyses currently used in a variety of animal models of anxiety and depression (e.g., Mitchell & Redfern 1992; Rodgers et al. 1992; Shepherd et al. 1994). As Allen (1992) points out, it is important to realise that EW analysis is not a replacement for other means of describing behaviour. On a more positive note, however, specific behavioural components which have emerged from these ethological procedures could potentially benefit from further scrutiny using EW notation. One ideal candidate for a detailed geometric analysis would be "stretch attend," a rodent posture recently shown to be very sensitive to anxiolytics (Rodgers et al. 1992; Shepherd et al. 1994).

Finally, Golani argues that the observer is restricted by language as a vehicle of perception and that the EW analysis reduces the confounding impact of such restraints. At some stage, however, the data emerging from EW analysis must be translated into that same restricted perceptual framework. Thus, in agreement with Bekoff's (1992) commentary concerning the ultimate need for an ordinary language for transmitting information, it could be argued that interpretation may be delayed but not eliminated by this form of geometric analysis.

In conclusion, EW movement notation provides a novel and impressively detailed technique; as such, any criticism should really be limited to the data. Clearly, more pharmacological studies are required to substantiate and possibly extend the utility of what seems to be a progressive approach to movement analysis.

Author's Response

The practicality of using the Eshkol-Wachman movement notation in behavioral pharmacology and kinesics

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Abstract: Eshkol-Wachman (EW) movement notation analysis consists of a stage in which the relevant movement variables are isolated by experts and a stage in which they can be readily used by anyone not skilled in EW. Because everything else that happens is constrained by these variables, once they are pointed out they can easily be discerned and scored. They constitute the skeleton of behavior; therefore, like real bones, they can be used to construct a taxonomy of behaviors.

Both commentaries commend the soundness of using Eshkol-Wachman movement notation (EW) in the analysis of movement and consider the results obtained novel and important. Both, however, also express a concern about the practicality of using this method: the investment in human resources is large (**Hadar**), and it is not clear whether the results justify the investment (**Shepherd & Dourish**); the cost effectiveness of this method in the face of an increasingly complex pharmacological context is, in Shepherd & Dourish's opinion, questionable. How much more so given that the analysis of movement is simple compared to challenges which confront behavioral pharmacology, such as the analysis of anxiety or social behavior. This provides me with an opportunity to address the issue of the practicality of using our method and results.

In evaluating the practicality of using EW, one should distinguish between a stage of establishing the relevant kinematic variables and a subsequent stage of using these variables in the measurement of behavior. The first stage consists of analyzing behavior with the help of tools borrowed from EW, including a variety of coordinate systems and symbols that help in isolating the relevant variables. This stage obviously requires skill in movement notation. Once a relevant variable is specified, however, it can readily be used by anyone, irrespective of skill in EW. Furthermore, because these variables are strictly defined geometrical quantities, all one need do is attach stickers to the rat's relevant joints and record their coordinates on the computer screen with the help of a computerized video tracking system. These coordinates can then be used to compute the dynamics of the defined variable(s).

Whereas it would be unrealistic to expect behavioral pharmacologists to engage in the first stage, they could easily engage in the second. Because the relevant variables are *collective ones* (Haken 1983), that is, variables to which *the system is enslaved* (e.g., pp. 265–66 of the target article), in the sense that everything else that happens is constrained by them, it is our experience that once they are pointed out to the observer they become almost self-evident and easy to score from the video record, with or without automatic tracking.

If I were a behavioral pharmacologist I would place any active drug-treated rat on a large enough glass platform and obtain a side and bottom view video record of its behavior, so that the parts of the trunk and all four legs could be observed simultaneously. Then I would examine, one at a time, each of the collective variables isolated so far.[1] For example, do the legs, just before stepping, release foot contact with the ground inside or outside the contours of the trunk when viewed from below (Fig. R1; Adani et al., in preparation)? Or do the forequarters often cross the plane dividing body-related space into left and right hemispheres without stopping there (Fig. 16 in target article; Einat et al., submitted)?

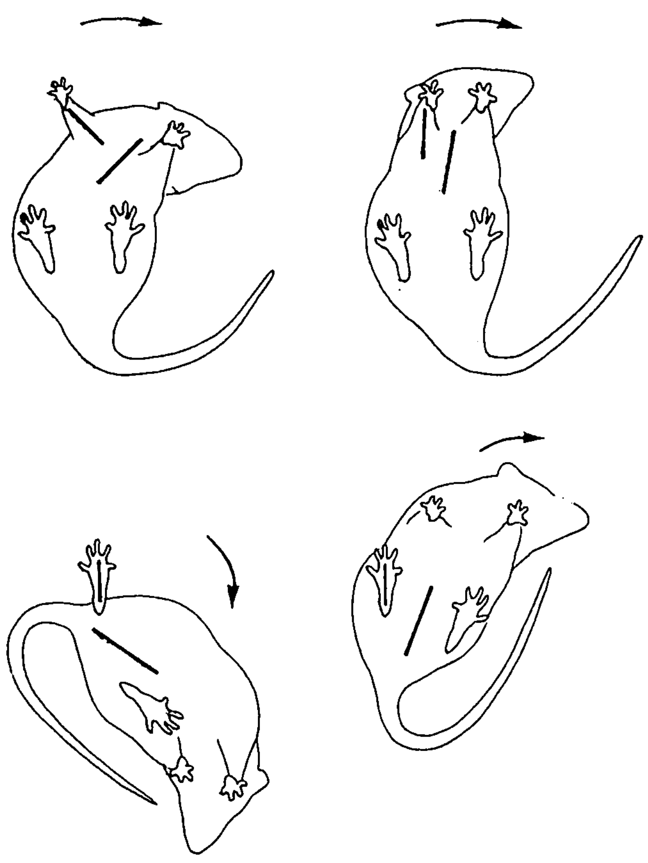

Figure R1. Bottom views of a rat performing a clockwise horizontal movement. A rat's left legs' relative positions just before joining a movement of the body by stepping in the same direction. In the two illustrations on the left, the feet are located outside the contours of the trunk just before stepping. In the illustrations on the right the feet release contact just before crossing the trunk contours.

Shepherd & Dourish do not see how EW could help in advancing the field from the level of complexity currently used in animal models of anxiety (e.g., Rodgers et al. 1992) and social behavior (e.g., Mitchell & Redfern 1992). In the first of these studies, the level of anxiety is represented by six variables (e.g., percent total for frequency of entries to open/closed arms in an elevated plus-maze; frequency of head dipping and stretch attend postures, etc.). In the second study, ritualized fighting is represented by 15 variables (the Grant & Mackintosh, 1963, behavioral categories: attack, submit, attend, crouch, etc.). The variables of both studies are intrinsically unrelated to each other, within and across situations; the changes in the level of anxiety and aggression are measured in reference to a relative baseline, and dozens of graphs are examined in the search for a statistically significant difference. In addition, the use of overall constructs such as aggression and anxiety ensures that all that can be established is that a drug enhances, reduces, or has no effect on each of these constructs. It is therefore possible to show that a drug belongs to a certain family of drugs (anxiogenics, for example) but difficult if not impossible to establish qualitative differences – between drugs and between sub-groups of drugs – belonging to the same family (for example, the data of the second study do not reveal any qualitative differences among the five examined antidepressants).

In contrast, a study based on EW analysis would start with a screening of drug-induced behavior in order to choose, out of the list of collective variables available so far, the variables whose dynamics are changed by the drug. A description of drug-induced behavior in terms of these variables would have several advantages:

(1) Like the bones of a vertebrate's skeleton, these variables are part of the skeleton of behavior. They should therefore be discernible in any drug-induced behavior: some will stay unchanged, others will change in this or that direction, and still others might be gradually eliminated. For example, since the building blocks of exploratory behavior are round trips that are performed from a fixed home base and are constrained by an upper bound on the number of stops per round trip (Golani et al. 1993), it would be enormously informative to know whether this particulate process stays intact, is eliminated, or is changed, and in what way, by a specific drug, or by a specific sub-group of drugs.

(2) Unlike the improvised mobility gradient generated by the physical structure of the elevated plus-maze, the

measurement of mobility in terms of the general mobility gradient variables should yield results that relate to an absolute reference (complete immobility), as well as to ritualized fighting, and to situations and preparations presently outside the scope of behavioral pharmacology.

(3) Because we search for qualitative differences, magnitudes of the same variable should differ across preparations by at least an order of magnitude (see comparison between dopaminergic stimulants in target article; see also Figs. 6 and 7 in Adani et al. 1991; Eilam & Golani, in press). This should increase rater reliability, reduce the load on statistical evidence, and provide a kind of a table for a qualitative classification of drugs of the same family in terms of their differential or common effects on behavior.

The absence of such a classification is only emphasized by the increasing complexity of the pharmacological context mentioned by **Shepherd & Dourish.** An increase in the number of newly discovered receptor types also implies an increase in the number of receptors in quest of a behavioral function. Instead of adopting Shepherd & Dourish's suggestion to use a more balanced approach that also includes pharmacology, I would rather help solve this problem by increasing the list of available collective variables that might be used to define precisely, for example, the common behavioral effects of a group of drugs sharing a common physiological mechanism.

Unlike the situation with behavioral pharmacology, where part of the ground work has already been done, the use of EW analysis in the context of human movement in communication (kinesics) has not yet begun (see however, Eshkol 1971). I thus fully support **Hadar's** analysis of what could be done with the help of EW in the field of kinesics. From a practical point of view, the description of the morphology of relevant variables in this field would demand a skill in EW and an interest in the study of human movement in and of itself. As evidenced in the field of kinesics, a motivation to use movement merely as a vehicle for gaining insight into cognitive or linguistic phenomena has not been sufficient.

Finally, **Shepherd & Dourish** are concerned that because the data derived from EW analysis must ultimately be translated into ordinary language, interpretation may be delayed but not eliminated. The answer is that suspension of interpretation should not be taken lightly. The rigor of a specialized language forces one to attend to aspects of movement that are left unattended with only the help of perception guided by ordinary language. Once these unattended aspects are captured, there is no reason why they should not be formulated in ordinary language.

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NOTE

1. The list of collective variables now includes the variables constituting the mobility gradient (p. 258, target article), several additional variables in body-related space (Adani et al. 1991; in preparation; Einat et al., submitted) and several variables relating to locomotor behavior in locale space, (i.e., relating to exploration and spatial memory; Eilam & Golani 1989; 1990; in press; Golani et al. 1993; Tchernichovski et al., submitted; Tchernichovski & Golani, submitted).

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Commentary on **Gregory R. Lockhead (1992) Psychophysical scaling: Judgments of attributes or objects? BBS 15:543–601.**

Abstract of the original article: Psychophysical scaling models of the form R = f(I), with R the response and I some intensity of an attribute, all assume that people judge the amounts of an attribute. With simple biases excepted, most also assume that judgments are independent of space, time, and features of the situation other than the one being judged. Many data support these ideas: Magnitude estimations of brightness (R) increase with luminance (I). Nevertheless, I argue that the general model is wrong. The stabilized retinal image literature shows that nothing is seen if light does not change over time. The classification literature shows that dimensions often combine to produce emergent properties that cannot be described by the elements in the stimulus. These and other effects cannot be adjusted for by simply adding variables to the general model because some factors do not combine linearly. The proposed alternative is that people initially judge the entire stimulus – the object in terms of its environment. This agrees with the constancy literature that shows that objects and their attributes are identified through their relations to other aspects of the scene. That the environment determines judgments is masked in scaling studies where the standard procedure is to hold context constant. In a typical brightness study (where different lights are presented on the same background on different trials) the essential stimulus might be the intensity of the light or a difference between the light and the background. The two are perfectly confounded. This issue is examined in the case of audition. Judgments of the loudness of a tone depend on how much that tone differs from the previous tone in both pitch and loudness. To judge loudness (and other attributes) people first seem to process the stimulus object in terms of differences between it and other aspects in the situation; only then do they assess the feature of interest. Psychophysical judgments will therefore be better interpreted by theories of attention that are based in biology or psychology than those (following Fechner) that are based in classical physics.

Is there any difference between attribute- and object-based psychophysics?

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I think I could agree with most critical remarks that Lockhead (1992a) has made about a sterile construction of psychophysical scales of intensity. It is doubtful, however, whether I can accept his conclusion that psychophysical judgements will therefore be better interpreted by theories based on biology rather than those based on classical physics that have been continuously imitated by psychophysics since Fechner and Stevens. The main reason for Lockhead's unhappy conclusion appears to be the conviction that classical physics is based on the measurement of isolated attributes which can have no significance for complex biological organisms, of which the human observer is obviously one, because attributes can vary independently of objects and so do not reliably predict objects in the ordinary world. This is especially true concerning natural objects which form unanalyzable wholes, although man-made objects or their parts are sometimes separable into isolated attributes. Because the complex settings of the ordinary world are fundamentally different from the simple setting considered in the laboratory, the whole scaling theory developed between the sound- and light-confined walls of psychophysical labs is wrong. Consequently, the old attribute-based psychophysics needs to be replaced by object-based psychophysics, although it should be admitted that Lockhead does not have so much to say about what that would be. In my opinion, this radical reform proposal is a consequence of a misconception about physical attributes.

According to Lockhead's reconstruction, with which many commentators did not agree (e.g., Dzhafarov 1992), the attribute-based Stevensonian psychophysical scaling model is based on four basic assumptions, that (1) the subjective magnitude of an attribute of a stimulus is some function of the physical magnitude of that attribute and that this attribute can be judged independently of (2) other attributes, (3) spatial context, and (4) time. Stevens himself believed that magnitude estimation arises from people's fundamental "ability to separate out of a complex configuration one single aspect and to compare that aspect with the same aspect abstracted from another configuration" (Stevens 1975, p. 66). Lockhead seems to consider this assumption illicit and responsible for the failure of Stevensonian psychophysics in general.

There are two logical steps in Lockhead's argument. First, he demonstrates rather convincingly that all existing brightness scales of the form R = f(I) are incomplete because the intensity of the light I is not the only attribute on which the judgements about brightness are based and that this is not because the subject simply failed to understand the verbal instructions or tried to cheat. The reason is the observer's inability to abstract veridically the magnitude of the estimated stimulus from all other attributes and, in particular, from spatial and temporal properties of that stimulus (cf. Allik 1989). From this observation, which I think is absolutely correct, Lockhead jumps to a more general conclusion that "people cannot process one attribute of an integral stimulus independent of other attributes." This, I think, is wrong. The problem is not in the inability to abstract attributes; it is much more likely to be in the inability of the visual system to carry out verbal instructions exactly. Both the inadequacy of Stevensonian psychophysics and the existence of geometric illusions are perhaps best understood as a failure of the visual system to carry out the exact measurement called for by the verbal instruction (cf. Morgan et al. 1990).

There seem to be two classes of stimulus attributes which are treated very differently in psychophysics. The first consists of (1) privileged attributes which are typically listed in the international system of measurement. These are the attributes usually assessed in physical sciences and engineering practice and carefully described in school textbooks. The second class of attributes consists of (2) physical properties which are not