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Isomorphism: Setting the record straight

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Summary. This paper attempts to set forth and correct recent misinterpretations of the Gestalt concept of isomorphism. It distinguishes the new "isomorphisms" currently being proposed from the original conception of Gestalt psychology. Finally, it reviews the implications of isomorphism tested experimentally by Köhler and his collaborators, through the demonstrations of steady cortical currents during perception. Experimental attempts to discredit the theory of cortical currents are evaluated.

I know of no concept in psychology that has been more misunderstood, indeed more distorted, than isomorphism. I would like, therefore, to review the concept, examine some of the misunderstandings, consider objections that have been raised, and try to summarize its present status.

I will start with Köhler's last statement of the principle of psychophysical isomorphism. It is the hypothesis that the structural properties of processes in the nervous system are the same as the structural properties of the corresponding psychological facts (Köhler, 1969, p. 90). This is, of course, a very general statement that has to be specified.

Before we proceed to specify, it must be noted that this concept refers to psychological and to physical events that have structure. Since some of the misunderstandings occur at just this point, it should be indicated that, in both cases, structure or organization is a functional concept. It refers to the interdependence of parts in segregated entities, so that what happens in one local region depends on events not only in that region, but on processes in every part of the system. Gestalt psychologists, in connection with perceptual and other phenomena, have demonstrated the dependence of local properties on those of the whole in which they occur. In the case of natural physical systems, too, it is the distribution within the system as a whole which determines what happens in a local region. This is true, for example, of currents flowing in a network of wires, or of a drop of water in a river. The flow at each point depends upon interactions within the system as a whole.

The nature of the correspondence between psychological facts and physical facts in the brain assumed by Gestalt psychologists has not been well understood. I have elsewhere reviewed some of the misunderstandings but have by no means exhausted my collection (Henle, 1977).[1] I thought I had reached the limits of these distortions when I found Gestalt psychology described as the "picture-in-the-head Gestalt school" (Kaufman, 1974) or isomorphism taken to mean "pictures inside the brain" (Gregory, 1966). But no; here is a more recent version of isomorphism:

> Circular objects were supposed to give circular traces, square objects square-shaped traces, and so on. This supposed direct correspondence of external characteristics with scaled down but essentially similar brain states, is to say that we have diminished replicas of reality in our heads. It is to say that for every house we see there is, while seeing it, a tiny toy house formed of electrical potentials in the brain... The brain model is supposed to be three-dimensional... When we consider seeing a house with a green roof, are we to suppose that the corresponding part of the brain turns green? This is not considered by Köhler, or other ... "toys in the brain" theorists. (Gregory, 1974, pp. 255–256)

Another writer thinks he is contradicting the theory of Köhler and Koffka when he states:

> the representation of a circle within the convoluted and intricately interconnected folds of cortical tissues need not itself be circular, any more than incident light or sound in perception need engender anything that is correspondingly luminous or noisy within the skull. (Shepard, 1968, p. 287)

A much-cited variant of the misconception of isomorphism as a "picture-in-the-head" explanation is Skinner's (1963):

> The search for copies of the world within the body, particularly in the nervous system, still goes on, but with discouraging results... Suppose someone were to coat the occipital lobes of the brain with a special photographic emulsion which, when developed, yielded a reasonable copy of a current visual stimulus. In many quarters this would be regarded as a triumph in the physiology of vision. Yet nothing could be more disastrous, for we should have to start all over again and ask how the organism sees a picture in its occipital cortex, and we should now have much less of the brain available in which to seek an answer.

It is obvious that, since isomorphism does not refer to "copies of the world within the... nervous system," there is no need for a homunculus to view the pictures.

The major difficulty in these discussions centers around the nature of the similarity with which isomorphism is concerned. Similarity, admittedly, presents difficult problems. In the present case, however, it is clear that no literal or geometrical similarity is intended, as in the accounts quoted, but a functional one. Let us consider some examples.

First, we may quickly dispose of the notion that Gestalt psychologists say that part of the brain turns green when we see that green roof. Köhler explicitly denies it:

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1 I have even seen Koffka's name spelled as if it were Franz Kafka's – an error which seems to me to have a certain bizarre appropriateness: we think of Kafka's K. who runs about trying to find out of what crime he is accused.

> If certain processes in the human brain are said to be the cortical correlates of phenomenal colors, it is not implied that in those processes themselves there is anything like such colors. According to our present views this holds for all sensory qualities without exception. They all have cortical correlates; but their own existence seems to be restricted to the phenomenal world. (Köhler, 1938, p. 194)

Again, Köhler remarks (1938, p. 195): "The cortical correlate of the color blue is not blue." So much for chromatic brain processes. Whoever invented them, it was not the Gestalt psychologists.

There are relations which are geometrical but which also have functional meanings. Between is a good example. Functionally, one process is between two others if the latter can influence each of the others only by means of the third, the one in between. Köhler (1930, p. 569) offers this analogy:

> I tell Mr. A a story which is meant for Mr. F, whom for some reason I cannot address directly. Mr. A is in the same situation and hands the story down to Mr. B, Mr. B talks with C, C with D, D with E and, at last, E with F. Thus A, B, C, D, and E are "functionally between" me and F in this case of an influence. At the same time, F may be nearer to me geometrically than D; for example, he may even be "geometrically between" me and D. Still, functionally, the opposite is true.

It is in the functional meaning of *between* that isomorphism is interested.

In the same way, functional meanings of such concepts as segregation, continuity, boundary, inside, and outside have been worked out (Köhler, 1938, Chapter 6). These meanings are the ones we would consider in looking for the cortical correlates of the concepts in question. Here is one final example, a very simple one, which I cite precisely because it is simple enough for a beginning of concrete research:

> [An] elephant... is a macroscopic object, a separate entity in physical space. If an image of the animal is projected upon my retina, cortical processes within a circumscribed region of my brain are immediately segregated as a particular macroscopic unit, which is my "psychophysical elephant"; and one phenomenal thing, the elephant-percept, appears in my visual field. Three people walk before me on the physical street as distinct physical entities; correspondingly there are three psychophysical units in my cortex and three people-percepts in my visual space. (Köhler, 1938, p. 218)

Thus, one perceptual unit corresponds to one psychophysical unit, three to three.

It may be appropriate to mention some recent attempts to rescue isomorphism, because they illustrate the very misconceptions I have been discussing. Roger Shepard, after putting forth the familiar picture-in-the-head interpretation – rather misinterpretation – of isomorphism, which makes a perceived green square correspond to a square arrangement of green neurons, suggests a "second-order isomorphism":

> Thus, although the internal representation for a square need not itself be square, it should (whatever it is) at least have a closer functional relation to the internal representation for a rectangle than to that, say, for a green flash or the taste of persimmon. (Shepard & Chipman, 1970, p. 2)

This almost sounds like isomorphism until we realize that the functional relation between two internal representations here refers "most fundamentally, to the tendency of a response that has been directly associated with one to be aroused, also, by the activation of the other." An example is a naming response: Presumably the square is more likely to elicit the same naming response as a rectangle than to elicit the naming response for a green flash. Second-order isomorphism seems to be identified with the subjective similarity of phenomenal objects. The functional relations that Gestalt psychologists discuss are relations within a process itself, not between a representation and some response. And isomorphism in Gestalt psychology concerns the relation between phenomenal and physiological facts, not that between two phenomenal facts. I see no reason to confuse Shepard's concept with that of isomorphism.

In a later paper, Shepard (1981, pp. 290–291) restricts second-order isomorphism to the case of unanalyzable or unitary stimuli, best illustrated by homogeneous colors.[2] In the case of a differentiated structure, he prefers to speak of a "complementarity" between object and its internal representation, a relation analogous to that between lock and key.

Internal structure of an object or scene implies what Shepard calls "an 'abstract first-order isomorphism' between a representation and its object" (1981, p. 291), this abstract first-order isomorphism underlying the second-order isomorphism. But this abstract isomorphism between an object and its internal representation is not a matter of similarity, but of complementarity. And the relation in which Shepard is primarily interested is that "between the global properties of the external object and the corresponding functional organization of the internal process" [i.e., "certain unobserved processes in the brain" (1981, p. 282)].[3] Thus, isomorphism and complementarity deal not only with different relations, but with different referents. Isomorphism is concerned with the relation between perceptual (and other psychological) processes and the corresponding brain processes; complementarity deals with the relation between external [distal] objects and the corresponding brain processes.[4] Whatever the merits of the concept of complementarity, it cannot, therefore, be taken as a substitute for isomorphism as used by the Gestalt psychologists.

Julian Hochberg, too, has suggested a "new isomorphism":

> Perhaps it will be possible to construct a new form of isomorphism in terms of a hypothetical (or mental) "processing space," without trying to identify a concrete set of physiological processes that are counterparts to the things that we see in the world of experience. (1974, p. 193)

Of course, nobody can object in advance to such a procedure, but it seems to me only to confuse matters to call it isomorphism which, by definition, is something else.

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2 As indicated above, the Gestalt concept of isomorphism does not apply to homogeneous colors.

3 In discussing Köhler's isomorphism, Shepard correctly recognizes it as a hypothesized relation "between brain states and perceptual experience" (1981, p. 284). But in the same context, the concept is described as "the conveyance of the physical shape of the external object into the subject's physical brain" (ibid.). These two statements are not equivalent. Aside from the infelicitous expression [shape is not conveyed into the brain], no distinction appears to be made between perceptual and physical objects. The identification of these two objects (naive realism) makes Shepard's discussion irrelevant to the Gestalt concept of isomorphism.

4 Again, "I am using *psychophysical* in the specific sense in which it seems to be used by contemporary psychophysicists – to relate external stimuli to internal responses that are also presumed to have a physical embodiment, however abstractly it may be specified" (Shepard, 1981, p. 282).

Robert Shaw and Michael Turvey have introduced still another isomorphism, reciprocal isomorphism or duality. "Advisedly, we can now speak of the epistemic transactions animals engage in with their worlds as being reciprocal isomorphisms, or dualities, between designated affordances and effectivities ..." (1981, p. 379). These authors state clearly the difference between their concept and the psychophysical isomorphism of Gestalt psychology: "The two approaches differ radically, however, with respect to what counts as the object of experience" – perceptual experience as contrasted with the experience "of the functionally specified environment itself ... – an affordance structure" (1981, p. 411). Nor does isomorphism refer to the same relation in the two cases. Shaw and Turvey illustrate their concept:

> We see most clearly the essence of the duality relation when stated in words: "An object X affords grasping Y by an animal Z if and only if the structure of X is isomorphic with the structure of Z"; and, dually, "An animal Z can effect grasping Y if and only if the structure of Z is isomorphic with the structure of X." (1981, p. 388)

Here isomorphism refers to a reciprocity or a complementarity or a fittingness between two structures, not to the identity of structural properties of the Gestalt concept.

While the above account does not exhaust the new isomorphisms now being proposed, it does suggest that it is confusing to apply the same term to very different conceptions, often conceptions in which both the relations and the referents differ.

Since these new isomorphisms imply criticism of the Gestalt concept, it is appropriate at this point to mention two additional criticisms. The hypothesis of "toys in the brain" – not isomorphism – raises the question of cortical representation of the third dimension; isomorphism has been criticized for not dealing with it. Shepard (1981, p. 292), for example, cautions: "We need to be especially careful in considering the role of resemblance in the internal representation of three-dimensional objects."

This limitation has been acknowledged by Gestalt psychologists. Thus Köhler remarks: "It has not yet been possible to apply our physiological assumptions to all these phenomena. The reason is simply that nobody knows the physiological facts which underlie visual depth, kinesthesis, and learning" (1958, p. 153; cf., Köhler, 1965, p. 81). It has, therefore, not yet been possible to deduce the implications of isomorphism for three-dimensional perception.

Isomorphism has also been described as implying a passive conception of perception, the organism simply registering passive copies of stimulus patterns without contributing to them. Köhler rejects such criticisms:

> The 'self,' though functionally depending upon processes in the organism, is a phenomenal correlate only of a limited part of brain events. And 'objective' percepts ... are quite as much the correlates of intense processes in the same brain. That these processes, occurring in the same nervous system, should be passive copies of stimulus-patterns is certainly an idea which can no longer be seriously held. (1938, p. 92)

Now, more specifically, what does isomorphism mean?

One of the earliest findings of the Gestalt psychologists may be summarized under Wertheimer's principle of *Prägnanz:* that groupings, phenomenal objects, apparent movements, and so forth tend to be as simple and regular as the conditions permit. This same principle of simplicity and symmetry was earlier formulated by physicists. Mach, for example, observed that when a physical system approaches a state of equilibrium or a steady state, more and more of the forces balance each other, and the distribution of materials and forces tends to become increasingly simple and symmetrical. Thus Köhler suggests that a first specific case of isomorphism may be seen in this parallel: When perceptual phenomena show this tendency toward simple and regular structure, the corresponding brain processes – in accordance with physical principles – show this same tendency.

Such processes are too complex for a first investigation. Köhler started more modestly (cf., Köhler, 1940, Chapter 2). From the curious (though familiar) tendency of certain perceptual figures to reverse themselves during prolonged inspection, it occurred to him that the cortical processes corresponding to the figure might produce an obstruction to their own continuation, so that the percept could no longer continue in its original location; thus it moves to the other possible area. In turn, the new process appears to weaken itself, so that the original figure is seen again, and so on. It occurred to Köhler that such a weakening might be found also with figures that are not reversible, and this led to the investigation by Köhler and Wallach of figural aftereffects (1944). Here, too, changes occurred in the area of a percept which again suggested that the cortical process corresponding to a phenomenal figure obstructs itself. Köhler was able to formulate certain rules which the corresponding brain process must follow to account for the phenomena of figural aftereffects. From this point, he searched for a physical process which both followed these rules and was a process which could occur in the medium of the brain. Actually, only one such process could be found which satisfied both sets of conditions: "electric currents which originate and spread in the brain tissue as a continuous or volume conductor" (Köhler, 1969, p. 103).

There is no need to discuss such cortical currents, since there seems to be no doubt about their existence. The dispute concerns their relation to perception.

For Köhler, the next step was to demonstrate that such direct currents correspond to organized percepts. He began to record cortical currents from the heads of human subjects while they were watching slowly moving visual objects. And just as the phenomenal elephant corresponds to one psychophysical unit, three phenomenal people to three psychophysical units in the cortex, so when one visual object is presented, a single current is recorded; when the object is exposed three times, three responses are obtained. Sounds gave comparable results. Then the investigation was extended to animals in whom the electrodes could be placed directly on the exposed brain, and comparable results were obtained – stronger, of course, because the current did not have to pass through skull and scalp. "Are these currents really related to facts of perception?" asks Köhler. "Most probably they are" (1969, p. 117). He relates that a sleeping animal shows no responses to perceptual stimulation; but one has only to pinch the animal's ear, and in a few seconds a perfectly clear response is obtained.

Köhler's first report of his work on cortical currents appeared in *Science* in 1949 (Köhler & Held, 1949). Soon afterwards, Lashley and his coworkers published a challenge to the theory that direct currents are cortical correlates of visual objects (Lashley, Chow, & Semmes, 1951). Lashley placed strips of gold foil on the visual area of one monkey's brain and inserted gold pins into the visual cortex of another monkey. His argument was that, if cortical currents are significant for pattern vision, such vision should be severely disturbed by such a procedure. He thought that, since gold is such a good conductor, it should deflect the flow of cortical currents. No such disturbances were found. But Lashley's "gold-plated monkeys" – as he called them in a letter to Köhler – do not settle the issue. As Köhler pointed out:

> When laid on the cortex (or inserted in the tissue) Lashley's gold foils must have been polarized at once, must therefore have become unable to conduct, to deflect cortical currents, and thus to disturb pattern vision. (1965, p. 67)[5]

Another challenge came from Sperry and his collaborators (Sperry, Miner, & Myers, 1955; Sperry & Miner, 1955), who also took measures to disturb the flow of cortical currents. This was done by inserting metallic needles into the cortex of a cat or by inserting insulating material into cuts in the visual cortex. Again the animals performed well after these drastic procedures. Once more, however, the research cannot be accepted as evidence against the theory of cortical currents. Postmortem examination of the brains of the experimental animals revealed, in Köhler's summary, "a picture of utter devastation" (1965, p. 68). The animals, who may have been partially blind during the tests, nevertheless made difficult discriminations. Köhler, on three occasions, suggested the presence of an experimental artifact and expressed the hope that the experiments would be repeated with all necessary experimental controls (Köhler, 1958, 1959, 1965).

It is very hard to understand why, in the literature, the story ends with the work of Lashley and of Sperry, accepted for the most part without any question, while Köhler's replies are not mentioned. The following recent examples are typical:

> Both direct experimental tests (Lashley, Chow, & Semmes, 1951; Sperry, Miner, & Myers, 1955) and subsequent neurophysiological research have made Köhler's hypothesis of direct current flows highly unlikely. (Beck, 1982, p. 1)

> An experiment of Lashley, Chow, and Semmes (1951) showed that Köhler's proposal ["hypothesized field of electrical potentials and direct currents... in the cortex"] was simply wrong. (Attneave, 1982, p. 12)

> This rather vague hypothesis ["the Gestaltist 'brain field' theory"] tied their ideas to a physiological mechanism – which was later shown to be inappropriate (Lashley, Chow, & Semmes, 1951). (Palmer, 1982, p. 99)

> Brain fields of the type hypothesized by the Gestalt psychologists were taken seriously enough by Lashley, Chow, and Semmes (1951), by Sperry, Miner, and

5 A much more detailed analysis of Lashley's paper is contained in a long letter which Köhler wrote to Lashley, who had sent him the paper in manuscript. The letter, of which the first page is lost, is in the Library of the American Philosophical Society. Lashley urged Köhler to publish this reply simultaneously with Lashley's article.

Myers (1955), and by Sperry and Miner (1955), whose experiments, in the eyes of most, have put to rest forever any serious belief in these notions. (Pomerantz & Kubovy 1981, p. 428) [This last statement is unusual in mentioning, by reference only, Köhler's rebuttal.]

A more recent development is the work of Pribram. He implanted aluminum hydroxide cream in the brains of monkeys, a procedure which produces irritations evidenced by changed electrical activity. Retention of a visual discrimination was not disturbed, although original learning of the same discrimination by other animals was very markedly retarded after implantation. Pribram writes that Köhler said about these experiments: "That ruins not only my D.C. field but every other current neurological theory of perception" (Pribram, 1971, p. 111).

Unfortunately Köhler has not commented on these experiments in print. But there was an exchange of letters between him and Pribram, which Karl Pribram very kindly sent to me; Köhler's letter is also in the possession of the Library of the American Philosophical Society.[6]

Pribram (May 12, 1958), reporting the results just cited, asks what Köhler makes of them and remarks that his own interpretation (i.e., his interpretation at that time) does not do violence to Köhler's experiments and to his interpretation. Köhler's reply (February 13, 1959) does not sound like that of a person who has just abandoned his theory:

> I wish we could soon have a good talk, first of all, about the Aluminum hydroxide experiments. My first question would be: What exactly does this material do in the tissue? Your older experiments seemed to prove that it does not greatly disturb fundamental visual processes. The assumption is that these processes would be disturbed if the synaptic currents of the tissue were strongly affected. This would be true not only from my point of view but also according to Barron and Matthews, Eccles, and others. Consequently, according to your results, these currents cannot be seriously affected. Once more, therefore, what does the chemical do? How does it distribute itself in the tissue? There is one possibility. If its distribution were statistically, or macroscopically, even, all currents would be weakened, but their distribution would not be altered, and as a consequence, the organizational characteristics of the currents would remain approximately normal. On the other hand, if the currents are responsible for the formation of "traces", they might now be too weak to establish normal or stable trace patterns, and thus your new findings about the difficulty of new *learning* could be understood.
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> This, of course, is the old physicist talking again, who wants to know more precisely about physical and chemical action in given brain experiments before he decides what certain experiments mean. You are referring to some cognitive mechanism involved in the situation. The old physicist does not feel safe when he hears this expression. At times, it is necessary to use such preliminary suggestions. But sooner or later, we shall have to talk physics and chemistry of the brain. Don't you agree?

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6 Acknowledgement is gratefully made to the Library of the American Philosophical Society for permission to publish Köhler's letter.

Pribram (1980) likewise cites the work of Lashley and Sperry and their collaborators, as well as his own work, as throwing doubt on the relationship between cortical currents and perception. "Direct current fields... seem to be unrelated to the structuring of percepts." In another place, Pribram (in press) refers to these three lines of research and concludes: "This led Köhler to remark that not only his theory but every other brain theory of perception had been jeopardized." Köhler's written discussions of the findings of Lashley, Sperry, and Pribram – which in no way suggest that his theory was in jeopardy – are not mentioned.

I can only wonder whether Pribram misinterpreted a remark of Köhler's, a joke perhaps. In any case, after 1959, Köhler wrote several times on his theory of cortical currents, without regarding it as "ruined."

Pribram (in press) speculates that Köhler was beginning to think in terms of "microfields centering on synaptic events" as substituting for or underlying the macrofields. But there is no need for speculation at this point. As early as 1957, Köhler and O'Connell suggested a hypothesis broached earlier (Köhler & Wegener, 1955) that "the currents shown in our records are those of cortical synaptical potentials" (Köhler & O'Connell, 1957, p. 40). Again, "most probably, the sources of perceptual currents are activated synapses in the cortex" (Köhler, 1958, p. 153).

In sum, careful reading of the record shows no evidence that Köhler gave up, or modified, his theory of cortical currents as the correlates of organized perceptions under the impact of any of the experiments so often cited against it. These experiments simply do not bear on the theory.

Since Köhler's work has not, to my knowledge, been followed up, I do not know whether the theory of cortical currents as correlates of pattern vision is correct. But I do know that implications of theories must be correctly drawn to test a theory, and that this has not always been done in the present case (Lashley, et al.). Experimental controls seem to have been neglected (Sperry, et al.). And the specific effects of interventions must be understood before results may be interpreted as disproving a theory; again, this step has here been neglected (Pribram). It seems to me that, until this situation has been corrected, we cannot know whether the theory of cortical currents is correct or whether it is promising.

Isomorphism will not go away if we misunderstand it or if we try to ridicule it to death. Koffka (1935, pp. 62–63) writes: "That *some* isomorphism was necessary has been held by most psychologists since the times of Hering and Mach." He cites Hering's theory of color vision, an "almost casual" isomorphism in G.E. Müller, and a more far-reaching but practically unknown isomorphism in Mach. Koffka remarks that Mach's isomorphism looks identical with that of Wertheimer and Köhler, neither of whom knew about it. He adds: "I found the passage in Mach to my great surprise by mere accident."

If my review is accurate, the theory of cortical currents remains essentially untested beyond Köhler's early tests. Why don't we give it a chance by further testing, rather than rule it out in advance?

Even if the theory of cortical currents should prove incorrect, the question of isomorphism remains. It is a heuristic not to be ignored. It involves finding cortical processes that will account for the specific functional properties of psychological facts. A hypothesis about any such processes would lead to new psychological data, just as the theory of cortical currents led to new discoveries in perception and to new hypotheses in the fields of memory and attention.

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