

Egon Brunswik: 1903-1955

The Psychology of Egon Brunswik

EDITED BY

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A Critical Consideration of Egon Brunswik's Probabilistic Functionalism

ROBERT WARD LEEPER

THE CIRCUMSTANCES AND NATURE OF THE PRESENT PAPER

Early in 1954, a letter from Gardner Murphy invited me to join Egon Brunswik and himself in one of the symposia at the International Congress of Psychology at Montreal in the following June. The symposium was to have the cosmic-sounding title, "The Relation of the Person to His Environment." Murphy's paper was to deal with concepts of the boundaries between the person and the environment, Brunswik's with his ideas of similarities and differences between thinking and perception as "ratiomorphic" types of functioning. My own paper, Murphy proposed, might examine the implications of field theory for psychology.

In the half-year that followed, the three of us did a lot of exchanging of suggestions and criticisms on our preliminary drafts. In this period, I soon became convinced that, instead of dealing with the topic that Gardner Murphy had originally suggested to me, my paper might more appropriately provide a commentary on Brunswik's ideas. This was eventually its form.

After the Congress, Murphy's paper was published (1956); but Brunswik's paper, now included in the present volume, was never published, and my commentary on it similarly was never submitted for publication. (For abstracts, see Brunswik (1955a) and Leeper (1955).)

My failure to publish this paper left me with a feeling of unmet obligation. The preparation for this symposium not only had necessitated a more intensive study of Brunswik's ideas than most psychologists have been able to make, but also had given me an unusual opportunity to get

acquainted with Brunswik's thinking in almost the last year of his life. My further acquaintance with Brunswik's publications, furthermore, convinced me that there was some considerable need not only for some clearer and simpler statement of Brunswik's main ideas, but also for a critical re-examination of those ideas on the basis of a wider array of examples than Brunswik commonly had used.

In the period since the Congress in 1954, three important sets of material have been published that add very significantly to the papers dealing with Brunswik's work. These are:

1. The several papers—and especially those from Brunswik—published in the *Psychological Review* in 1955 from a Symposium on the Probability Approach in Psychology at the University of California in Berkeley in July, 1953;
2. Brunswik's *Perception and the Representative Design of Psychological Experiments*, published posthumously in 1956;
3. The summary of Brunswik's ideas and research by Leo Postman and Edward Tolman in Volume 1 of Sigmund Koch's *Psychology: A Study of a Science* (1959).

These publications, it seems to me, still leave a great need for clarification and critical discussion of Brunswik's contributions, but they certainly put us into a much more favorable situation for such an attempt now than we were in 1954. Also, in my own personal case, it has been possible to give a much longer consideration of Brunswik's ideas than I could in 1954. Consequently, although the present paper attempts the same type of role that my briefer paper tried to serve at the Montreal Congress, it has attempted a more inclusive discussion of Brunswik's work, even that it by no means covers all aspects of that.

To some persons, it will probably seem inappropriate for a paper in a memorial volume to be as bluntly critical as the present paper will be on a number of scores or for it to suggest that there are so many points where Brunswik's ideas need to be rounded out into a more comprehensive psychological theory. Such criticisms and suggestions may seem inappropriate not merely because there is no opportunity for Brunswik to reply and correct those points where I may have failed to catch the intent of his writing, but also because such a paper may seem inconsistent with the customary tone and purpose of such memorial volumes. However, what seems most important is that our present discussions should take Brunswik's ideas really seriously and try to find some means whereby they can be brought into the main streams of contemporary psychology. It seems that Brunswik truly was making some very important and original contributions. But it also seems that, if these contributions are

not to be lost to most psychologists, we must get a much clearer idea of what they are, what their strengths and limitations are, and what modifications and developments they might receive.

In our discussions of these matters, there is obviously a basic difference between Egon Brunswik and myself. Brunswik was primarily a creative worker—not only in theoretical matters, but also in his ability to move naturally back and forth between theoretical discussions and ingenious experimental work suited to yield striking demonstrations of his concepts. But he was relatively unconcerned with trying to make his presentations simple or clear. The important task, as he saw things, was to develop and state some fundamentally new modes of thought. In our correspondence about the Montreal symposium, I urged various clarifications of his paper, particularly to take into account the fact that many of his audience would be persons for whom English would be a more or less unfamiliar language. He acceded to some suggestions regarding the length and complexity of his sentences, but for the most part he waved such suggestions aside. "If you are going to present difficult ideas," he finally declared, "you simply have to present them in difficult terms."

My own tendencies are different. For one thing, I am convinced that, when a person is dealing with complicated theoretical issues, he needs to try to state things in the simplest possible terms, because I am convinced that a theoretical worker, otherwise, is likely to create difficulties not only for other persons, but also for himself. For another thing, I have a basic distrust of long logical leaps that do not come down fairly often to make sure that they are keeping contact with a broad base of empirical realities. I feel that there may be some gains from trying to approach Brunswik's work with these different major objectives. But, even if so, it can easily be seen that the difference in possible contribution is the difference between the person who is primarily engaged in creative work and the other person who, if he is making any theoretical contribution at all, is doing so merely through tidying up the contributions of others. I hope the present paper may have some contribution of this latter sort. At the same time, I realize that cleaning women sometimes throw away some extremely useful things and sometimes rearrange objects in some awfully odd ways. Maybe this will happen in the following. The risk must be taken, however, because it is important that Brunswik's ideas not be relegated to some museum, but be brought into the living give-and-take of psychological controversy, involving them in the dialectic process that Boring so often has praised, especially in his years as editor of *Contemporary Psychology*. As Boring has said, such vigorous discussion is probably indispensable for bringing out the important potentialities of any

complex body of thought. Such considerations, then, have prompted the character of the present paper.

THE LIMITED USE OF BRUNSWIK'S IDEAS

It may be that I underestimate the degree to which Brunswik's ideas have been studied and used by psychologists. However, it seems safe to say that only a very small percentage of psychologists have anything except some very general and frequently rather inaccurate impressions regarding Brunswik's contributions. As far as I can judge, both from printed statements about Brunswik's work and from querying a number of psychologists about this topic, the common conception of Brunswik's work, insofar as this work is known at all, is made up of the following impressions:

Impression 1

Brunswik was somewhat of a bridge between the older psychology of Germany and Austria, on the one hand, with its main emphasis on perceptual phenomena, and American experimental psychology, on the other hand, with its stress on learning and with its predominantly biological and objective orientation. His influence probably came partly through his contributions to Tolman's thinking. This relationship and also the perceptual ancestry of his thinking probably indicate that it is more soft-minded than the usual American tradition.

Impression 2

Brunswik was much interested in the philosophy-of-science viewpoint which developed in Vienna. Hence, he may have played some part in producing some of the present background assumptions of much of American psychology. However, since the pronouncements of this philosophy of science turn out to be more equivocal than originally had been claimed, they are less revolutionary than was expected, and this phase of Brunswik's contribution may be mostly just of historical interest, rather than something significant for current psychology.

Impression 3

Brunswik spoke a lot about the need for "representative design" in psychological research, but the meaning of this is rather obscure. It may be mainly an advocacy of use of real-life situations as much as possible and a disparagement of artificial laboratory situations. This, in turn,

must mean that he had some old-fashioned sentimental resistance to the necessary methods of scientific work.

Impression 4

Brunswik spoke a lot about the value of correlational studies of perception and perhaps of other sorts of phenomena. This had something to do with his desire to demonstrate that perception is generally a fairly accurate process. However, it is hard to see what significance such correlational methods might have, other than their lending some support to this very general point.

Impression 5

In general, then, Brunswik probably proposed no ideas that are not fairly well represented in our present-day thought in experimental psychology. Any careful study of his writings may perhaps safely be left to those who are primarily interested in the history of psychology.

If the above is even an approximation of the thinking of the majority of psychologists, it means that there are some recognizable points of relationship between this image of Brunswik's work and the actual facts of the matter. But it also means that there has been a very inadequate grasping and utilization of Brunswik's concepts. If such actually is the case, and if there is danger that it may continue, it is important to try to learn why there has been this limited use of Brunswik's contributions, and it is important to try to counteract these factors.

SOME MAIN REASONS FOR THIS LIMITED UNDERSTANDING

There have been, I believe, four main factors that have tended to account for the limited understanding and use of Brunswik's contributions. These seem to be the following:

1. Brunswik was contributing some genuinely new ideas on certain scores, and these were ideas, in many cases, which were not in keeping with the Zeigist, as Boring calls it (1955), of the age in which they occurred. For one thing, he was stressing perceptual problems in a period in which American psychology had not yet overcome its behavioristic prejudices against working on perceptual phenomena because of their earlier alignment with discussions of subjective experience and introspective or phenomenological methods of observation. For another thing, he was proposing some new means of thinking about perception and about other phenomena. He was using concepts that the majority of us

had not practiced with. He had developed some rather well-chosen terms for referring to these concepts, but these terms also were unfamiliar to us. Consequently, his writings presented some inevitable difficulties such as one ought reasonably to expect when a new conceptual system and a new terminology are proposed. Part of the difficulties of communication were inevitable. They were difficulties such as always will be met whenever any worker proposes some fundamentally new modes of thought.

2. Other difficulties in understanding Brunswik's proposals came simply from difficulties of style. A great deal of the difficulty comes from the compactness with which Brunswik wrote and from the large number of technical terms that he characteristically crammed into single sentences. There are difficulties, too, from the cumbersome and involved character of many of his sentences. If I were to try here to document adequately such statements, it would require a considerable sample, and there is no space or point in that. But, I believe that any reader can establish this point for himself by turning to the several papers from Brunswik that have been printed in this volume, including the papers he wrote for presentation at meetings. These were not papers that a listener could read and reread, and they ought to have been simpler than the printed papers. But they are papers, like the rest of Brunswik's writing, which require rereading after rereading before one gets each part sufficiently well mastered to grasp, adequately, the larger units of thought.

I know in my own experience that I have had to read and reread Brunswik's papers a number of times, working slowly to relate his statements to background material that I could call to mind, before I could get a clear and usable understanding of much of what he was saying. His writing is rewarding with such repeated reading. It is not like writing which seems very impressive on first acquaintance, but which becomes less impressive the more carefully one analyzes its logic. Brunswik "wears well," as one might say. But it is not writing that communicates much to those who feel that they must read it rapidly or read it merely one or two times.

3. Even though Brunswik did a magnificent job in delineating some of the main historical trends of psychology, especially in his monograph on "The Conceptual Framework of Psychology" (1952), there are occasional respects in which Brunswik doubtless has struck the advocates of other points of view as having misunderstood some parts of their interpretations. For instance, speaking of Adler's work, Brunswik said: "... Adler's major contribution, the attempt to shift earlier reduction schemes from the sex drive to the desire for mastery and prestige, is merely a change of content." (1952, p. 60.) On this, it might well be

maintained that the concept of personality as basically a matter of a learned "life style" is a much more fundamental contribution.

In speaking about Gestalt psychology, Brunswik spoke about it as recognizing vicarious functioning through the principle of transposition. But, he added:

"Since this principle of 'transposition' ignores 'families' of cues the members of which do not formally resemble each other but are held together merely by association . . . , recognition of vicariousness remains limited to one of its comparatively trivial aspects. . . ." (1952, p. 62.) A Gestaltist would feel troubled by this for, even though he would agree that it is appropriate to speak about transposition only when the equivalence between two patterns of stimulation is one that is independent of training, this point does not at all mean that Gestalt theory is unconcerned about other equivalences that are learned. The work of Köhler with chimpanzees, for instance, certainly involved lots of instances where chimpanzees had to learn that one object or one method of dealing with things was equivalent to some other. Similarly, a Gestaltist would be troubled by Brunswik's characterization of their work as having been concerned particularly with ambiguity and illusion, as being almost solely subjectivistic or phenomenalist in approach, and of having a mode of approach in which "... the intricate problems of psycho-environmental (central-distal) stimulus-response coordination are, by both Köhler and Koffka, summarily dismissed by allusions to a vaguely conceived kind of pre-established harmony (or extended isomorphism) between the structural principles of the surroundings and the field dynamics within the organism." (1952, p. 63.) It is true, for instance, that Gestalt psychologists have spent much time in studies of perceptual illusions, but the reason for this interest has been the desire to learn how it is possible for the organism to perceive veridically so many features of its environment. The work with illusions, for example, helps to eliminate the overly simple principle that people otherwise tend to trust that we perceive things as we do because the perceptual mechanisms merely reflect external realities or peripheral stimulation.

In the same way, an S-R psychologist might feel uncomfortable about Brunswik's description of the work of Watson and Hull as having been concerned, not with the stimulus situation and with the results of behavior, but with correlations between receptor events and happenings in the effector organs. Such a theorist might well say, "True enough, Watson typically talked in these terms, but his experimental work of course was rarely in those terms—he described, for example, the apparatus in which he placed the animals he tested, and he described the errors they made

and the successful reaching of goal-boxes. So, he was not mainly dealing with 'proximal-proximal' relationships!"

In the huge territory that Brunswik described in his aim to portray the gradual convergence of psychological theory and research toward its current character, such errors of characterization, if indeed they are such, are relatively infrequent by comparison with the very penetrating and informative statements on a vast and complex array of types of work. However, I believe it still is true that a number of other groups of psychologists have felt that, at some points, Brunswik was describing their work in terms that were not valid.

4. Another factor that has produced some considerable degree of difficulty in understanding and using Brunswik's ideas is that, with a number of his major concepts, Brunswik had not yet worked out the implications of his thinking nearly as adequately as they will need to be worked out. In a number of cases, key terms are inadequately defined. In a number of matters, the meaning or implication of major principles is not explored with more than one or a few examples in each case. Even when Brunswik wrote about some matters repeatedly, the successive discussions tended to repeat the examples and make essentially the same statements that already had been given in previous discussions. Consequently, some degree of confusion and uncertainty prevails at a number of points.

For example, the term "ecology" is a key term for Brunswik. Representative research design calls for a representative sampling of situations from an "ecology" or "natural-cultural habitat." But, what is an ecology or natural-cultural habitat? Postman and Tolman, in the paper in which they attempted to summarize Brunswik's views in Koch's volume, said:

"Representative design" thus refers to investigations in which the external ecology of the organism is studied in a sample of situations. . . . It must also be emphasized that representative design does not refer to the sampling of *variables*. . . . When variables are sampled, there are as many universes as there are variables. But there is only one universe of environmental situations; that is, there is only one ecology for a given organism. (1959, p. 521.)

One might well wonder whether this was Brunswik's intent. When a person moves from a farm to a city, or is confined in jail, or finishes medical school and starts his practice, does he still have the same ecology? To the best of my knowledge, Brunswik's papers do not give anything that explicitly and clearly answers this question. My own impression is that Brunswik felt that an individual might be considered from the standpoint of various ecologies. Thus, speaking about his own experiment on size constancy, he said: "More drastic mismeasures of size, such

as of the moon, are probably mostly cases of going beyond the confines of the ecology of manipulable things." (1965, p. 489.) Now, if there is an ecology of manipulable things for a given person, there must also be another ecology that includes the moon, sun, stars, clouds, and flashes of lightning. There presumably would be other ecologies.

Even though this term was so central for Brunswik's major discussions, he did not state, however, how he would have dealt with questions such as these.

Another instance concerns the functions governed by the "lens model." As will be explained later, it seems that there were two types of examples that Brunswik used in speaking about this concept. He moved back and forth between these two types of examples without raising the question of whether two somewhat different sets of phenomena might be involved in them.

Brunswik's general idea of a need for a broad sampling-base for psychological generalizations ought to have led him to consider a wide array of examples for each major proposition that he was making. Instead, the successive discussions in different articles and monographs tended to repeat the same examples presented previously in the same context. From this, it has been more difficult for other psychologists to see what possibilities there were in Brunswik's concepts.

On a good many of these points, I think that part of the origin of these difficulties was the fact that Brunswik worked more nearly alone—with less interaction with other psychologists—than would have been optimal for the development and clarification of his proposals. It would have been better if his work could have been the center of a more lively and extensive series of controversies. There was some of such, as in the July 1953, with its two papers by Brunswik (1955b, 1955c) and the critical papers by Postman (1955), Hilgard (1955), Krech (1955), Feigl (1955), and Hammond (1955). But there was less of this than there should have been. It would have been better if more discussion could have occurred while it could have had the criticisms and reactions of Brunswik. That is now impossible. But, in the lack of that, it seems to me that the most genuine tribute to him that we can give is to be as vigorous and forthright in our criticisms and in our proposals for revision of his work as we can be. Hence the character of the present paper.

THE MAIN SUBSTANTIVE IDEAS FROM BRUNSWIK

To set the stage for the later evaluations and revisions that I will suggest, there are some advantages in grouping Brunswik's concepts as

falling within two different groups. In the first place, Brunswik proposed a number of fundamental concepts regarding the nature of the phenomena that psychology is investigating empirically. In the second place, taking into consideration the nature of psychological phenomena, he felt that a number of rather distinctive methodological principles ought to be recognized and used by psychologists. In some ways, it seems he was more interested in such methodological problems than in empirical questions. But, if so, this was only because he felt that empirical research will be wasteful and inefficient until it is based on methodological principles appropriate to the subject-matter of psychology, methodological principles founded, not primarily on logical or philosophical grounds, but by reference to the nature of psychological phenomena. Brunswik spoke of this relationship as "the methodological postulate of behavior-research isomorphism." Thus:

... one may . . . demand that the "order," or pattern, of research "ideas," or design, should be the same as the pattern of the "things" studied, which in our case is behavior. Research may be said to have reached an adequate, "functional," or "molar" level of complexity only if it parallels, and is thus capable of representing, behavior in all its essential features. We may call this the methodological postulate of behavior-research isomorphism. (1952, p. 25)

In line with this mode of thinking, let us speak first of the relatively empirical or substantive principles that Brunswik emphasized. His most important principles seem to me to be the following. Let us state them without any attempt, for the present, to evaluate them.

(1) *The Adaptive Significance of Behavior and of Psychological Processes.* Psychologists need to adopt the general biological evolutionary point of view. Psychological processes are mainly a means of biological adaptation, mainly matters of "the readjustive value of behavior in coming to terms with the physical or social environment," to quote from the last paper which Brunswik gave (in December, 1954, before the Section on History and Philosophy of Science of the AAAS).

(2) *The Dependence of Adaptation on Responses Related to Things Distant in Time and/or Space.* As Brunswik phrased it, "Forced to react quickly or within reasonable limits of time, it (the organism) must respond before direct contact with the relevant remote conditions in the environment, such as foodstuffs or traps, friends or enemies, can be established." (1952, p. 22.) Particularly is this true with man and the other higher animals. Their biological survival depends on their taking into account the relatively remote factors that they can take into account because of their fine distance receptors, their great learning ability, and their capacity for social communication.

(3) *The Limited Ecological Validity of the Cues and Means Which Must Be Used in Dealing with Remote Factors.* This molding of behavior to deal with factors remote in space and/or time has to depend on factors that are of imperfect ecological validity—that is, that are more or less unreliably related to the objective factors and possible distal achievements that are the really important things for the organism. Even though some cues are more nearly valid than are others, none are absolutely trustworthy, and no means that the organism can use can be counted on as having utterly invariable results.

(4) *Lens Functioning as an Aid in Dealing with Such a World.* In this semierratic world, organisms are helped by their use of processes that may be understood to some extent by means of a "lens model" of organismic functioning. Just as a lens permits a camera to take a picture in relatively dim light because it takes scattering bundles of rays of light and refocuses each bundle on a point on the film, so the lens-functioning of the organism permits multiple use of cues (or means), thereby permitting better information (or more certain action) than the organism could achieve merely through one cue (or means) or another. Also, just as a lens would permit the use of a narrow beam of light focused only on any part of a lens, rather than requiring that the same part of the lens would be used in each case, so also does the lens-functioning of the organism make possible a vicarious substitution of one means or cue for another. Thus, if two people cannot communicate verbally with each other because they speak different languages, they may revert to gestures or to some other means of communication. Consequently, even though the organism lives in a world that is extremely heterogeneous, there is more stability in the achievements of the organism than we could expect were it not for this lens phenomenon.¹

(5) *The Organism's Need of Dealing in Terms of Probabilities.* Even with this lens-functioning, however, the organism cannot be sure of effective adaptation. The best that the organism can do is to live in terms of probabilities of different costs and different possible gains. Some highly unlikely reward may still be worth struggling for, and some highly unlikely disaster may be worth guarding against, but what the organism must always do is to try to live in terms of the best estimates of "probability times cost" and "probability times possible rewards."

(6) *The Rationomorphic Character of Cognitive Processes.* It would be a mistake to understand psychological processes in rationalistic

1. It may seem that, in this discussion of the lens model, I am introducing a number of revisions into Brunswik's concepts, rather than merely summarizing them. However, these are the ideas that are implicit, it seems to me, in Brunswik's examples and abstract statements on this matter.

or intellectualistic terms. But, in some more basic biological sense, psychological processes nevertheless are something which "... involves the particular type of orderly interaction we find best exemplified in syllogistic reasoning or in mathematical calculation." (1965, p. 487.) For example, the individual might have learned that, when he hears thunder, he may also expect some rain. From this general premise, and from noting at a given time that there is some thunder, he will tend to anticipate that rain is likely to come soon. The same holds true when a number of cues are taken into consideration and when some compromise is reached with regard to the divergent testimony they give. The organism bases its behavior on reasoning-like combinations of premises to reach conclusions.

(7) *The Lack of Complete Equivalence of Different Lens-Functions.* As we have mentioned above, Brunswik placed major emphasis on the idea that the organism needs to be described by a "lens model" or needs to be understood as capable of "vicarious mediation" or utilization of any of a number of cues or means required to attain required ends under varying conditions. However, Brunswik did not regard the "lens" as a completely perfect means of substitution of one cue or means for another. As he said, "Imperfections of achievement may in part be ascribable to the 'lens' itself, that is, to the organism as an imperfect machine." (1952, p. 23.) He spoke particularly, in his last writings, of some differences that he believed tended to come in consequence of whether the individual tended to make a judgment about an environmental situation by means of sensory perceptions or by means of "thinking." This would be merely one instance of a host of examples of the fact that, when the organism chooses to use one means of response rather than another, some variations in the distal achievement or behavior-results will come because of the particular means chosen.

(8) *The Importance of Situational Determinants of Behavior.* Psychology cannot be made up solely of principles to the effect that such and such species of organisms (or types of individuals) tend to show such and such behavior. Must of our empirical research, and probably even the greatest part of that empirical research, needs to be concerned with questions about what situational factors produce what effects. As Brunswik said:

It may well be that in many contexts individuals in a population are more homogeneous or stereotyped than are situations in an ecology, and that the ascertainment of ecological generality may be a more challenging [and profitable] task than that of responder-population generality. . . . Ebbinghaus needed only himself as a subject to lay the foundation for much of modern learning theory.... (1955b, p. 202.)

In general, in these principles, Brunswik was emphasizing the idea that the specific means by which the organism adapts do not matter very much. There is more stability in what the organism accomplishes than in the means which the organism uses to accomplish such things. There is more stability in what the organism accomplishes than in the cues by which it judges what it has to cope with.

THE MAIN METHODOLOGICAL PRINCIPLES FROM BRUNSWIK

From such concepts about the nature of psychological processes, Brunswik derived various propositions regarding how psychologists ought to proceed in their work of research and theory-construction. As I understand his methodological principles, his main proposals were the following:

(1) *We Need an Adequate Basis of Sampling for Whatever Abstract Principles We Propose.* The material with which we are working in psychological research is not like some chemical that can be secured in some purified form. Instead, there is almost infinite variety in the situations in which behavior occurs. Hence, we cannot take some one or some few samples of some broad class of situations and be sure, from our observation with such inadequate samples, that what has been manifested there, even if quite clearly and significantly, will also be manifested by other examples from the range of situations that would be covered by the abstract principles that we are tempted to use in reporting our results.

Psychologists have learned, Brunswik said, that a research worker must be careful to get adequate samples of types of individuals whom he wishes to compare. But we have been very negligent (perhaps because of the huge labor that this rule would involve) of the fact that, when we talk about the effects of different types of situations, we need some adequate sampling of them just as truly as we need some sampling of individuals. The logic of the two matters is the same. Thus:

Everyone knows that encountering, say, a wife who is taller than her husband . . . does not justify the inference that wives . . . are always, or are overwhelmingly (or, Brunswik might have said, frequently), taller than their husbands. . . . What the instances mentioned do demonstrate, however, is . . . that it is possible for a wife to be taller than her husband. . . .

(In the same way, since experimental research typically involves merely a standardized sample of a certain type of situation.) Experiments in the biological and social sciences are often formally analogous to the instances referred to above, . . . they do demonstrate a mere possibility.... (1956b, p. 54.)

It is difficult, however, for psychologists to make their conclusions as modest as their evidence. To guard against the temptations of over-generalization, we must realize that there is a sampling problem involved whenever we make abstract statements.

(2) *Representative Sampling of Natural-Cultural Habitats as Essential for Significant Quantitative Principles.* Psychology needs quantitative principles. It is not sufficient merely to know that "this factor has this type of influence in some situations." But, if we are to have good quantitative principles, this calls for much more than just demonstrating a "statistically significant difference" or a "statistically significant correlation." Something like that is merely the prelude for other work to determine quantitative relations. Particularly, any quantitative statement needs to be based on evidence sufficient to indicate the relative weight of some factor in a universe of situations which can be effectively described so that other workers—whether in other research or in practical applications of such rules—can know what domain the quantitative generalization applies to.

Such domains could be defined artificially or arbitrarily, but the work of psychology would be endless if this were our procedure. Instead, we need to deal with domains defined in some more externally given fashion. Hence we need to deal with the natural-cultural habitats or ecologies of organisms. Our studies need to give us a representative sampling (whether by random sampling or by some more stratified means) from such domains. Out of such representative sampling—and only by this means—can we get the data that will permit us to make good quantitative principles.

(3) *The Further Reasons for Representative Research Design.* The reason for working with natural-cultural habitats or ecologies is more than just the need for drawing samples from domains that can be meaningfully specified. Representative research involves working with real-life situations, as contrasted with the artificially simplified and stereotyped situations generally used in experimental studies (in "systematic research design," as Brunswik terms it). The use of such real-life situations has three virtues.

- a. The different variables in real-life situations are merely irregularly related to each other. Unlike what is true of the majority of experiments on learning, for example, there is no one action in real life that will always be rewarded, and no discriminable cue that always will indicate the same referent. A sampling of real-life situations gives an opportunity for the organism to demonstrate its modes of functioning in probabilistic situations.
- b. Because real-life situations have not been artificially simplified, they offer abundant opportunity for the organism to display the phenomenon of vicarious functioning.

- c. In representative design, real-life situations have not been artificially simplified, with some variables eliminated from the situation. Hence these situations permit interactional effects to occur which we need to learn about.

Such considerations are important, Brunswik said, because the need in psychology is not solely for "... rigor of fact-finding, inference, and communication," but is a need also "to establish exact study on an adequate level of complexity." (1952, p. 1.)

(4) *Psychology Needs to Seek for Probabilistic Laws.* A search for strict laws—a use of the "nomothetic approach," as Brunswik defines this—is appropriate in some other sciences. It would be appropriate also in psychology if it were feasible for psychologists to make allowance for all the many chance influences that occur in the objective environments with which the organism is dealing. If psychologists were omniscient, superhuman, they conceivably could have all the data for predicting that, in a given case, the behavior of the organism actually would produce such and such unusual effects because of the chance variations of conditions affecting the validities of cues and/or means. But, neither the organism nor the psychologist can predict the chance developments that will occur in real-life situations. Hence, the organism has to deal with its environment in terms of probabilities. The psychologist also, in trying to understand the functioning of that organism has to work in terms of probabilistic laws that will take into account the uncertainties in the cues that the organism will receive and the uncertainties in the means that it employs to try to attain its ends. Psychology should try to estimate these probabilities as precisely and exactly as possible, just as it is true that studies of parent-child relationships should attempt to determine as precisely as possible what the correlation, say, is apt to be between the characteristics of parents and the characteristics of offspring. But these probabilistic laws will be descriptive merely of the probabilities for any given individual, just as statements in genetics attempt to state the probabilities that dominant or recessive traits will show in an offspring of parents of a given genetic type. It is unrealistic to believe that we can ever make definite and exact predictions of what a given organism will do in a given situation, just as it is impossible to predict what set of chromosomes will be selected by the reduction-division of a given reproductive cell. We can attempt to estimate probabilities exactly, but this is basically different from attempting to form strict laws. Psychological laws must be based on studies of situations which involve the same sorts of uncertain relationships that exist in abstractly described ecologies.

(5) *Psychological Research and Theory Ought to Deal Mainly with Distal-Distal Relationships.* At quite a few points, Brunswik's

statements either *appear* to call for casting psychological principles in terms of statements about the relationships of "distal focal variables" (relationships of environmental situations to behavior products) or quite unmistakably advocate this sort of formulation.

Thus, what seems a very clear advocacy of distal-distal principles is given in the concluding chapter, on "Convergence toward an objective functional approach," in his monograph on "The Conceptual Framework of Psychology." Referring first to some earlier writing by himself in 1934, Brunswik said:

The present writer has spoken of a "psychology in terms of objects" . . . in which organisms are described, and differentiated from one another, by reference to the—predominantly distal—stimulus or result variables with which they have "attained" stabilized relationships. By applying this approach to distal-to-distal functional arcs bridging over the entire organism without descending into it [see Fig. 1, Chap. 1, this volume, Ed.], one may further gain in scope and at the same time get around the hazardous construction of intervening variables. . . . In specifying this proposal by urging positive ascertainment of focusing, and of the width of vicarious functioning in the proximal versus the distal region(s), we can avoid focal-arc atomism (sec. 8) in spite of the ignoring of intratorganismic mediation. The full-fledged pattern of functionalistic research can be realized in this manner, thus removing what must seem the most cogent basis for criticism of the empty-organism approach. (1952, p. 72.)

Various other statements by Brunswik might very easily be understood in the same sense of advocacy of "distal-to-distal functional arcs bridging over the entire organism without descending into it." Thus, in a much earlier paper, Brunswik said:

Thus, both for reception and for action, it turns out that the special manner in which any thing is mediated (or done) is not especially essential or significant. One and the same means-object may be represented at different times by very different stimulus configurations. And one and the same goals may be reached equally well by very different kinds of movements and means-object manipulations. . . . The really significant question always is: What are the kinds of such objects and final goal-effects which the organism is able to attain independently of all the varying circumstances with a relatively large degree of accuracy and probability . . . ? (1936, p. 125.)

In his final paper in December, 1954, Brunswik said:

We have conjectured that the emphasis on wide-spanning functional correlations at the expense of attention to the intervening technologic detail is one of the major characteristics that distinguishes psychology from its predecessors

. . . the functional arcs that span toward, and gain their feedback from the remote, "distal" environment . . . are the really important arcs. (p. 509.)

Many other quotations could be given that similarly would tend to leave the impression that Brunswik favored the same proposal advocated by B. F. Skinner (1950, 1957)—that psychology ought to avoid any development of inferences regarding intervening processes or variables, ought to have nothing to do with any supposed introspective data, and ought instead to formulate its principles solely in terms of "functional relationships" between objectively observable situations and objectively observable behavioral results.

(6) *Psychological Research and Theory Ought Generally to Take the Form, Not of Distal-Distal Principles, but of Distal-Central, Central-Distal, or Distal-Central-Distal Principles.* To make this statement, and to submit it as descriptive of Brunswik's thinking rather than as a possible alternative to it, is to hazard a more controversial description of Brunswik's ideas than has been the case in any of the rest of this paper. But, even though I have arrived at this conclusion only in the very last possible moment for revision of this paper, I believe the evidence is very clearcut that indicates that this was Brunswik's basic mode of thought, even though he expressed it sometimes in ways that could be understood in different terms from those he meant.

Let me review the reasons for picturing this as Brunswik's main conception. We may note, first of all, that he deplored "hostility to theory and to central inference." (1952, pp. 47-49.) He saw such hostility to inferential concepts as a mark of immature thinking about the philosophy of science. He felt that much of the avoidance of inferential terms has been a consequence of fear of not escaping the subjectivist connotations of older psychology, and he felt that such tactics involve the cost of losing the "heuristic advantages to be gained from the 'apperceptive mass' attached to traditional terms. . . ." (1952, pp. 44-47.)

In the second place, we may note that Brunswik spoke of the lens model as a means of conceiving the relationships between "focal variables" rather than necessarily between distal factors. For example, in the section of his 1952 monograph where he spoke of "Stabilized achievement and vicarious mediation," (1952, pp. 16-21), he ended this section with a discussion entitled "Central-distal versus peripheral focusing of achievement." In this, he made what I now regard as a really crucial statement of his outlook:

Recent psychology has shown that variables located in certain "areas," "layers," or "regions" of the environment or of the organism seem more often

to be focal than those in others. Some of the most crucial changes of emphasis in contemporary psychology are based on the recognition of the relatively nonfocal, vicarious, "generalized" role of the sensory as well as of the motor periphery, coupled with the comparatively focal character of the central as well as the distal regions, both situational and historical, in the case of the higher animals at least. (1952, p. 21.)

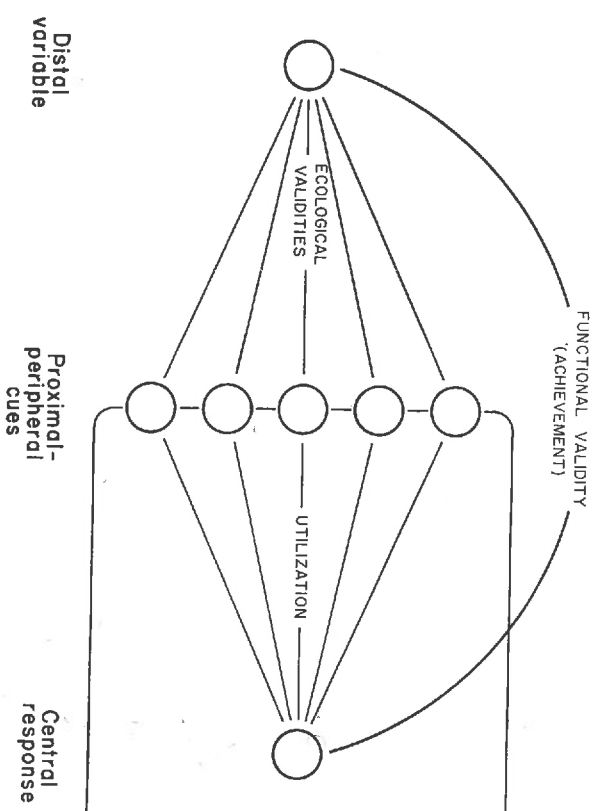


Fig. 1. Diagram given by Brunswik to illustrate "the lens model as applied to perceptual constancy." (Adapted from E. Brunswik. Representative design and probabilistic theory in a functional psychology. *Psychol. Rev.*, 1955, 62, 193-217.)

As expressing this conception, it is worthwhile to examine carefully Figure 1, which reproduces what Brunswik gave in a diagram of "the lens model as applied to perceptual constancy." (1955a, Fig. 8, p. 206.) In this diagram, it is to be noted that the one focal variable is the distal variable, the other is marked as the "central response."

One might ask, "Why didn't Brunswik treat the individual's judgment of the size of an object as a distal variable, since, of course, Brunswik was recording the overt judging response of the person? Why speak about 'central responses' when what was observed by the experimenter was the final behavior product (for example, the subject's saying '8 feet')?"

In a certain sense, of course, the implications of this question are

unimpeachable. But, it seems that Brunswik's view could be stated, in reply, in some such terms as these: "When we have a person look at an object and make a judgment about its size, he might indicate his judgment or his perception of its size by any of a long series of means. He might reply orally, he might write down his estimate in numbers, he might raise his hand to indicate the size of the object, he might compare the object with some other object, he might make a line on the ground with his foot, he might check one item from a multiple-choice question, and so forth. So, there is a great deal of possible vicarious mediation in his indication of what he sees. But, since there are such high correlations between the estimates that he would communicate by such different means, we are forced to infer that there had been some focal variable within the organism, some central response, which then could furnish the basis for any of a great diversity of forms of producing an effect on his environment (in this case, for communicating his judgment to the experimenter)."

In line with this thinking, we might well say that Brunswik's conception would be expressed somewhat more fully, not by the diagram given in Figure 1, but by the somewhat more elaborate diagram which I propose in Figure 2. I do not know of any *diagram* in Brunswik's writings which exactly corresponds to this particular diagram, but Brunswik's Fig. 1, Chap. 1 and his comments on it are quite close. And, many parts of his discussion apparently assume this sort of thing. His sympathetic comments about Tolman's work, for instance, suggest this form

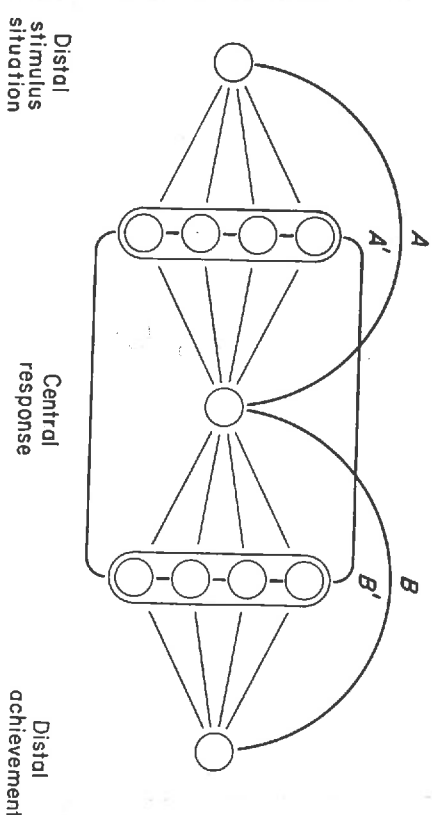


Fig. 2. The double-lens model which seems to be required to portray diagrammatically the basic model which Brunswik's discussions implied.

of statement (1952, pp. 67-70). His very sympathetic comments about the inferences in psychoanalytic theory regarding motives, defense mechanisms, and personality structure also are in line with this conception.

Now, if we go back to the quotation made above from Brunswik's 1936 paper, we can see that this quotation can be understood in terms of such a double-lens model. Furthermore, when one reads Brunswik's papers with this conception in mind, one realizes that, when he spoke about "wide-spanning functional arcs" or "wide-spanning functional correlations," the context typically reveals that he was speaking, not about distal-to-distal relationships, but about relationships between distal and central factors, or about distal relationships as mediated by such and such central responses. I am reminded by all this of a correction which he jotted in the margin of a letter that he sent to me on May 19, 1954, when we were corresponding about the paper that he was to present at the International Congress of Psychology a month later. In dictating the letter he had apparently said that "... our symposium ... concentrates on distal-distal (*S-R*) problems exclusively, so far as I can see." In the margin, however, he added the word "central" to change this to "distal-central-distal" problems.

One other way that Brunswik expressed the sort of thing that is represented by Figure 2 is that he spoke frequently about the need for "macro-mediational studies"—studies that would be planned "from above" through previous observations of "distal-distal" relationships. The distinction he drew between such macro-mediational studies and reductive-nomothetic-systematic attempts is that such studies would not attempt to dissect out the details of the two lens functions (the *A'* and *B'* of Fig. 2), but would accept such vicarious functioning and would deal in terms of the wide-spanning functional arcs *A* and *B*.

In all such comments as the above, we must emphasize that Brunswik did not want to speak about central processes in introspective or phenomenological terms. He wanted to deal with them as processes inferred from objective data. But, nevertheless, it is important to realize that his basic mode of thought was not one of talking about perceptual phenomena, for example, in terms of distal-distal relationships, but in a different way dictated by his belief that it was efficient to develop concepts about intervening central processes.

(7) *There Is a Place Also for Reductive and Systematic Studies, but Only as Quite Accessory to Studies Which Permit Vicarious Mediation.*

When Brunswik was pressed on the score of whether he saw any role for reductive studies and for systematic experimental research, he insisted that he saw these as having certain important functions, but

that their relative role had been vastly overemphasized. In the 1953 symposium, for example, after some vigorous criticism by the other discussants, Brunswik included the following statement in his final *In Defense of Probabilistic Functionalism: A Reply*:

Probabilistic functionalism is not ... hostile to reduction. It merely places correlational achievement-mapping of generalized functional arcs at the top of a hierarchical pyramid ...; this is followed by the macro-mediational analysis of vicarious attainment strategy. As to the third level in this pyramid, the reductive study of micro-mediational tactics, I agree with Professor Hilgard that this and the entire nomothetic approach should not be "replaced" by the probabilistic approach of the two top levels, although I would like to place reductionism in a marginal position to psychology unless it is executed in firm contact with the two functional aims. In order to reduce, we must know what to reduce. We must reduce "from above," that is, starting from such high-complexity functional units as the lens model. ...

The nomothetic-reductionist-systematic type of approach has in the past been overstressed at the expense of the probabilistic-functional-representative approach. ... We have had all of the former and nothing of the latter for too long. Now we must balance psychology in the molar and molecular realm. (1955b, p. 237.)

In the course of our preparation of papers for the 1954 International Congress, I got very much the same comment from Brunswik when I urged that there were many values in mediational studies and that most of his own work actually was mediational or reductive, including his paper at this Congress. Brunswik's comment was essentially that he "of course" recognized the need of mediational studies to round out our psychological knowledge and to permit us to make more discriminating predictions, but that psychologists had been giving a one-sided emphasis on systematic research and that he probably had slipped into a too-strong emphasis on nonmediational studies to try to redress the balance. If such is the case—and Brunswik certainly stated it in very clear terms—it calls for a revision of many of the pronouncements from Brunswik regarding the proper methodology for psychology.

(8) *The Prime Importance of Partial-Correlation Methods as a Means of Analysis.* Despite what has been said in the preceding section, most of the statements Brunswik made regarding the analysis of significant factors in complex situations have placed emphasis on partial correlation as the means of answering questions of "how?" Thus, he said:

The challenge of further isolation (of significant variables) must be met by after-the-fact, mathematical means, as in the study of individual differences. For example, we may use partial correlation as a mathematical means of holding constant a certain variable. ... It must also be noted that, in contradistinction

tion to systematic design, the process of analysis may be stopped at any point, falling back on the nonreductive aim of functional research, together with the assurance that the unresolved part of the associations is safely within the fold of the ecology to which the investigation has been geared from the beginning. (1955b, pp. 202-203.)

In such generalized statements about methodology, therefore, Brunswik was not describing the sort of work that he had done on perception versus thinking, on the role of different probabilities of reward in animal learning, and on the learning of perceptual cues. Instead, he was calling for a more consistent use of representative design than his own research actually demonstrated.

A TENTATIVE PRELIMINARY EVALUATION OF THESE PRINCIPLES FROM BRUNSWIK

The main difficulty with Brunswik's proposals, it seems to me, is that he has overstated his case. He has drawn too sharp a contrast between his own proposals and those advanced by other psychologists. He has not made sufficient use of his own precept that, before one makes broad generalizations, he ought to have a very careful and extensive sampling of the instances such as the generalization purports to cover, and that from this sampling he ought to see whether there is a sufficient basis for the broad and strong statement that he is inclined to make. Furthermore, because he did not try to canvass a wide diversity of examples, he did not locate and correct some serious shortcomings in the definitions of some of his key terms (as in the case of the term "ecology," which we already have discussed).

Because of the overgeneralizations in Brunswik's writings, these writings tend to invite criticisms and perhaps overstatements on the opposite side, and the constructive gains that might have come from Brunswik's work are in danger of being lost. Perhaps even at that, the strategy that he adopted, probably quite unintentionally, may have been more worth using than a strategy of much more careful statements. Maybe it is true that new proposals have to be exaggerated in order that they gain attention and consideration. Or, perhaps it is inevitable that, as Boring has suggested in two of his last papers (1954, 1955), it is more or less inevitable that truly creative persons have a strong measure of egoistic interest, and some lack of appreciation of the contributions of others, else they are not likely to have the strong motivation needed for the development and presentation of some new ideas or findings.

My own basic conviction, though, is that overstatements are risky—

that they too much tend to invite countercriticism and counterattacks and that, in the hurry-burry of this, the important positive contributions get trampled and lost. And, in Brunswik's case, as I have said above, I think there are some highly worthwhile original proposals. Consequently, I think it is very important to re-examine Brunswik's principles, asking how we can state them in some more justified fashion and how we can show their organic relations to various other bodies of thought. The following discussion will be attempting to give a sort of restatement that I believe is demanded in the light of some broader sampling of materials from which to reason.

Most of the points that will be discussed are methodological points. Some points about the nature of psychological phenomena will be included in the discussion of these as needed, and then the discussion will end on several points where the main concern is just with empirical hypotheses.

SOME MORE SPECIFIC PROPOSALS ON A NUMBER OF MAIN CONCEPTS FROM BRUNSWIK

(1) *The Need for Adequate Sampling for Any Abstract Statements.* Perhaps the most important concept from Brunswik, it seems to me, is his very general methodological point that we must be careful not to make abstract statements that are broader than our sampling of cases really would warrant. As he said, as cited above, a research study in a very restricted experimental situation may be sufficient to establish the point that a certain relationship exists in at least that one instance, just as the data on a single couple can establish the point that it is possible for a wife to be taller than her husband. Furthermore, as Brunswik said,

Quite often the demonstration of a mere possibility . . . is all that is necessary and desired of a piece of research, and may be fully sufficient to establish tentatively a principle for purposes of further verification and thus to stimulate further research; in all cases of this kind the systematic experiment is in place and may save the burdens that would go with a proof of ecological generality. In other cases, a systematic experiment may serve to exclude certain trivial factors from the explanation of a phenomenon. (1956b, p. 55.)

However, if we make statements of some other sort, such as that "all cases of . . . involve . . ." or "most cases of . . . involve . . ." we must

have adequate sampling of instances to warrant such statements. Brunswik has rendered an extremely important service in calling our attention to the fact that sampling theory is just as relevant and just as indispensable when we make abstract statements about types of situations as when we make abstract statements about types of persons or types of organisms of any sort.

(2) *The Need for Representative Sampling as a Basis for Generalized Quantitative Principles.* This seems to me like another excellent principle from Brunswik. Brunswik did well to point out that the task required by representative research design is a very huge task, but that it is a task that we cannot shirk if we want to make abstract statements of a meaningful quantitative sort about the relative contributions of different factors to some sort of behavior or achievement. The demonstration of quantitative relationships within a particular experimental situation is merely a first step in developing an adequate quantitative principle. Until we get the means to specify some larger domain of situations of which that experimental situation is an example, and until we have secured a representative sampling of instances from that larger domain, we really do not have an *abstract* principle of a quantitative sort, but merely a quantitative finding applicable to that one instance, comparable to the finding that "it is possible, judging from this one married couple, for a wife to be 14.7 percent taller than her husband."

As said earlier, the domains that we sample by research could be defined artificially or arbitrarily, but the work of psychology would be endless if we followed this procedure. Instead of that, we need to get domains which are worth talking about, worth knowing about, and yet which can be identified clearly enough so that other workers can know what we have undertaken to sample. Then, by some technique or other, whether by random sampling or stratified sampling or whatever, we need to get a representative sampling of instances from that domain; otherwise our quantitative statements cannot be used.

(3) *The Importance in Psychology, however, of Qualitative Principles.* As in the quotation given above, Brunswik has granted clearly that there can be some real value in research that proves nothing more than that a certain factor can exert a certain type of influence in a certain direction. But, in his discussions of psychological theory, Brunswik has not adequately indicated, it seems to me, how important are such qualitative principles in psychology (and in other sciences) and how much of psychological knowledge, at least for some long period, must consist solely of such qualitative principles.

For many purposes, we are not greatly concerned about the details of

quantitative relationships, but wish only to know that such and such a factor has such and such an influence in at least some cases. This is not to say that we will not be interested in quantitative knowledge as soon as it can be acquired. But, in the meantime, a more sketchy sort of knowledge can have great value. Much of medical knowledge, for instance, is of this sort, and yet is quite valuable in spite of limitations of a quantitative sort. For example, there must surely be individual differences in susceptibility to scurvy or pellagra under conditions of vitamin deficiency, but it is not very important, practically speaking, to know about these differences. Once the qualitative principle has been established—that such diseases can come from certain nutritional deficiencies and that they may be prevented or cured by such and such means—this knowledge easily can be used by employing diets that have some considerable margin of safety in them, and most of the value can thus be attained that could come through much more elaborate quantitative knowledge.

In the same way, a great many psychological problems could be dealt with more successfully and intelligently if we but knew the types of factors that are important and the directions in which their influences are exerted. It is a good thing for us to develop these principles into as nearly adequate quantitative principles as we can, and hence it is important for us, as we were saying above, to get as adequate a sampling-basis as we can for our generalized statements. But, useful and important knowledge does not start merely with the kind of knowledge that can be secured only through representative sampling.

(4) *The Importance in Psychology of a Sampling Procedure More Economical than Representative Sampling.* Particularly as related to human life, since cultural factors can so enormously change human behavior, but also because human beings are adapted for living under such a terrific diversity of natural environments, and because the world in which we live is so infinitely complex and variegated, it is an extremely difficult thing to get any adequate representative sampling of "natural-cultural habitats" of human beings. But, we ought not to proceed as though our choice had to be a selection between representative sampling, on the one hand, and no significant breadth of sampling, on the other hand. There is another method of sampling that can be used quite economically and which is adequate for testing certain sorts of generalizations, at least in rough and tentative ways. We might speak of this method as a technique of "testing the limits," borrowing a term from Rorschach work.

For example, when a psychologist is tempted to generalize, from research with a particular situation, that "all learning requires an intent

to learn," the danger is that he may be extending his finding to a vastly larger area than is justified. The thing for him to do, then, is to consider other instances of learning that seem least likely to exemplify the influence that he observed in his particular experiment. He might ask whether we can plausibly maintain that animals have an intent to learn. If he decides that such extreme instances seem to indicate that he has overgeneralized, then he may try to locate some more restricted category within the field of learning, so that he may test whether he can find some more modest but still fairly large territory where he will find no exceptions (even in seemingly extreme instances within that category). By such successive retreats from his extreme instances, he may ultimately find some rough indication of the limits of application of his finding. Or, he may demonstrate to himself that he cannot identify any basis for telling when the principle applies and when it doesn't, and may realize that he has to come to such a conclusion as, "Under some conditions, which I have not found the means of specifying, learning requires an intent to learn."

Brunswik to some extent illustrated the use of this method of sampling in connection with his experiment on perception versus thinking. In the specific instance that he used, it seemed that thinking tended to yield a high proportion of exactly correct answers, along with many gross errors in other cases, because of a tendency for thinking to take merely one single track or another in trying to reach a solution of the task. Perceptual judgments, on the other hand, all tended to be at least approximately correct through some multiple use of cues or a multiple-track type of approach.

In assessing the significance of his study, however, Brunswik did not depend merely on his one study. As he summarized the matter:

Ending on a note of caution, we should like to stress that the representativeness of our two versions of a common cognitive task is open to some doubt. Many specific conditions could be listed under which it is perception which is bizarre while it is thinking which is mellow and given to compromise. Aside from deductive considerations, only representative design could definitely prove us right or wrong in our conjecture that the juxtaposition which we have presented is more typical than its reverse. (1956b, p. 93.)

In judgments about the intelligence of a person, for instance, our *thinking* certainly tends to use multiple cues. On the other hand, when a person looks at a TAT card, he tends to *perceive* it as clearly and obviously portraying one sort of personal situation rather than another—something much like what Brunswik has spoken of as the single-track

switching that may be more typical of thinking. Furthermore, when people do engage in thinking which is clearly single-track, we cannot count on its being accurate in any case, because it may be using faulty premises. Thus, in the old days of blood-letting in medicine, the major premise on which the practice depended was that "People who are ill are suffering from bad blood."

My impression is, however, that, although Brunswik recognized some of these considerations that can be urged against taking his experimental results as illustrative of perception and thinking more generally, he also tended to speak at various points about this study as though his data were more representative than we have a basis for believing that they are. For, if there are such exceptions as mentioned above, it is only in some special sense that he could have been justified in using, in the title of his Montreal paper, the expression "... a Functional Differentiation between 'Perception' and 'Thinking.'" If the conclusion is that the two different types of distribution of errors are found *only in general* to differentiate between perception and thinking, as Brunswik is saying, then it must follow that he was using some more fundamental means of saying what phenomena should be classed as perceptions and which as instances of thinking. And, one wonders, then, whether we ought not to challenge the traditional means of classification and propose some new classification that would cut across the old categories. He may have had this in mind, in part, in using the two terms (in parts of this same discussion) of "certainty-geared interactions" and "uncertainty-geared interactions." But it is hard to see that these terms are warranted when we find that he spoke as follows:

"... certainty-geared interaction may go wrong... when the single cues representing the constituent variables are not in reality foolproof, that is, when certainty-geared interaction lacks its necessary counterpart in the ecology. Take here the earlier confinement of airplane altimetry to the air-pressure cue, and the resultant crashes of planes in mountainsides whenever the cue was misleading." (1965, p. 490.) Still further, we cannot say that explicit logical thought of a single-track sort would be geared at least to propositions which the individual would regard as certainties, even when these are not actually such. In many cases, an individual knows that some course of action has merely a faint hope of success, and yet he uses it because it seems less uncertain than other possible courses of action.

(5) *The Question of the Proper Type of Laws for Psychologists to Strive for.* The preceding discussion leads us to another methodological point stressed by Brunswik, but which seems in need of

revision. Brunswik frequently drew a contrast between the physical sciences, which he saw as seeking for strict laws, and psychology, which he said must search merely for probabilistic laws.

Rather than portraying this question as an all-or-none matter, it seems to me that Brunswik would have done better to speak of this question as a question of a continuum. What we want in psychology would be categories and principles which, although cast in highly abstract form, would enable us to make as nearly precise predictions as possible. So, in our consideration of instances of perception and thinking, if we find that there are merely general tendencies for thinking and perception to yield the sorts of pattern of errors that were found in Brunswik's experiments, this knowledge is more uncertain than we wish, even though it has some value. What we naturally try to do is to reclassify the various instances of cognitive processes and see whether we can find some different classes where we can anticipate, with higher likelihood, the two sorts of patterns of performance which Brunswik described. When there are so many uncontrollable factors both within the organism and within the environment, we cannot hope to reach the point where our predictions can be precise in individual cases; but we at least are seeking for laws that will be as precise as possible.

As long as some large assortment of unidentified conditions remains constant, we can predict quite definitely, of course, what a single individual will do—we can predict what he will eat for breakfast, what route he will follow in going to work, and what words he will mispronounce. But such predictions are of little use for psychology. Precision of that sort is unimportant in a science, because sciences are basically efforts to develop highly abstract knowledge, which will facilitate dealing with new instances. When the world and the organisms in it are so complex, and when there is so much loose play in the separate parts of each, it seems inevitable, all right, that psychological laws must be probabilistic. But, as Brunswik emphasized at one point, at least, so are many of the laws of physics. The gas law of physics, for instance, is merely a statement of a statistical likelihood. The important point is not simply that we must deal in probabilistic laws; the point is also that we want to make these laws as nearly exact or strict as we can.

(6) *The Need for a More Functional Concept of Ecologies.* I have mentioned, above, the lack of any sufficient indication of how Brunswik meant the term "ecology" to be applied. It is, nevertheless, a key term. If we are to hold, as Brunswik did, that behavior is a function of complex situations and that we ought to emphasize representative research design—that is, the study of a representative sampling of real-life

situations from an ecology or natural-cultural habitat in each case—we have got to have some better meaning for this term. There can be no proceeding with representative research design if we cannot say what the ecologies are that such research should sample.

On this problem I think that we need to work backwards from a consideration of the point discussed in the previous section—namely, that we want the means to make predictions that will be as nearly exact as possible. If this point be granted, then it would follow from this that, whenever we get some additional knowledge which enables us to specify a highly important parameter, this gives us the means of defining an ecology about which it will be worth our while to study and generalize.

Take the example of size-constancy which Brunswik mentioned so frequently. In the experiment which he performed, no object that was viewed was more than about 2 miles away. Within that range, the product-moment correlation between the log of measured bodily size and the log of estimated bodily size was .987 for one individual, .993 for another. But these data were gathered during the daytime, and there was no opportunity for estimates of the size of stars. Suppose the individual studied had been an old-time shepherd or an ancient Polynesian mariner steering his course by the stars. In such a case, lacking our modern knowledge of the distance and size of stars, such persons would have made extreme errors, and the product-moment correlation that included such instances would have tended even to be negative. But, what interest would there be in such a correlation? The main fact would still be, somewhat as Brunswik said, that those old-time individuals still would have made highly accurate size-judgments regarding near-by objects, just as modern men do, and that the estimates regarding the size of stars belong in a special class, which we need to discuss separately. Hence, there is some point for the expression that Brunswik used in his Montreal paper when he spoke of "the ecology of manipulable things."

But, if we proceed in this fashion, as I believe it would be quite necessary for us to do, it seems that it leads to quite a different basic mode of thought from that which Brunswik usually implied. It means that the definition or identification of any ecology is not prior to knowledge of cause-and-effect relationships, but would be affected by whatever knowledge we had which would warrant separating off some category of cases as ones significantly different, on the average, from other cases. Within the ecology thus separated off, we still would have the question, "What are the relative weights of the various uncontrolled factors in determining the effects seen?" or the question, "How accurate, for example, are judgments of intelligence (or size or monetary value and so forth)

within the limits of the situations thus separated off?" But we would be depending on our earlier partial knowledge of such cases to help us separate off those cases that are functionally similar to one another in some important ways. Otherwise we cannot get abstract principles well adapted to our objective of providing the means for relatively precise predictions.

The situation faced by psychology is basically the same as the situation faced by an individual with reference to the life situations to which he must learn how to respond as well as possible. He has to learn that the behavior that is effective in one type of situation is not effective in another, and that the factors that are very highly correlated with success in one type situation have only a much lower correlation with success in a different type of situation. In each type of situation, there will be variations that will go beyond what he can anticipate; but the individual nevertheless could well express the lessons from his learning by saying, "I have learned that, at different points, I am in such and such different ecologies, and I've had to learn different laws about how to proceed and what to expect in those different ecologies. My knowledge still is probabilistic, but it is a darn sight better when I recognize these different settings in which I'm operating than when I disregard them or do not know about them." In the same way, although it is true that psychology wants to describe ecologies in more abstract terms than the individual person would be likely to do, the attainment of good explanations by psychology depends on the recognition of a great host of ecologies, separated out by some understanding of the key factors operating in various life situations, even for a given individual.

(7) *The Limitations of Partial Correlation as a Means of Developing and Testing Hypotheses about Functional Relationships.* As said previously, Brunswik decried the general stress on experimental or systematic research because he believed that it accomplishes only what can be done, and in a safer fashion, by mathematical analysis "after the fact," using partial-correlation techniques to keep other factors constant and to determine whether a particular factor is one from which some predictions can be made. Thus, replying to some criticisms from Postman in the 1953 symposium, he said:

"Since, in principle, under representative design all variables are allowed to vary and none are held constant artificially, their role can be ascertained after the fact. For the same reason, there is a gain rather than a loss of information, contrary to what Postman seems to fear." (1955b, p. 239). In his main paper in the same symposium, Brunswik compared systematic research to soap operas and popular novels and

movies. All of these deal in clichés, in cases "... by no means impossible or nonexistent, but made prominent out of all proportion to its frequency, and to the detriment of all other types of incident." (1955b, p. 215.) Because of this, Brunswik said,

... the suspicion arises that the didactic role which systematic experimentation obviously plays in the mental economy of the scientist, by virtue of the simplicity and order it both requires in the design and furnishes in the result, may outweigh the fact-finding competence of systematically designed experiments....

The main function both of art and of systematic experimentation, then, is to shake and mold us by exaggeration and extreme correlation or absence of correlation. But exaggeration is distortion, and this distortion must, in science, eventually be resolved by allowing the more palatable systematic design (of research) to mature into, and to be superseded by, the more truthful representative design (1955b, p. 215.)

Part of this interpretation we may agree with. As has already been said, it seems quite appropriate to say that good quantitative principles can be based only on representative sampling of the ecologies to which they are supposed to apply. But, the other implications of this interpretation are ones we must reject. What can the statement mean that "Since, in principle, under representative design all variables are allowed to vary and none are held constant artificially, their role can be ascertained after the fact"? After what "fact"? Put in plainer words, the question is, "After we have conducted a series of observations and measurements, and have recorded our data, can we then go back and tease out some relationships which we had not even suspected might exist?"

It seems to me that the answer to this question is quite different from what Brunswik implied. *Sometimes* the data will have included material from which additional relationships can be discovered. But the fact that a huge number of variables had been present in different quantities and in different timing does not mean at all that a research worker will have noted those variables or that he will have record that such and such values for them were associated with such and such values on other variables.

When we go back over the history of scientific work, whether in psychology or medicine or physiology or whatever other field, it seems that almost no principles have been discovered, or hypotheses developed, by mathematical analysis after the fact—that is, after the data have been gathered. The work of scientific discovery depends mainly on one or the other of two special origins of hypotheses. One origin is chance observations in which certain factors have been related to each

other in some exaggerated or unusual and hence striking way, as when Semmelweis in the 1840's got his clue to the cause of child-bed fever when one of the other physicians died, with the typical symptoms of this disease, within four days after having cut his hand with a scalpel during the course of an autopsy on the body of a woman who had died from this disease. The other main origin is deductive reasoning, arguing more by analogy, as it were, from general premises that reach out into a wider territory than actually had been established up to that point.

With either of such origins for his hypothesis, the scientific worker then admittedly arranges conditions in a fashion intended to exaggerate the hypothesized relationship. He wants the situation to be didactic—that is, he wants it to teach him something, wants it to establish or give fairly clear disproof of a *qualitative* finding. The early workers on behavior, for example, expected that the disease probably was communicated from one person to another, but when they arranged conditions so as to accentuate the likelihood of such transmission, it did not occur. Hence they had to turn from this hypothesis, whereas a representative research design might have given rather ambiguous testimony on this score. They had to have some other chance conditions which gave them the altogether unexpected idea that the disease came from diets heavy in the use of polished rice. The same point could be illustrated through reference to Harlow's work on the effects of certain factors in the experience of infant monkeys, or in any number of other psychological studies.

Admittedly, as said before, the determination of quantitative relationships calls for the representative sampling which Brunswik has emphasized. But Brunswik has misjudged, it seems to me, in saying that the original development of hypotheses can come by partial-correlation techniques, and in speaking disparagingly about other methods as didactic, exaggerating, artificial, and so on. I believe that the consideration of a wide sampling of scientific work suggests that the original development of hypotheses and original demonstration that "there really is something there" have to come by these means in most instances.

(8) *The Limitations of Distal-Distal Studies as Compared with Medial Studies.* As has been said, Brunswik seemed generally (except when he was pressed with criticisms of this proposal) to prefer to picture the main work of psychology as the development of concepts about distal-distal relationships.

There certainly are many problems that can be cast in such terms. Many practical questions can be well phrased in this way. For example, the question whether a reduction in the maximum speed on highways would reduce accidents can be answered by studying the accident rates

under two different laws. Valuable facts can be learned without having to get any light on why the rate is different under one law than under another. Similarly, changes of industrial productivity can be demonstrated under different conditions of temperature, humidity, and lighting without bothering to ask why the productivity changes with changes in these environmental conditions.

Not only practical questions, but many straight research questions can be phrased in this fashion. Skinner, for instance, has demonstrated important differences of performance under different schedules of reinforcement in lever-moving experiments with rats and pigeons.

In fact, the possible case for distal-distal formulations may be made to sound fairly impressive by pointing out that, in almost all of psychological research, the observational data are concerned solely with distal stimulus situations and distal achievements. From this, the argument can be raised, "Since almost all of the data in psychological research are data on distal factors, nothing can be added by trying to develop constructs about the processes which intervene between the distal stimulus situation and the distal achievement."

However, here again, before we draw such sweeping conclusions, we need to see what sort of sampling we have for our generalization. To begin with, we can note that observation of proximal responses often is important. The skilful athletic coach does not observe merely whether the athlete clears the bar in a pole-vault, but *how* he does it. The same for the violin teacher, the teacher of typing, or the foreman training men in laying bricks. In social psychology, it has been found important to study the different observable techniques that different leaders use in trying to get things done by a group, rather than study just the assignment of task and the final group accomplishment.

In the field of cognitive processes, there is increasing evidence that there are important effects that come from the type of cognitive activity in which the individual engages in trying to cope with a complex problem. Brunswik's own experiment on perception versus thinking was a demonstration that those two different cognitive activities in that situation yielded different sorts of judgments. The work of Bruner, Goodnow, and Austin on different strategies in concept formation revealed the same point in their situation (1956). Many other studies of thinking, such as those by Katona (1940), Hanfmann (1941), and Bouthilet (see Leeper, 1951, p. 745), have established the same point.

Consequently, it seems clear that psychology would deprive itself of valuable information if it confined itself solely to distal-distal formulations. Distal-distal studies are valuable means of *initial* exploration of

complex phenomena. But, except as they lead on into more differentiating mediational concepts, they leave us with rougher functional correlations than, at least for many purposes, we need to develop.

(9) *The Need to Elaborate the Lens Model.* In many contexts, when Brunswik spoke about the lens model he was speaking about the capacity of the organism to combine evidence from several cues to reach some better representation of some feature of the environment. At other points, instead of speaking about such multiple use, he was speaking about the organism's capacity to choose between one means and another and yet work toward the attainment of some common end in all of the different cases. Brunswik seemed to move back and forth between the two concepts as though he saw them as indistinguishable. Thus, in his final monograph, he said:

The limitations in the dependability of single-cue variables force an uncertainty-gear probabilistic strategy upon perception. In order to improve the cognitive "wager" . . . the perceptual system must accumulate and combine cues. Thus we arrive at a more complete understanding of the principle of mutual substitutability or "vicarious functioning" of means (or cues) which Hunter, Tolman . . . and most other behaviorists looking for a structural criterion have incorporated into their basic definitions of behavior or purpose. . . . (1956b, pp. 140-141.)

Now, it may be that the combining of different cues (or means) is like the phenomenon of choosing between alternative possible means. But the "choosing between" in the case of means would seem a closer parallel to those cases in which the organism shifts from one single cue (or group of cues) to another and yet continues to make much the same "cognitive wager." And, on the side of the use of cues would seem to be the fact that the organism often acts redundantly to make more certain that the intended effect will be realized. The small child, for example, not merely tells its mother by words that he wants to go home, he also tugs at her hand.

It would seem as though Brunswik is quite right in saying that these two things have some abstract quality in common, and may both deserve to be designated by some common term such as "lens functions." But, there may be some important differences between them, too. The shift from one means to another without sacrifice of the end to be attained is something which calls for a consideration of cybernetic mechanisms (goal expectations, motives, purposes, or whatever we choose to call them). This may be the same phenomenon, entirely, that is involved in the multiple use of cues. I want merely to suggest, however, that the

appreciation of some important similarities should not divert us from trying to find out whether, in some other respects, we have two significantly different operations here.

(10) *The Need for a More Adequate Exploration of the Implications of the Ratiomorphic Model and of Alternatives to It.* Brunswik emphasized the ratiomorphic model as one of his main proposals. But, he did not go very far in spelling out what he meant by it. He indicated that it definitely was not a rationalistic or intellectualistic construct, and that it did not imply that cognitive processes necessarily were completely accurate. He also suggested that behavior, and more particularly cognitive activity, including both perception and thinking, ". . . involves the particular type of orderly interaction we find best exemplified in syllogistic reasoning or in mathematical calculation." (1965, p. 487.) That is, I suppose we might say, cognitive processes are ones that utilize not only the particular conditions observed at the moment, but also more general premises or beliefs derived from earlier experiences. The individual notes not merely the apparent blueness of the tree-covered hill that he sees, but also takes advantage of some generalization that he has developed that tree-covered hills look blue only under such and such conditions of distance and/or illumination. The use of such considerations may be an extremely swift process, as in spatial perceptions, or a more slow-moving process, as in much of explicit logical thought. But, in either case, a conclusion is drawn whose validity can be checked, or, in principle, might be checked, against the independent reality that is portrayed.

This is a proposal of considerable interest, but it leaves us with hardly more than the beginning of work on the question of how all this is accomplished. Brunswik might have replied to such a criticism by saying that, in a distal-arc functionalism, there is no need to ask about the "how" of things. But, as we have seen, Brunswik was not content to leave any question in this way. His more fundamental proposal has been that distal-distal studies would identify various phenomena in need of more careful study and that a more adequate understanding might then be secured by mediational studies planned "from above."

Let us illustrate the problem, taking a couple of instances that illustrate some rather common phenomena. Suppose we have people look steadily at such a drawing as that shown in Figure 3. At least after staring at this for a short time, people find it impossible to continue seeing it as a merely two-dimensional drawing. They find it impossible to stare at it for longer periods of time without having it reverse in perspective. Furthermore, the rectangular pieces are not seen as lying in the same plane—

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instead, it is as though the person were seeing a rectangular strip of paper that had been folded, with somewhat different angles at different points, along lines at right angles to the long edges of the rectangular strip.

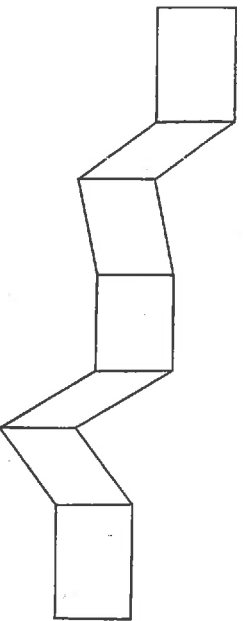


Fig. 3. A simple visual figure that illustrates the ambiguities in the proposition that perceptions are "ratiomorphic."

Now, it might be said, "This perception is ratiomorphic, because people, at least in a civilized environment, have more dealings with objects which have 90-degree corners than with objects with any other sort of corner. What the person is doing is seeing the drawing in such a way that it could be a strip of paper creased in such a right-angle way."

But, this doesn't get us very far. The individual also has had a lot of experience that acquaints him with the fact that the surface of a sheet of paper is merely two-dimensional and with the fact that a drawing on such a sheet of paper does not shift from one moment to another. Why would it be, then, that the individual perceives or "draws his reasoning-like conclusion" from one sort of background-knowledge or training rather than from another? Merely to say that such perceptions are ratiomorphic or reasoning-like does not carry us very far, but presumably some other principles can be found that will enable us to say that one sort of effect rather than another will occur.

Take another instance: any worker in the field of personality is well acquainted with the fact that an emotionally maladjusted person often responds as though he were reasoning from premises that he knows are false and that he rejects under other circumstances. For example, a person may commonly feel and say, "I'm worthless; I do more harm than good; it would be better if I did not try to accomplish things because I create more difficulties than I make contributions to counterbalance them."

In such a case, the person may have quite sufficient ability to recognize

that all persons have real limitations, and he may be very discriminating in his judgment of the balance of good and bad in other persons and even in himself when he is considering his life in a reflective mood. He has other premises from which he might reason, therefore. But, if his common emotional responses are reasoning-like, they must be derived from premises other than he respects intellectually. Or, he may accept some social or religious philosophy which actually cannot be the set of premises from which his actual interpersonal relationships must be derived.

There are instances such as an experienced touch-typist can testify to. If such a typist has covers over his various keys, he will not be able to look at the keyboard and say what keys can be used for different purposes—which is the back-spacer rather than the margin-release, which is the key for the hyphen, which for the asterisk, and so on. But he can put his hands on the keyboard and demonstrate by his finger movements that, in some sense, his psychological processes are such as though he were saying, "Such and such a key has the asterisk mark on it which can be used after depressing the shift key; I want to type the asterisk; therefore that key is the one to hit." He does not go through such a process now. So, what point would there be to saying that his behavior is syllogistic-like?

What I am saying, to put it more briefly, is that our psychological concepts need to be, in great part, something much more specific than a ratiomorphic model, as developed only by a few brief comments, provides us with. We need a whole series of more differentiated concepts about perceptual processes, learning, the use of habits, motivation, selective forgetting, and other matters. In such a more developed theory, what would tend to be found, I suspect, is that reasoning and reasoning-like processes are merely special cases of some more general phenomenon or principle. But, to make some good judgment about this, we cannot leave either the ratiomorphic model or such a possible alternative model in such a very indefinite form as Brunswik left his concept. The implications of theoretical concepts must be spelled out in much greater detail.

(11) *The Need for Considering the Possibility of Highly Selective Organizing Characteristics of Cognitive Processes.* As we said at the start of the summary of Brunswik's main concepts, one main principle that he espoused was that our methodological and broad conceptual principles ought to be decided in the light of the nature of psychological phenomena, rather than as *a priori* philosophical principles. In keeping with this spirit, there is a problem to which Brunswik ought to have given major attention, but on which his views are indicated only in rather un-

certain indirect ways. In part, this is the problem of whether inner psychological processes can be predicted at all precisely from outer environmental influences, or at least from exact information about receptor stimulations. In part, it is the further question whether, if cognitive processes cannot be very definitely predicted from such means, whether there are other means of learning what cognitive organizations have been produced and whether such knowledge can be important in predicting what will occur in different stimulus situations.

It may be that cognitive processes are highly correlated with, or highly predictable from, the distal conditions that have affected the individual, at least provided we take into account not merely the current distal conditions but also the earlier distal conditions from which various habits might have been formed. On the other hand, it may be that cognitive processes depend on selective organizing activity to such an extent, and these in turn depend on essentially chance factors to such an extent, that we cannot achieve a satisfactory means of predicting behavior just by studying distal-distal relationships, but must center much of our research and theory on problems of how to judge what mediating processes occur within different individuals, as Lewin emphasized (1943).

Clearly, with any species that has great learning ability, there can be no sufficient predictability of behavior merely from the immediate stimulus situation affecting the organism. The whole significance of learning is that it makes the organism able to respond to a situation in a manner different from that which would be manifested if earlier learning situations had been different. We could find endless examples of the fact that people from different cultures respond to the same objective situations in different ways and even that different individuals in the same culture, or from the same family, for that matter, make different responses to the same kind of situation.

However, the possibility still remains that, provided earlier stimulus situations also are taken into account, the behavior of the organism may be highly predictable from the whole sequence of stimulus situations that the organism has met. Such was the view in Clark Hull's theory, for instance. Hull believed in stating his principles in terms of inferences about habit-strength, reaction-potential, reactive inhibition, and so on, rather than merely in terms of distal-distal relationships. But, the use of such mediational concepts in Hull's approach was not terribly essential, because Hull believed that these inferred factors or intervening variables could be estimated fairly closely by taking into account the relative numbers of reinforcements and non-reinforcements, the timing and intensity of various stimulations, the quantity and biological appropriateness of

reinforcements for the species in question, and a few other such factors. What is inner, Hull essentially was saying, may be fairly well predicted from these refined measures of what has been outer.

In general, Brunswik's discussions of perception and judgment were a good deal in this same vein. *When Brunswik spoke about the multiple cues available to the organism in any situation, he did not generally put emphasis on the idea that the organism might select certain cues rather than others, or that different individuals might use quite different cues and make quite different cognitive responses in the same objective situation.* Instead, with his lens model, he put emphasis on the tendency of the organism to combine various cues in order to reach some better over-all judgment. In his discussions of size constancy, for instance, he placed considerable emphasis on the very high correspondence between the individual's perceptual judgments and the properties of external realities.

When he discussed judgments of intelligence and personality, Brunswik stressed evidence that different judges tend to use somewhat different bases of judgment, tend to differ in the weights they assign different cues, and even sometimes treat some cues as though they were positively correlated with the trait to be judged when, in actuality, the correlation is negative. Still, the general impression that Brunswik tended to convey was that such differences were somewhat minor matters—that, even though different judges might differ in the accuracy of their judgments, there would tend to be a good deal of agreement. Brunswik generally was speaking as though the organism tends to perceive things in about the terms that the ecological validities of those things would warrant.

Let me make two quotations from Brunswik to illustrate what I mean. These quotations, from his two last publications, both have reference to the same experiment of 1953.

This writer and Kamiya . . . have demonstrated (with the use of $N = 892$ separations between adjacent parallel lines in a roughly "proportionate" sample of shots from a current motion picture) that the long-recognized gestalt factor of "proximity" possesses a certain modest ($r = .12$) but statistically significant ecological validity as an indicator of mechanical object unity. Its utilization as an organizing principle for perceptual "figures" is thus a probabilistically adjustable mechanism. The realization of this fact may help to open the door for a possible "reduction" of the hitherto unabsorbed gestalt dynamics into learning theory. (1955b, p. 241)

Later, speaking of the same study, he remarked:

Since . . . all ecological validities represent a challenge to the organism for utilization, and since it appears that certain cues are on the average being utilized roughly in proportion to the degree of their validity . . . our findings

lend plausibility-support to a viewing of the Gestalt factors as cases of successful cue-utilization subsumable under the principles of learning theory. (1956b, pp. 122-123.)

The physiological hypothesis that would be consistent with such views by Hull and by Brunswik is the idea that the nerve impulses that arrive in the brain from any peripheral stimulation arrive in the brain as basically independent travellers and continue to exert influences in the brain in the same way. As Hull phrased the matter,

"According to the 'law of reinforcement' . . . every one of the receptor discharges and receptor-discharge perseverations active at the time that the to-be-conditioned reaction occurs must acquire an increment of habit strength. . . ." (1943, p. 206.)

The alternative to this sort of proposal is that which has been urged, even if in somewhat different terms in the different cases, by Lashley (1938, 1942, 1960), Tolman (1948), Köhler (1929), Köhler and Adams (1958), Bruner (1957), and Leeper (1963). The fundamental proposal of this alternative view is, in the first place, that the functional units of brain activity are not the individual nerve impulses, but complex dynamically organized processes. According to this view, the outstanding fact about the brain is that it takes the incoming nerve impulses and makes new functional units of a much larger scope out of them—functional units that stress certain properties of peripheral stimulation and leave other properties unrepresented, just as a radio amplifies some wave-lengths of what comes in from the antenna and leaves other wave-lengths weak and ineffective. In the second place, the essence of this alternative view is that there are many factors that, practically speaking, are chance factors, but which are powerful determinants of what perceptual organization occurs. In the third place, this alternative interpretation suggests that it is possible, frequently, to find what perceptual or cognitive organization has in fact occurred and that this knowledge can be extremely important in predicting what particular effects will be seen in the life of the particular individual in different objective situations.

In other words, according to this view the processes involved in the formation of cognitive processes are somewhat analogous to those that determine the shapes of snowflakes. All snowflakes are made up of about the same materials and get formed under very nearly similar circumstances, and yet they are of most diverse types. They do have some internal dynamic organization so that each snowflake builds itself symmetrically, yet different snowflakes become more or less different from one another under "almost identical" conditions.

Let me illustrate concretely what I mean, using Boring's "wife-and-

mother-in-law" figure which I employed in an experiment reported in 1935. We can dictate pretty heavily what the individual will see in such a *vieldewig* stimulus-figure, either by drawing the figure so that it favors one possible organization rather than another or by giving some prior training with such loaded examples. Or, as Botwinick, Robbin, and Brinley (1959) have demonstrated, there are some personal characteristics that may exert some degree of influence on what will be seen, as through the fact that the age of the perceiver tends to be related to the age of the person perceived from such drawings. But, aside from such loading of the dice, it seems that it is almost a chance affair as to which organization a given individual will get with the Boring figure. Yet, in any case, the individual does get one clearcut organization or another, and, if we wish to make predictions about details of his behavior with reference to the figure, it is worthwhile to learn what perceptual organization he achieved. We can ask him, for instance, to point to the tip of the nose of the person he sees. Depending on what he does, we can then predict whether he will report that the mouth is visible or hidden, we can predict roughly what age he will judge the woman to be, we can judge what details he might gloss over if he were copying the drawing, and so on. The perceptual organization is decisive for all of these further aspects of his overt behavior. On the other hand, if one knew merely that the person had been presented with the Boring figure as distal material, even on some series of trials, the only prediction that could well be made is that the person might show one pattern of responses or the other.

Now, is such an ambiguous figure a good paradigm of the environmental situations which the individual meets? Are most environmental situations thus ambiguous, *vieldewig*, ambivalent, or are most situations *eindeutig* except for minor shadings of the perceptual or cognitive responses they evoke?

I think Brunswik's discussions tend to suggest that such a stimulus-material is an oddity. Most of the stimulus-materials which we perceive, I think he would have said, we perceive in a veridical fashion. We perceive trees as trees, rain as rain, books as books, and so on through the great host of everyday environmental realities.

For many common stimulus-materials, one might agree. However, if we want to base our portrayal, not on some limited sampling of situations, but on some broader sampling, I believe we need also to take account of those more complex and subtle situations that are important for personality functioning and for our more complex social processes. Particularly in them, as Peter Madison and I have urged in our discussion of personality (1959, Ch. 6), it seems extremely important that such

situations typically are, in high degree, *viedende* situations. The situation that brings a sense of panic to one person brings a sense of security to another; the behavior or personality characteristic that one person views with shame is viewed with pride by another person, and so on down the line. The tactics that one nation sees as buttressing its national honor and international standing are seen by another nation as the poorest possible course of action.

I do not mean to imply, of course, that it is impossible for us to make considerable progress—or even great progress—toward learning what objective training conditions or learning situations tend to produce one such personality effect rather than another. We have made some progress on this task already, and it is important to push our research as vigorously as possible. But, what I do mean to imply is that:

1. complex life situations, such as are involved in the learning of personality habits, are extremely complex situations, with very inconsistent and conflicting relationships in them (as Brunswik expressed it, with very low ecological validities for most factors); and
2. in such situations, the individual tends to crystallize certain perceptions and concepts rather than others, selectively stressing certain things and neglecting other things, constructing certain patterns rather than others, and then subsequently tending to perceive what confirms what he already has learned to perceive.

In such situations, and with such processes operating, it seems that distal-distal studies, even when they take into account earlier learning situations as well as the current stimulus situation, can yield no more than very rough predictive principles. It seems to me, therefore, that our concepts in psychology generally need to be mediational principles, rather than distal-arc principles. The latter may have value as a prelude to other work, as Brunswik said, but I think that his tendency to stress distal-arc studies as strongly as he did came in part because of implicit acceptance of an interpretation of cognitive activity that had undue faith in the degree to which cognitions correspond with ecological validities.

How are we to decide between different alternative possibilities like these? How are we to determine the areas in which such examples as the Boring figure would be a valuable paradigm and those other areas in which its implication would be misleading?

On this basic conceptual problem, I think we are driven back to a basic principle of Brunswik. We cannot decide such questions merely by examples or by experimental evidence which indicates that such and such effects can occur as possibilities. We are concerned with a quantitative question, with a problem of the degree to which some example is

representative. And, when we are faced with questions like this, a really satisfactory answer cannot be secured except by a type of research method that we tend not to use because it presents us with such a high task—namely, the method of representative research design.

There are some very important values, therefore, in a number of important concepts which Brunswik hammered out. I believe that the major concepts, however, can be adapted into a primarily mediational type of approach to psychology. Indeed, beyond that, I believe that they can be adapted, and need to be adapted, into a primarily perceptual or cognitive type of approach to psychology, and I believe that much of Brunswik's own research actually was in this direction. There are some important modifications at some points, however, to permit this graft to "take" and grow rather than to be rejected or neglected.

SUMMARY

The present paper is a development from one which I presented in a symposium shared with Egon Brunswik and Gardner Murphy at the International Congress of Psychology at Montreal in 1954. My paper in that symposium was primarily a commentary on Brunswik's paper, now printed for the first time in the present volume, and on his ideas more generally. In the study and discussions preceding that symposium, I became convinced that Brunswik typically had not been expressing the underlying trend of his thinking and that, for an adequate utilization of his contributions by psychology, he should be presenting a somewhat different formulation than he usually urged. In rewriting my paper for this eventual publication, I have become more convinced than ever that some considerable reformulations are necessary to permit optimal use of Brunswik's contributions. This paper has attempted to sketch some of these main points.

Brunswik's work has been less discussed and less used by psychologists than should have been the case. Most psychologists have had only a vague idea of what he stood for. They have not had the means of assimilating the major contributions from it. In turn, the limited attention to Brunswik's ideas during his lifetime prevented him from receiving the healthy criticism and from making some of the revisions and restatements that he might have made otherwise. It is important now to try to make up for what might better have occurred earlier.

Several factors helped to account for this relative neglect. Brunswik's emphasis on perceptual phenomena and on probabilistic considerations was not much in keeping with the *Zeitgeist* of American psychology dur-

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ing most of his life, even though there has been more congruence in the last ten years or so. His ideas and terms were new enough that, combined with his terse and difficult style, they made his writings and papers difficult to understand and criticize. The amount of influence of Brunswik's writings has probably been limited as well by another factor—to wit, that other psychologists have felt that various of Brunswik's descriptions of their concepts did not correspond with the real intent of those concepts. Further difficulties have existed because Brunswik did not spell out adequately the meaning of some of the crucial terms that he used. There were various factors, therefore, which perhaps make it understandable that there has been merely a limited discussion of his concepts. They have considerable significance for psychology, however, and this earlier paucity of discussion should be remedied.

In summarizing Brunswik's main ideas, it is worthwhile to group them under two headings. In many ways, Brunswik seemed more interested in broad methodological questions, or the problems of theory-construction, than in more particular empirical questions. However, his view was that certain methodological principles were important, not because of any *a priori* philosophical analysis of science in general, but because of certain empirically demonstrable points about the nature of psychological processes and about the nature of environmental factors related to those processes. Any discussion of his system, therefore, needs to rest on a discussion of his psychological assumptions.

Some main points that Brunswik emphasized regarding psychological phenomena were these: Psychological processes are adaptive biological processes. Very commonly, they are means of dealing with things distant in time and/or space. The cues on which the organism has to depend in such dealings are always of limited ecological validity, and the means which the organism must use to try to produce the environmental effects are of limited validity, too. In this situation, the higher organisms, especially, have achieved increased effectiveness through a means of functioning best described by a "lens model." Organisms have the means for multiple use of cues and means, and they have the capacity for choosing between alternative possible means for attaining any particular objective. In all of such responding, because of the limited ecological validities of cues and means, the organism is dealing with probabilistic situations rather than with the highly invariant situations that we tend to set up in experimental research because of considerations of expediency and because of our desire to establish strict laws of behavior. In their cognitive activity, organisms are engaged in processes that are basically reasoning-like or ratiomorphie. Despite the fact that this is true both of perception

and of thinking, there probably are important functional differences between these two sub-types of cognitive activity, and we should explore these differences. The lens functions are not completely equivalent to one another.

On the side of methodological principles, Brunswik urged that such empirically demonstrable points regarding psychological phenomena create a need for certain related ideas about the proper procedures for psychological research and theorizing. More particularly, some of the main points he advocated were as follows: Any abstract statement must rest on some adequate sampling of the domain that it purports to cover. Since most psychological propositions speak about types or classes of situations, we must have adequate samples of such classes or domains of situations. Particularly for any adequate quantitative principles in psychology, we need to have a representative sampling of natural-cultural habitats or ecologies of the organisms considered. In the representative research design which we ought to use for such purposes, we ought to deal with real-life situations; these involve the probabilistic features which psychological processes actually have to be concerned with; they also give wide opportunity for significant lens functioning; and they permit a study of interactional effects generally eliminated in experimental situations. From psychological research, it would be a mistake to expect that we can achieve strict laws—the probabilistic nature of what the organism must deal with signifies, in turn, that our laws can only be probabilistic laws. Because of the capacity of organisms for vicarious or lens functioning, psychological research should not generally be concerned with process details or intervening processes, but should generally be concerned with relationships between distal stimulus situations and distal achievements. However, after distal-distal studies have defined problems that might be given more intensive study, there is a need for systematic or analytical research guided by such prior exploration "from above." Even so, the main means of analysis of more particular factors should be partial-correlation studies of distal-distal data.

These major concepts from Brunswik are a stimulating and significant set of proposals. Some of Brunswik's concepts and some of his terminology ought to become part of our common psychological heritage. However, any adequate use of Brunswik's contributions is not apt to be made until we re-examine his notions and remove from them the over-generalizations that he sometimes made and spell out more clearly the implications of some of his principles.

The point from Brunswik on which there should be least argument, though it is a principle widely neglected in the theorizing of psychologists,

is that abstract generalizations should be restricted to what the sampling of situations actually gives a research worker some good warrant for saying. A point of almost equally obvious character is that, for significant quantitative principles of any abstract sort, as contrasted with quantitative principles related merely to particular experimental situations, we will have to make the very ambitious studies suggested by Brunswik's concept of representative research design.

In Brunswik's discussions, however, there is not sufficient indication of the value of *qualitative* principles in psychology—principles identifying certain variables as having certain relationships to other variables, but not attempting to estimate the quantitative aspects of these relationships. Brunswik ought also to have indicated that, as a means of testing many abstract statements, it is possible to use a much more economical means of sampling than representative sampling. A considerable degree of testing can be done by a technical examining of extreme or possible limiting cases.

There can be no doubt but that, as Brunswik said, the predictions from psychological principles cannot be better than probabilistic statements. There are so many uncontrollable factors both within the organism and within the environment that predictions cannot be exact. However, the contrast that Brunswik drew between probabilistic laws and strict laws was overstated. Even though the predictions in psychology must be probabilistic, our aim remains that of learning to understand as many variables and relationships as possible, and in as definite terms as possible, so that our predictions can become as nearly precise as we can make them. In our search for such relatively more precise means of prediction, we need to investigate a great diversity of different ecologies for different purposes. Different ecologies cannot be identified satisfactorily in any *a priori* manner; the reason we group some set of situations together as falling within some type of natural-cultural habitat is that we know, from our understanding of significant causal factors, that this is a set of situations worth studying separately from other sets of situations. Therefore, systematic research often needs to be the prelude to definition of ecologies and to the planning of representative research, rather than always the reverse of this.

When Brunswik proposed that partial-correlational analysis would be the chief means of teasing out special factors, he was proposing an idea that might seem feasible on first consideration; however, even though a wealth of factors may have been present in the situations studied by representative research, the investigator typically would not have noted the presence of most of such factors, and his records would typically not

contain the data from which such heuristically noteworthy analyses could be made "after the fact," as Brunswik expressed it. Instead of speaking so disparagingly of case-material and of experimental methods as exaggerating, distorting, didactic, Brunswik might well have said that the original identification of significant variables and relationships commonly has to come through contact with unusual situations that involve some relationship in some unusually clear or exaggerated fashion.

Studies of distal-distal relationships can have many values, especially for exploratory investigation and for rough practical purposes. However, the lens-functions of the organism fall so short of complete equivalence that it is very important for psychologists to explore mediating or intervening processes as well as the more broad-arching distal relationships. Actually, a great portion of Brunswik's own research was concerned with mediating processes.

An example of the more detailed analysis that is needed is the point that Brunswik's discussions of the lens model covered two partly different phenomena: on the one hand, the *combining* of different cues or means to gain more assurance than could come through any *either-or* selections; on the other hand, the selecting or *choosing between* one cue or means and another.

There is need of much analytical work in psychology. It does not take us very far, for example, to say that cognitive processes are ratiomorphic or reasoning-like; we need to understand how to predict whether the organism, as one might say, will reason from one set of premises rather than from another, or will engage in a reasoning-like process that stresses certain factors rather than others.

Brunswik did not give much attention to the question of whether many stimulus situations have the potentiality of supporting, in different individuals, or in the same individual at different times, drastically different cognitive processes analogous to the widely different perceptions that can occur with reversible illusions and other ambiguous figures. It may be that such examples are oddities. On the other hand, it may be that, especially in such fields as those of personality and social psychology, almost all situations are highly *violdentig* or ambiguous. In such fields it may be of major importance to realize that cognitive processes tend to select only a portion of the cues that might be used and tend to construct dynamically organized processes that differ enormously from person to person and from culture to culture. If this is in truth a widespread phenomenon, it would mean that Brunswik ought not to have placed so much stress on studies of distal-distal relationships, but ought to have placed much more emphasis on the idea that he expressed at some points

that cognitive processes are a means of producing, within the organism, a representation of the objective environment, and that many of our formulations need to be distal-central-distal formulations, rather than distal-distal formulations. The extent to which there is some occurrence of markedly different representations of the environment within different individuals, even under what seem like very similar training situations, is something that we can determine only by extensive empirical study. Only by some more adequate representative sampling of life situations can we determine the degree to which the character of cognitive processes justifies a distal-distal type of approach or the degree to which, on the contrary, we will need to make mediational studies and use mediational concepts such as are suggested by cognitive studies with ambiguous stimulus materials.

There may be a number of changes and refinements, therefore, that ought to be made in Brunswik's principles. However, it is well worth the labor to make any such changes, because Brunswik has given us a considerable number of concepts that are indispensable to help psychology become a more mature and careful field of scientific work.

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