Form Discrimination as Related to Military Problems: Proceedings of a Symposium (1957)

Pages
281

Size
6 x 9

ISBN
0309361516

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PREFACE

Two years ago, prompted by the paucity of organized quantitative information in the area of visual form discrimination, one of us made a limited tour of several laboratories known to be engaged in various aspects of research in the field. For a variety of reasons which will become evident, that junket proved both rewarding and frustrating. In discussing the tour, we were struck by the number of people who were working on some of the same aspects of similar problems, the lack of knowledge in certain areas of form discrimination, the widely differing and ingenious experimental approaches being made, and the volume of research that an apparently small group was doing. (Inevitably, too, our compendium of workers in the area became proliferated as communications became better.)

In that discussion we concluded that it might be extremely stimulating and productive if the relatively small group of researchers actively working on form discrimination could get together for an organized but informal exchange of ideas. The thought was not at all designed to eliminate overlap or even duplication of research on the problem, but, rather, to speed up communication within the group and, hopefully, to identify critical areas in which little was being done, and thereby stimulate more work.

It was initially considered that a symposium on form discrimination should properly include discussants representing all sensory modalities as well as, perhaps, personality and social psychology. The restriction to visual form discrimination resulted from the facts that (a) by far the most work on forms has been done in vision, and (b) the inclusion of other modalities and disciplines would have necessitated many more days or weeks of meetings than could practicably be arranged. It is hoped that some future symposium will serve to bring together some of these areas where manifestly analogous form discrimination problems are being investigated so that further cross-fertilization may occur.

We cast about for some time for a source of support for the proposed symposium, and were most fortunate in gaining the busy ear of Stan Ballard, just then in process of becoming Executive Secretary of the newly reconstituted Armed Forces—National Research Council Committee on Vision. Because of the impressively increasing number of military applications of research in visual form discrimination, it was thought that support of such a symposium might be an appropriate activity of the Committee. The Executive Council of the Committee, happily, concurred, and its Chairman, General Byrnes, appointed a working group composed of Stan Ballard and ourselves to arrange the symposium.

In preparing a program for Committee sponsorship there seemed at least three goals to be met:

The first was to get a reading on present military requirements for research on form discrimination, and some idea of the degree to which they were being met. To attain this goal we planned three steps: to get a statement from a representative of each of the Services as to critical operational problems which involve form discrimination; to provide a fairly close look at some of the representative research that is presently going on in the area; and to provide an opportunity for the military to identify critical areas in which the data necessary for problem solution are lacking and are not being generated.

The second goal was to try to accomplish the same things as the first, in essentially the same manner, but within the broad frameworks of psychophysics, psychophysiology, and psychology in general.

And the third goal was to try to provide an atmosphere which would best promote realization of the first two.

In attempting to meet our goals we decided fairly early that participation and attendance should be carefully limited to people who are active in areas which bring them into contact with problems in form discrimination or who are actively engaged in research on form discrimination. Initially, we were thinking in terms of twenty to thirty participants; a group large enough to include representatives of a variety of experimental approaches plus adequate military representation, yet small enough to preserve an informal atmosphere and allow time for free interchange of ideas. First we listed all the workers we knew who were doing forms research; then, to check ourselves, we went through the Vision Committee Bibliography and the Tufts Bibliography on Human Engineering, pulling the names of people who have published research on visual form discrimination during the last five years. At this juncture, we realized that our original estimate had been a bit conservative and we prepared for the possibility that thirty or forty might be involved, but still hoped it would function as a small, intimate group.

With a tentative program in mind we mailed, to likely candidates, invitations to attend and participate. And people who were doing forms research or who had related problems seemed to crawl out of the woodwork. When we finally put the program to bed the evening before the symposium we were still getting requests for attendance or participation. On the first day of the meetings three additional people arrived with prepared presentations, so that we had to make last-minute changes in the program. Since the symposium we have learned that some others had prepared presentations but had not made themselves known. Eventually, over thirty-five participated formally in the program; an additional fifteen or so participated informally in discussions. Over seventy-five attended some part of the proceedings.

The program of the symposium, modified only to include titles of papers and to reflect changes that occurred forms the Table of Contents of this volume.

Session I was designed to define the problem of form discrimination and cast it into perspective within the framework of military applications and the rubrics of psychophysics, psychophysiology, and general psychology. The second session was to provide up-to-date information about some representative technical and procedural aspects of conducting research on form discrimination. Session III presented experimental results relating form to the elementary visual detection process. It served to point out those few pieces of research which have been conducted on the role of target shape at or near the brightness or contrast threshold. Julian Hochberg's paper, not read at the symposium owing to time limitations, is published here in order to supplement the other two presentations. Session IV essays the jump from detection to higher-level characterization of the stimulus, and acts somewhat as a catch-all session for presentations of unscheduled papers and those difficult to fit in elsewhere. The final session was left wide open. It was expected, however, that the ball would be kept rolling with afterthroughts from the participants in Session I, so that the primary goals of the symposium might be realized. It was also hoped that general discussion would promote summarization and conclusion-drawing, either orally or implicitly, by all of the people at the symposium. Ideally, every person attending should have been able to make some judgments about the degree to which current research on form discrimination is meeting his requirements -- as a military person with operational problems, as an experimentalist with methodological problems, or as a theoretician with conceptual problems. The degree to which Session V succeeded in this aim will never be precisely known, but the results represented by the informal discussion seem encouraging.

The entire proceedings of the symposium were recorded on tape, and have been transcribed except for papers left or supplied afterward by several of the participants. We have edited the material loosely in an attempt to preserve the atmosphere of the symposium. None of the participants has seen the results of our editing, so we alone are responsible for any inaccuracies in the transcription that may be found, for omissions made for the sake of brevity and for the format in which the proceedings appear.

Many people contributed substantially to the symposium and to this volume. Those who took time from their crowded schedules to present papers so effectively and to cooperate so well in supplying manuscripts and graphic material made both the symposium and this volume possible. The audience, which almost completely disappeared by metamorphosis into participants, added to the symposium the always-hoped-for, but hardly-ever-achieved, tempo and flavor which we have tried to retain here. Dr. Paul D. Coleman of Tufts installed the sound and tape recording system. He literally heard every word that was said and made it possible for papers without manuscript and for the entire discussion to be presented here very nearly in their original forms.

Mr. Paul Ronco, with Miss Lee Jenkins and Miss Dona Macaulay, managed the logistic details of the symposium, so often taken for granted, with acumen and without spilling a drop. The administration of Tufts was generous in providing facilities and services in support of the meetings.

Mrs. Mary O. Gavrelis transcribed the tapes and assisted in editing the manuscript. Mrs. Robert Kelly prepared the manuscript in its final form. The staff of the Publications Office of the National Academy of

Sciences provided expert guidance and direct help in carrying the manuscript from rough copy through publication.

We are grateful to all.

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May we now start the Symposium on Form Discrimination as Related to Military Problems. You will be interested to know that this is the first official function of the recently reconstituted Armed Forces-National Research Council Committee on Vision, and for that reason there are some of us who hope very much that this symposium will be a success. I trust that you join us in this hope.

This is not a regular meeting of the Vision Committee but it's one of the other activities of the Committee, namely, a symposium on a specialized subject: an invitational symposium on a specific subject. The Vision Committee consists of about 80 individuals, half of whom are nominated by the three military services and the other half by the National Academy of Sciences-National Research Council. Among the "civilian" members here today are Clarence Graham, Glenn Fry, Lorrin Riggs, Dick Blackwell, Mason Crook, Bob Boynton, and Joe Wulfeck. Some of the rest of you are members nominated by the military.

The Vision Committee is run by an Executive Council of which Brigadier General Victor A. Byrnes is the Chairman. He is also the Chairman of the big committee and he is the Air Force representative on the Council. So, if any of you who are associated with the Air Force have problems that could come before the Vision Committee they should be sent to him.

General Byrnes is very sorry he cannot be here. He asked me to send you his regrets and his best wishes for the success of this meeting.

The appropriate person in the Army to receive visual problems is Colonel Charles S. Gersoni of the Walter Reed Army Medical Center, and the appropriate person in the Navy is Captain Clifford P. Phoebus of the Office of Naval Research.

Large meetings of the entire Committee are to be held each year; the first one will be held in May 1957 at the National Institutes of Health in Bethesda. The Committee also has working groups which study specific subjects. The work of the Committee is supported by the three Services through an ONR contract with the National Academy of Sciences.

The Secretariat would like to have as complete a collection as possible of reports on work pertaining to vision, and if each of you would think of putting me on your distribution list for reports, either classified or unclassified, I would appreciate it very much.

This symposium has been organized by two eager young form discriminators whom I would like to introduce to you because they'll be taking

SESSION I

THE SIGNIFICANCE OF FORM DISCRIMINATION

Chairman: Colonel Joe G. Gillespie

GILLESPIE: I want to thank Dr. Ballard and Dr. Taylor for inviting me here to participate in this meeting today. I have heard so much about the Vision Committee that I am really looking forward to see what happens at one of these meetings. I am sure that I am speaking not only for the Air Force but for all of the military departments when I say that we're all very keenly aware of and interested in the work of this committee and the research represented by members of this committee. As Dr. Ballard said, I am particularly interested because I have been involved in matters pertaining to reconnaissance during the two wars that have come along while I have been in military service. I am quite familiar with a lot of the problems that have existed for a long time, and I seriously doubt if the advent of missiles and satellites is going to do anything to improve them. As a matter of fact, I am sure that the problems will become more complex rather than less complex. For this reason we certainly have to depend on the work that you people represent here in trying to solve some of these problems.

I'd like to call up the members of the first group: Mr. White, Dr. Harker and Dr. Bersh. They will discuss military applications of form discrimination.

THE SIGNIFICANCE OF FORM DISCRIMINATION IN NAVY OPERATIONS

Carroll T. White

One's first reaction to the question of the significance of form discrimination in naval operations is to assume that it is a matter of limited interest. On pursuing the subject in more detail, however, it becomes clear that problems involving the discrimination of form and pattern exist in practically all phases of the Navy's work. I would like to mention a few of the major areas in which more knowledge along this line is needed.

The most fundamental situation is that in which the unaided eye is used to search for and identify an object which is in its "normal" condition. In this case the information regarding the object is mediated by light, and is affected by what may be termed "natural processing", e.g., atmospheric haze, turbulence, illumination, etc. The physical attributes of the object must also be considered - including its color(s), pattern, texture, and geometrical shape (or shapes, if its outline changes with a change in aspect). The interactions among these factors, and between these factors and the object's environment complicate the picture still further.

A second area of interest, closely related to the first, is that in which the physical characteristics of the object are intentionally altered in order to make it either more or less detectable or discriminable. The whole field of camouflage could be considered here; and also the work done to establish those combinations of shape, pattern, and color which are most easily seen against various backgrounds, so that items such as life-rafts may be designed which will increase the probability of early rescue. The complications brought about by the "natural processing"of the light-mediated information must be considered here, too; in fact, the best solutions to problems of camouflage and enhanced visibility will undoubtedly come with an increased understanding of the laws governing the visibility of the unaltered object in its natural surroundings.

When optical aids are introduced into the light-path between the object and the eye of the observer, another set of factors must be considered: the degree of magnification, the size of the field provided, and the various types of distortion which might be introduced by the optical system. In this general area we should also include studies regarding the use of polarizing filters and colored filters of all kinds, which might change the boundary contrast between an object and its surrounding.

My discussion to this point has dealt with what may be called first-order optical displays. In distinction to these we might consider a photograph as an example of a second-order optical display. We have here an opportunity to study the various means of recording, processing, and viewing photographic records in an attempt to derive more information from them. Color relationships can be varied by the use of special filters, the gray scale can be controlled in the processing of the film, and even the time dimension can be manipulated by the use of time-lapse or high-speed photography.

Form discrimination as such is usually not thought of in connection with the displays of electronic systems such as radar and sonar, but there is every indication that it should be studied in relation to such systems. The psychophysical work which has been done in these fields has been primarily concerned with intensity and amplitude discriminations, but even in this work there has been evidence that other factors are operating, factors that cannot easily be isolated or quantified, factors that seem to separate the experienced from the non-experienced operator. Many investigators believe that subtle differences in the shapes of elements of radar and sonar displays may be one such factor. These differences have not as yet been described and standardized in any existing vocabulary. One area in which form discrimination very definitely plays an important part is Sonar Target Classification. Here the shape, pattern, consistency in time, and other, still undefined, characteristics of the visual display are used, along with auditory signals, in an attempt to determine just what it is that is giving the returns. This is a major problem facing the Navy today, and I can assure you that form discrimination lies at the heart of it.

The distinction between first-order and second-order displays might also be made for radar and sonar, but it is hard to know just where to draw the line. The simplest way might be to refer to the most commonly used displays ("raw" radar or sonar) as first-order and to any display which is the result of special processing of the raw data, second-order.

The part that form discrimination can play in first-order electronic displays has already been mentioned. Now, let us consider the ways we may expect it to be of importance in second-order electronic displays. A great number of kinds of special processing is possible, but a consideration of three basic types should serve the purpose. These may be called spatial, temporal, and symbolic processing.

Spatial processing could refer to any display system in which changes in the geometry of the target representation are brought about in an attempt to improve the operator's performance. For example, changes in the strength of target returns might be represented by changes in the size or shape of the display elements, rather than by the usual changes in intensity.

Temporal processing could refer to any display system in which the time dimension is manipulated in an attempt to improve an operator's performance. The application of the principles of time-lapse photography to electronic displays is probably the best example of this. Such a display technique brings about some unique problems of form discrimination, since we are here dealing quite often with forms and patterns that do not actually exist in any physical sense, but emerge somehow in the perceptual processes of the observer. Emergent form might be a good term for this.

Finally, we have symbolic processing. Here the form in question must convey much more than, say, the mere existence and present location of a target. A single symbol may be required to indicate the exact nature of an object, its altitude, course, and speed, the degree of threat, etc. Symbols such as this are used in filtered electronic displays, and their use will increase a great deal in the near future. All the services have data handling systems which employ such symbols, and new systems are being developed all the time. Unfortunately, there has been no agreement as to the characteristics of such symbols, so every system has essentially a unique symbology. The significance of this fact can be appreciated when it is pointed out that many of these systems will be expected to work in conjunction with one another, and that operators may be expected to work with more than one such system. It is apparent that an attempt should be made to establish an effective standard symbology before the situation gets completely out of hand. I should like to suggest that the Vision Committee consider the problem of symbology. If this does lie within your province, any contribution would be most welcome and useful.

I hope that this brief discussion has helped to emphasize the importance of the study of form discrimination for a better understanding of human information gathering and processing, and has demonstrated the fact that form discrimination is a very real problem in many phases of the Navy's operations.

THE SIGNIFICANCE OF FORM DISCRIMINATION TO CURRENT ARMY PROBLEMS

George S. Harker

In speaking of the significance of work on form perception to problems of the Army, I should like to start with a consideration of the basic unit of the ground forces, the foot soldier. His physical environment in the most part is identical to that you and I might experience every day. Yet, the foot soldier learns to respond to this environment in terms of its military features; defilade, cover, concealment, avenues of approach, defensible positions, points of observation, etc. This is form discrimination in a most general sense, a sense which is highly significant to the military duties of scouting, patrolling, forward observation and perimeter guard.

The form sense has been characterized as the capacity to recognize the primitive outline of objects in the visual field. The implication is that a threshold exists for form even as there is, for example, a threshold for brightness. The critical elements, i.e., the psychologically sufficient dimensions of this threshold function are, however, yet to be determined. Will they, when determined, encompass the above mentioned problem of discrimination of form in context? Without denying the significance of the threshold concept, I wish to assert the significance of form discrimination in this latter sense. Form perception is a function of both the sensory receptors and the higher nerve centers. It involves the integration of processes at several levels. It is not limited to one modality since form discrimination has visual, kinesthetic and auditory components.

I should like to differentiate two modes of experimental attack upon problems of form discrimination. Both have the same ultimate objective, the demonstration of the manner of functioning of the human and the expression of his capability in terms of physically defined, psychologically sufficient, variables. However, the order, manner of approach and area of application are different.

In those circumstances in which a fairly definite task is specified and the method of stimulus presentation determined, job simulation techniques can be used in conjunction with selected conditions to sample a range of the physical variables present. Such an approach permits the accumulation through laboratory investigation of information on task capability as a function of operating conditions. In effect, it compresses years of operator experience with a task into months of laboratory investigation. The deduction of human function and the dimensionality of the psychologically sufficient variables is secondary and, due to the psychological complexity of the task, is frequently very difficult. The degree to which the information so obtained is generalizable to tactical situations is a function of the authenticity of both the instrumentation and the experimental situation.

The second approach stems from a recognition of the human as the primary communality in a number of possible task situations. Since the task is not specified and the operational situation is subject to selection, attention is directed to a scrutiny of the capacities and the functional interrelations of the response systems of the human, i.e., the human becomes the subject of the investigation. In the visual sense modality, one is committed to determine those factors which transform a complexity of stimulation into a figured perception. These must be determined for degrees of stimulus complexity which range from the absence of differential stimulation, i.e., total darkness or total brightness to the complex situations present in the natural environment. In this regard, the study of the processes of concept formation are meaningful, as are studies of the influence upon the human's behavior of delimiting the stimulus-response categories. The concepts of stimulus over-determination and response availability, or the correspondence of the number of stimuli to the number of responses, also have a place in the study of form discrimination.

In seeking to provide the soldier with mechanical, electrical, and/or optical aids we deal with the perceptual processes of the human at several levels. Depending upon the complexity of the information to be conveyed and the mode of presentation selected, we may be required to work with a threshold type response to form or with a form identification process. For example, the problem of reporting details of the physical environment via a monochromatic flourescent screen is a matter of concern in several military efforts. In some instances the operator's task is to detect the occurrence of a pip, or configuration of pips, against essentially a dark background. In other instances, the task of the operator is to interpret as a picture the relative strength of a multitude of returns. Irrespective of the manner of probing the environment, IR, Radar, or Optar, and within the limitations imposed by such factors as the angle of the illuminant beam, the relative reflectivity, and the incident angle of the surface, the granularity of the presentation, and the inherent noise, the meaningfulness of the display is determined via the processes of form perception. In some instances where the display is three dimensional, as in stereoscopic displays, the added feature of binocular fusion is available to the operator to elicit those perceptual processes associated with viewing the environment from two laterally separated positions.

Suffice it to say, our ability to aid the operators of existing equipment will in a large part depend upon our understanding of the processes by which the human formulates a concept of a unique form from distinctive contours. Our ability to effectively direct efforts to improve existing systems will hinge on our understanding of the dimensions of the human's ability to resolve critical features of form, particularly in relation to the potentially limiting weaknesses of the present system. And, our ability to select systems for future development which offer more promise than present systems will depend upon our understanding both of the fundamental processes by which the human discriminates form, and of the interrelation of the various elemental mechanisms and their proportional contributions.

The threshold concept of form discrimination assumes significance as the degree of impoverishment of the stimulation increases, i.e., as the dimensionality of the stimulation is reduced. Such impoverishment often occurs in the effort to provide technical aid for the soldier and to operate in extremes of the natural environment. To the degree that we understand the human's responses to these stimulus reduced situations, we will be better able to approach the synthesis of the human's responses to the generalized stimulation of the natural environment. One comments on the effort to solve the problem of the stimulus reduced situation by complicating the stimulation available to encompass the full dimensionality of the natural environment. This is not an infallible answer. The relation of stereoscopic ranging sensitivity to increases in base and/or optical magnification and the perceptual consequences of attempting to substitute one for the other is simplified when ranging into an impoverished visual field. Stereoscopic ranging against a sky background or over the open sea does not deviate from theory as does ranging into a rising wooded slope. Understanding of the underlying processes operative within the human and their interrelation is necessary in order that equipment development can efficiently exploit the human's capability.

To sum up, the significance of form discrimination for current problems of the Army is associated with areas of special emphasis resulting from equipment or armament development and with a general interest in the functioning and capabilities of the human as a basis for the guidance of future technical developments.

AIR FORCE REQUIREMENTS FOR FORMS RESEARCH

Philip J. Bersh

Since I learned only in the past few days that I was to be a member of this panel my remarks will necessarily be informal, somewhat scattered, and perhaps somewhat simple-minded. I won't pretend to cover in any comprehensive fashion all of the Air Force's interest in form discrimination problems. Instead, what I will do is attempt to cover briefly about four or five areas with which we at Rome are or have been concerned. I should point out that one of our major functions is to support an intelligence laboratory and therefore our emphasis here will be upon the reconnaissance and intelligence aspects of the form problem.

Let me begin with photographic interpretation. Where the photographs to be interpreted are of large scale and high quality and where the time available for interpretation is considerable, by and large the major practical limitations upon the identification of targets or the extraction of other information are those imposed by deficiencies in the skill or experience of the interpreter. However, modern reconnaissance vehicles are of necessity being designed to travel higher and faster with correspondingly detrimental effects upon both the quality and the scale of the material collected. Furthermore, collection systems are being designed to gather ever increasing volumes of data at ever increasing rates. To this we must add the fact that reconnaissance data constitute a highly perishable commodity, one whose timeliness limits are constantly being reduced. In the future, therefore, the photographic interpreter may often be called upon to interpret very small-scale, highly degraded material and do it at faster rates than any to which he has been accustomed. Such a prospect points to several important problems. Among these we may mention the following. What are the limiting values as far as identification and interpretation are concerned? As for the physical characteristics of photographic materials - and by the way you'll recognize here that what I say about photography will apply in stage, you might say, to infrared interpretation and to radar interpretation. Among the characteristics of obvious interest are scale itself, granularity, sharpness or contour gradient, density, and contrast. Of course, we as psychologists might prefer first to translate these variables into stimulus conditions or stimulus variables and then to study the influence of these stimulus variables. Actually, we want ultimately to know not only the minimum values for such physical characteristics but if possible their optimum values and combinations of values. The latter information is needed to provide quality standards or goals toward which development of special processing procedures, optical aids, image enhancement methods, etc. must be directed.

The second question of importance is how such characteristics influence the time required for recognition or interpretation. Research emphasis in the area of form discrimination has typically been upon problems of discrimination proper. Relatively little attention has been given to the speed factor. Data on the influence of relative stimulus variables upon time, as well as accuracy, measures would permit determination, where required, of appropriate presentation rates for photographic materials.

The third type of problem arises once again because we must somehow expect timely intelligence from large volumes of photographic materials. There are simply never enough skilled photographic interpreters available. We must preserve their skills and experience for the work to which they are best suited by seeing to it that they are not burdened with useless material or material which contains nothing new. We should have an interest in screening processes by perhaps less skilled individuals who must make more elementary types of judgments, judgments to the effect that there is something here to be seen or judgments to the effect that, well, here's a photograph taken of this area once and here's a photograph taken of this area on a later occasion. There is or is not something new in this photograph, some change in form or configuration of forms which might have military significance, and so on.

In many respects form discrimination enters into visual reconnaissance in much the same way as in photographic interpretation. In order to avoid detection or interdiction, the reconnaissance vehicle must fly higher and faster or perhaps lower but faster so that we must again be concerned with limiting values of the stimulus variables required for detection or recognition of targets. These set for us the effect of altitude and speed limits within which visual reconnaissance is feasible. Although this field has had a rather extensive history of research effort, almost all of this research has been concerned with the detection of homogeneous targets against homogeneous backgrounds. Where air-to-ground visual reconnaissance is concerned, both emphases are perhaps somewhat misplaced. Only recently have workers in this field turned their attention to recognition responses, and not as yet, to any great extent, to targets and backgrounds which are not uniform in luminance. The need for research on form recognition in situations involving target background complexes of non-uniform contrast cannot be emphasized too strongly. Even an attempt to generalize the laws of contrast attenuation by the atmosphere to a large number of situations requires a technique for evaluating contrast in a non-uniform case.

I now pass to low altitude, high speed pilotage. In recent years the Air Force has evidenced considerable interest in the potentialities of low altitude, high speed flight. By low altitude I mean 500 feet or less. By high speed I mean 600 miles per hour or somewhat faster. There is a limit on the speed because you don't want to get into the noise barriers, etc. at those levels. The basis for Air Force interest is obvious: low altitude flight may escape radar detection. At the same time the high speed is required, among other reasons, to make interception difficult if detection should occur. The combination of low altitude and high speed effectively prevents navigation by methods other than visual control. Since relatively small deviations from the planned flight path can mean abortion of the mission we must do all we can to aid the pilot in navigating accurately as well as in avoiding obstacles. One possible form of help is to provide him with appropriate maps or charts. Such charts would portray important landmarks and potential obstacles. Two classes of problems are involved in the development of such charts. First of all we must select salient cultural and terrain features for portrayal on the map as landmarks. Of course, we have no real choice where obstacles are concerned. It is clear that the brief time available to the pilot for recognition may constitute as critical a factor in the selection of check points as is the relative identifiability of various terrain and cultural features. One might expect that recognizability and recognition time would tend to co-vary with relevant stimulus conditions, but much research is still needed to demonstrate this.

Another major factor for consideration is, of course, the fact that shape distortions are introduced because of the obliquity of the pilot's view. He cannot look directly below the plane because the ground is passing by him too rapidly. He cannot look immediately in front of the plane because of the plane itself. Therefore his angle of view is really an acute angle. After suitable landmarks are chosen and obstacles noted there still remains the problem of representing such features on the chart in the most effective fashion. It should be noted that the pilot has very little time during which to consult the chart while he is navigating. Furthermore, he must not only use the chart to anticipate obstacles and turns in his course but he must correlate the chart material with what he actually sees. It is vital, therefore, that chart representation be such as to give literally the maximum of information in a minimum of time.

Such requirements raise questions of the following variety: should the chart symbols be abstract or should they incorporate some measure of realism or resemblance to the features they portray? How much detail should be portrayed on the maps, that is, how many cultural features, how much terrain information? A great deal of clutter will make the discrimination of the landmarks and obstacles difficult. In addition, there are the usual considerations of color schemes, topographic information, etc. all of which may influence in an important way the accuracy and speed with which the pilot responds to the configurations on the map.

In the development of countermeasures to be used against airborne radar systems a knowledge of parameters involved in form discrimination is, of course, of considerable value. Generally speaking, form discrimination is important both to radar navigation and radar bombing. Navigation is accomplished by identifying returns, for example, from cities, in order to establish fixes with an error of essentially zero, and by utilizing the returns to determine wind characteristics in order to correct a pre-established course. In the former case the geometric arrangement of multiple returns may be used to identify correctly a specific area. Occasionally the form and arrangement of geographic features (mountains, lakes, rivers) may be used as adjuncts to the normal identification process.

Form discrimination is of even more obvious value in the bombing stage. By means of sector scan with short range, the pilot or bombardier can use the actual shape of the target or offset point to determine the bomb release point with extreme accuracy. Built-up areas, such as factory sections, may have characteristic shapes which make possible complete accuracy of identification. Of course, the purpose of countermeasures is to deny such information to the enemy radar observer. In order to do this we must really understand the fundamental basis for form discrimination. Only in this way can we decide what features of radar returns our countermeasures should attempt to distort, delete or obscure from the observer.

It's rather strange that I as a representative of Rome have spoken mostly about airborne problems when Rome is concerned almost entirely with ground problems, for example, automatic displays for intelligence analysts. Displays of this type can provide a useful summary of up-dated intelligence which is drawn from storage at the user's request. The function of the display is to represent changes and trends in significant events in a manner which will permit valid strategic inferences to be drawn and timely decisions to be made. The strategic indications which constitute the input to such displays represent categories of information well-suited to shape coding. The question of how best to code, however, presents a number of special problems. In many situations there are literally hundreds of categories with which we must be concerned. Therefore we have the following questions: What is the limiting number of symbols that can be effectively responded to by the analyst during a single presentation? What kind of shape code alphabet should be used? Can one alphabet be constructed in which each item of information is uniquely referenced by one discriminable symbol? Or should a fixed number of symbols be keyed interchangeably at different presentations to different categories of information? The selection of a code format will depend upon the interaction of many factors which include total number of indicator categories to be coded, the ability of the analyst to master the entire alphabet, and the degree to which each analyst is required to use all of the information for his own particular function. Applied research in developing new shape code alphabets will be necessary to resolve these questions.

I'd like to conclude by making a few general points and I'll have to make them rapidly, I guess. The form factor in form discrimination is not always of primary importance. For example, the content of reconnaissance photographs is, after all, determined by the relation of the target to the background: there is little we can or would want to do to change the shapes or spatial relationships of the images on a photograph. A case can therefore be made for concentrating our study upon those features of the photographic material, such as contrast, over which some control can actually be exercised in an operational setting. The need for training research should be emphasized. There are after all limits to our capability for improving degraded images, for removing shape distortions, and for increasing contrast. In spite of all our efforts to improve his task an observer may still be confronted with material highly unsatisfactory for recognition or interpretation. Perhaps it's time we began to pay attention to systematic procedures for training observers in the recognition of blurs and distorted shapes.

GILLESPIE: The next group of papers will deal with visual psychophysics.

FORM DISCRIMINATION IN PSYCHOPHYSICS

Ailene Morris

A "form" is an extent which is bounded or limited in a certain way.

It can be tactual, aural, conceptual, or visual. As the eye follows the boundary lines of a form, it travels different distances and rests at points of interest in various positions. Different forms elicit distinct complexes of sensations in and about the eye.

We perceive differences from one moment to the next for one sense receptor, from one receptor to the next, or from one sensory area to the next. In presenting visual forms we are differentially stimulating various retinal receptors. Form discrimination has been called "local discrimination"; that is, we can measure it by the smallest separation of two contours which can be recognized as discrete.

It is usually agreed that an abrupt change in gradient is called a "contour", that contours produce shape, and that shapes typically have meaning. You may believe that perception of contour, shape, or form is the fusing of a number of sensory experiences or that the "whole is more than the sum of its parts"; nevertheless, the eye responds in a singular way, it discriminates differences.

Let us define some of the terms used in Visual Psychophysics.

If the STIMULUS is	an area	a spot	one line	the smallest element of a pattern	an object
JUST NOTICEABLY DISCRIMINATED as.....	different	different	laterally displaced	resolved	nearer or farther
RELATIVE TO	another area	its background	a second continuing line	other elements	a second object
the THRESHOLD is ...	minimum perceptible	minimum detectable	minimum vernier acuity	minimum separable acuity	minimum depth perception
	(1)*	(2)	(3)	(4) (5)	(6)

The form and context of visual stimuli determine their discriminability.

*All numbers in parentheses in this discussion refer to sketches, see Figures 1 and 2.

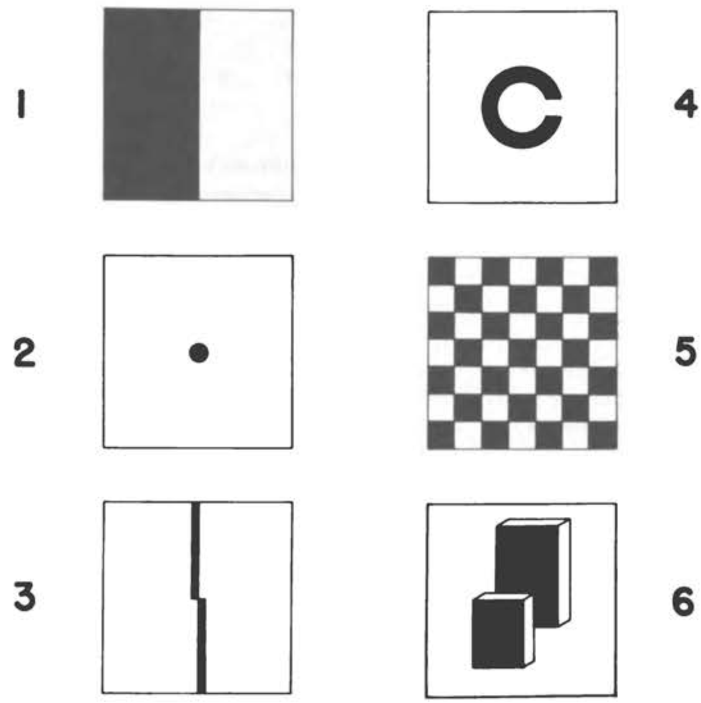

Fig. 1

In quantitative form discrimination experiments psychophysicists have demonstrated, and are still studying, the significant effects of various parameters such as luminance conditions, adaptation time, contrast ratio, color, movement of target and eye, retinal location, multiple targets and signals, and many more.

However, the observer is often required to report more than simple detection of "something" present. He may be required to recognize, identify or classify the stimulus formation. He must now draw upon the developed ideas at his command and associate them with the sensations which are his immediate response to the external stimulus. This more complex response has also come under close scrutiny by experimenters. The perception, recognition, and recall of forms has been shown to vary significantly with present mental set, which includes attention and distraction, and with past experience, which would be anything from recent instructions to extensive training. Moreover, recognition varies according to the complexity of the stimulating target and background.

Recognition or identification of a form requires seeing of more details than initial detection. The visual angle of these details must exceed the threshold for visual acuity of that eye under those conditions. The appropriate threshold measure depends upon the nature of the detail. The luminance difference between areas above and below the horizon must be greater than the minimum perceptible threshold [7]. The angular size of

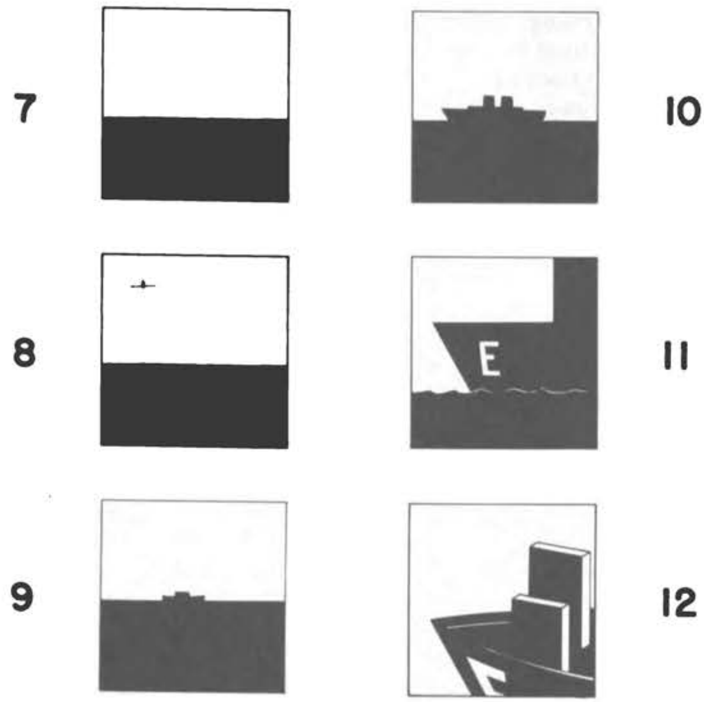

Fig. 2

an approaching target seen against a sky must be above the minimum detectable threshold [8]. To see a ship on the horizon the contour displacement must exceed the minimum vernier acuity threshold [9]. A gap in a silhouette must exceed the minimum separable acuity threshold [10]. Internal pattern such as identifying letters or numbers must be of greater visual angle than the minimum separable acuity threshold [11]. And for more complete information about the object such as its width, depth, or relative position, certain details to be discriminably nearer the observer than other details must be physically displaced in excess of the minimum depth perception threshold [12]. The hierarchy of "simple detection--recognition--identification classification" does not refer to various types of visual performance, but describes the target characteristic to be detected and the level of the discriminative response required of the observer.

GENERAL COMMENTS ON THE PSYCHOPHYSICAL STUDY OF FORM DISCRIMINATION

H. Richard Blackwell

In keeping with the announced purpose of this symposium, I may well begin by remarking on the considerable importance visual recognition seems to have assumed in a variety of military problems. In spite of physical detectors of steadily increasing capability, the human eye continues to be widely used in direct detection and recognition of military targets by all three services. Visual detection and recognition seem to be particularly important to the Army, in which the individual military unit, the combat soldier, cannot be equipped with complex detector equipment. Visual recognition is particularly important to all three services when they attempt to locate the enemy on the sea or ground from reconnaissance aircraft, because of the limited capability of most physical detectors for target identification. The importance of visual recognition is obvious in what might be called "literal" visual displays such as photography. It is equally obvious in the interpretation of the displays produced by most military sensory devices, which are "non-literal".

Form discrimination represents a primitive form of visual recognition. With the considerable contemporary importance of all types of visual recognition, we might expect our knowledge of form discrimination to be well developed. At the risk of showing my ignorance of stores of knowledge to be presented subsequently at this symposium, I venture to suggest that our current knowledge is of little use in solving any practical problems of military interest.

Since I was requested to offer general comments, I will venture to suggest certain inherent difficulties in the psychophysical study of form discrimination. The first of these is the problem of what might be called the "framework of discrimination". Form discrimination must always be made with reference to one or another form, either supplied or implied. Data obtained in such studies are completely lacking in generality, and one must repeat form discrimination experiments endlessly in order to investigate all possible frameworks of discrimination. Even if one had all this information, in many practical military recognition problems the framework of discrimination supplied or implied is unknown. For example, we do not know to what extent recognizing a tank on the battle field represents recognition of a _ding an sich_ or to what extent it represents recognition of the difference in form between a tank and typical foliage. I believe that in practical visibility problems and in visual display problems involving literal displays we must identify the framework of discrimination before practical use can be made of form discrimination data. In some non-literal display problems, it is at least conceivable that the framework of discrimination can be supplied explicitly. Perhaps such displays will be most effective when the framework _can_ be clearly identified for the user.

A second problem in the study of form discrimination concerns what might be called the "psychophysical error". I refer to the usual procedure of attempting to understand form discrimination data in terms of the stimulus objects presented to the eye rather than in terms of the neural correlates of these objects. I consider it highly unlikely that we can ever understand form discrimination so long as we relate our behavioral response data to the stimulus objects. We must become psychophysiologoical rather than psychophysical in relating cause and effect. I am confident that the psychophysiological approach is possible and I do not mean to suggest that we can study form discrimination by electrophysiological means. We can, I believe, build psychophysiological theory from psychophysical measurements. Let me be specific. Crozier, and subsequently Marshall and Talbot, have pointed the way to mathematical theories of the neural correlates of visual stimuli, in which the behavior of psychophysical data may be used to infer the characteristics of the neural correlates. In my opinion, visual detection data related to the effect of stimulus object size can only be understood in terms of the topographical mapping of retinal stimulation onto the relevant CNS centers by a one-to-many, many-to-one spatial map. Later in this symposium my associate, Dr. Kristofferson, will describe a particular neural mapping theory we have derived from visual detection data. It is not crucial that our mapping theory is correct. If any mapping exists (and we can scarcely dispute the anatomical and physiological evidence that it does), then the neural maps, and not the stimulus forms which we present to the eye, are the proper material to use in understanding form discrimination data. Dr. Kristofferson and I hope to study form discrimination, using neural maps constructed on the basis of detection data as the "stimulus" with which to correlate psychophysical response data.

It may seem unwarranted to use detection data to explain form discrimination, since our contemporary science has tended to explain detection in quantum or photochemical terms and form discrimination in judgmental or informational terms. I suspect the dichotomy of explanative principles is more a function of the modes of thought of the investigators than the indication of a difference in visual functions. It seems to me apparent that what we call detection is merely the lowest level discrimination which can be made between neural signal and noise. There is an infinite series of levels of discrimination between detection and precise identification. The precise discrimination required of the observer depends primarily upon the framework of discrimination supplied or implied. In these terms, the neural map of stimulus objects derived from detection data must provide the stimulus for the neural discriminatory mechanisms. Our problem is to understand the neural discriminator in these terms.

I would like to direct one last comment at Attneave-type informational analyses of form discrimination. Consideration of spatial stimulus patterns in terms of informational bits seems to me to ignore the neural mapping which must occur across space and to imply classes of spatial discrimination which I do not believe can exist. I will gamble, rather, on the existence of neural time-space scanning mechanisms which evaluate the CNS area stimulated by object forms in terms of a continuous functional map of the neural correlate of object forms.

Of course, the only test of general speculations of this sort is experimentation, and my associates and I hope to make such tests in the coming years.

INFORMATION THEORY AND FORM DISCRIMINATION

Wilson P. Tanner, Jr.

In this brief discussion I would like to make five points:

(1) The application of information theory to the study of form discrimination must be based on experimental data.

(2) Experiments in form discrimination and experiments in communication have the same characteristics. On the surface, at least, it appears that information theory should be useful in studying form discrimination.

(3) It is a basic concept of information theory that "any event is evaluated against the background of the whole class of events that could have happened." According to this concept the identification or perception of form depends not only on the stimulus presented, but on the ensemble of stimuli the observer was expecting.

(4) The concept of capacity is highly interesting and at the same time elusive. It suggests the necessity of buying time with error, or accuracy with time.

(5) Information theory is one of a class of theories comprising the broader field of statistics. Many of these contain the same concepts. Perhaps another member of this class, decision theory, may be more applicable.

In order to make the first point, I will resort to a quotation from Dr. Shannon's editorial, The Bandwagon(1):

> "Indeed the hard core of information theory is essentially a branch of mathematics, a strictly deductive system. A thorough understanding of the mathematical foundation and its communication application is surely a prerequisite to other applications. I personally believe that many of the concepts of information theory will prove useful in other fields and indeed some results are already quite promising, but the establishing of such applications is not a trivial matter of translating words to a new domain, but rather the slow tedious process of hypothesis and experimental verification.
>
> "If for example, the human being acts in some situations like the ideal decoder, this is an experimental and not a mathematical fact, and as such must be tested under a wide variety of experimental situations."

I share Shannon's view that the applications must be based on experimental data. When these data become available I think we will be able to formulate psychological theories far more precise than any we now have available.

It does not follow, however, that the value of information theory will be realized only in the distant future. I think that the impact of information theory on psychology is much the same as that on the communication engineers. J. R. Pierce, in a paper delivered at the IRE Convention, finds that, because communication theory presented a systematic approach with definite theorems, the communication engineers had great expectations. There have, however, been no rush of new inventions, and many engineers have become disenchanted. Perhaps information theory was oversold or overbought.

According to Pierce, information theory makes a positive contribution to the understanding of communication situations. It suggests new measures and new experiments to the engineer and to the psychologist. As a way of thinking it is of immediate value in many scientific areas.

Now, let us examine a block diagram describing an experiment in form perception or discrimination. There are a set of stimuli presented to an observer, usually one at a time. They may be presented once each, or several times each in a random order. The set of stimuli and the method of selecting the order of presentation define a message ensemble.

They may be presented tachistoscopically, they may be presented as incomplete pictures, they may be presented at low intensity, or in a number of other ways. Many of these conditions are equivalent to low signal-noise ratios, or adding noise in the channel.

Finally, the observer or subject is asked to state what he "perceives" or to identify which of the stimuli he thinks was presented after each presentation. The correlation between the transmitted stimuli and the responses completes the definition of a communication channel.

There is a striking resemblance between this block diagram and that used by Shannon(2) in defining a communication channel. On the surface at least this seems to be a situation to which information theory applies.

The third point, that form identification depends not only on the event, but on the ensemble of events that might have occurred, is one made in much more detail by Quastler (3) in his introductory remarks in Information Theory in Psychology, which he edited.

The measure of the rate of information flow in a channel requires the determination of uncertainty on the average before an event and of uncertainty on the average after the event. The change in uncertainty represents the transfer of information. This change is the same from the standpoint of the receiver or the transmitter. From the receiver's standpoint, it is the difference in his ability to state the transmitted stimulus before and after the event, the reception. From the transmitter's

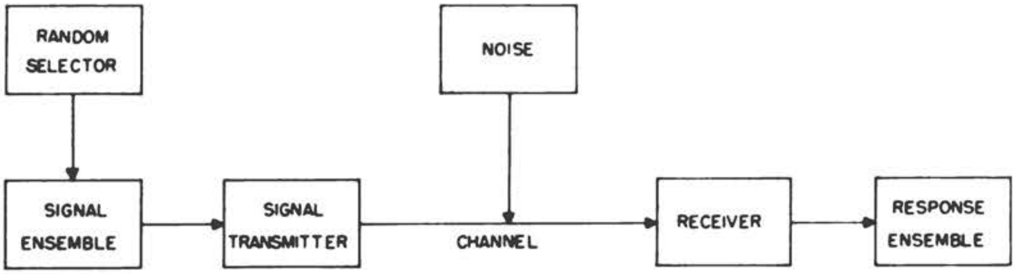

Fig. 1

standpoint, it is the difference in his ability to state the receiver's response before and after the event, the choice of the stimulus to be transmitted.

The concept of capacity states that a channel cannot transmit information beyond some maximum figure determined by the conditions defined for that channel. This means that the uncertainty following the event depends on the uncertainty preceding the event, for the change on the average can be no greater than that allowed by the channel. The uncertainty preceding the event is defined by the set of signals or stimuli which the receiver feels might be presented, along with the probability or degree of expectancy associated with each. These probabilities sum to one, meaning that the response always specifies one member of this set of stimuli.

From this point of view, a "perception" may merely represent the hypothesis selected. If this is the case, then it is not necessary for the detail of perception to be traced through every stage of a sensory system. All that is required is enough information upon which to base a choice. The detail is supplied by the hypotheses or alternatives in the observer's ensemble of expectancies. Of course, if the detail is supplied in this way, then it is possible to experience fine detail unrelated to the event in the environment. In other words, the detail might be experienced, even if the decision is wrong.

The concept of a channel capacity has already stimulated many efforts in psychology. Quastler(4) and Fitts(5) have made efforts to determine maximum capacities for human beings, and Miller(6) has considered the problem from the standpoint of memory and information storage. These are all nice studies, but I'm sure none of these authors feel they have had the last word.

Several problems arise. First of all the environment (in this case the set of stimuli and the conditions of presentation) place an upper bound on the capacity which might be achieved in any given experiment. That is, even if a perfect observer exists, he cannot accumulate information at a rate greater than the capacity of his environment to transmit this information.

Regardless of the environment or experimental design we choose, there is always the problem of how well matched is the human being to receive information through this channel. We need to know much more than we do now before we can establish a channel which we can say with confidence permits an observer to exhibit his capacity.

There are further problems. The environment may have a uniform capacity over time, while the observer may be able to operate at a high capacity for short periods of time only. Such channels are currently not a part of information theory.

The rate of presentation, the number of stimuli employed in an experiment, the observer's knowledge of the set to be employed, the method of ordering the presentations, and the observer's knowledge of the method of ordering all appear to be important variables in a form discrimination experiment if one takes the information theory point of view seriously.

So far, we have restricted our comments to concepts contained in that part of information theory which we might call Shannon theory. Wiener(7), in an editorial comment, regrets such a restriction. He feels that this theory, essentially a coding theory, is only a special case of a broader class of theories which together constitute the area of statistics. Quastler(3) discusses the equivalence between information measures and measures used in other parts of statistics. Uncertainty and variance are related. Information transmission and correlation are related. All we have said so far applies if some other area of statistics, such as decision theory, is more suitable.

It may be, for example, that the evaluations of the events are not based on information alone. That is, maybe accuracy is not the only criterion of a good response. Maybe the utilities or values of the possible choices are also considered. Intuitively, it seems likely that they should be. If this is the case, then decision theory has a great deal to contribute.

Again, the concept of capacity applies. Since rates of information are measured in terms of a reduction of uncertainty per unit time, it is possible to trade time for accuracy. Within a given channel capacity we can achieve a rate in two ways: (1) by making decisions quickly and accepting some error, and (2) by delaying decisions until more information is available, reducing error. Too great a demand for accuracy may require so much time that the decision is no longer useful, even though with proper coding the rate of information may be maintained with this delay.

Information theory and its role in form discrimination have been dealt with in a very general way; so general, in fact, that other problems could be substituted for form discrimination and these remarks be equally applicable. I have not mentioned some excellent papers treating the subject in specific ways. In a more specific exposition the studies of

Attneave(8), Fitts and his colleagues at Ohio State, Selfridge(9) and Pollack(10), to mention only a few, would command attention. These papers are unfortunately outside of the scope of the present paper.

To summarize, information theory appears to furnish a scheme for the study of form perception. The concept of channel capacity points out the role of the observer's a priori uncertainty, specifically, how the channel capacity and the a priori uncertainty place a lower bound on the a posteriori uncertainty. This concept further points out the limitations placed on the observer's performance by the environment, either experimental or natural.

The fact that decision theory may be more applicable introduces concepts of values and costs in addition to those of information theory. The buying of time with error now becomes a relevant concept.

And, in the last analysis, the applicability of these concepts depends on experimental verification.

DISCUSSION

ARNOULT: I'd like to ask Dr. Blackwell to elaborate a little more on the necessity he sees for working with the neural correlates of the stimulus. I see this as very desirable but I don't quite see the absolute necessity of it.

BLACKWELL: The necessity, of course, depends upon our familiar experimental evidence. Although we are restricted here to general comments and theories we all must realize the importance of certain data. Both psychophysical experiments and electrophysiological experiments make it perfectly clear that considerable blurring, if you will, occurs between the stimulus, which is outside the eyeball, and the final central nervous system correlate. It seems to me just common sense in science that if an important transformation makes a major alteration in a particular parameter you can't ignore this in your theorizing. That is, it seems to me very unreasonable to start with a stimulus and expect a reasonable qualitative relationship between that stimulus (the physical world) and the thing we call psychophysical response. If you have a major distortion, as in my opinion you do from stimulus to the neural correlate, how in the world can you expect it to be sensible going from S to R rather than from physiological to psychophysiological. Consider Dr. Fry's book and Dr. Boynton's review of it as one simple example of what I am getting at. There the concern is only with the first blurring loop, namely, the eyeball itself and the tremendous distortion of the stimulus which occurs before the image is formed on the retina. As Dr. Fry has shown, in many cases where the eye is only slightly out of focus one point becomes two points. It seems to me that this is rather crucial to form discrimination. If one point is really two, how can we begin to talk about extensity? Add to that the tremendous amount of spatial overlap that all experiments suggest physiologically and it seems to me that we have to view the stimulus through the very much blurred mapping function that must occur. One thing I am hoping to do some day is to build a little optical device which blurs the physical stimuli in the way that the neural pattern does. I want to see what form discrimination is all about. I want to look at a square and a circle through this blurring eye that my little homunculus has and see if I can figure out hypotheses to go on. For example, I will venture to say from just some preliminary calculations that one thing is at once obvious; that is, that you can just forget about all the difference in these forms except the sharp points. Now people sort of half know this but the tremendous significance of this is not clear. For example, people can talk about straight edges as opposed to curvilinear edges. Well, I am quite certain that with the tremendous amount of blurring which occurs in the stimulus-to-neural transformation we can forget about "straight" or "curvilinear" and begin to talk about "what is it about the edge?" Is it a discontinuity and if so of what type? This is my argument for why I think you just can't expect to be successful without going into the guts of the problem.

ARNOULT: I would like to emphasize that I accept that point of view, and I think for a complete explanation of form discrimination and recognition we will certainly have to do that. I still am reluctant to accept experiments on neural correlates as an exclusive method of approach. I think that there is a great deal that we can learn which will further this kind of work by dealing simply with -- I hate to use the term -- the old "empty organism" type of approach where we are concerned with stimulus and with response and the relations between these two.

BLACKWELL: That is not to be denied. But I would say that that has been tried for a hundred years and I think the successes are extremely limited.

HENNEMAN: I'd like to back up Dr. Arnoult in that I have two questions to pose for Dr. Blackwell. The first one is: do we know enough or are we likely to know enough neural physiology in the next forty or fifty years to do us much good? Secondly, particularly in equipment design, there are problems of the appropriate response, some stimulus data, signals, and what have you, rather than some conception or experience or consciousness. Isn't that the problem and can't we be or haven't we been fairly successful in using the empty organism approach? I am still unconvinced by Dr. Blackwell.

BLACKWELL: At least I stirred up a controversy to occupy the ten minutes that you wanted used up. Your first point: remember I didn't say that we were to study this problem electrophysiologically. I said that, in my opinion, we can infer the neural correlate from psychophysical measurements. This is the crux of the idea I will discuss later in the symposium. I'd say there are ways to dig out from detection information what I think this neural map will be. Marshall and Talbot did this with visual acuity inferring the probable characteristics of the neural correlates of acuity objects from the behavior of the psychophysical data; and that's the way I'm proposing to do this, rather than electrophysiologically. Actually, we're doing this physiological research but it looks to me like a long time before it will take this form.

The second one: of course, the objective here is not "consciousness". Heaven forbid I would use a word like that. You used it, I didn't. I'm interested in behavior, only, too. All I am saying is that it seems to me unlikely that you will be able to correlate well, objective stimulus and psychophysical response, which is what we are trying to do. It seems to me rather successful progress when we realize the importance of the first transformation and look at cause and effect in terms of the neural correlate and the response. The objective is still to predict physical stimulus conditions. What I am saying is that the theory will succeed in terms of the neural correlate. It's like atomic physics. You don't say it just chemically. You go into mathematical models based upon properties and then your theory becomes predictive of real things, not of little ping-pong balls moving around in orbits.

TANNER: I'd like to get into this argument just a little bit. The first thing I would like to say is that if we're dealing with inferred neural correlates, I think that we must remember that we're dealing with models. It may be very much like a computer model or something on that order and at this point I would like to quote Quastler again, perhaps not exactly because I don't have the quote in front of me, to the effect that this is a very useful technique. The only thing that we must remember is that we musn't get them mixed up with reality.

MORRIS: Apparently Morris says it's stimulus-response, Blackwell says it's a neural response, and Tanner doesn't believe either one of them.

HARKER: The fact I want to present is some experimental evidence that we've developed with respect to stereopsis depth. I'd like to comment that, working from the neurological fact that the retina is unidimensional, but that space, as we know it, is three-dimensional, we have demonstrated that without changing convergence or accommodation or stereopsis we can change the perceived depth between two objects in the physical environment. Now, I believe that once we have done this we are able to see the significance of cues in the physical environment for this problem from thinking in terms of the retina as unidimensional. Now this I take as being the very simplest case of Dr. Blackwell's idea of neuro-anatomy application. In other words, the organism was not empty. It had a unidimensional retina. Starting from this we were able to build our experimental concepts and able to demonstrate the perceived distance of a single unitary stereopsis may be changed by changing defined aspects of associated objects, the monocular cues.

GILLESPIE: The next three papers will deal with the significance of form discrimination in psychology. Our first speaker is Dr. Clarence

FORM PERCEPTION AND SENSORY PROCESSES*

Clarence H. Graham

I have been asked to discuss the topic of sensory aspects of form perception. From a certain valid point of view I might be expected to follow such a line as enumerating some stimulus configurations and considering theoretically the kinds of physiological processes that may be concerned in the sensory inputs for different perceptual processes. The point of departure for such an account might involve a consideration of the area-intensity relation and its physiological implications for different sizes and shapes of figures or the significance of the intensity-time function for the problem of identification of figures. (It would probably happen in the case of the latter topic that I might spend time considering, on theoretical and experimental grounds, some problems arising in connection with duration threshold as a datum in many types of perceptual experiments.)

I admit that the types of discussion I have here mentioned might be profitably carried out, but I am not going to follow this indicated line. Rather I shall try in a few minutes to deal with a more pretheoretical problem, the general significance of the concept of sensory as related to perceptual and what the analysis may mean for the theoretical understanding of perceptual functions generally, with of course, overtones of specific meaning for the particular topic of form perception.

The general area of form perception embraces and touches a vast array of perceptual categories. We are here confronted not only with the general topic of form perception but such related areas as: figural after-effects, space perception, illusions, "figure and ground", grouping of units, "structuring", interaction of the senses, and many other topics.

I shall not go into the general problem of the kinds of conceptually descriptive functions applicable to experiments in the different topics, for I have discussed this topic in other places. It is sufficient to say that all such functions are specific cases of a general one that specifies response as a function of stimulus aspects, specifiable conditions of the organism (including instructions), time and number of presentations. In a word, the data of perception can be systematized within the general scheme of this relation. In this context perception is the class name for sets of

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relations expressible by a general equation involving stimulus, response and conditions of the subject. In the history of the study of perceptual discrimination, the two types of variables that have played predominant roles in experiments are those specified as: (1) stimulus parameters taken with conditions of the organism constant and (2) conditions of the organism taken with stimulus parameters constant.

What, then, is meant by the term sensory at least as it relates to form perception? A useful answer might be given by a considerable number of people who would say that the basic sensory problem in form perception is the analysis of cues. If one accepts this statement one could quickly become involved in a reductionistic analysis tending very quickly toward the general problem of the physiology of form perception. I find the understanding of the physiological variables (not necessarily physiology) of form perception to be desirable and theoretically of great importance. In some ways it is the modern development of inner psychophysics. On the other hand, I do not underestimate the psychophysical approach, the modern successor to outer psychophysics and inheritor of the general name. The analysis of cues is limited in the case of psychophysicists to careful specification of stimulus conditions for the determination of given responses. The history of an important problem in form perception is of interest here, because it provides an example of interplay between psychophysics and physiological theory.

A description of contour was given in good approximate form almost a hundred years ago by Mach when he specified that a contour occurs when the gradient of an intensity distribution of light shows a sudden change in space. In these terms Mach specified the nature of a contour in terms of the second derivative of the spatial distribution of intensity. From the psychophysical point this formalistic description (which Mach changed when he had to) had some advantages and allowed a first approximate description of a cue, that is, the essential stimulus condition for a given response. Later on it became obvious that this generalization was related to the problem of intensity discrimination concerning which a number of approximate descriptions (in terms, for example, of photochemical processes) are available. In the latter case, the stimulus-response correlation became analyzed further into a reductionistic scheme. It is obvious that this sort of analysis applied to many of the phenomena of perception must involve all manner of chemical and nervous components as well as interaction effects. I could talk about specific examples of these variables but do not feel that a simple multiplication of instances is profitable.

When we think of sensory analysis, we think of the elements that are said to be the products of the analysis. What is elementary about the stimulus component of the psychophysicist or the physiological processes of the psychophysiologist? For that matter, what was elementary about the sensations of the early psychologist?

First, consider the problem from the point of view of the psychophysicist. A cue variable is specified when experiment shows the stimulus conditions that are required for a given response. Can it be shown, we may ask, that one specified stimulus cue is more elementary than another for the production of a given class of response? The answer to this question is, of course, a theoretical one. The subject does not give the answer; it is the experimenter's and theorist's statement that gives a basis for making the judgment. It often happens when the account is available that we are no longer interested in the question because we have something better, a relation.

The interpretation as to elementary products of analysis that holds for the psychophysicist holds also, but in more specific detail, for the psychophysiologist. His specification of cues is similar to that of the psychophysicist; it is only in terms of theoretical physiological considerations (augmented when need be by experimental physiological observations on preparations) that he gives a process account for the intact organism. Judgments as to comparative elementary aspects of parts of the account have no significance for other than classificatory purpose. Finally, it should be mentioned that the situation as regards sensation in an earlier day was no different from the one that holds in the case of analytic elements today. Titchener, for example, did not claim that sensations were observed. They resulted from logical analysis, theory if you wish, applied to perception.

This discussion of elements indicates that operationally and with respect to experimental procedures and outcomes the presumed differences between a sensory discrimination and perceptual discrimination is meaningless. It is, however, a fact that within the general body of perceptual relations, different theoretical representational schemes exist, involving different levels and numbers of sub-representations; in a word, different kinds and degrees of theory are interspersed among the modal subject matters of perception, form perception in particular. These theories are parts of what will eventually become an inclusive network of relations that describe and explain the field of perception. Included in these connecting relations are some that, due to historical factors, have received the term sensory. The continued use of the term can be justified only as a matter of convenience, not substantive content.

These are my opinions concerning the interrelations of sensory and perceptual processes and I should like to make a plea concerning the general study of perception. Specifically, it is this: since sensory theory is an integral part of perceptual theory, then let us hope that the future will see the integration of subject matters and theory within the same array of connections. To disregard the so-called sensory in a consideration of perception can only be done at the cost of relevant information. The consequence may be possibly bad, because uninformed, experimentation. Examples of the error in the recent literature are not far to seek. Their disorganizing effects may provide areas of confusion for years to come.

THE SIGNIFICANCE OF FORM DISCRIMINATION FOR PSYCHOLOGY IN GENERAL

Olin W. Smith

Dr. Taylor suggested that I comment briefly upon the significance of form discrimination for psychology in general. In doing so I am going to depart from the traditional concepts of form discrimination to include three-dimensional as well as the customary two-dimensional forms which are usually investigated. This departure from tradition seems warranted since the scope of the subject is greater than that covered by the usual areas of research.

For purposes of discussion, I shall discuss form as the describable dimensions of a visually discriminable object. The logical reduction of this position is that if a point is discriminable as an object, it is describable as having form.

In my opinion, there are several key questions of importance for psychology. The first question will be discussed only because the answer has been assumed implicitly, but seldom asserted. It is: is form discrimination necessary for survival of man as a species? I would answer this with an unqualified Yes. If we assume that vision is necessary for survival of the species, then a visual field which reflects no discriminable silhouettes is, by definition, a homogeneous field. It provides insufficient stimulation for survival behavior. Moreover, discriminable forms are a pre-requisite for absolute judgments of distance. Here we assume that the behavioral space that is necessary for the survival of our species is the scalable space of absolute judgments. Examples of acts based on absolute judgments of distance are the leaping of a ditch, the final pressure on the brake at a stop light, etc. The space for these acts is normally determined by discriminable forms having a retiform distribution. In this space the errors of judgment in locomotion are determined in part by judgments of distance which in turn are dependent upon judgments of forms and their pattern. From this we are willing to conclude that the discrimination of three-dimensional form is necessary for survival, as well as is discrimination of silhouettes or outlines.

At this point a distinction should be made between the two- and three-dimensional attributes of objects which are necessary for the correct discrimination and identification of forms. It has generally been conceded that the silhouette of an object must be discriminated in order for the form to be correctly identified. This is not as all-inclusive as might be supposed, since there are classes of forms which are incorrectly identified when the outline is the only basis for discrimination. For instance, a sphere is judged to be a disk when the lighting of its surface is unfavorable for the discrimination of differences in depth of adjoining parts of its surface. Similarly, a cylinder is judged to be a flat rectangular surface until the differences in distance of its surface are made available for discrimination. We shall return to this problem later in this paper. These examples prove only that the silhouette is not necessarily sufficient for the correct discrimination and identification of form.

We have seen that the visual discrimination of form is necessary for survival of man. It also determines at least, in part, other survival activities such as learning, food seeking, and reproduction. For these reasons I judge it to be of crucial importance as a topic for psychological investigation. However, this evaluation is only my biased opinion. It might be preferable to determine whether past psychologists have considered it to be equally important. The answer to this question is less readily given.

Historically, the form problem has been marked by three major periods, each of which was followed by a major revolution. The earliest school was created by the Italian artists of the Renaissance. They sought to show how the patchwork of colors and lights could be unified by means of the optical geometry of perspective so that two-dimensional representations would reflect ray sheaves to the eye which would approximate those of the model. They understood the effects of brightness and color contrasts, of shadow, and of perspective. They gave to psychology the original list of the monocular cues to distance as well as original notions for form discrimination.

The first revolution was initiated and maintained by philosophers and psychologists who concerned themselves with the problems of the origins of perceived space and form. This revolution was started by Bishop Berkeley in 1709 and continued through Helmholtz into the twentieth century with Titchener. This was the era of the associationist, whose ideas were punctuated by sensations, mental chemistry, and local signs.

This setting led to the second great revolution, which was sparked by the leaders of the Gestalt school in the second decade of this century. The Gestalters were concerned with the totality of impression as revealed by the phenomenal report of the observer. Their subject of investigation was both static and moving forms. Their efforts resulted in a plethora of "laws" of organization for nearly any describable form. Practically, of course, their laws are, with but few exceptions, merely Monday morning quarterbacking. Their lasting contribution of great value is found in the initial formulation of problems which are as yet unsolved.

The third revolution has been in progress for only a short time. This revolution originated not as a reaction against any specific theoretical or philosophical school, but as an attempt to cope with urgent practical problems. The evidence for this revolution is found in the demands for independent criteria for forms. These have included (1) the problem of the fidelity of representation on the radar screen, (2) the problem of veridical versus schematic representations of space, and (3) the form to be taken by lights for optimal safety in landing aircraft, etc. As a result of these challenges, psychologists have attacked the problem of objectification of forms which is still in its infancy. The products of objectification hold much promise for the future since they make possible the measurement of forms as well as their generation. These will be the measurement devices which are so urgently needed at present for both practice and accompanying theory.

Studies of form prior to the third revolution have been restricted, for the most part, to two-dimensional geometrical illusions such as the Müller-Lyer, to transparent or reversible forms such as the Necker cube, and to silhouettes of solid figures which may or may not be reversible, i.e., Rubin's vase. The empirical results of these studies have contributed little to the understanding of form.

It seems clear to me that in the past our colleagues have had a continued interest in the problem of form. Their most productive results for present-day purposes seem to have been in suggesting problems. It is evident that the process of measurement of form, its objectification, is going to be the big break-through for the study not only of silhouettes, but of three-dimensional forms and their combinations which constitute space as we know it.

The third question I should like to pose is one which I have already implied: is form discrimination independent of space discrimination? It has traditionally been considered as a separate problem, but at present it is no longer feasible for theory to treat them as separate. One reason is that plane surface projections of form have already proved adequate for motor tasks such as the accurate tossing of balls at targets in pictured spaces. This involves very complicated problems of object identification, space identification, as well as judgment based on the scale of the projection. Accordingly, I wish to propose the argument that form discrimination in general cannot be understood independently of distance discrimination, and vice versa. For many specific problems, this would be an unnecessarily complicated and an unworkable approach, since their solution will undoubtedly be reached independent of the larger theoretical objective. Nevertheless, theories of these problems must be inextricably linked. In the first place, as I have discussed earlier, the perception or judgment of distance is dependent upon the existence of forms in the visual field. In the second place, the judged dimensions of a form may depend upon the different distances at which parts of the form are judged to be located. This is the case even when the form is judged to be on a plane surface. Here the surface is actually not on the horopter but at varying distances from the eye, which situation is implicit in the judgment of a plane. The form would have other dimensions if judged to be located on a different plane, or if three-dimensional.

The judgment of the position of a plane surface at any degree of slant or tilt can be based upon judgments of rate of change of distance from the observer of adjacent portions of the surface. Similarly, curved surfaces, as previously noted, can be judged by means of discriminable differences in rates of change of the distance of adjacent portions from each other or from the observer, whose position gives a convenient zero point.

This logic was applied to a series of unpublished observations on judgments of the degree of curvature of a perfect demi-cylinder. The cylinder was mounted in a frame with its central axis horizontal and perpendicular to O's line of regard. It was illuminated evenly so that when the surface was covered by a plain white paper jacket, the light reflected to the eye from the surface was homogeneous and without shadow. The lateral edges of the cylinder were obscured, as was any view of the apparatus or its immediate environment except for shielding drapes.

Under these conditions, O saw a plain white rectangular surface against a gray field. The surface appeared to be completely flat and even when the demi-cylinder was rotated and the gray background was exposed, no curvature was reported but rather that a gray curtain appeared to wipe out the white area.

The purpose of the experiment was to determine which of the traditional cues or variables of distance perception, or what combinations of these, were either necessary or sufficient for differences in perceived distance on the surface. The criteria for perceived differences in distance were judgments of form. These were made by matching the perceived form to one of a series or scale of fully exposed forms which varied through degrees of concavity to flat and then through degrees of convexity until elliptical forms were reached. Various patterns with varying numbers of discriminable forms were placed on the surface of the cylinder. All patterns were viewed both monocularly and binocularly, and from several distances. We tested most of the traditional "cues" or variables, and found no one to be necessary for judgments of curvature. Similarly, no one cue or variable was sufficient. When we combined cues, their effects were not additive in any simple fashion. The effects of each differed according to the context in which it was observed. This suggested that interaction effects were the important variables, as a later experiment demonstrated.

These observations indicated that, although no single cue was necessary for judgments of curvature, the presence of discriminable forms at a number of different locations on the surface <u>was</u> necessary. From these observations we also concluded that differences in distance on the surface of the cylinder had to be discriminated in order for veridical judgments of curvature to be obtained. These judgments were necessary in order for the true form of the cylinder to be judged as curved rather than flat. Thus, discriminable form proved to be necessary for the discrimination of distance which was necessary for the determination of three-dimensional form, illustrating the inextricable linkage of the problems.

In summary, the perception of form is necessary for survival of man as a species. Psychologists have considered the problem from many points of view in the past, demonstrating their concern with its importance, but it is only in recent years that the need for objectification of form has become evident. Similarly, it is only recently that form discrimination has ceased to be an isolated problem, but has become a part of the complex of space perception. I have not even mentioned its further linkage to the problems of motion, which psychologists such as my colleagues Drs.

Gibson and Hochberg at Cornell and I are now considering. We have high hopes that an integrated approach to these problems will emerge from our efforts.

THREE INFERRED FACTORS IN THE VISUAL RECOGNITION OF BINARY TARGETS*

E. Rae Harcum

The problem considered in this paper concerns the extent to which tachistoscopic identification of "supra-detection" targets is equivalent for the various meridians of the visual field. A retinal meridian is defined as the array of visual receptors stimulated by a linear target on a frontal plane, passing through the point of fixation.

The published data indicate that the retinal meridian mediating best visual performance varies somewhat with the specific task. There is, however, some evidence on which to base predictions about supra-detection pattern thresholds along the various retinal meridians. Many experiments, such as those by Leibowitz(1) and by Higgins and Stultz(2), have reported visual acuity for circular line grids to be higher for horizontal or vertical line inclinations than for diagonal ones. These so-called "retinal astigmatism" results have led directly to the present problem, since they do not conform to data obtained from sensitivity scans along the various meridians. The line grid may be a special case in that individual lines extend for relatively greater distances along meridians than, for example, Landoldt C acuity targets or small circular spots used for contrast sensitivity thresholds. Perhaps some sort of spatial correlation or informational analysis propensities are operative along the meridians. The former has been suggested by Kristofferson and Blackwell(3) relative to the detection of rectangular forms. The latter has been suggested by Attneave(4) as important in contour perception.

However, assuming dot pattern identification to be closely related to reading experience, best performance would be predicted for the horizontal meridian and poorest for the vertical meridian. For example, bilingual Chinese students were found by Chen and Carr(5) to read and mark through Chinese characters more accurately and faster when the material was arranged vertically than when it was arranged horizontally. The reverse was true for English letters and Arabic numerals. Exception to these results was directly related to length of residence in the United States. Aulhorn(6), investigating reading speed of German text and letters

*Supported by Department of the Army Contract DA-36-039-SC52634 with the Engineering Research Institute of the University of Michigan, operating under a Tri-Service Charter, administered by the U. S. Signal Corps.

as a function of the angular rotation from horizontal, found that reading time increased with clockwise angular rotation to about 135°. It then decreased slightly for 180° rotation (i.e., upside down and right to left).

Two other studies, which may be relevant here, employed a task in which an observer was searching for a linear target pattern in a complex visual field. Shetler, Berbert, and Finney(7) report that if a visual field be randomly filled with dots and a linear dot pattern is presented through the diameter of the field, then horizontal arrays are seen better than vertical or either diagonal. In an experiment by Knight(8) observers were asked to look for a horizontal, vertical, or diagonal column of figures, letters, or digits imbedded in a field of similar elements. Horizontal patterns generally required less time to locate than vertical ones. Neither diagonal was seen as quickly as horizontal or vertical and diagonal targets sloping up to the right were not seen as quickly as those sloping up to the left.

The above data from several visual tasks indicate differential visual performance for linear target patterns in different directions from fixation. However, one cannot be confident of predicting which meridian will mediate best binary pattern recognition. Results obtained on the present task would not necessarily conform to retinal sensitivity data. The specific visual task under consideration requires the observer to reproduce complex tachistoscopic targets, the elements of which, if exposed by themselves or for long durations, would be above detection thresholds. These linear targets of binary elements are similar to those often used in information-assimilation or in perceptual span experiments. Therefore, the task being investigated measures the extent to which the visual system can assimilate information exposed momentarily along different meridians. The present task has relevance to both visibility and visual display problems. Although the experiment to be reported here was exploratory, the results obtained have been verified by a subsequent experiment employing additional experimental controls.

The targets were presented binocularly in a Gerbrands tachistoscope. Background luminance, provided by flourescent flash lamps, was approximately 5 foot-lamberts. The target template consisted of nine typewritten zeros arranged in a straight line and separated by single typewriter spaces. Twenty individual patterns were formed by filling in certain of the zeros with the typewritten number sign (#).

The center zero in the target registered with the fixation point. Each experimental session of 80 exposures contained all patterns at each inclination. An error of reproduction was defined as either filling in a zero that was unfilled in the target pattern, or not filling a zero that was filled. In Experiment I nine observers each completed four sessions with the target exposed for .2 seconds. The observers were told in advance the target inclination before presentation. In Experiment II four new observers did not know target inclination prior to the .25 second exposure. Each completed only one session. (In discussing the following results the terms target inclination and retinal meridian will be used synonymously.)

Mean errors per target presentation in Experiment I for the four meridians are shown in Figure 1, arranged by sessions. There is a slight increase in errors as the pattern is rotated in 45° steps clockwise from W-E. These meridional differences are not significant. Virtually identical conclusions with known and with unknown target inclination were found in the repeat experiment with ten naive observers. However, with ten practiced observers the identical results were statistically significant for both known and unknown pattern inclination. It is interesting to note that the least practice effect occurs for the W-E meridian, which presumably is most practiced by reading English.

Figure 2 shows the summarized results for both experiments. The W-E meridian shows fewest and the NE-SW meridian shows most errors in Experiment I. The curve is virtually flat for Experiment II. Absolute error values are not comparable in the two experiments, due to the observer and exposure duration differences.

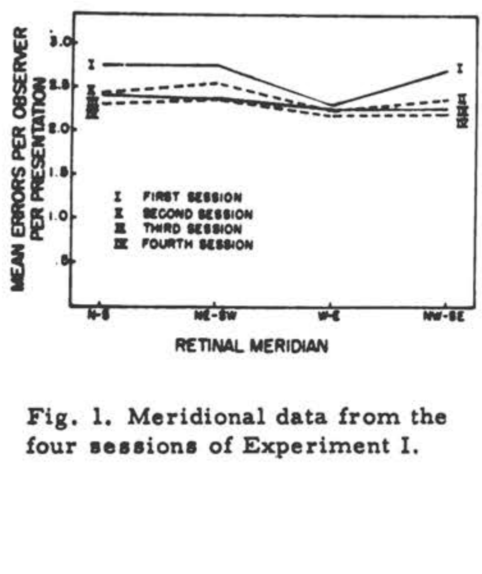

Fig. 1. Meridional data from the four sessions of Experiment I.

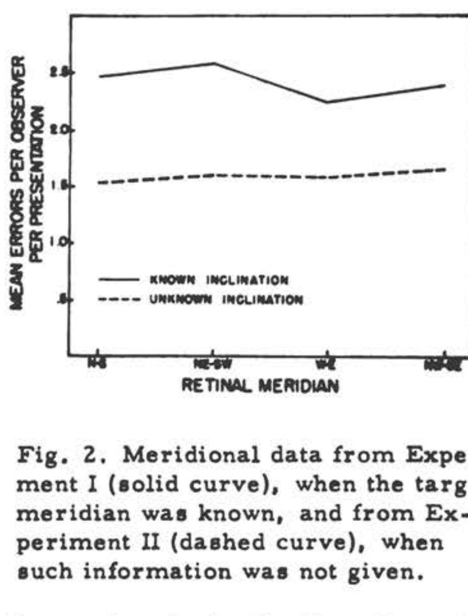

Fig. 2. Meridional data from Experiment I (solid curve), when the target meridian was known, and from Experiment II (dashed curve), when such information was not given.

Figure 3 shows the data for both experiments for the four elements on either side of center (i.e., half-meridians). Half-meridians are designated by compass points. Again absolute values are not comparable in the two experiments. However, both curves appear nearly sinusoidal. The small meridional differences apparently masked much larger half-meridional differences. Generally, a small number of errors on one half-meridian is balanced by a large number of errors on the opposite half-meridian. Obviously, some one meridian must show approximately equal errors on the two half-meridians. The meridian showing maximum difference between opposite half-meridians will be at right angles to this "null" meridian. The rest of this paper will attempt to explain why a given half-meridian shows maximum, minimum, or intermediate mean errors.

The two curves in this figure appear slightly displaced horizontally with respect to one another. This difference may be real, but is undoubtedly influenced by observer sample. This point can be proved by reference to Figure 4, representing individual half-meridional data from Experiment I. The grouped data mask large observer differences relative to location of the maximum and minimum mean error half-meridians. All observers show a group of adjacent half-meridians in which the number of errors is relatively greater, and another group of half-meridians on the opposite side of fixation in which errors are relatively fewer. Those half-meridians with greater errors are most apt to be upper-left of fixation.

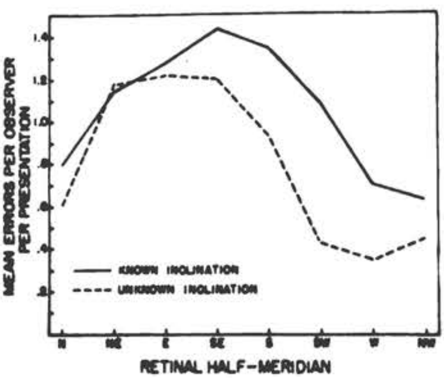

Fig. 3. Half-meridional data from Experiment I (solid curve), involving knowledge of target meridian and from Experiment II (dashed curve), in which such knowledge was not given.

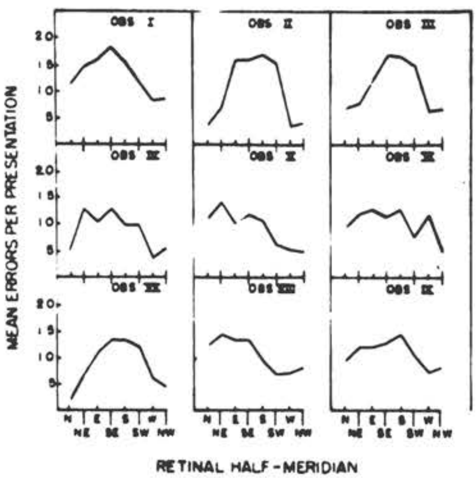

Fig. 4. Individual half-meridional data from Experiment I.

Figure 5 shows mean errors made on each target element in Experiment I. The relations among half-meridians are remarkably consistent for the individual elements. The decrease in errors at element separation four from fixation is evidence that the present task is not limited by element detection. It is presumably due to an "end segregation" phenomenon in which elements near the beginning or end of the targets stand out due to their unique position within the target. Therefore, end segregation is the first supra-detection factor to be demonstrated. It should be noted from this figure that NE and SW are the only halves of the same meridian whose data points are virtually superimposed.

Figure 6 shows the same data as Figure 5 plotted such that the central element is flanked appropriately by the other elements. This plot illustrates a "primacy" effect seen in all meridians except SW-NE. The primacy effect refers to the progressive increase in mean errors per element position as one proceeds from one end of the target to the opposite end. There appears to be some set or concentration of attention favoring certain half-meridians at the expense of the opposite ones. This is the second supra-detection factor to be mentioned. Such a phenomenon has already been found in two other areas of psychological research. Ebbinghaus(9) found primacy as well as end segregation in rote memorizing series of nonsense syllables. Both factors are also seen in perceptual span experiments as, for example, in an experiment by Crosland(10) using unrelated letters.

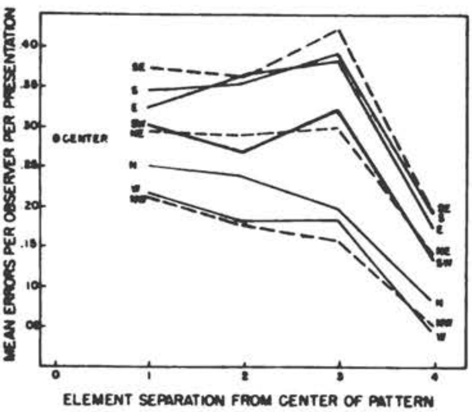

Fig. 5. Element position data from Experiment I, all plotted on one side of center.

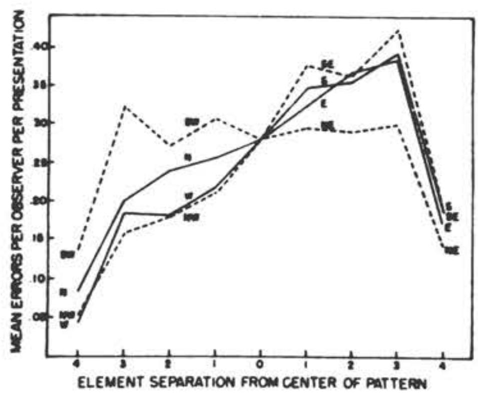

Fig. 6. Element position data from Experiment I, both sides of center.

The third supra-detection factor to be inferred from the present data determines which half-meridians are favored and which are not favored by primacy. This factor is called here "consistency of scan". The scan in psychological terminology might be considered as a sweep of attention. On the physiological level it might refer to temporal-spatial sequences of heightened sensitivity across the retina. The term consistency refers to the reliability with which the scan proceeds in a given direction. For example, the meridian in which errors per opposite half-meridian are nearly equal would have a low consistency of scan in that the sweep would be equally likely to start at either end and proceed to the opposite end, so that the primacy effects would cancel. A meridian with highest scan consistency would exhibit maximal differences in mean errors on the opposite half-meridians because one half-meridian would be much more often favored by primacy than the opposite half-meridian. A given observer in a given experimental situation will exhibit one meridian with highest consistency of scan. The meridians on either side of this meridian will have somewhat less consistency. Consistency will continue to decrease for meridians at greater angles to the highest-consistency meridian according to some probability function, until the perpendicular meridian is reached. This meridian will have no consistency.

The cause of consistency of scan remains to be considered. It is hypothesized that each observer on a given visual task subconsciously selects a reference point in the visual field from which the scan proceeds. This point would be most advantageous for the specific task, or one most advantageous for similar tasks with which there has been much experience. Presumably for the present task, practice in reading English would be an important determiner of direction of scan. Since English reads consistently from left to right and from top to bottom, consistency of scan should be greatest for the W-E or NW-SE meridians. Since the NE-SW meridian might be presumed for the observers in Experiment I to have conflicting left-right versus up-down scan components, it is reasonable to expect minimal differences between northeast and southwest half-meridians compared to the opposite halves of other meridians. Also, the meridian should and does result in the largest total number of errors because of the conflicting directional scan tendencies.

The present data agreed reasonably well with the results of Knight. To account for his data, Knight hypothesized "systems of tentative responses" which are built up as a result of experience. Maintaining that sensory response cannot be divorced from learning, Knight felt that the primacy of horizontal and vertical over the diagonals was due to the learning of sets favoring horizontal and vertical movements.

Functional physiological differences in the visual system may, also, determine scan consistency. For example, right to left scan sequences may be easier for some observers in spite of practice with any task. Or the scan may proceed from retinal areas served by the dominant to those projecting to the non-dominant cerebral hemisphere.

In the present study there was no way of determining the actual point of fixation before a target was presented. Also, the exposure time was not rapid enough to rule out with absolute certainty the possibility of eye movements after stimulus presentation. Since the data with known and unknown inclination are quite similar to one another, however, the observers probably did not fixate differently for the four inclinations. The repetition of this study gave identical ranking of the meridians in terms of mean errors, and extremely similar half-meridian results for both known and unknown target inclinations with an exposure duration of .1 second, which is less than the latency for voluntary eye movements.

Evidence that conscious set of the observer is extremely important for the identification of the patterns is found in an incidental observation made during the course of Experiment I. When the experimenter inadvertently told the observer that an inclination different from what was actually presented was going to be presented, the observer invariably was not able to report the pattern at all. Knight, also, found that observers were unlikely to see a particular target configuration if they had been instructed to look for a different one. In the repetition of this study using naive observers, there was an advantage of known over unknown inclinations, but this advantage was not shown by practiced observers.

SESSION II

SOME TECHNIQUES FOR INVESTIGATING FORM DISCRIMINATION

Chairman: Robert M. Boynton

BOYNTON: The first year I was in graduate school at Brown University I had a seminar with the late Dr. Walter Hunter. Dr. Hunter was very fond of saying that the results of an experiment are never any better than the methods that were used to deliver them. I've never had any reason to doubt this statement since, and what we're going to be dealing with in this session is primarily methodology. I hope we'll have time for some discussion.

TOWARD A PSYCHOPHYSICS OF FORM*

Malcolm D. Arnoult

Perhaps the greatest single barrier to the development of systematic knowledge about form perception is the difficulty involved in describing the stimulus. There is a limited number of forms which have names agreed upon by most people - geometrical forms, which are characterized by certain mathematical regularities in their contours, and "meaningful objects", which may or may not have universal form characteristics - but these classes of forms are only a small fraction of the infinite variety of forms which can exist. We also have other ways of describing forms - angular forms, curved forms, mixed types, ink-blot forms, etc. - but these classifications are far too gross and non-quantitative to serve as the basis for a scientifically useful taxonomy of shapes.

Our inability to describe form stimuli adequately has as its primary consequence the fact that we are severely limited in the kinds of generalizations we can make from experimental results. If, in a given experiment, we chose properly the people who serve as subjects, we can generalize our results to other people, and we can specify a degree of confidence in the generalization which depends upon the "significance" of

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*This research was carried out at the Operator Laboratory, Air Force Personnel and Training Research Center, Lackland Air Force Base, Texas, in support of Project 7706, Task 27001. Permission is granted for reproduction, translation, publication, use, and disposal in whole or in part by or for the United States Government.

the results. We cannot, however, specify other stimuli to which the results are applicable because, in the typical experiment of this sort, the stimuli were selected arbitrarily and consequently represent a non-random sample from a population having unknown parameters. At the same time, we are rarely content, as scientists, to limit our conclusions to the particular stimuli we happened to use, even in applied research.

In a recent paper(1), Attneave and I suggested that this problem might profitably be broken into two parts. The first part is what we have called the specification of the stimulus-domain. The stimulus-domain is a population of forms which has determinate statistical parameters and which may be randomly sampled. We described a variety of such clearly defined hypothetical populations and indicated ways of drawing "random" samples of forms from them. The advantage of using stimuli of this sort is that the results of any experiment in which they are used can be generalized with a specifiable degree of confidence to the entire parent population; that is, the results presumably could be replicated in a similar experiment which used a different sample of stimuli from the same stimulus-domain.

Such a procedure for generating "nonsense" forms for experimental purposes is clearly better than the methods heretofore available, but it still does not solve the second part of the problem. We ordinarily are not interested in nonsense forms as such. What we really need are sets of nonsense forms which are representative of sets of natural forms occurring in our visual world. In order to do this we need to know the psychologically important statistical parameters of natural forms in order to construct stimulus-domains having the same parameters. We must find quantitative measures which isolate that set of psychological properties which we call form; these measures, like form itself, should be invariant under transformations of size, place, color, brightness, orientation, and, perhaps, projection. They will be of two kinds: first, those measures which describe a form in terms of a set of numbers and operations, from which the form may be reconstructed by reversing the analytical process; and, second, those measures which describe in quantitative language what we have called the Gestalt properties of forms, i.e., important properties of the form as a whole. The first class includes the various techniques for contour analysis, while the second includes such measures as angular variability, symmetry, dispersion, and jaggedness, all of which may be given quantitative expression.

The purpose of this paper is to outline some of the ways in which these kinds of measures may be utilized in the development of a psychophysics of form. It is standard procedure, of course, in perceptual research for the units of variation of the independent variable to be psychophysical units rather than physical units. We have, therefore, measures such as the sone, the veg, and the gust, all of which were developed so that stimuli could be varied in terms of specifiable psychological units. What sort of comparable scale could be developed for the variable of form? In the examples cited, the physical variation and the psychological variation are both unidimensional. What kind of psychophysics is required when both the physical and judgmental sides of the relationship are multidimensional? It may be that some day a general mathematical solution to this problem will be developed; meanwhile, it would appear that the best approach is to develop separately a psychophysical description for each of the discriminable aspects of forms. We would like to have psychophysical scales for complexity, meaningfulness, size, closure, beauty, jaggedness, and coherence, to name just a few.

Implicit in such a program is the assumption that these and similar judgments about the attributes of forms are determined wholly, or at least very largely, by the physical characteristics of the forms. For every psychological dimension along which forms may be ordered there is assumed to exist a measurable aspect of physical structure which shows a corresponding variation. The physical measure may be simple or complicated, it may be single or multiple, it may be obvious or obscure; nevertheless, it is assumed that there is some physical measure or combination of measures which will account for the major part of the variance in the judgments. In the case of size judgments, we have preliminary evidence that about 90% of the variance of the judgments can be accounted for by measuring the area of the stimulus, with virtually none of the variance being attributable to measures of characteristics such as compactness or jaggedness. If these results hold up, i.e., if the same simple relationship is found in other samples of form stimuli, then it will be possible in the future to use size and area in the same way that brightness and intensity are now used. The attribute of complexity, on the other hand, is an example of a psychological dimension which can be accounted for equally well, but in a more complicated way. In a recent study(2), Attneave showed that about 90% of the variance in judgments of complexity could be accounted for by a weighted combination of the following measures: the number of sides of the form, symmetry, angular variability, and P^2/A (the ratio of the square of the perimeter to the area). As before, these results need to be replicated before we can proceed with confidence (at the moment there is some doubt whether the number of sides or the logarithm of the number of sides is the better physical measure), but it would appear that complexity may be satisfactorily accounted for in terms of the structural characteristics of stimuli alone.

The major gain resulting from the psychophysical specification of an attribute such as complexity lies in the increased generalizability of research results. Suppose we are interested in studying the relation between stimulus complexity and difficulty of discrimination. One way of defining complexity is in arbitrary physical terms; we may equate complexity with number of parts, or number of sides. The results we get will be generalizable to all other stimuli whose parts or sides are countable, but the results may not make psychological sense. For example, we would be sure to find that a star, which has ten sides, is easier to discriminate than an irregular polygon having only five sides. On the other hand, we may avoid the problem of higher-order invariances in physical structure by having our stimuli rated for complexity before the experiment begins. Our results are now pretty sure to make psychological sense, but we would be hard put to predict the results we would obtain with a new sample of stimuli. By utilizing the psychophysical relationship demonstrated by

Attneave, though, it is possible to construct a sample of stimuli having known physical characteristics and known complexity; furthermore, it is possible to generate independent sets having equal complexity. We have the advantage of using stimuli which are scaled along a psychological dimension without losing the advantage of generalizing the results along a physical scale.

It would be premature, of course, to assert that a multidimensional psychophysics of form has been developed. Many difficult problems remain to be resolved before we can say that this approach will work. One of the most serious of these problems has to do with attributes such as meaningfulness and familiarity. It has been shown that the ratings given to stimuli on these attributes are highly related to the amount of past experience the subjects have had with the forms(3). What, then, of the assumption that the ratings can be accounted for on the basis of structural characteristics alone? The current hypothesis is that meaningful objects, such as the natural objects of our visual world, demonstrate statistical regularities in their structure, and that all objects displaying those same structural regularities will be judged to be meaningful, even when the factor of past experience has been controlled. On the basis of preliminary data it can be said that meaningfulness and familiarity are significantly related to such measures as compactness, angular variability, and number of sides. Whether these measures or others will account for a sizeable proportion of the variance of judgments of meaningfulness and familiarity remains to be seen.

Another problem which affects the usefulness of this type of psychophysical approach has to do with the reliability of the ratings of stimulus attributes. So far we have evidence on this point only in the case of complexity ratings. Reliabilities have been computed under the following circumstances: (a) the same stimuli scaled by subject samples differing greatly in background and educational level; (b) a subsample of stimuli scaled while embedded in two kinds of larger stimulus populations; (c) the same stimuli scaled by seven-category and by five-category rating scales; and (d) the same stimuli scaled both by a rating method and by paired-comparisons. In all these comparisons the reliability coefficients have ranged between .93 and .97. Similar comparisons are not yet available for other stimulus attributes.

A related problem is that of determining the reliability of the obtained psychophysical relationship. If a new sample of stimuli is randomly drawn from the same stimulus-domain, will the physical measures previously identified be adequate to account for the judged complexity of the new sample? As yet no data are available to provide an answer to this question. Finally, there is the problem of evaluating the generality of the psychophysical relation. How well will the physical measures which describe a given attribute for one stimulus-domain account for that same attribute for stimuli from another stimulus-domain? Ideally, we would want to have a predictive equation of the form:

$$\text{Complexity} = f\,(a + bx + cy + dz + \ldots + kn)$$

which would predict judged complexity for any imaginable stimulus. Unfortunately, we cannot predict what will be the total number of physical measures relevant to the judging of all attributes of all kinds of stimuli, nor can we know that we have constructed all possible kinds of stimuli until we have included all possible physical characteristics.

The development of a psychophysics of form still lies far in the future. I have tried to describe here a program of work which could result in such a development. Only the barest beginning has been made, but the preliminary results are encouraging. We have nibbled at the problem successfully. It remains to be seen how large a bite we can take without getting methodological dyspepsia.

PSYCHOLOGICAL FACTORS IN FORM DISCRIMINATION STUDIES

Robert B. Sleight

Quantification

In this day of growing emphasis on operations research there is every reason to pin the behavioral scientists to the wall and demand some answers in quantitative terms. It cannot continue to be sufficient to say that it is desirable to have highly motivated radar operators or pilots or photo interpreters. We need to go further and say that if such and such circumstances prevail the men will be motivated to such and such a level and the outcome will be a specified percent improvement.

In order to obtain quantitative data on military problems involving form perception, we must first understand clearly the nature and scope of our subject matter. One of the primary building blocks of form perception is visual acuity.

"Boy, I wish I'd said that." Such was my serious reaction when I recently came across the following succinct statement in Woodworth and Schlosberg's revised edition of Experimental Psychology.

> "We have come a long way from the simple classical theory which accepted visual acuity as a measure of the fineness of the retinal mosaic. We now know that visual acuity represents a dynamic interaction of many factors, ranging from purely physical spread of light, through interaction at retinal and higher levels, and finally involving the higher neural processes that are characteristic of perception." (Underlining by Sleight)

Let us take a more thorough look at this statement. For assistance I will call on the old reliable Dictionary of Psychology edited by Warren.

First, consider dynamic. This means "pertaining to the causes and effects of behavior and mental activities, often with special emphasis on motivation". Interaction we may define as "a relation between two units or systems of any sort, such that the activity of each is in part determined by the activity of the other". Physical is a term which, although common, warrants the note that it is "distinguished from the realm of mental phenomena". By retinal we simply mean "pertaining to the receptor organ for vision". Higher neural processes is not a phrase explicitly defined in Warren's Dictionary, but here we would assume that it refers to the "complex cognitive processes". And finally we come to perception by which is meant "a mental complex or integration which has sensory experiences as its core".

We have seen here what I feel is appropriate emphasis on the complex mental processes.

Even if we are willing to agree that this emphasis is reasonable, how do we go about incorporating it into form discrimination studies related to military problems?

A first step is to quantify these variables--both the physical and the mental.

The data for the influence of the physical environment factors are fairly well specified. For example we have much objective information on the effect of temperature, noise, and illumination. It should, however, be prepared in a form which can be included in computer analyses of military system effectiveness. Next we must obtain quantitative data on the higher neural, or cognitive, processes.

I know this is easier said than done, but as one who has been brought up short frequently by engineers and mathematicians, and who has had to provide some numbers, I am convinced it can be done. Admittedly the values are not as precise as the scientist would like, but they are better than qualitative statements. Sometimes the extrapolations may seem to be "going beyond the data"; but even a partially correct answer is far superior to no answer at all.

Field Conditions

How may we obtain quantitative descriptions of both the physical variables and those related to complex processes?

The solution may lie in maximizing realism. Wherever feasible experimentation should be conducted under actual field conditions. Admittedly this may take considerable planning and on first thought it may appear expensive. I believe, though, that most of us have seen instances where with the cooperation of the military people we could have joined in a field maneuver or participated in training. The services are getting very research conscious and cooperation is usually forthcoming even when "mind reading" psychologists are the investigators.

In peace time it is through the relatively large scale maneuvers and exercises that we can come closest to realistic conditions--high validity by definition is thus accomplished.

Simulators

Many of the military man-machine problems are becoming so complicated or futuristic that the only practical solution is simulation. Simulation studies will undoubtedly increase as system complexity continues to grow. We must then face the problem of how to incorporate the appropriate realism into the simulator programs. How do we ascertain what is appropriate realism?

I'm afraid we must go about it the hard way. We must continue to run well-planned experiments in order to determine cause and effect relationships. We will utilize to the fullest the findings of laboratory researches. We will want to look very critically at the literature on such factors as stress, motivation, attention, emotion, attitude, etc.

Also we may want to include these factors in our controlled simulator tests. These are the psychological factors, which although often spoken of, are still mysterious and only partially understood.

Even the more-or-less physical aspects of the military environment are not easy to specify. For example let us consider some of the multitudinous facets of pilot or observer environment aloft. Among them are illumination, color, noise, vibration, temperature, humidity, air movement, oxygen, and pressure. In a flight simulator how many of these items should be simulated?

Post-Session Interview

In both simulation and field studies, the post-session interview is a valuable tool. Such interviews can give information regarding the manner in which individual observers intepreted their instructions and thus may account for some of the variation in the data obtained as well as to point out ambiguities. Perhaps a more important function of the post-session interview is to gain insight into the mental processes used by the observers in responding to an experimental situation. This is most valuable in complicated problem solving situations in which the observer has to respond to a multivariate stimulus. One may frequently learn what elements of the stimulus are most salient, the order of responding to these elements and the interactions among the various aspects of the stimulus. For example, in the photo interpretation task, the observer is responding to a highly complex visual presentation in which the meaning of what he sees is in some way related to his training and experience. It may be possible through properly designed interviews to learn more about the relative significance of the target detail and of the associative detail in the image which makes possible correct identification.

Even though much of the data obtained from post-session interviews may be of a qualitative nature, it is likely that these data will indicate which of the quantified variables have had a significant influence on the observer.

Interpretation of Results

Results of psychological experiments, even simple sensory ones, are not easily interpreted. The problem of interpretation becomes more difficult with increasing complexity of the stimulus situation.

We would like to be able to say at the conclusion of a simulation study that the results indicate the probable success of the real system in that the desired military information will be obtainable from it. Before we can make a valid prediction of this nature, we must consider a number of aspects, some physical (in the equipment and in the environment) and some psychological, that have affected the nature of the data. In particular we will have to evaluate the ways in which the simulation program may have deviated from the conditions of actual operation of the completed system.

In Summary

Through quantitative statements of both physical and psychological factors we can arrive at the best prediction of man-machine performance.

PATTERN RECOGNITION: A PROBABILITY APPROACH*

Nancy S. Anderson

One of the most important problems in pattern recognition is to find an adequate specification of relevant stimulus characteristics. The history of earlier efforts to specify physical characteristics of stimuli has not resulted in any accepted classification system.

There is now an application of methods from information theory which lead to specification and control of the physical aspects of the stimuli(1, 2). This approach is probabilistic and makes the following assumptions: An individual's perceptual responses are determined on the stimulus side by both (1) the specific physical properties of a particular figure and (2) the characteristics of the population of figures from which the figure is chosen. It is the latter point which will be emphasized here.

*The concepts and research presented here originated in the Laboratory of Aviation Psychology at Ohio State University, through support from a contract between ONR and the OSU Research Foundation, Dr. P. M. Fitts, Project Supervisor, with contributions by Dr. O. S. Adams, Dr. J. A. Leonard, Dr. M. Rappaport, and Dr. M. Weinstein.

This approach has many advantages. For example, we rarely perceive an object as the exact same pattern of stimulation more than once. Thus with the probabilistic approach we can study form constancy by the study of response correlates of the population characteristics.

At a given moment, recognizing, naming and classifying are responses in which the particular object viewed is placed in a subclass belonging to a larger population of objects having similar characteristics. What we have done is to formulate certain of the population characteristics using information measures and studied the effect of these on perceptual responses.

First let us consider the hypothetical populations of patterns which could be utilized. One could investigate all possible forms constructed in all possible physical dimensions. A restriction on this class of forms might be to consider all forms in a two dimensional space. This space can be described by considering a two dimensional matrix of cells which are filled in by choosing from all possible colors and brightness levels. A further restriction might lead to all forms in a two dimensional matrix which is constructed by choosing cells of various brightness levels. The latter case has been adequately described by Attneave(1).

The population of forms we used, referred to from here on as <u>metric</u> figures, were restricted by the following rules. Consider a two dimensional n x n matrix. Starting from the left we blacken each column of the matrix by choosing a number corresponding to a column height ($\leq n$). The specific numbers for column heights are sampled from a table of random numbers. We can now define certain characteristics of this population of metric figures.

<u>Complexity</u>

In generating stimuli by a random process in a matrix, we define complexity as the average amount of information required to specify a figure, i.e., the degree of uncertainty existing in regard to a randomly generated figure before one sees it.

Consider the 4 x 4 matrix in Figure 1. One can generate N figures, where $N = r^c$, r = number of rows and c = the number of columns. Specifically, we can generate 4^4 or 256 or 2^8 figures which contain 8 bits of information, (information = $\log_2 N$). Consider the 8 x 8 matrix in Figure 1. Here we can generate 8^8 or 2^{24} figures which contain 24 bits of information.

<u>Redundancy</u>

The randomly generated set of figures above insures that on the average each detail occurs equally often and independent of every other detail. Any other sampling procedure generates fewer figures and by definition figures possessing a degree of relative redundancy, where redundancy is defined as

$$R = 1 - \frac{\text{actual information}}{\text{maximum information}}$$

With respect to the generation of metric figures, redundancy is a function of the degree of restriction imposed on the sampling of column heights.

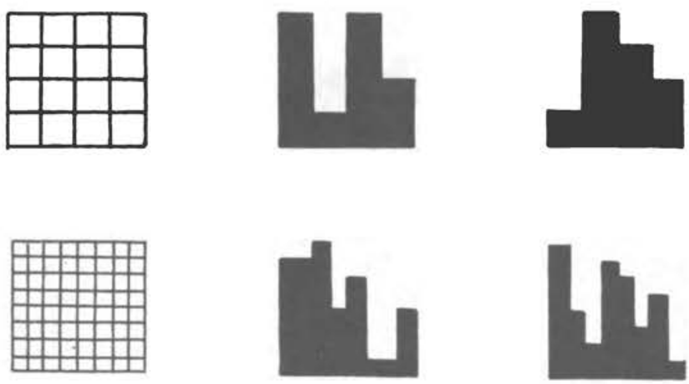

Fig. 1. Construction of metric figures. Two matrices, consisting of 4 x 4 and 8 x 8 cell units, are shown with illustrations of asymmetrical figures constructed by sampling contour details with replacement (see the two center patterns) and by sampling details without replacement (right hand patterns).

For example, column heights can be sampled at random with the restriction that once a given column height occurs in a figure, it cannot occur again in the same figure. This procedure corresponds to sampling without replacement (see Figure 1) and generates $N' = c!$ figures. In the case of the 4 x 4 figure, there are 24 figures generated by this procedure and this results in 42.75% redundancy. Using this approach we can study as many variations of redundancy as there are sampling restrictions.

Let us consider another way of introducing redundancy in the metric figures. If we generate part of a figure by a random process, we can generate another part of the figure defined by the first part. As an illustration in Figure 2, each figure is constructed in one half of the matrix and a corresponding figure in the other half of the matrix by (1) identical reproduction (the second part can occur under or next to the first part of the figure), (2) a mirror image or (3) a reciprocal code reproduction. All of these types of methods generate symmetrical figures with the same degree of relative redundancy.

We can now investigate not only the effects of redundancy *per se* but also the effects of introducing redundancy by different methods.

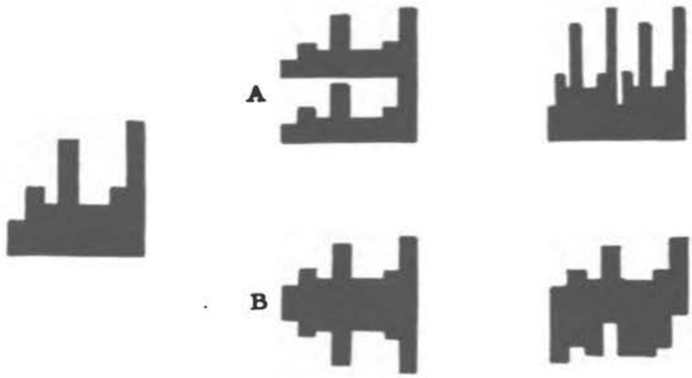

Fig. 2. Four types of redundancy. On the left is the asymmetrical figure. Row A contains figures showing redundancy as repetition of the figure contour (below or adjacent to the figure). Row B contains figures showing mirror image symmetry and reciprocal-code redundancy.

Noise

Given a signal, noise is defined as any source of perturbation that modifies the message during transmission. The techniques described above lend themselves readily to specification of noise effects. For example after a given figure has been generated, its contour can be distorted

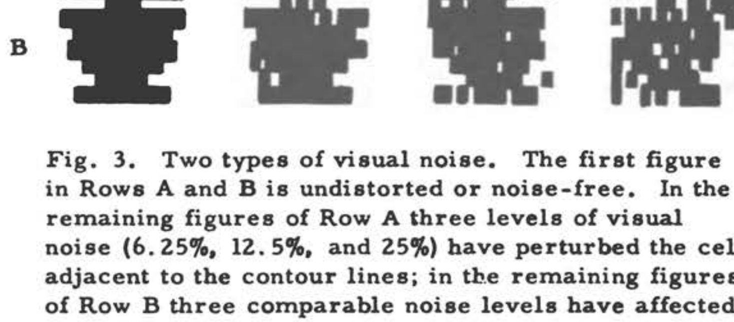

Fig. 3. Two types of visual noise. The first figure in Rows A and B is undistorted or noise-free. In the remaining figures of Row A three levels of visual noise (6.25%, 12.5%, and 25%) have perturbed the cells adjacent to the contour lines; in the remaining figures of Row B three comparable noise levels have affected all cells of the matrix. Noise level is here defined as the probability that the brightness of a given cell will be reversed.

at random by the addition or deletion of a unit cell. The level of noise, i.e., how many columns and by how many units, can be determined by specified laws of probability. This type of noise (see Figure 3, part A) can be considered analogous to amplitude modulated noise of auditory signals.

We can consider another method of introducing noise in which unit changes affect not just the contour of the figure but any cell within the figure or the remaining matrix background (see Figure 3, part B). For example, we can choose a noise source which would change some of the black cells of the figure into white and the white cells to black thus placing no restriction here with respect to what unit cells of the original matrix are altered by noise. Here too, we can study the effects of different types of noise. One can think of this as another way of approaching the classical problems of form constancy and the perception of shapes or forms masked by more or less complex forms.

There are several physical characteristics of figures which can be studied jointly with the more statistical parameters. For example, we can vary the size of the unit in a matrix or the area of the matrix, and the orientation of the figure. Results of the effects of these parameters together with the statistical parameters have been discussed by Rappaport(3) and Fitts, et al(2).

I would like to mention one word about methodology of investigating the population parameters. In all the experiments using metric figures, the subjects were to recognize a figure in a sample of figures chosen from a population with certain defined characteristics. We are interested in the ease of recognition or identification as a function of the population parameters.

Summary

A method is described for generating classes of figures that can be specified precisely in probability terms. The figures described are referred to as metric figures to emphasize their quantitative nature. The use of samples of these figures permits a systematic investigation of probabilistic stimulus parameters which characterize populations of figures. Some of the parameters which we have investigated are those of complexity, redundancy, and noise. We feel that this statistical approach to the study of stimulus correlates is supplemental to an approach that emphasizes characteristics peculiar to individual figures and that both kinds of variables can be investigated in the same experiment.

DISCUSSION

BLACKWELL: I'd like to comment on Dr. Arnoult's remarks about physical and psychophysical scales. I certainly agree that multidimensional scales are involved in form discrimination. There's no question of that. To turn your question to me back on you, let me hear again why you feel that the psychophysical scale necessarily would be more useful than the physical.

ARNOULT: Well, I tried to indicate in the example I gave that if you define a physical stimulus attribute only in the physical sense without reference to people's responses to it, you may come out with something that just does not make good psychological sense. I gave the example of a physical scale of complexity which would predict that a star would be more difficult to learn than a five-sided irregular figure. I think we all agreed that that's unlikely to occur even if the star were not a familiar object. It has a high order of invariance to its physical structure which is not measured by a simple counting of the number of sides. Now what I'm getting at is this: we need to do two things, particularly in the study of perceptual learning. We need to have some kind of objective measure which we can use as a basis for generalizing our results to other stimuli. At the same time, we need, as we do in all psychological studies, some psychological measure of stimulus parts and our stimulus in general, so that we can order them in a psychologically appropriate way. Now, either of those two things can be accomplished alone but they can only be accomplished together if you set up a psychophysical relation between the physical attributes of the shape and the judgments that are made about it. It is this psychophysical relation which you vary as your independent variable rather than either side of it alone.

BLACKWELL: I think that again the question becomes: Which works out better in practice? I suppose you are arguing for the use of the scale of subjective rather than physical intensity. Certainly, history has shown that this would get us nowhere in certain classes of visual problems. The physical scales prove most useful in detection work, for example. In my opinion the psychophysical scale has no use here whatsoever. We know, as a matter of fact, that it has reference to quite a different problem. If the physical scale were to certify the fact of a psychophysical correlate of it, as it does according to Stevens, at least, then I don't think I quite agree. It seems there were a few more facts. It would be just as wrong to take a psychometric scale in place of a physical scale when the physical scale obviously worked, as it produced meaningful order, as to do the thing which you speak of, which is the reverse. Now, it seems to me that in this case, without having the fact, you can't argue necessarily that one good scale is any better than another good scale. Clearly your example was loaded. If you took a physical variable, which would make nonsense, to illustrate the point that this is a bad scale, then by inference a psychophysical scale would be a good scale. Well, I'm sure that if you took a bad psychophysical scale, and we've got lots of them around, you could make equal nonsense. Again the only proof of the pudding is in the success of a scale in allowing you to generalize. I don't see any a priori reasons why a physical scale, or even maybe a neural correlate scale, isn't just as likely to succeed as your psychophysical scale.

ARNOULT: Well, I think we have got a thoroughgoing misunderstanding here and perhaps we should start over from scratch. Specifically, just as I rebelled a little bit this morning at what I detected to be a statement on your part to the effect that the method you proposed would be an exclusive method, likewise, I would not assert that mine is the exclusive method. I think it is a method. I think it shows some promise. I think perhaps it should be pushed as far as it can go. I will predict, and give you a thousand to one odds, that it will turn out ultimately to be inadequate.

GOTTSDANKER: I think the interesting thing of this discussion is that the contenders have completely shifted positions since it has started.

GILMORE: There has been quite a lot of discussion about the problem of describing, say, two-dimensional forms. It seems to me that the wave number characteristics of similar and different figures might be a very useful way of determining how different these figures are. I have a feeling that the key would be, say, the two-dimensional frequency response of the human being observer.

ANDERSON: I would like to make one comment on that. I think that some of the concepts from information theory, including some of the aspects of Fourier analysis, for example, and the concept of redundancy, would aid in this.

ARNOULT: I think that we could also add that that type of analysis would be of the sort that I mentioned under the heading of contour analysis, that is, descriptive techniques from which a form could be reconstructed after the analysis had been made. A number of suggestions of the sort of the Fourier analysis and others have been made for this sort of thing.

I think it's quite possible that you not only want to describe the forms but that you may want to use this description to relate to certain experimental phenomena that you obtain.

BOYNTON: Dr. Arnoult, there's one aspect of what you had to say that bothers me a little bit. It was pointed out earlier today that, where forms are concerned and where subjects are going to be called upon to discriminate, let's say, one form from another, the possible population of forms among which these discriminations have to be made is very important. Suppose we have an experimental situation where we're asking subjects to tell us whether Form A is, say, the same as or different from Form B, to take a very simple case. Now, I would presume that you had hoped eventually on the basis of your type of work to be able to predict in advance, on the basis of various measures that you can make of an objective and physical nature ultimately of, say, Form A, how discriminable it is going to be in the experimental situation. But, it seems to me that how discriminable it is going to be depends to an alarmingly important extent on the nature of the forms with which it is going to be compared. The kind of measures you are talking about, if I understand them, would be no different in an experiment where there were forms around very much like Form A as compared to another experiment where maybe Form A stands almost alone as a form of this particular type. In one case it might be easily discriminated and in the other case only with extreme difficulty.

ARNOULT: On the contrary, I think the desired result we would like to achieve would be one in which we, for example, might be able to order the forms in terms of similarity, based on the quantitative similarity of physical measures, that is, how close they lie on some physical scale and that this will get to be the same problem. If you have two mathematical descriptions or have mathematical descriptions of two shapes, then you should be able to predict how difficult discrimination will be, by comparing the equations, let's call them, for these two shapes.

BOYNTON: I think that answers my question exactly.

BLACKWELL: Suppose you turn to complexity as your nicest psychophysical variable. Discriminating two very difficult multi-sided P^2/A figures I can imagine would not be equal in complexity. It seems to me this analysis will break down at this point (and now I'm back to my consistency with the nervous system) so that, in my terms, these two must be compared. I've got to know what the mapping functions look like, and then decide on the computer function that would tell them apart. I wonder if complexity has a prayer of allowing you to make this discrimination that Boynton pointed out.

ARNOULT: There is only one possible answer to that. We have now the cleanest in experimental designs and two hypotheses. Let's get back to the laboratory!

BOYNTON: If nobody else seems in the mood to talk, I've got something I'd very much like to say about Mr. Sleight's remarks and his insistence that experimentation should always be done, insofar as possible, under actual field conditions. I admit I'm prejudiced on this point. I wonder, for example, how far physics would have gotten if in the early days the experimenters had gone about their business with this kind of viewpoint. I think there are a number of things you can say about the so-called field conditions that make me wonder how adequate they are for getting the answers to these problems. In the first place, you very often cannot control the conditions sufficiently. Sometimes, unless you have an unusual degree of control you can't specify what the independent variable really is. Correlations simply are not sufficient, in my opinion, to get you very far in trying to infer something about causal relationships. Next, you can't manipulate the subject matter which you're dealing with well. I am sure that a lot of the things that happen in the cyclotron do not happen in real life and those that do probably could never be measured there. Thirdly, and perhaps most important, I wonder about the generalizability of specifically applied research, if that's what's being implied here. I think sometimes that by the time data which provide the answer to a specific problem have been obtained, the problem may no longer exist. Technological advancement may have rendered the problem obsolete, whereas the more fundamental type of research, I believe, has more generalizability and can be always used in different situations later on.

SLEIGHT: I certainly would have no argument with the points you made. My main thesis was that wherever it is possible to obtain the requisite control in the field situation, let's use the field situation even if it takes a little more time and a little more planning, especially planning.

HARKER: I feel a sympathy for both points of view, having been on both sides of the fence. I would like to comment this way: I feel it's probably a matter of order. If one tries to start in the field, I am afraid he is going to go down in defeat. If he starts in the laboratory, I think he can then go to the field possibly for a verification, if you will, or a field demonstration, to obtain the quantitative values. But I feel very strongly that one should not go to the field until he has a clear grasp of his variables so that he can know when he has control of significant variables and when he does not have control. In other words, where can he give way to the exigencies of field expedience and where must he hold tight rein in order to get meaningful data? You're placed in so many compromises so rapidly when you try to see large portions of the field that unless you have full control, or know what you're going for, I'm afraid you'll come back with nothing.

SLEIGHT: The day is arriving more and more when you do have control of the field conditions, including large numbers of men. I dealt with a situation very recently where we had almost exclusive control of 500 men in a communication situation for a long period of time. Your point about starting in the lab first instead of in the field is certainly valid in some cases. In other cases we don't know the problem and the best place to find the problem is in the field where it exists and the best people to find it are not necessarily the military people. In the past, the problem has been observed very casually by the military people, put into a certain verbiage, and transmitted thusly to the scientist. He doesn't really know when he gets to the lab to study something whether he is studying the right thing in the right way.

MORRIS: Then, after the problem is specified, do you take it back to the lab?

SLEIGHT: If you're unable to obtain sufficient control and adequate facilities to conduct the study in the field, yes, indeed.

BOYNTON: I believe Dr. Graham has a question.

GRAHAM: I should like to remind you that certain scientific approaches to the area of perception would be in the tradition of the field experiment; an example would be the Brunswik-Thurstone type of analysis. I am not pleased about this, but it seems to me to be a coherent, systematic kind of approach and I suppose the only thing you can say about it is: What does it give us? Well, the essential thing is that we don't know because not enough experiments have been done in the line of this tradition. It would, it seems to me, be generally worthwhile to investigate the outcome of the experiments directed in the line of this sort of theoretical attitude.

BOYNTON: I suppose it's unnecessary to add that it's probably a good thing that we have people with different points of view about applied versus fundamental research and that both kinds are very much needed. Time for one more question.

WHITE: I think the basic question here is partly trying to find out by your work what question has been asked by whom. If you're interested in advancing the basic science of perception of forms or something, I think very definitely the laboratory is where to do it. If, however, you have to come up with some good information for the military in a certain situation, you could get into a lot of hot water by taking the results of a perfectly wonderful laboratory experiment and applying them to the field situation. As Dr. Boynton said about the study of physics, you bring it back in the purest sense to the laboratory to study it. I think the situation is almost reversed when you have a teacher involved in a situation; the laboratory situation is apt never to exist in reality and the field situation is the more real study. As I say, it's what level of answer you're looking for. If you're looking for basic things, laboratory; if you're looking for applied things, in the great majority of cases, you have to use the field.

A MULTIDIMENSIONAL PSYCHOPHYSICAL METHOD FOR INVESTIGATING VISUAL FORM

Donald W. Stilson

Gibson(1, 2) has stated unequivocally that psychophysical methods will be a necessary step toward the understanding of form perception. Whether or not one accepts this view, psychophysical methods offer a potential avenue of attack on the problems associated with visual form perception. There are many reasons for the relative neglect of psychophysical methods in the history of form perception, and three of them are particularly pertinent and will be discussed here.

Emergence and psychophysics. The Gestalt and phenomenological psychologists have contributed most to the recognition of physical form as an important determinant of perception, and they have tended to view perceived form properties as unanalyzable "givens"(3, 4, 5). It has been said that perceived form is an emergent property of whole figures which can not be analyzed in terms of the parts of the figure.

At first, acceptance of such a view may appear to rule out the possibility of psychophysical analysis of form. However, this is not a necessary consequence of accepting the emergence concept. Emergent properties are presumably characteristic of a single figure whereas psychophysics is concerned with perceived similarities and differences between stimuli. Hebb(6) has argued that one does not need to reject the possibility of emergent properties of the single figure in order to defend the view that perceived similarities and differences between figures are dependent upon physical differences between their parts. Thus it is possible to espouse the emergence notion and at the same time use psychophysical methods in the study of form perception without the uneasy feeling that the two are logically incompatible.

Meaning and psychophysics. It is quite possible that the perceived similarities of shape are dependent in part upon non-physical aspects of the stimuli. For instance, Gibson(1, 2) has argued that two-dimensional figures should be excluded from studies of form perception on the grounds that they are always seen as representing something other than themselves or as having "meaning" for the observer.

It seems reasonable to begin by investigating the role of the physical properties of the stimulus in the determination of form perception, though admittedly, to the extent that non-physical factors are relevant to perceived similarity of forms, a psychophysical approach can not be wholly successful. In any event, with respect to Gibson's comments, the role of "meaning" must be viewed as an empirical question.

Unidimensionality and psychophysics. Perhaps the most important hindrance to the use of psychophysical methods in this area has been the conviction that an adequate account of form perception could not be accomplished within the limitations imposed by the unidimensional psychophysical methods which are most familiar. In general, the perceived similarity of two-dimensional figures is not representable in terms of a single psychological continuum, i.e., the perception of similarity of forms is a multidimensional process. In addition, it does not appear that any single physical measure of form is likely to account for all of the variance in perceived similarity. That is, the physical aspects of the stimulus which are relevant to perceived similarity of forms are multidimensional also.

Procedures have been available for some time which permit multidimensional psychophysical scaling. It is the purpose of this paper to describe these methods and to show how they might be used to enhance the prediction of response variability which is dependent on differences in stimulus form.

The Method

Unidimensional psychophysics involves the construction of (a) a psychological scale or dimension along which stimuli are ordered, (b) a physical dimension along which the stimuli are ordered also, and (c) some function or transformation which permits as estimate of the value of a stimulus on the psychological scale from knowledge of its value on the physical scale. Basically, the introduction of multidimensionality does not alter this formulation. In the multidimensional case, the results will consist of (a) a psychological space of two or more dimensions in which the stimuli are imbedded, (b) a physical space in which the stimuli are imbedded also, and (c) a transformation relating the dimensions of the two spaces. Then, if the values of a stimulus on the dimensions of the physical space are known, it is possible to apply the transformation to obtain an estimate of the value of the stimulus on each of the dimensions of the psychological space.

It is also possible to construct what may be called a psychophysical space. This is a multidimensional space containing both the physical and psychological dimensions obtained for a set of stimuli, i.e., each of the dimensions of the psychological space and each of the dimensions of the physical space are assigned projections on each dimension of the psychophysical space. This makes a compact representation of all of the psychophysical variance possible.

The remainder of this section will be devoted to outlining methods for the construction of each of these three spaces and for defining a transformation (or set of prediction equations) relating the physical space and the psychological space.

Constructing the physical space. To begin with, it is necessary to choose a set of physical measures which are applicable to the population of forms which is to be investigated. Clearly, there are an unlimited number of measures which might be selected in the hope that they would be related to the perceived similarity of forms. However, many of these measures will overlap considerably so that some of them can be discarded without loss of information.

Suppose that a set of physical measures has been selected and that a sample of figures has been drawn from the population under consideration. The values of the physical measures obtained for each stimulus in the sample can be recorded as shown in Table 1. The entry m_{2B}, for example, is the value obtained for stimulus B on physical measure 2.

Physical Measures	Stimuli			
	A	B	...	N
1	m_{1A}	m_{1B}	...	m_{1N}
2	m_{2A}	m_{2B}	...	m_{2N}
⋮				
q	m_{qA}	m_{qB}	...	m_{qN}

Table 1. Physical measurements of N stimuli on q physical measures.

The next step toward obtaining the physical space is to find some measure of similarity of each pair of stimuli which is a function of their values on the q physical measures. One of the first possibilities which comes to mind is a measure of correlation, though some other type of index of similarity might be used. For convenience, suppose that a correlation measure has been selected to measure the similarity of each pair of stimuli. The set of correlations between all possible pairs of stimuli may be subjected to a factor analysis to obtain a set of factor loadings or projections for each stimulus on each of the $\underline{k}$ orthogonal factors or dimensions. This set of factor loadings can be represented symbolically as shown in Table 2.

	Dimensions of Physical Space			
Stimuli	1	2	...	k
A	p_{A1}	p_{A2}	...	p_{Ak}
B	p_{B1}	p_{B2}	...	p_{Bk}
⋮				
N	p_{N1}	p_{N2}	...	p_{Nk}

Table 2. Factor loadings of N stimuli on $\underline{k}$ dimensions of the physical space.

Note that the factor analysis just described is a "Q-technique factorization," i.e., it is the correlations between stimuli over physical measures which is factored rather than the correlations between physical measures over stimuli.

As far as the present analysis is concerned, it is now possible to provide a complete physical description of each stimulus by giving its projections on these $\underline{k}$ factors. The space defined by this factor analysis will be referred to as the physical space.

If such a space has been constructed for N stimuli, then it is possible to obtain estimates of the loadings on the $\underline{k}$ factors for any new stimulus, provided that this stimulus comes from the same population from which the original stimulus sample was drawn. To show this, suppose that k= 3 and that the new stimulus, X, is to be assigned loadings on the three factors of the physical space. Now select any three stimuli from the original sample (for which the loadings in the physical space are known). Denote these three stimuli A, B, and C. Next, obtain the values for X on each of the $\underline{q}$ physical measures and calculate r_{AX}, r_{BX}, and r_{CX}. Since the sum of the cross products of the factor loadings of a pair of stimuli is equal to the correlation between the stimuli, it is possible to write

$$r_{AX} = p_{A1}p_{X1} + p_{A2}p_{X2} + p_{A3}p_{X3}$$

$$r_{BX} = p_{B1}p_{X1} + p_{B2}p_{X2} + p_{B3}p_{X3}$$

$$r_{CX} = p_{C1}p_{X1} + p_{C2}p_{X2} + p_{C3}p_{X3}$$

The only unknowns in this system of equations are p_{X1}, p_{X2}, and p_{X3}, the factor loadings of the stimulus X on the three dimensions of the physical space, and these values can be estimated by solving the system of equations.

The accuracy of these estimates will depend upon (a) the reliability of repeated measurements on the physical measures, (b) the proportion of the total variance associated with each stimulus which is accounted for by the $\underline{k}$ factors of the physical space, and (c) the representativeness of the sample of stimuli used in the factor analysis. In general, one would expect the reliability of the physical measures to be high, in the neighborhood of .98 or .99. In addition, a small value of $\underline{k}$ will probably permit the reproduction of a relatively high proportion of the variance associated with any stimulus. However, it may be difficult to obtain adequate representation of the stimulus population with the relatively small stimulus samples which the calculation of a factor analysis imposes. It appears that this potential lack of representativeness of stimulus samples is likely to be the major source of error in the estimation procedure just described.

Constructing the psychological space. The method used to construct the psychological space permits the assignment of values or projections of each stimulus on each of $\underline{r}$ orthogonal dimensions. These dimensions are derived from an analysis of the perceived similarity of the stimuli. The basic procedure, which is closely related to factor analysis, has been described in detail by Torgerson(7) and elaborated by several others(8, 9, 10, 11).

In order to construct a multidimensional space, it is first necessary to have a measure of the psychological similarity of pairs of stimuli which satisfies the four requirements of a mathematical distance function. The first of these requirements states that the distance between any pair of stimuli, A and B, must be greater than or equal to zero. The distance from stimulus A to B must be zero if and only if A is identical to B. The distance from A to B must equal the distance from B to A for all stimulus pairs, A, B. Perhaps the most stringent requirement is that the triangular inequality hold, i.e., the distance from A to B plus the distance from B to C must equal or exceed the distance from A to C for all triplets of stimuli, A, B, and C.

It may appear that an empirical measure of psychological similarity satisfying these requirements would be hard to find, but in general, this is probably not the case. Rather, the difficulty is to select a function from among the many possibilities which will exist for most problems(12). Several methods based on judgments of stimuli presented in pairs or triads have been discussed elsewhere(7, 8, 9, 10, 11, 13), but a method having particular pertinence in the study of form perception will be discussed here. This procedure has been used with some degree of success in the study of triangular shape(10).

This distance function is based on an experimental procedure used by Attneave(14) to eliminate the effects of an observed interaction between the judged similarity of triangles and their relative orientation. Attneave found that for certain pairs of triangles, A and B, judged similarity was higher when A was presented to the right of B than when A was to the left. In order to eliminate this interaction, Attneave developed a procedure for measuring perceived similarity in which the stimuli were presented one at a time. The method involved training subjects to pair a short word with each triangle in a paired associate learning situation. Then, on a test trial, the frequency with which each pair of triangles were confused was recorded. Triangle A and B are said to have been confused if the word correctly associated with A is given in response to B or if the word correctly paired with B is given in response to A. It was assumed that two triangles were likely to be confused if they were seen by the observer as being similar.

The measure used by Attneave, frequency of confusion, does not satisfy the conditions of a distance function. However, it is relatively easy to convert these values to any one of several functions which probably will satisfy the four requirements of a distance function given above. One such distance function is described below.

The first step in obtaining values for this measure of psychological similarity is to form a matrix in which each entry is the number of subjects who confused a particular pair of stimuli on the test trial. For example, Table 3 indicates that 5 subjects gave the response correctly paired with stimulus A when stimulus B was presented on the test trial. When A was presented on the test trial, the response correctly associated with B was given by 3 subjects. The diagonal entries of this table are the numbers of subjects giving the correct response to a particular stimulus on the test trial. The sum of any column in the table will equal the total number of subjects in the experiment, provided each paired associate word (or nonsense syllable) is given only once by each subject on the test trial.

In order to obtain a measure of the distance between a pair of stimuli, say B and C, the square root of the sum of the squared differences between the entries in rows B and C is obtained. From the table,

$$d(B, C) = \sqrt{(3 - 11)^2 + (20 - 15)^2 + (8 - 31)^2 + \ldots + (5 - 3)^2}$$

Stimulus with which Response is Correctly Paired	Stimulus Presented A	B	C . . . N
A	25	5	10 . . . 2
B	3	20	8 . . . 5
C	11	15	31 . . . 3
. . .			
N	9	8	17 . . .30

Table 3. "Confusion matrix" for obtaining distance function based on paired associate learning.

An analogous calculation can be performed for columns B and C, i.e.,

$$d(C, B) = \sqrt{(10 - 5)^2 + (8 - 20)^2 + (31 - 15)^2 + \ldots + (17 - 8)^2}$$

can be obtained. Since d(B, C) will not necessarily equal d(C, B), the arithmetic mean of the two can be used as the final measure of the distance from stimulus B to C. If one wishes to make this distance function independent of the total number of subjects used in the experiment, each of these values can be divided by $(\sqrt{2n^2} + \sqrt{2n^2})/2 = \sqrt{2} \cdot n$, the maximum possible value of the distance function when n is the number of subjects. For most problems, this function will satisfy approximately the four requirements given earlier.

Use of the function just described implies that the perceived similarity of two stimuli is not dependent only on the frequency with which they themselves are confused. Rather, the distance between two stimuli is dependent upon the relations between themselves and, in addition, the relationships of each of them to all other stimuli in the sample. Of course, not all distance functions which could be defined have this property, and whether or not this is desirable will depend on the aims of a particular investigation.

Once a set of interstimulus distances has been obtained, it is possible to apply the multidimensional scaling procedure to obtain a set of orthogonal axes or psychological dimensions(7). The projections of each of the N stimuli on these r dimensions define a psychological space based on the perceived similarity of the stimuli. The results of such an analysis are symbolized in Table 4. Each entry in this table is a projection of one of the stimuli on one of the r psychological dimensions of similarity.

	Dimensions of Psychological Space			
Stimuli	1	2	...	r
A	a_{A1}	a_{A2}	...	a_{Ar}
B	a_{B1}	a_{B2}	...	a_{Br}
⋮				
N	a_{N1}	a_{N2}	...	a_{Nr}

Table 4. Projections of N stimuli on r dimensions of the psychological space.

Now that methods for constructing a physical space and a psychological space have been described, the next step is to find a procedure for predicting values on each of the psychological dimensions from knowledge of the projections of a stimulus on the dimensions of the physical space.

Relating the psychological space to the physical space. In order to simplify subsequent discussion, a summary of the results obtained so far is given in Table 5. From inspection of the table, the nature of the prediction problem is evident. It is necessary to find some function of the k physical dimensions which is in some sense the best predictor of each of the r psychological dimensions. It will be necessary to have r such prediction equations. A linear regression equation with k predictor variables may be defined for each of the psychological dimensions, or, if one chooses, multivariate curvilinear prediction equations may be used. Unless the deviation from linearity is marked, the latter will have little advantage over the linear method due to the lower stability of the coefficients of the curvilinear equations under cross-validation.

	Dimensions of Physical Space				Dimensions of Psychological Space			
Stimuli	1	2	...	k	1	2	...	r
A	p_{A1}	p_{A2}	...	p_{Ak}	a_{A1}	a_{A2}	...	a_{Ar}
B	p_{B1}	p_{B2}	...	p_{Bk}	a_{B1}	a_{B2}	...	a_{Br}
⋮								
N	p_{N1}	p_{N2}	...	p_{Nk}	a_{N1}	a_{N2}	...	a_{Nr}

Table 5. Projections of N stimuli on dimensions of the physical space and on dimensions of the psychological space.

Suppose that the coefficients of the r prediction equations (one for each psychological dimension) have been obtained (using either linear or non-linear methods), and that a stimulus, X, not included in the original sample, is to be assigned values on each of the dimensions of the psychological space. Using the procedure given earlier, the projections of X on the dimensions of the physical space can be calculated, i.e., p_{X1}, p_{X2}, ... p_{Xk} can be estimated. These values can be plugged into each of the r prediction equations in order to estimate the projection of stimulus X on each of the r psychological scales.

Operationally, the regression equations relating the physical and psychological spaces provide a physical interpretation for the dimensions of the psychological space. In other words, if one wishes to characterize a particular psychological scale, the relative weightings of the physical dimensions which enter into the prediction equation for that scale are examined.

So far, methods have been described for the construction of a physical space, a psychological space, and for defining a transformation which permits the prediction of each psychological dimension as a function of the values of a stimulus in the physical space. In addition, a method for estimating the values of a new stimulus on the dimensions of the psychological space has been described. In short, a multidimensional psychophysical method has been outlined.

Constructing a psychophysical space. In order to simplify the representation of the relationships between the dimensions of the physical space and those of the psychological space, it is possible to "imbed" all of these dimensions in a single, psychophysical space. The first step is to form the $(r + k)$ by $(r + k)$ matrix of all correlations between the dimensions of the psychological space and those of the physical space. This amounts to calculating the correlations between all columns of Table 5. Factorization of this matrix yields factor loadings for each dimension of both spaces on the factors or dimensions of a psychophysical space.

If desirable, it is possible to plot the psychophysical space graphically one plane at a time, and thus to represent the relationships between the physical and psychological dimensions of the stimuli. This provides a convenient graphic display of the relationships between the physical and psychological scales which may facilitate interpretation of the results. A similar procedure has been applied previously(10).

Discussion

I do not consider psychophysical scaling procedures an end in themselves. Rather, the method just described is conceived as part of an S-R formulation which may be applicable when the physical and psychological characteristics of the stimulus are complex (as they usually are in the case of form perception). Actually, stimulus and response are thought of as terminal variables which are "mediated" by the conceptual dimensions of the psychological space. Consequently, it is more correct to write S-P-R, in which the "P" represents the psychological space linking stimulus and response. Note that the "response" referred to here is not the response used in obtaining the measure of psychological distance between stimuli. Rather, the "R" represents any response measure which is predictable from knowledge of the values of the stimuli on the dimensions of the psychological space. In the case of form, the response might be a measure of stimulus generalization, tachistoscopic recognition threshold, or any other variable which fluctuates as a function of stimulus shape. The ultimate usefulness of a method such as the one described here will depend upon the number of response measures which can be predicted from knowledge of the values of stimuli in the psychological space and the accuracy with which these predictions can be made. Generally, it would be expected that a given dimension of the psychological space is most relevant to one or two response measures whereas other response variables are predicted best by some other scale(s) in the psychological space.

In attempting to formulate S-R relationships when the stimulus is complex, an attempt to characterize the physical properties of the stimulus on a unidimensional scale is likely to prove futile. In addition, the psychological properties or dimensions of a stimulus class which are relevant to a particular response measure will depend upon the nature of the response. For example, dimension I of a hypothetical psychological space may be most relevant to a measure of stimulus generalization whereas dimension II is virtually unrelated. On the other hand, tachistoscopic recognition thresholds may be best predicted from knowledge of dimension II and essentially unpredictable from dimension I. At the same time it is possible that no unidimensional ordering of the stimuli exists which permits adequate prediction of both of these response variables. To illustrate this, suppose that stimuli A, B, C, and D have been ordered on a single scale as a result of some psychological scaling procedure and that the ordering is B, A, C, D. If it is the case that these stimuli are best described in a two-dimensional psychological space, the ordering on dimension I might be B, C, A, D and A, B, D, C on dimension II. If a measure of stimulus generalization ordered these stimuli A, C, D, B, then this measure would have a rank-difference correlation of zero with the ordering on the unidimensional scale, -.40 with dimension I of the hypothetical psychological space, and +.80 with dimension II. Tachistoscopic recognition threshold may order the stimuli B, C, A, D, and the correlation of this measure with unidimensional ordering is +.20, with dimension II it is -.20, and with dimension I, +1.00. In short, the example involves two response measures, neither of which correlates greater than .20 with a unidimensional psychological scale, though each of them correlates at least +.80 with one of the dimensions of a two-dimensional psychological space. Under such circumstances, the multidimensional result will permit far better prediction of either of the two response measures than can be achieved using the unidimensional psychological scale.

The previous discussion of the relationship between psychological scales and response measures can be applied equally well to the relationship between the physical properties of the stimulus and scales derived from perceived similarity. That is, a physical measure of the stimuli may be unrelated to a unidimensional psychological ordering of the stimuli, but multidimensional scaling of perceived similarity of the stimuli may reveal one or more dimensions of the psychological space which are predicted quite well from knowledge of the physical measure. Consequently, multidimensional psychological scaling of complex stimuli may enhance both (a) the prediction of the psychological characteristics of the stimulus from its physical properties and (b) the prediction of response variables from knowledge of the psychological attributes of the stimulus.

Earlier, I suggested that linear regression equations might be used in relating the dimensions of the physical space to those of the psychological space. In the past, most psychophysical investigations have involved the fitting of curvilinear functions, and some people will undoubtedly be disturbed by the linearity assumption. However, in the case of complex stimuli, it is probable that the increased predictability of the psychological dimensions of a multidimensional space will more than make up for the loss of information incurred by the linearity assumption. Whether or not this will prove to be true in the case of form perception must remain an empirical question. Presently, the only directly relevant evidence comes from a psychophysical study of triangular form(10). In this investigation, it was found that approximately 70% of the variation in a five-dimensional psychological space obtained for the perceived similarity of triangles could be predicted from knowledge of the physical measures of the stimuli. A linear multidimensional psychological space and linear regression methods were used to obtain this result, and it was replicated with two independent samples of observers and two samples of triangles.

A final question to be discussed here is the reproducibility of the dimensions or scales of both the physical and the psychological space. The extent to which the procedures described here will serve a useful role in response prediction will depend to a large extent on the sampling stability and the test-retest stability of the scales in both the physical and the psychological space. If the dimensions of these spaces are not reproducible, then, of course, these methods will be of little value.

As I stated earlier, the reliability of the physical measurements will probably be high, and this will contribute to the stability of the dimensions of the physical space. Whether or not this is true in the case of the psychological distances between stimuli is another matter. Experimental procedures can probably be perfected to the extent that reasonably high reliabilities are obtainable.

The instability of the psychological distances which is introduced by individual differences between observers is identical to the problem faced in unidimensional psychophysics and requires no special attention here.

I believe that the most serious threat to the stability or reproducibility of the scales in both the physical and the psychological space is the variability introduced by stimulus sampling. Attneave and Arnoult(15) have discussed this problem in detail (see also Arnoult, this symposium), and there can be little doubt that one of the most difficult problems connected with the psychophysical approach in the study of form perception is the development of adequate and workable procedures for obtaining representative stimulus samples from specified populations. Attneave and Arnoult(15) have suggested several ingenious random sampling techniques, and some of these will probably prove satisfactory when large stimulus samples are possible. However, it will usually be found that the experimental procedures or the calculations involved in a multidimensional psychophysical investigation impose severe restrictions on the stimulus sample size, and, of course, small random samples offer little guarantee of representativeness. Perhaps the most attractive alternative is to find relevant variables for stratifying the stimulus population and use stratified sampling procedures. Stratification would permit considerable reduction in sampling error if the stratification dimensions were relevant, but it has one obvious difficulty. It is necessary to know something of the relevant characteristics of the population before it is possible to stratify effectively, and the investigator will seldom have sufficient information for this in the case of form perception. However, there are a few obvious variables which permit stratification (e.g., symmetry), and as further information concerning the psychophysics of form becomes available, perhaps others will be found.

THE USE OF HIGH-SPEED DIGITAL COMPUTERS IN STUDIES OF FORM PERCEPTION*

Bert F. Green

Preparing stimuli for studies of form perception is often a vexing problem. The problem is especially acute when it is desired to introduce random elements, since this usually implies a need for a large number of stimuli.

Recently we have learned how to use a high-speed digital computer to make 35 mm. film strips. The resulting film may then be projected with 35 mm. film strip projector, or made into slides for use in a standard 2" x 2" slide projector, or reduced to 16 mm. film if movie projection is needed. Using a computer is much less laborious than manual frame-by-frame photography. The computer also makes possible a wide variety of experiments that otherwise would be completely unfeasible.

Using a computer to generate film strips is not as mysterious as it may sound, and is not fundamentally different from other film techniques or from other computer techniques. We have been using an experimental computer at the Lincoln Laboratory called MTC, which means Memory Test Computer and derives from the computer's original purpose of testing memory devices. MTC is very similar to Whirlwind, and in speed and versatility is in the class of the IBM 704, the Univac Scientific, Datatron and similar high-speed electronic computers that use magnetic core storage and can perform from 20,000 to 100,000 arithmetic operations per second. (For comparison, machines that use magnetic drum storage like the IBM 650 and Burroughs E101 are about 1,000 times slower and are probably not useful for making films.) One of the output devices of MTC is a cathode ray tube to which a camera is attached. The camera is an automatic Fairchild 0-15 scope camera like those used to take radar scope photographs. The camera shutter is normally open, and is closed momentarily when the film is advanced; thus, the successive frames of the film record whatever is displayed on the CRT between successive film advances.

The computer paints the CRT display by painting a succession of points, each point is displayed for a fixed time and at a fixed intensity. The location and sequence of the points is completely under the control of the computer program. No display scanning is built-in -- the sequence is completely arbitrary.

The problem, then, for the user, is to prepare a computer program that will give the particular displays he wants. For example, one of the main uses of the display in engineering applications is to obtain a printout

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*The research reported was supported jointly by the Army, Air Force and Navy under contract with the Massachusetts Institute of Technology.

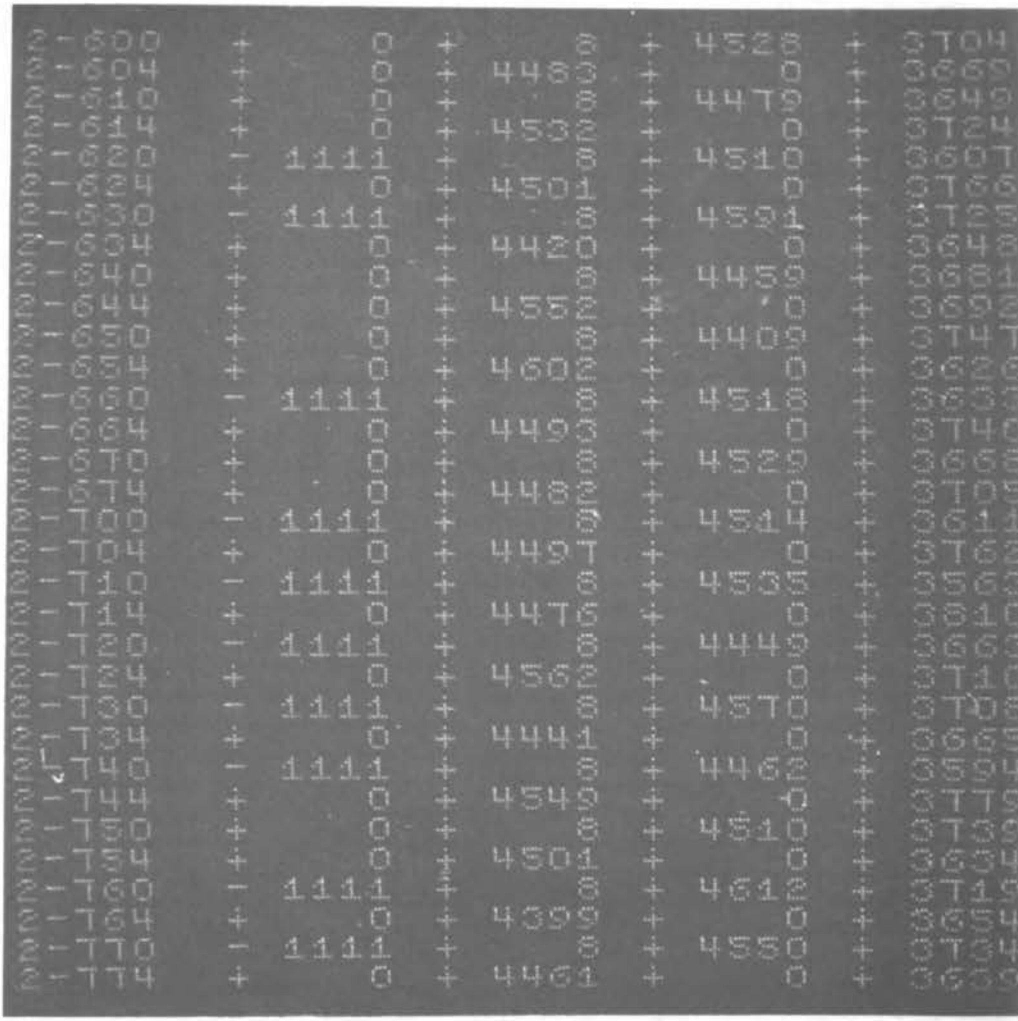


Fig. 1. Numerical printout.

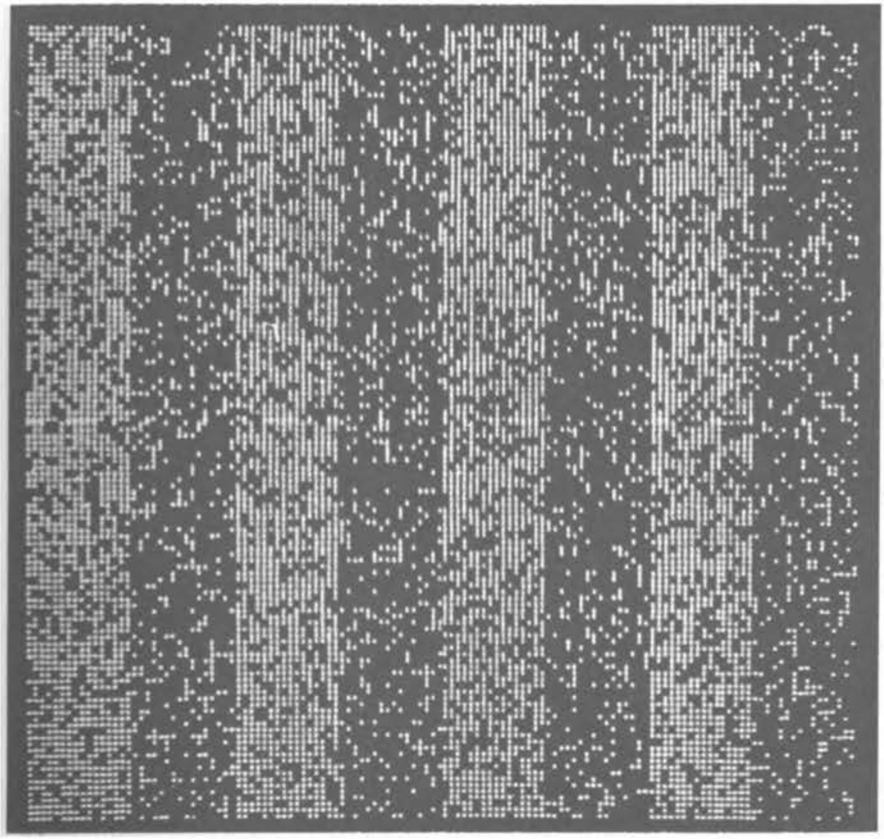


Fig. 2. Eight vertical bars ($p_1 = .75$, $p_2 = .25$).

of numerical results, as shown in Figure 1. In this case, the computer program is designed to paint sets of dots in the form of numerals corresponding to the previously calculated results.

A computer program, as you may know, is a sequence of elementary instructions to the computer, which are executed one by one. Typical instructions are add, multiply, store, and, for our purposes, display. Along with the display instruction on MTC it is necessary to specify two numbers, the x and y coordinates of the point to be displayed. To display a set of points, it is necessary first to set up the x and y coordinates of the first point, then to execute the display instruction, then to set up the second pair of coordinates, repeat the display instruction, etc. Moreover, the fundamental utility of a computer is the fact that whenever the same sequence of instructions is to be repeated several times, it need only be written once, along with a few instructions to effect the required number of repetitions.

Programming a computer can be rather complicated, since there are many details not mentioned here that must be cared for. Programming

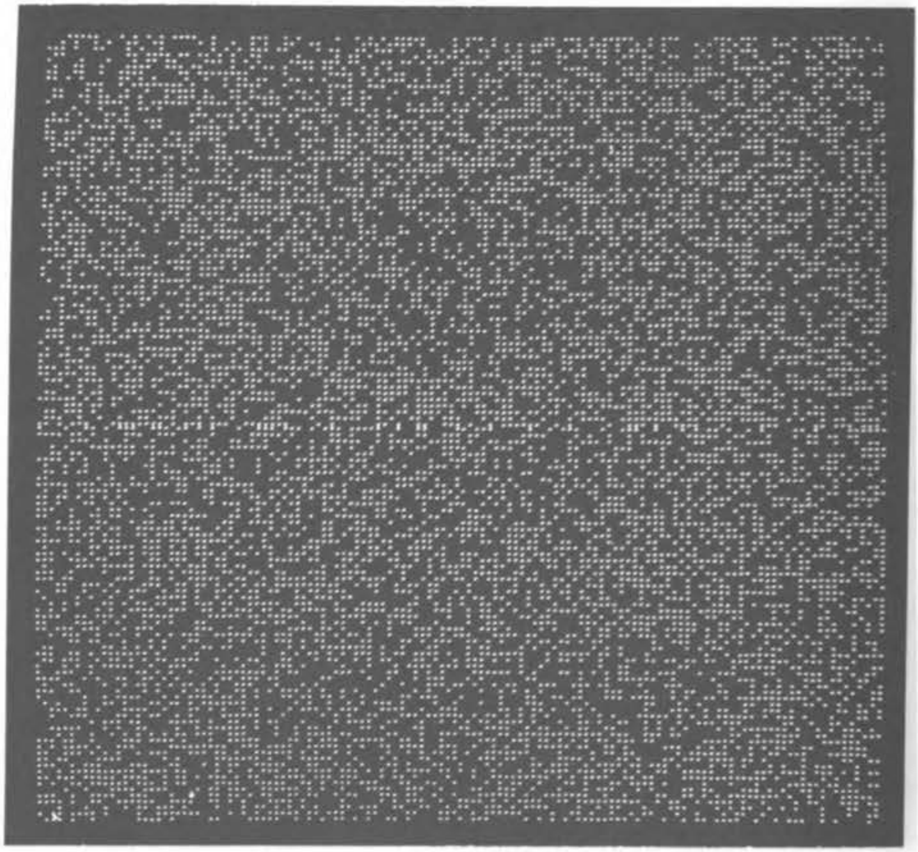


Fig. 3. Eight horizontal bars (p_1 = .53, p_2 = .47).

is much like designing equipment, and just as a new piece of equipment seldom works when it is first assembled, so a program seldom works right when it is first tried on the computer. A certain amount of debugging and patching seems to be inevitable. However, when it finally works, a well-planned computer program can be a very potent device.

The Psychology Group at the Lincoln Laboratory has four active projects that use computer-generated visual displays as stimuli. Three of these are unclassified and can be discussed here. Figure 2 shows an example of a pattern of vertical bars used in a series of studies in pattern detection. The pattern is formed statistically in a dot matrix -- each dot has a certain probability of occurring. Here the dots in the heavy bars have probability .75, while for the light bars the probability is .25. Figure 3 shows a similar horizontal pattern, but with probabilities of .53 and .47. In the experiment the observer is shown a series of such displays, and is asked to detect whether the pattern is vertical or horizontal. In this way, psychophysical curves of pattern detection can be obtained.

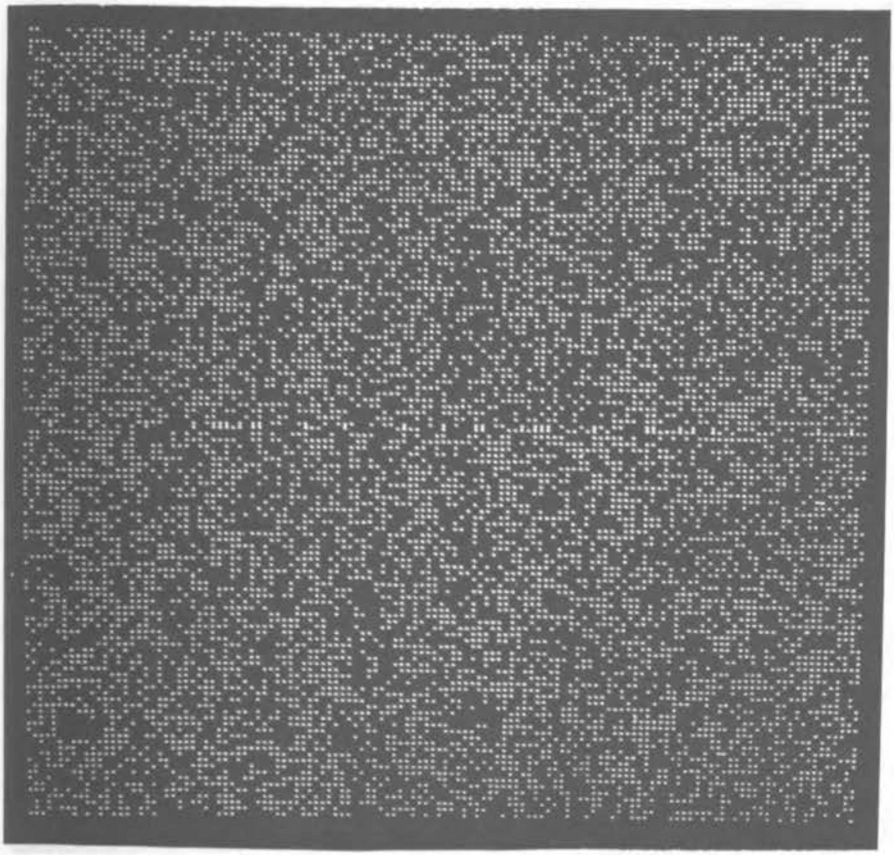


Fig. 4. Sixteen vertical bars ($p_1 = .55$, $p_2 = .45$).

One of the advantages of the computer is that several variations can be made with little change in the basic computer program. Figure 4 shows a display with 16 bars rather than 8. Figure 5 shows a variation in which the change from one probability is not abrupt between bars, but gradual. The probability levels are 1 and 0, but the probability changes in increments of .06 (1/16). Figure 6 shows the same sort of fuzzy contoured display, but with probability levels of 1/2 and 5/8, and the same probability increment of 1/16. We haven't actually done any experiment yet with these fuzzy contours, but it seems like an interesting possibility.

In another variation the pattern can be altered. Figure 7 shows a square within a square, with probabilities of .26 and .50. Figure 8 shows the same pattern with probabilities of .38 and .50. It is interesting to note here that it is possible to detect that there is something in the center of the larger square but the edges of this something are very indistinct.

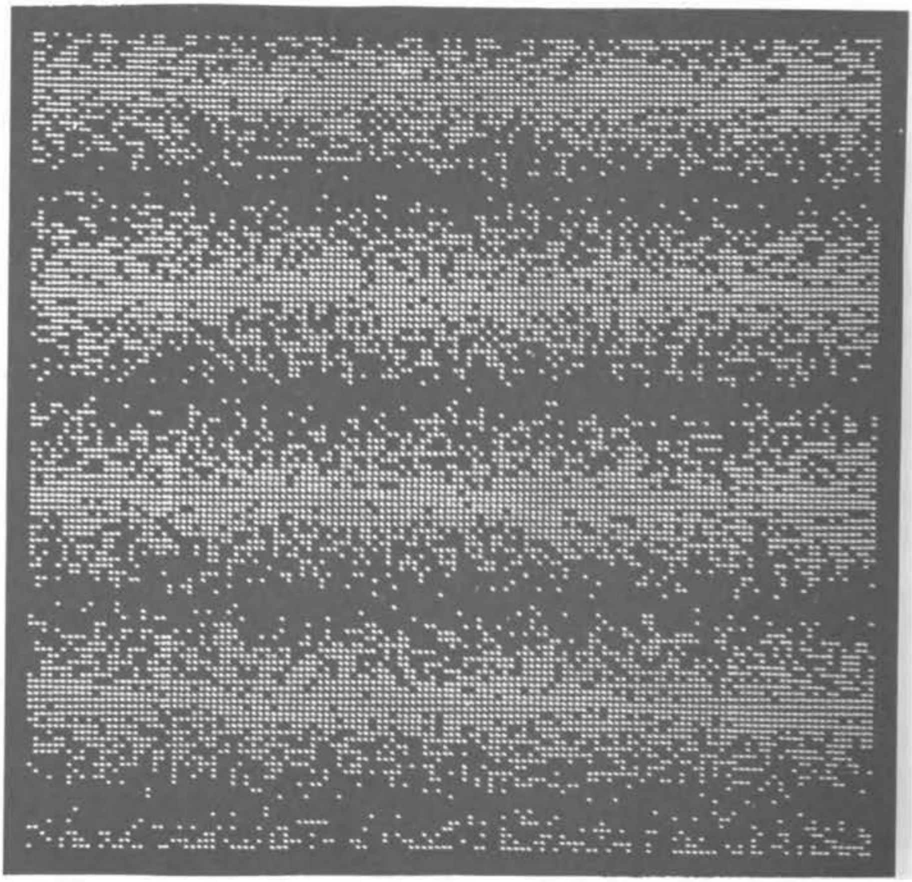


Fig. 5. Eight horizontal bars, with fuzzy contours ($p_1 = 1$, $p_2 = 0$, $\Delta p = .06$).

A second project using computer-generated displays is a study of how humans track targets on radar scopes and similar displays. In the initial experiment, a single track dot moved discretely through a field of random "noise" dots. At a pre-arranged point the track turned in one of five directions, which the subject was asked to identify. Figure 9 shows the path of one track, and also a typical noise field. In the experimental film strips, each frame contained just one track dot, and a random set of noise dots. The succeeding frame had the track dot in its next position along its path, and a new set of noise dots. Parameters in the study were amount of noise, and blips/scan ratio of the target; i.e., probability of a target dot on any particular frame. Each strip of 40 frames was generated by the computer in about 20 seconds, of which about 18 seconds were required for indexing the camera and 2 seconds for computing and generating the displays.

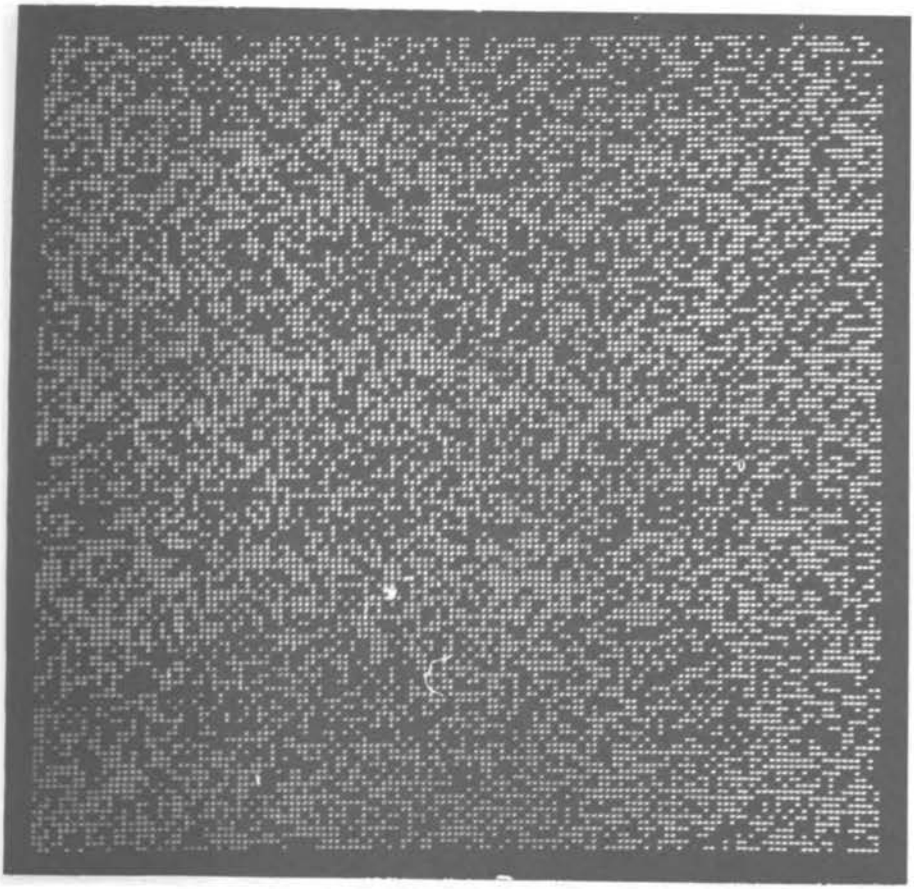


Fig. 6. Eight horizontal bars with fuzzy contours (p_1 = .63, p_2 = .50, Δp = .06).

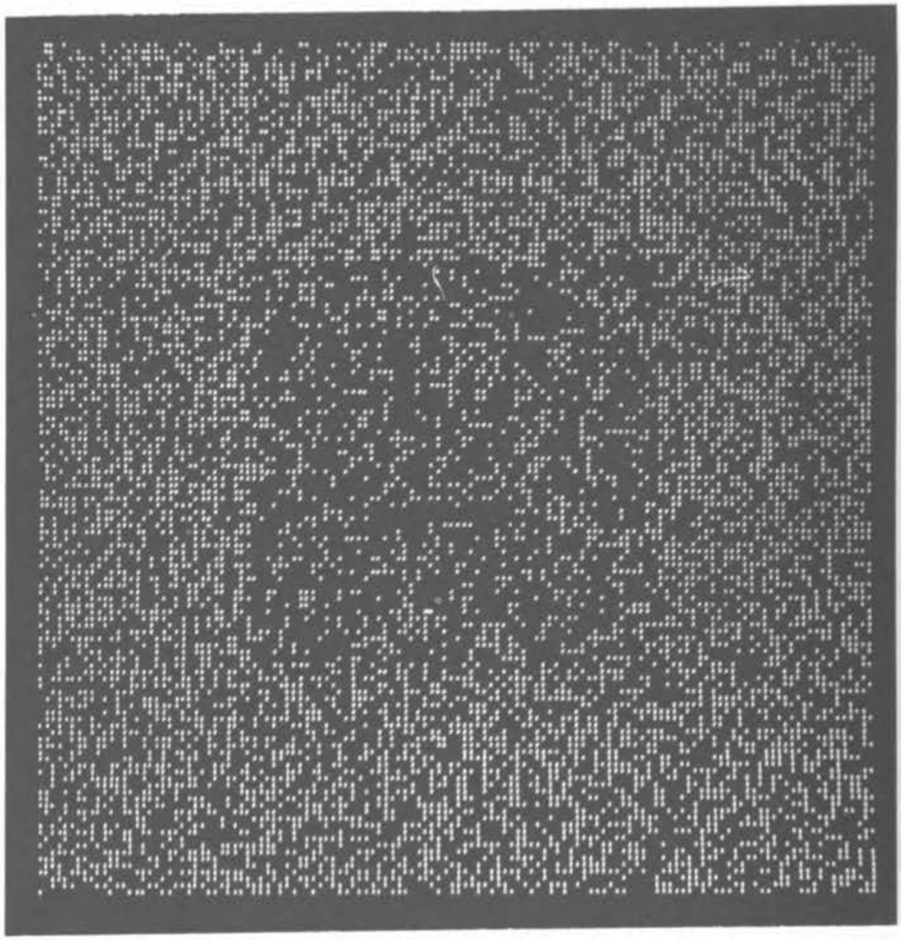


Fig. 7. Square (p_1 = .50, p_2 = .26).

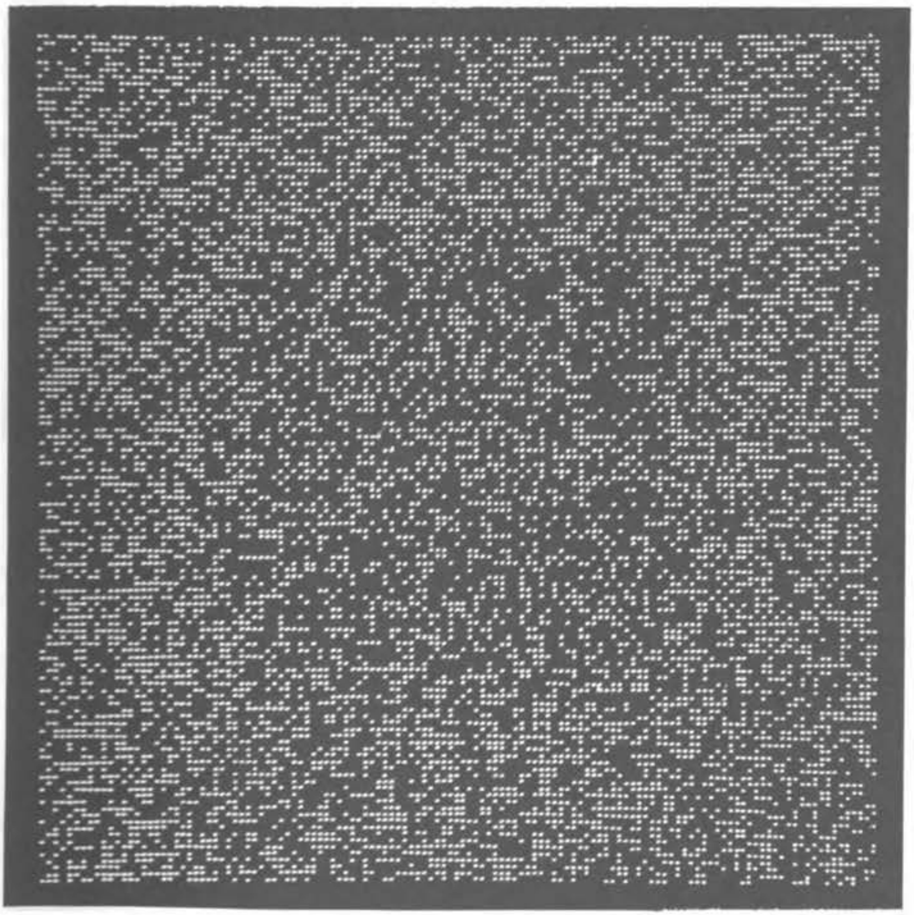


Fig. 8. Square (p1 = .50, p2 = .38).

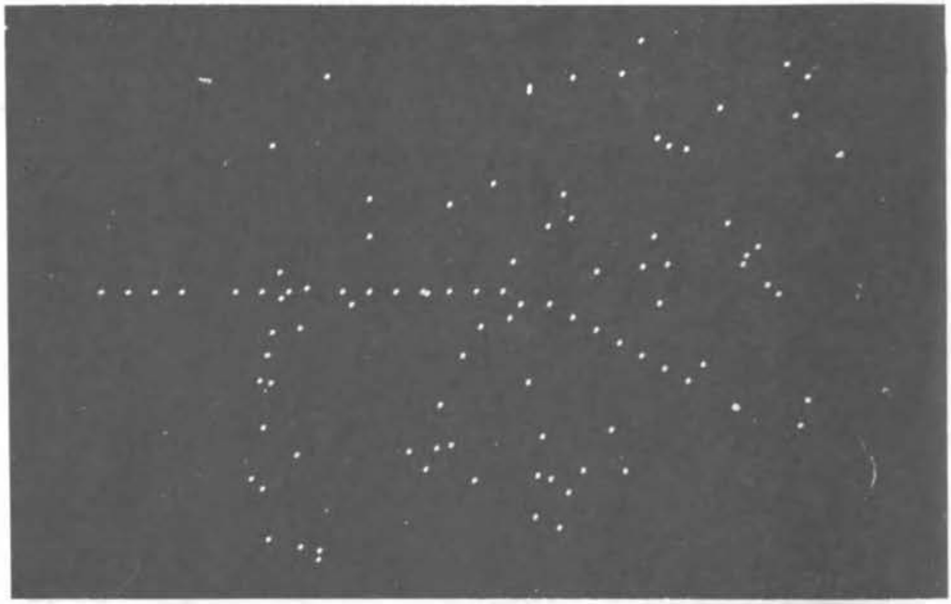


Fig. 9. Schema for radar tracking study.

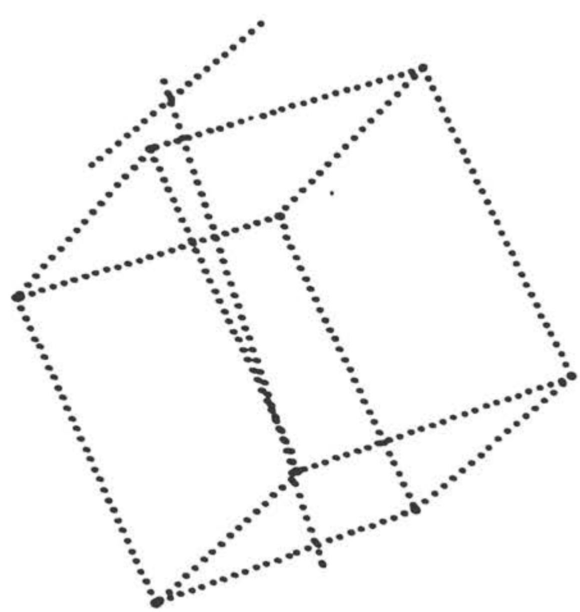


Fig. 10. Perspective display of cube and two random lines.

A third project based on computer-generated displays is an investigation of the use of relative movement to create a three-dimensional display on a CRT. Figure 10 shows a two-dimensional perspective outline of a three-dimensional cube. If this cube is now rotated about its center, its perspective outline will change in a regular way. Conversely if the two-dimensional outline is changed in the same regular way, it should appear as a three-dimensional cube. We should also be able to use rotation to present a three-dimensional configuration of dots. Several experiments are planned to evaluate this three-dimensional display technique either using movies as displays, or working directly with the CRT, without an intermediate photographic process. I have a short 16 mm. movie to show some of the possibilities.

BLUR AS A FACTOR IN FORM DISCRIMINATION

Glenn A. Fry

The method of generating blur about to be described is being used in connection with a study of photointerpretation which is being supported through a contract between the Rome Air Development Center and The Ohio State University Research Foundation and is being conducted in the Mapping and Charting Research Laboratory.

One version of this method of generating blur which is illustrated in Figure 1 can be used in conjunction with patterns involving lines and borders oriented in a vertical direction.

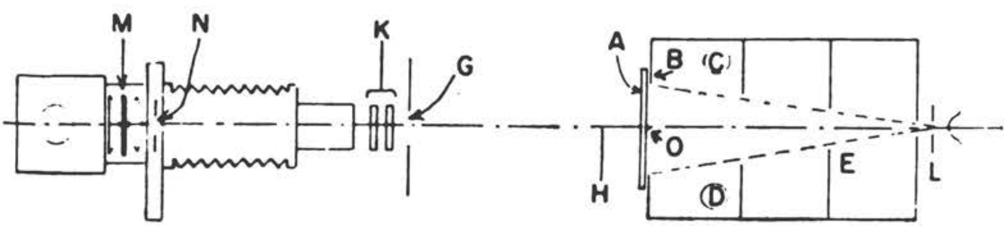


Fig. 1. Apparatus for generating blurred patterns.

This version of the method has been employed in a study(1) reported previously, and is easier to understand than the version of the method employed in the present project. The eye illustrated at the right side of the figure views through an artificial pupil. It sees a circular field produced by covering a hole at the back end of the viewing box with a piece of opal flashed glass A. This is uniformly illuminated by the sources C and D. The right half of this disc-shaped test object is illuminated by light projected through the diaphragm G. It is necessary to place a plate of ground glass between the two elements of the condenser in order to produce a uniform distribution of illuminance in the plane of the aperture at G. The left half of the opal glass is shaded from the aperture G by the screen at H. This screen has a vertical straight edge which lies on the axis of the instrument.

The shadow of the straight edge represents a gradient between the bright and dark halves of the disc. The form of the gradient depends upon the shape of the aperture at G. A circular aperture produces a gradient similar to that obtained by throwing the eye out of focus or by throwing a camera out of focus. An aperture having the shape shown in Figure 2 will produce a gradient which represents the integral of the probability curve. An aperture having the shape shown in Figure 3 will produce the type of distribution which is very useful for the study of Mach bands. By varying the shape of the aperture one can produce any kind of gradient. One can even simulate the type of gradient produced in the development of a photographic image.

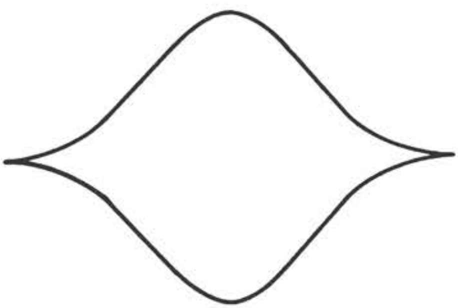


Fig. 2. Aperture for generating an ogive gradient.

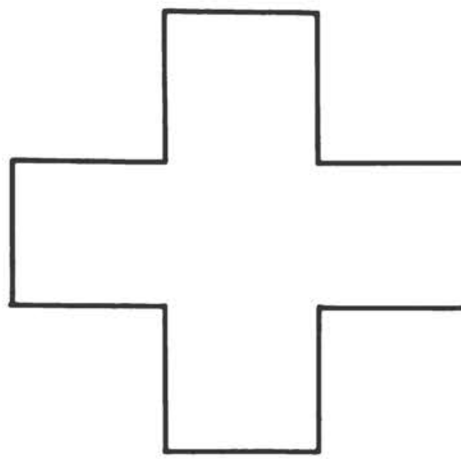


Fig. 3. Aperture for generating a gradient for the study of Mach rings.

One can vary in a quantitative way the amount of blur by changing the distance of the screen from the milk glass.

In the arrangement shown, the eye views the gradient through an artificial pupil and if the care is taken to provide proper correction lenses for the eye, blur inherent in the image-forming mechansim of the eye can be reduced to a minimum or at least held constant and then by varying the distance from H to A and by varying the shape of the aperture at G one can have complete control of the blur of the retinal image. The halation of opal flashed glass presents a problem. The same type of problem is encountered with TV screens. One can minimize this by using a piece of ground glass. This is more satisfactory from the point of view of halation but it introduces graininess.

The method of blur generation described above is limited to patterns which are composed vertical lines and borders, i.e., when the aperture at G is shaped like Figure 2. If the aperture at G is circular the method can be used with any kind of pattern for stimulating blur produced by throwing an eye or a camera out of focus.

One can use an ellipse at G to simulate astigmatism. Also one can rapidly rotate the aperture at G and produce any kind of radially symmetrical blur for any kind of target.

The technique described above for generating blur is by no means new. The method has been used, for example, by W. E. K. Middleton in a study described in his book Visibility in Meteorology(2).

There are other methods of generating blur. One method which has been used in a study(3) reported previously by the speaker is to view a transilluminated black and white pattern through a piece of ground glass placed at various distances in front of the pattern. The nature of the blur is dependent upon the scattering characteristics of the ground glass and one is limited to only one kind of gradient. Fortunately, however, the gradient is very much akin to the gradient that is produced by diffraction when an optical system is in focus. The amount of blur can be varied simply by varying the distance of the ground glass in front of the target.

A number of years ago Dr. Stanley Ballard suggested to the writer the use of a rotating prism. This method of generating blur must also be kept in mind.

In summary, it may be said that I have attempted to review several methods of generating blur. Although there is nothing particularly novel about these methods, instances abound in which investigators have generated blur by methods which are either time-wasting or do not lend themselves to quantitative specifications and it is important therefore to make an effort to widely publicize the available methods for generating blur in which the amount and kind of blur can be controlled.

Furthermore it does not appear to be generally appreciated that there are various distinct types of blur which are actually encountered in the use of the eyes.

Also gross misconceptions about the effect of blur appear to be present. One of these common misconceptions is that by making a pattern more and more blurred, we make it less and less visible. The most common cause of blur namely that of throwing the eye or camera out of focus leads to a complicated kind of imagery in which objects instead of just blurring out develop into miltiple images. Several examples(4) of this are illustrated in Figures 4, 5, 6, and 7. For each figure the camera has been thrown out of focus a given amount and test patterns of different

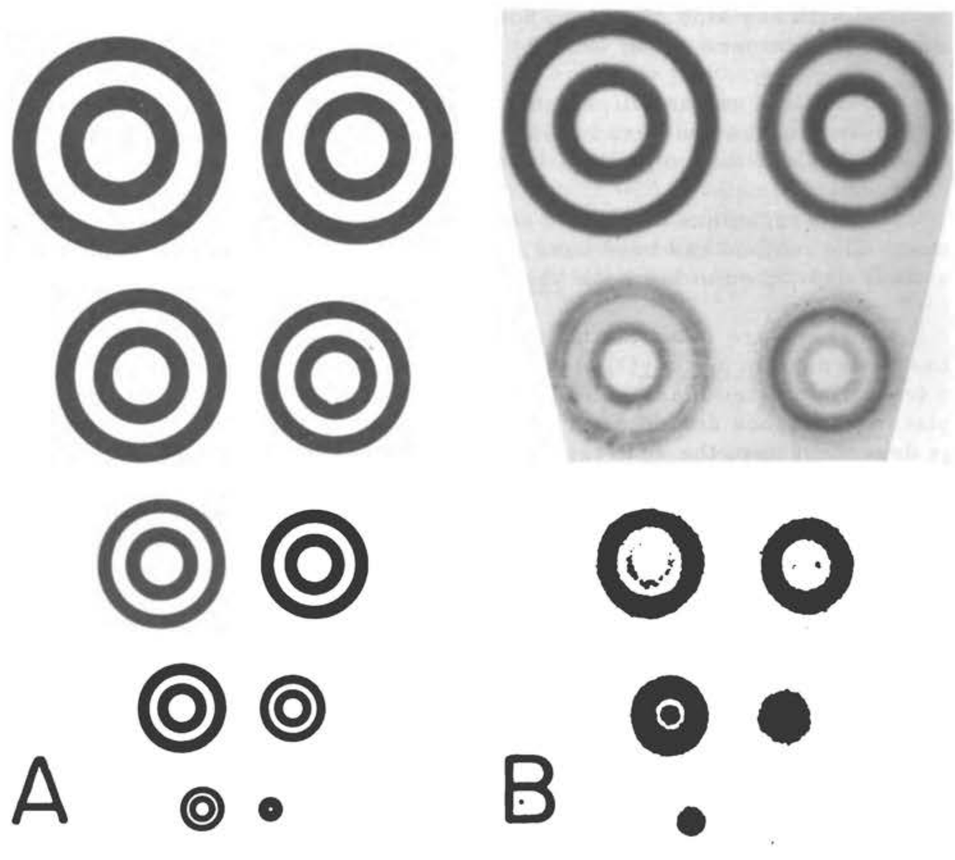


Fig. 4. Effect of throwing Verhoeff ring patterns out of focus.

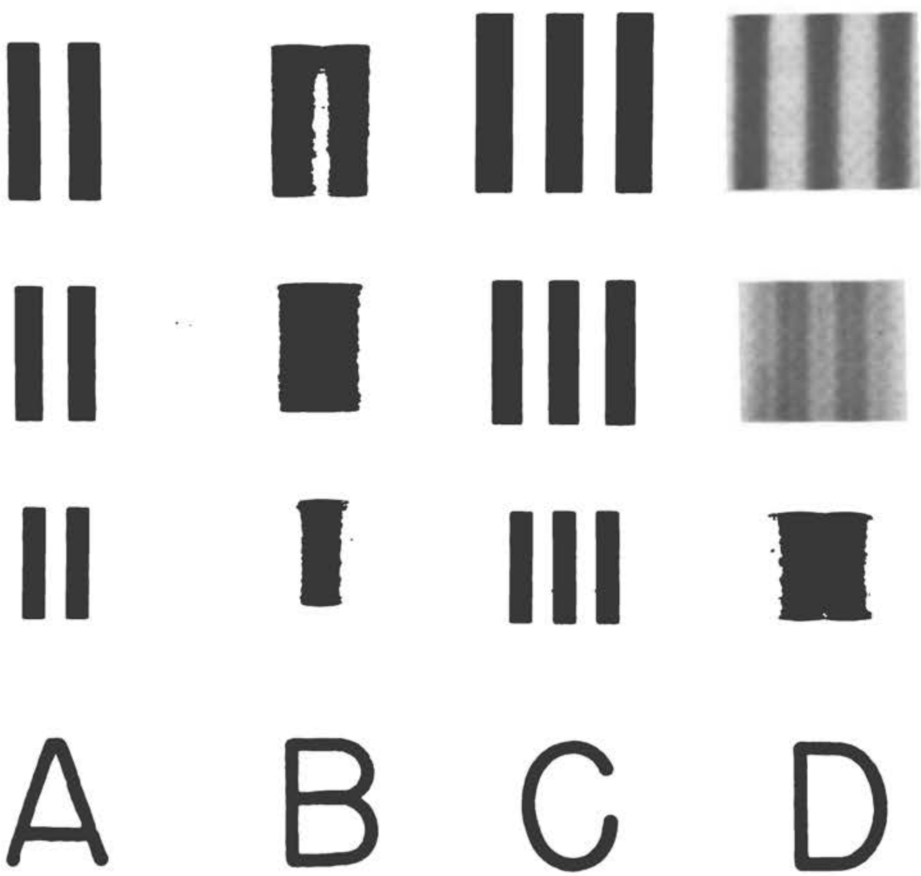


Fig. 5. Effect of throwing parallel bar patterns out of focus.

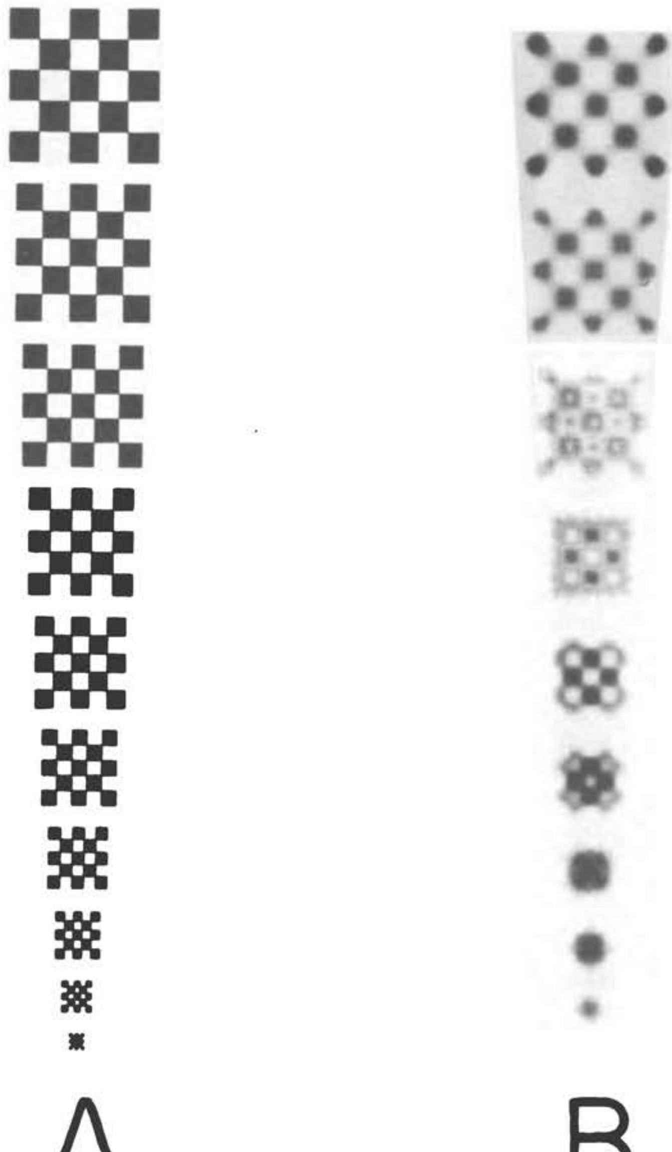


Fig. 6. Effect of throwing checkerboard patterns out of focus.

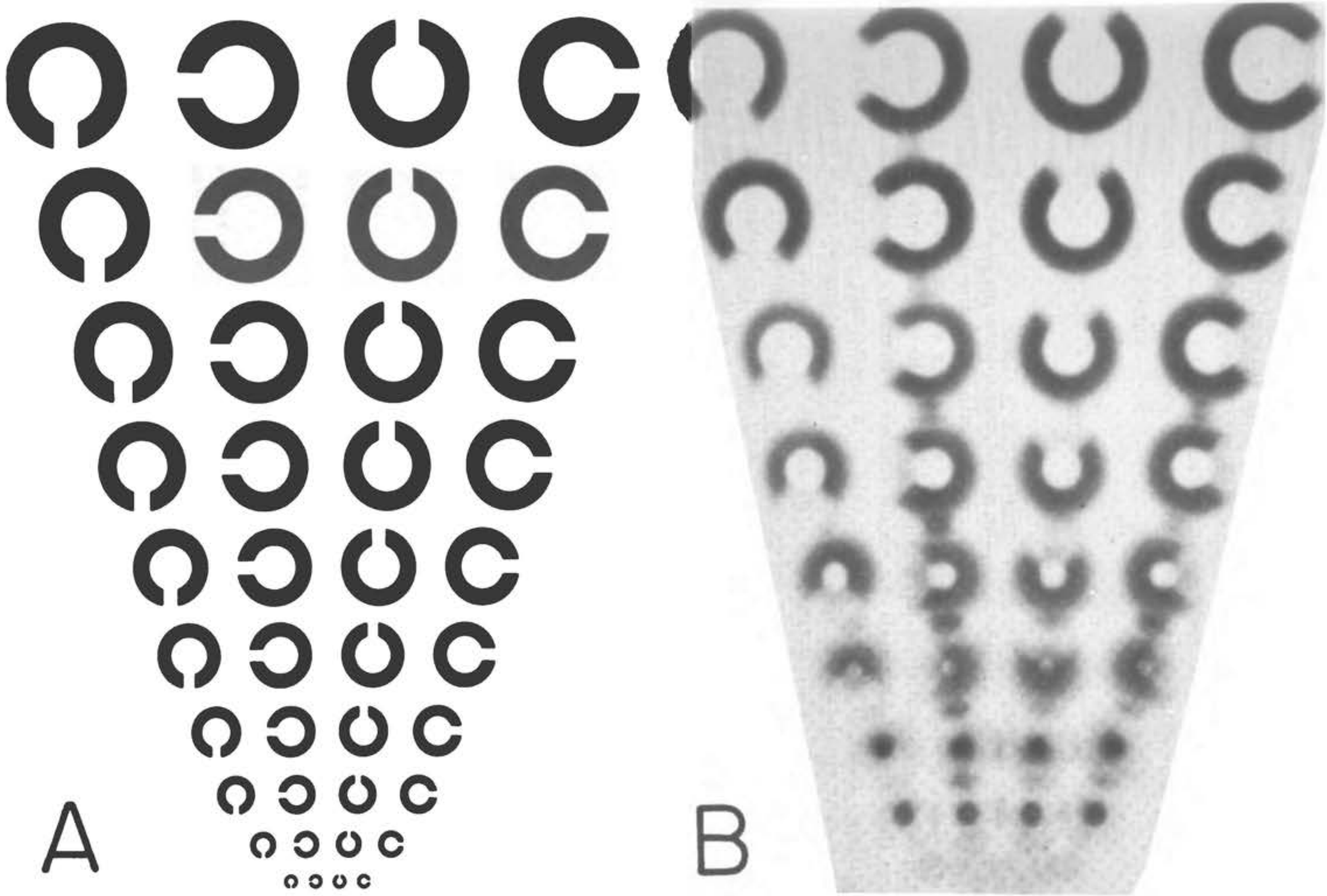


Fig. 7. Effect of throwing broken ring patterns out of focus.

sizes but of the same configuration have been photographed. The point to be made is that the number of images differs from the number of elements in each pattern, i.e., when the pattern gets down to the so-called limit of resolution. It is easy enough to deduce why such effects occur but at the moment the important thing is to make investigators aware of the complexity of the pattern.

DISCUSSION

TANNER: I'd like to make some comments on looking for the physical variables scale. In a form detection task the critical variable, if you were going to scale optimally, is whether you would scale as the ratio of the probabilities of the observation, whatever it may be, drawing from the various alternatives which you were testing. In other words, the likelihood ratio or something like this or a pretty fair alternative. This means that you have the scale in N minus 1 dimensions where N is the number of particular forms, figures as such. It is not at all necessary that this scale should be ordered monotonically with any physical variable. We may be looking entirely the wrong way when we're looking for physical variables, the scale and predicting on the basis of it. As a matter of fact, we can get a very nice quantitative scale in this way on the basis of purely qualitative observation. All we need to know is the probability that if each alternative occurs the observation will be made completely.

POLLACK: Could Dr. Green tell us something about the computer, starting from a non-mathematical angle?

GREEN: Well, do you know factor analysis at all? It's just like that. There are defined first the coordinates, say for example, of a line. Find the coordinates of the end points of the line with respect to a fixed axis and then rotate the axis and compute the new three-dimensional coordinates that would come from this rotation using trigonometric formulas of a reasonably simple nature. Having this, you then have to project it perspectively onto a two-dimensional surface and the projected coordinates as displayed are now the end points, say for a particular line. Then you draw the line between them by a reasonably simple routine. Am I saying anything. . . ?

POLLACK: I guess it's just a time scale that I'm really asking about. You don't actually compute point for point where the figures should go next and give this instruction to the computer point by point in time, but you give the computer the rule that you would use if you actually constructed a three-dimensional figure on a two-dimensional background?

GREEN: Yes, yes. I'd tell it how to compute rotations, for example.

KRENDEL: What about the two dots? We have just two dots moving around in the computer?

GREEN: Well, they're just two dots but each has inside the computer three coordinates, X, Y, and Z and I proceed through exactly the same mathematics so that the configuration of dots then gets projected in exactly the same way. It happens that with only two dots it looks kind of strange because they don't seem to hold together, and that's your fault, not the computer's, I think.

THE USE OF FACSIMILE EQUIPMENT AND CONTROLLED VISUAL NOISE IN FORMS RESEARCH

Alexander Weisz

I want to go very quickly over some material that we presented at a Vision Committee meeting four years ago. At that time it was entitled "The Technique for the Experimental Investigation of the Recognizability of Letters Reproduced by Facsimile System and Degraded by Gaussian Noise". This afternoon it's "Noisy Forms", subtitled "How to Produce Invisible Forms without the Use of a High-Speed Digital Computer". The interest here I think is because the technique has been used for six years at Tufts for producing stimulus materials for investigating the legibility and recognizability of letters, familiar forms, and unfamiliar forms. Essentially what you have is a facsimile scanner and printer into the circuit of which you can throw the output of a gas tube noise generator with matched attenuator so that you can control signal-to-noise ratio with 1 DB steps. What you get out of this is noisy copy. I think it's best that we look at Figure 1 now so that you see what the appearance of the copy is like.

There are four 26-letter stimulus cards. As you go clockwise from the upper left corner you can see that the signal-to-noise ratio is going down and that recognizability gets less. What's happening is that as your background gets grainier, you're chopping more holes in the form itself and you're reducing the contrast between background and figure.

Well, as you can see what we have is a technique for reproducing non-uniform figures and backgrounds that has some application to many electro-visual systems, in addition to the facsimile link. It is a technique that gives data that are very amenable to information theory, since you can specify channel capacity or signal-noise ratio or normalized energy requirements for recognition at any given probability level. The major variables that you can play with here are obviously signal-noise ratio and definition.

The equipment that we have used has a definition of 52 vertical lines

Fig. 1.

per inch. By varying the size of the stimulus material which you scan you can use a greater or lesser definition, more or fewer elements constituting the figure. This you can do at the equipment. When you come to the testing situation you can maintain an equivalent visual angle by varying your viewing distance.

I think the only other point that has to be made is that there's a requirement for considerable equipment here in order to turn out materials. However, we've been fairly successful in simulating much more simply the kind of degradation of the figure that you get with the facsimile kind of technique by using some overlays on photographic copy. We used clean copy originally with some overlays to cut down contrast, with zipatone sheets to knock white holes in the black figure and then a final overlay of a negative or a positive of noisy background in order to put black mottling into the background. You no longer have exact quantitative control of your difficulty level, that is, specification of your difficulty level, but you can come up with gradations in difficulty level.

FACSIMILE-GENERATED ANALOGUES FOR INSTRUMENTAL FORMS DISPLAYS

Mason N. Crook

The first thing I want to do is to show a few more samples of noisy copy. Figure 1 is not an example of experimental test copy; at least it isn't yet. It's just a demonstration of the way this technique could be used with a photograph of a natural scene to produce degradation in varying degrees. Only one degree is shown here. I have some sets of photographs similar to that one with actual facsimile noise copy in various steps which can be looked at afterwards if anybody is interested.

Figure 2 is what we call a sample of familiar forms (I'll come back to familiar forms a little bit later). It shows a series of noise levels progressing clockwise from the original in the upper left. You can carry noise level as far as you want until anything you put in there is completely unrecognizable, of course. The noise levels there are: on the upper left, the so-called original, plus thirty (this is in terms of signal-noise ratio, in decibels, and the plus thirty is an entirely nominal value since there is, in effect, no noise at that setting); upper right, minus nine; lower right, minus thirteen; and lower left, minus seventeen.

Figure 3 illustrates another feature of the facsimile system; that is, the effect of definition. Now, we've been using definition here to mean the ratio between the size of the object and the size of the printing element. The printing element in the system is at a fixed definition of 52 per inch in either direction determined by the scanning line in one direction and by the limit of resolution in the other direction. As you decrease the size of the object, it gets chopped up more. Each element then constitutes a larger fraction of the figure so that eventually you would run into a limit, just in terms of the definition of the system when it is printing clean copy. In addition, definition of the system interacts with the noise to pull the limit up higher when you superpose noise. We have printed copy when we wanted to use definition as an experimental variable. We have printed copy in different sizes and presented it in a viewing situation in which the viewing distances were varied to equate visual angle. It's conceivable that you get into some secondary variable here, but I think only to an inconsequential degree, so far as the things we've been dealing with are concerned.

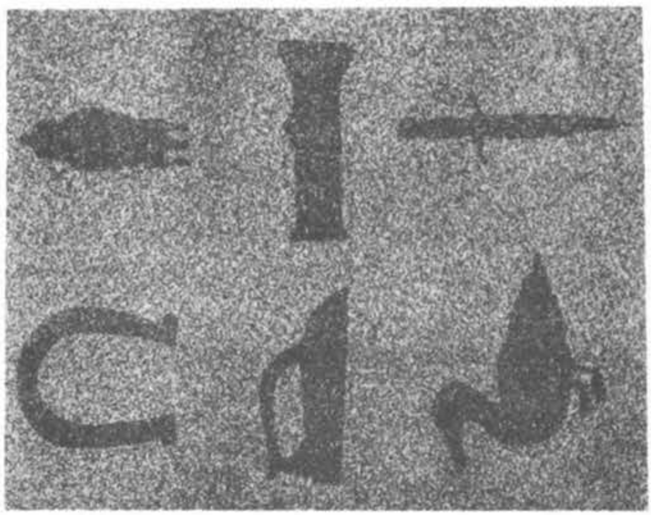
This way of generating noisy copy has certain advantages. In the first place the copy is stable for a few weeks and therefore is convenient to use for experimental testing. You go back to it and it is exactly the same for the period over which you need it. It's a good deal faster than any point-by-point hand manipulation of individual spots in copy, of course. It's quantifiable in terms of signal-noise level and definition with respect to meaningful physical aspects of the way the system works and therefore also reproducible. We hope that, at least with respect to these characteristics, it has some generality beyond the particular situation of printed messages with which it was originally used. We would hope that effects on

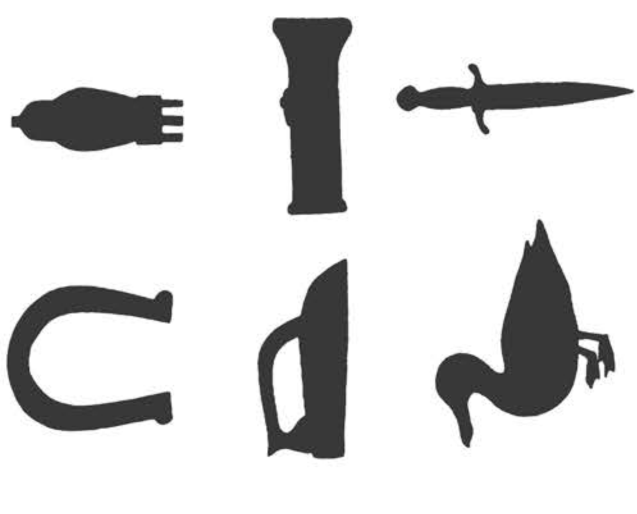
Fig. 1.

perception, which we can relate systematically to these variables as determined in this situation, might have some application to other viewing situations in which instruments are used that are subject to random noise and which have limits of definition or limits of resolution. That is one reason for going ahead to see what we can do with it after the original job for which it was developed was terminated. We used it originally in the "Infomax Project" in which we were concerned with printed messages transmitted through noisy radio channels near the limit of reception. If we are going much beyond that kind of operational situation and that kind of test material, of course, we have to begin to concern ourselves with other kinds of forms. Before I mention other kinds of forms, I might say in passing that I have some samples here of the materials that we used to produce simulated noise, or fake noise, that Weisz mentioned. This is an overlay which has a large number of irregularly shaped small dots. This material can be obtained in various characteristics, various dot sizes and so on. That overlay, combined sometimes with a plain diffusing acetate sheet and always with a photographic negative of regular facsimile noise, makes a pretty good simulation of the actual facsimile noise, at least from

Fig. 1. (cont'd.)

the standpoint of the psychological effect. You present it in a viewing frame, an ordinary printing frame. The reason for doing this is that the actual use of the facsimile equipment is a little laborious. When you get down to setting up copy for substantial experiments you're likely to find yourself running off copy for four solid days at a stretch, with additional handling beyond that. With this procedure you can handle material a good deal faster. You can carry through some of the preparatory steps of an experiment a good deal faster and the total amount of material that has to be handled is very much less. From that standpoint it is a great deal more convenient. The limitation of it is that it has no strictly quantifiable relation to the facsimile noise, the density characteristics of which can be stated statistically in a fairly definite way. This is kind of an empirical substitute. We have a rough calibration of sets of this material calibrated against actual facsimile noisy copy but we have not used it in such a way as to make it necessary to depend on that kind of calibration. We've used it mainly for comparisons within an experiment between types of forms or other variables.





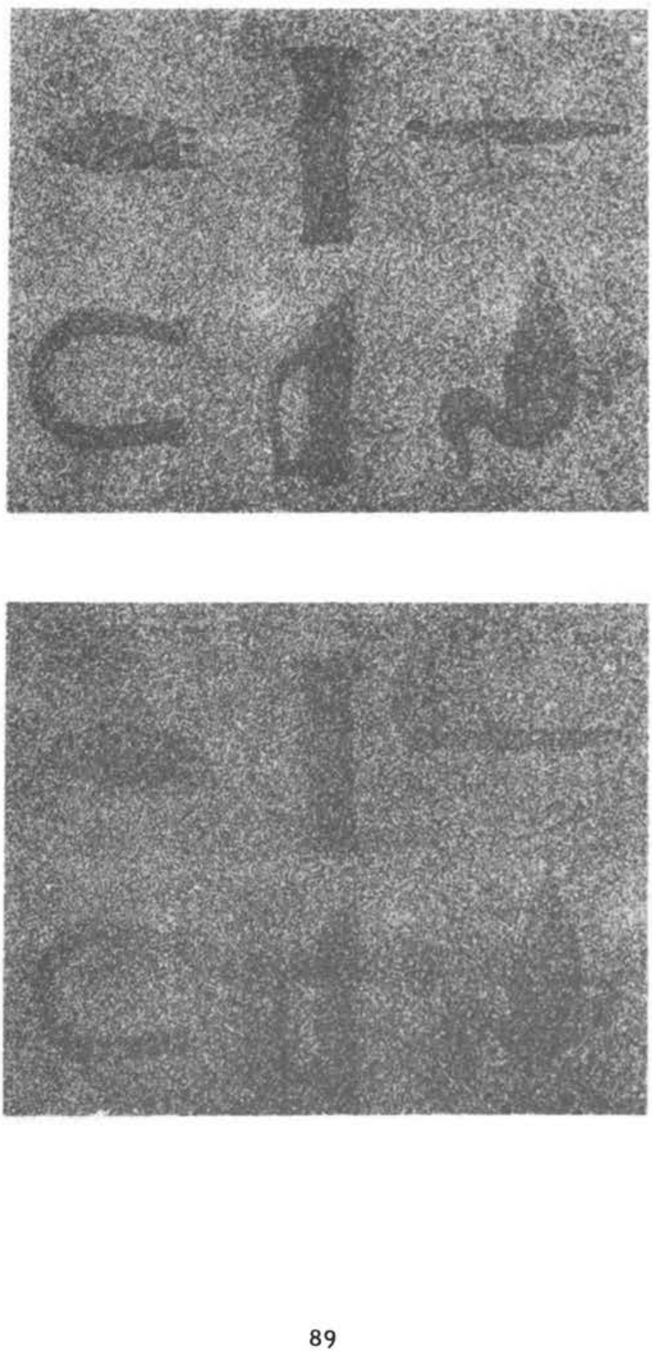


Fig. 2.

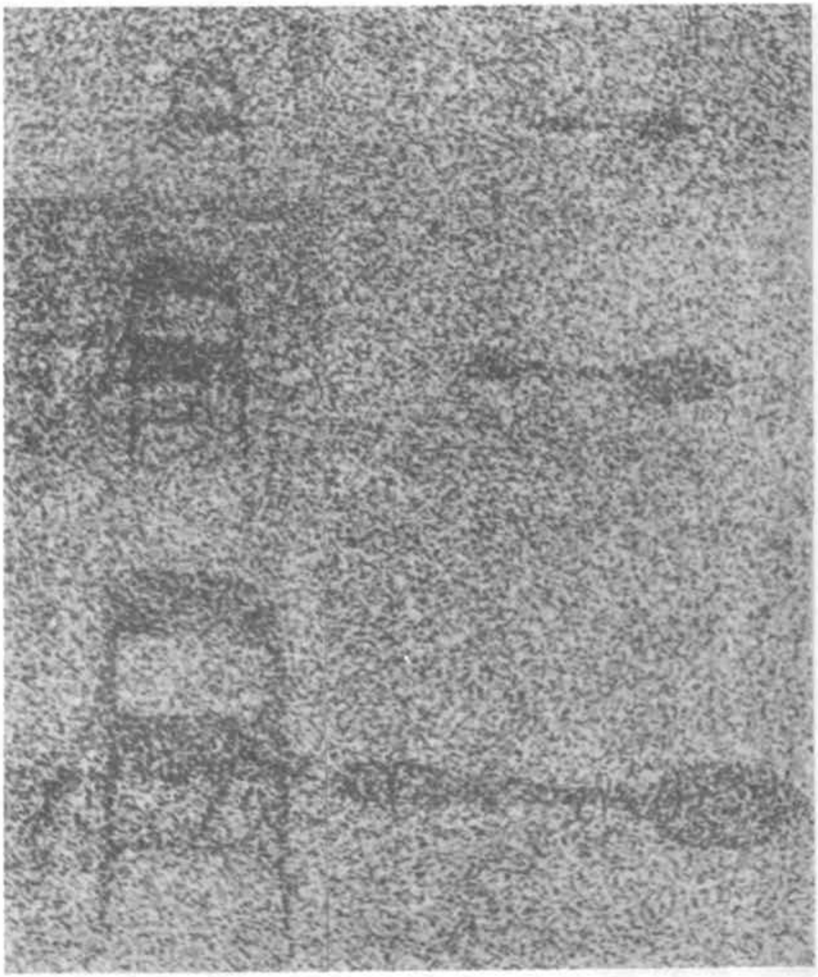


Fig. 3.

Going on to other kinds of test material, we have been involved for the past two or three years in studies of forms of much more general kinds. We wanted to work both with familiar and unfamiliar forms. Figure 4 shows a set of forms that constitutes a sample of so-called familiar forms. One difficulty in this whole area, of course, is that you have no standardized test material like you have in a Landolt C or a Snellen E. If you're going to make any cross comparisons or if you want any standardized test material to be carried through a series of experiments, you have to make it up for your own purposes. I certainly wouldn't want to represent this as an adequate sample of all familiar forms, but at least it's a sample that we selected with some care and have used in several experiments. If we continued to work with this kind of material and got enough of a breathing spell we would probably revise the set. The forms are certainly not all equally good. The snake below the middle, near the middle on the horizontal

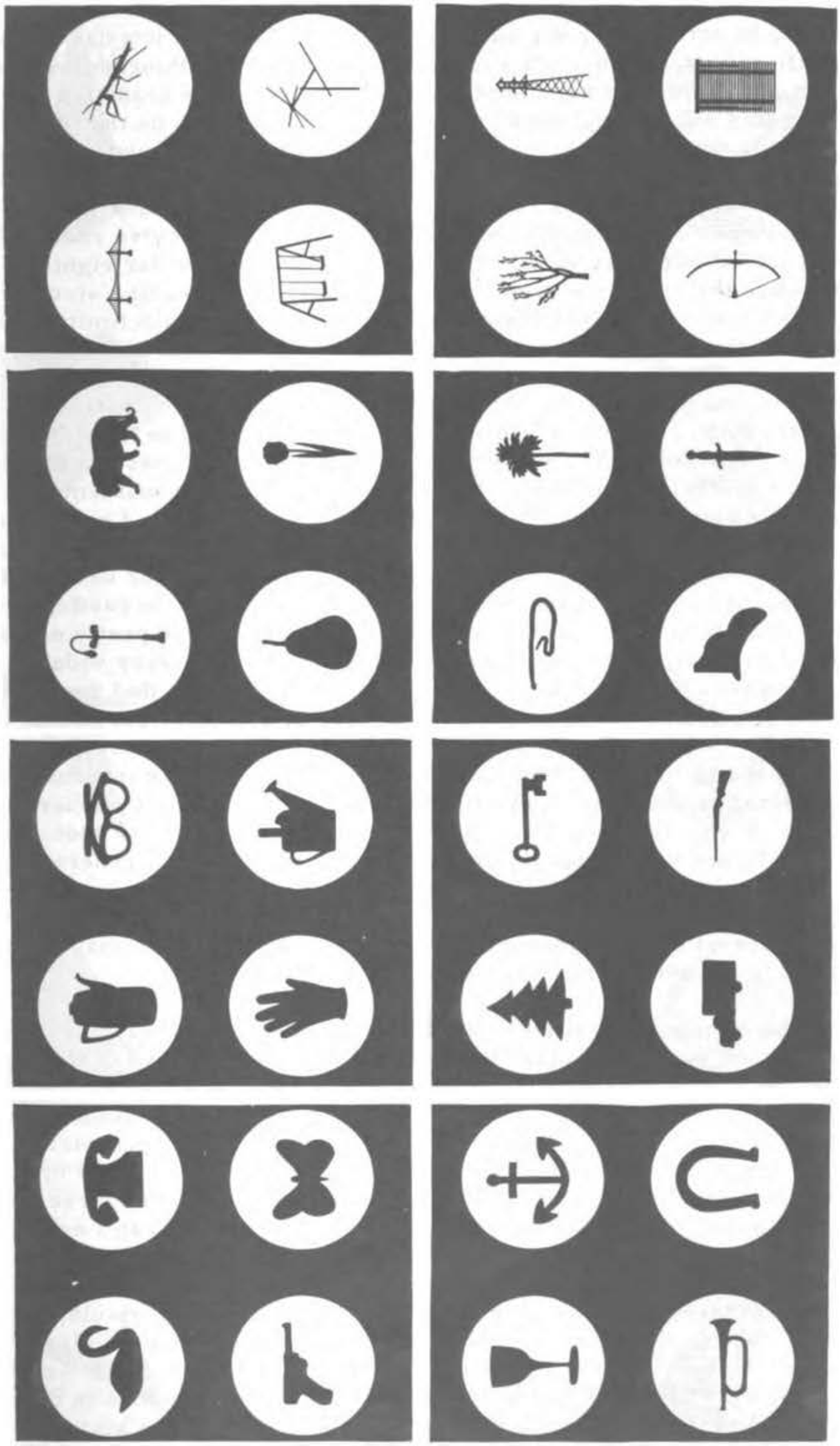


Fig. 4.

direction, is not a very good snake in the first place and it's called so many different things that it's very hard to score. One thing we were aiming at in setting up this sample of material was getting a gradation of difficulty and this we accomplished. The first double column on the left is what we call "easy" forms; the next double column, "intermediate"; the next double column, "difficult"; and those terms we have referred to as the difficulty categories. They're graded on a kind of rough classification for difficulty of recognition under marginal conditions and they're reasonably well separated in this respect. The double column on the far right is what we've called the "fine line" set. We wanted a set that had fine structural detail for comparative evaluation when we were dealing with limiting situations like interactions with acuity and definition in the reproduction system.

We also wanted unfamiliar forms and here we were confronted with an entirely different kind of a problem. I was interested to hear Dr. Anderson's comments on a hypothetical population of all possible forms. We toyed with this conception a little and I wondered what mathematical meaning the idea of sampling such a population would have. I gather that it may have some, but we decided very early that in the kind of situation we were working in it didn't have much practical meaning for us. We had to sample a much more manageable kind of a population. In casting around for ways of setting up forms one thing that became obvious pretty early was that you can't, with any single technique, generate a very widely diversified set of forms readily. The population of forms that you have to deal with is a population that can be generated by whatever particular technique you are using. The so-called stimulus domain is determined very largely by the technique. What you get by connecting points in a matrix of straight lines is obviously very different from what you get by generating ink blots. If you want to get rid of symmetry, take half an ink blot. It still doesn't look very much like the kinds of things that you generate by our more common geometrical methods. So it's pretty obvious, not only on this broad scale, but even when you get down to the specific rules, that you get different kinds of forms to some extent depending on what particular methods and what particular rules you are using.

The method we settled on at that stage was very similar in general conception and even quite close in some details to the method described by Arnoult and Attneave referred to here this afternoon.

In Figure 5 the forms were produced by positioning predetermined numbers of points randomly in a matrix and connecting the points by lines, subject to certain rules. You start out to connect points, of course, but you get yourself into a trap pretty soon. You find that you can't complete the figure so you have to have rules about what to do in cases like that. The rules are quite arbitrary and the only important thing is to be consistent about them and to be able to describe them. The particular type of form will depend, as I say, to a perceptible extent on the particular rules that you pick, but that is not very important at the state at which we are working now. The figures in the top row are four-sided, those in the middle row are eight-sided, and those in the bottom row are sixteen-sided.

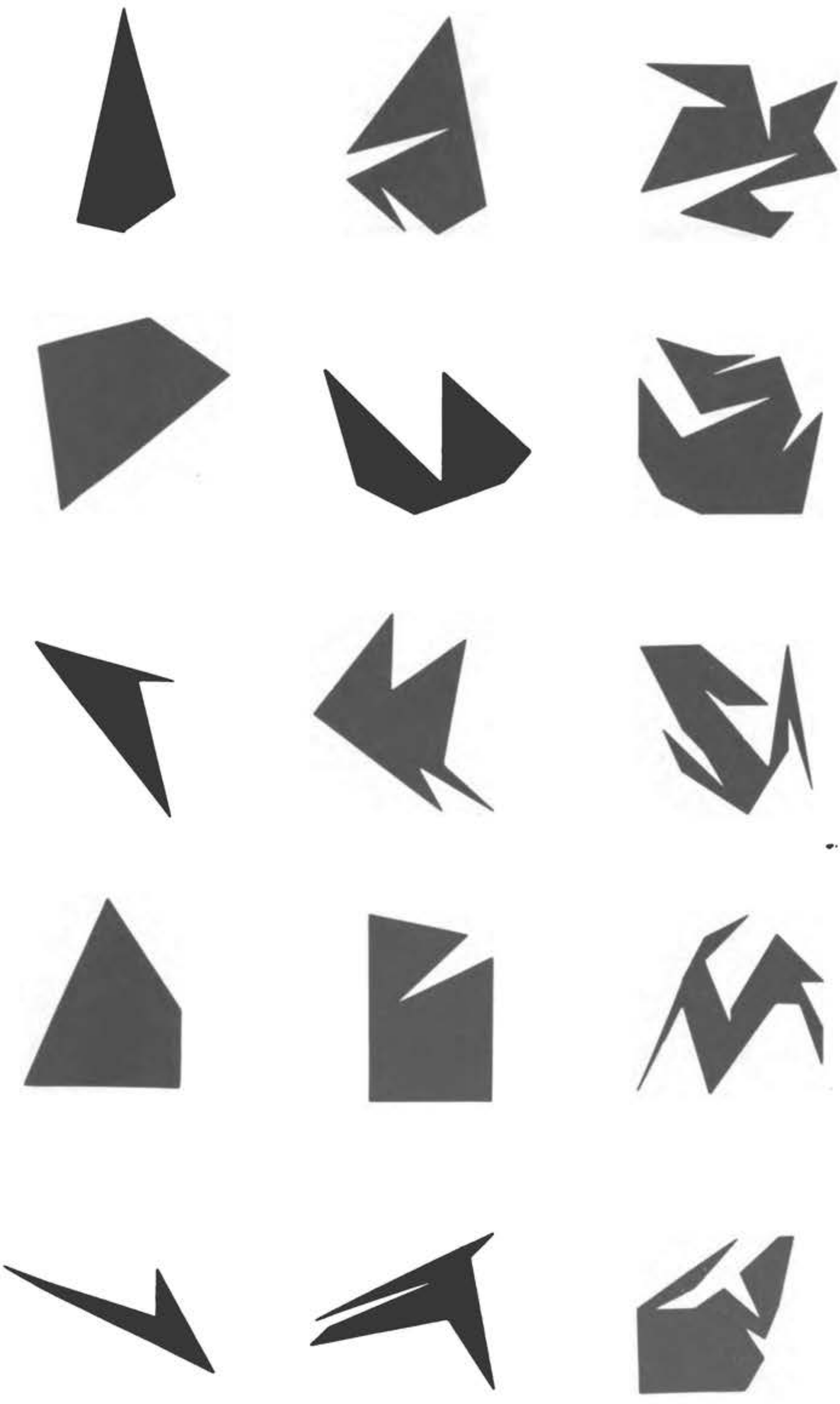


Fig. 5.

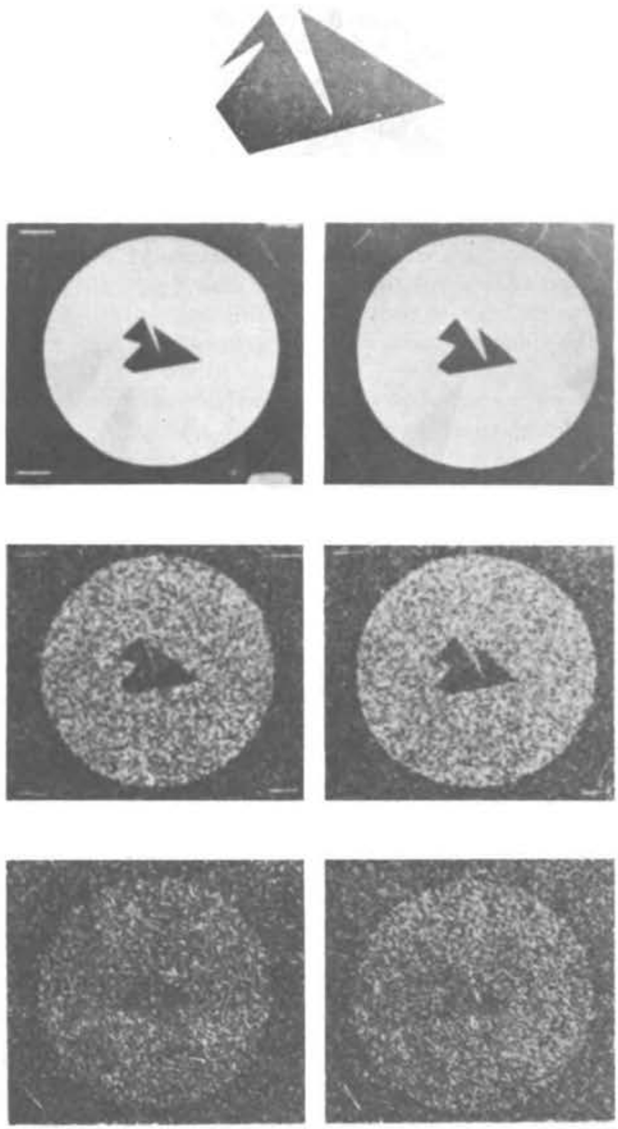


Fig. 6.

If you see one that looks six-sided instead of eight-sided, that's because of the way the dots happened to fall. This is one dimension of complexity which is psychologically meaningful in that you would agree impressionistically that the complexity order is the same as the order of the number of sides. We were interested, as one question in connection with the effectiveness with which people could recognize forms of this kind, in whether there is any simple discoverable relation between complexity and recognizability. We were testing recognizability by matching methods. The subject always had a clear standard to compare. We were usually

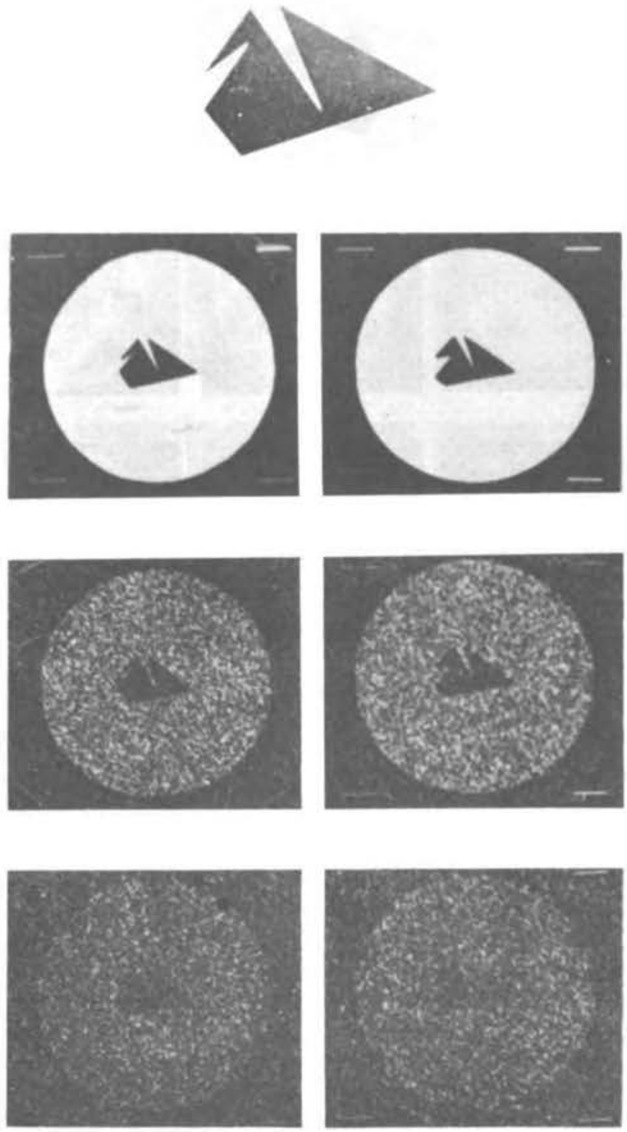


Fig. 6. (cont'd.)

testing by putting the test form itself under noise and asking the subject to say "same" or "different" with respect to a standard. For this set of forms we got results several different times that say that eight-sided figures, that is, those of intermediate complexity in terms of this particular scale, are easier to identify than the more or less complex. So far as these particular data go you would not conclude that there is a linear relation between complexity and recognizability. You may have an optimum. The reason I'm a little cautious about stating this result is that we don't have it on a large enough population of forms. I think we have it on a large

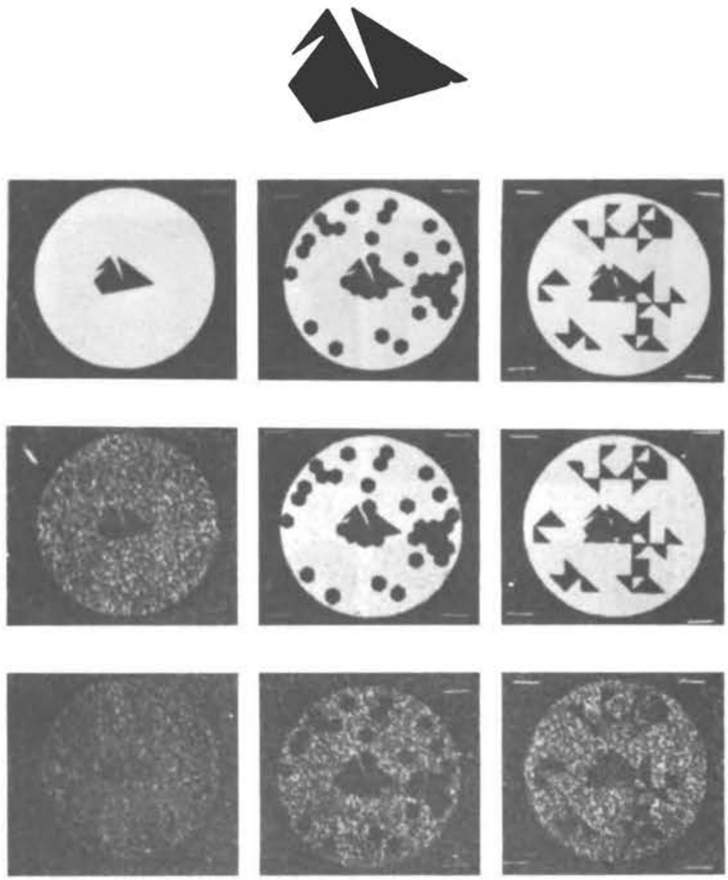


Fig. 7.

enough population of people, but we need to try it on more forms before we'll know whether it's going to hold up.

One other thing that we introduced into this particular system was a method of producing modified steps. Figure 6 shows one form which is presented in four stages of modification from the original; the first one being zero, identical with the original, on the left; the other three progressing to the right. These are produced in the same way other people have produced similar things, that is, by moving predetermined numbers of the points predetermined distances in the matrix. This gives you a method of setting up a kind of a statistical-geometrical scale of similarity. You can, of course, pretty readily get results that are related in some systematic way to degrees of similarity, so defined; but not necessarily related in a linear way. You get some rather peculiar things in the range where you are close to the original and the degree of difference is small, but I don't want to go into a discussion of results here this afternoon.

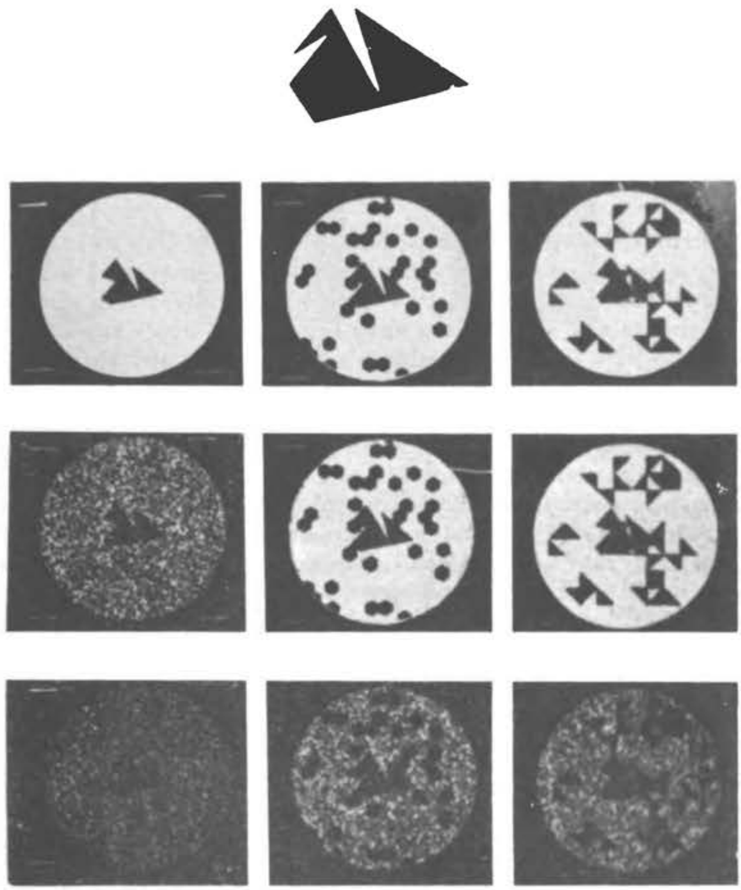


Fig. 7. (cont'd.)

We've developed a lot more forms, including some with curved edges, which we haven't had time actually to use in any experimental comparisons. We may be able to find out whether Blackwell is right about this business of straight and curved edges now. I think we have some kinds of materials that might possibly be applicable there.

In this general process of generating materials we have also been trying to develop a somewhat analogous method setting up statistically controlled backgrounds.

Figure 7 shows a variety of testing conditions for one original form. There are two modifications of the form. The whole left-hand side is the original and the whole right-hand side is one modification of it, combined with different degrees of noise. We have been calling those larger spots "background", which seems like a rather ambitious name for them, but that is as far as we have got. You might equally think of them as clutter or as another level of noise. We're going on with the business of making backgrounds this way by a process of using contingent probabilities. The original spots are put in there by randomization method. They sometimes fall into clusters and sometimes don't. We're producing clusters by contingent probabilities of varying degrees and we've got quite a set of additional backgrounds that give you a much broader coverage of varieties of patterns, but I do not have any figures prepared to demonstrate these.

One further development here which we would like to carry through is introducing a brightness differentiation among elements of the background. Of course, in all this you're back to point-by-point hand manipulation of the elements and it becomes very tedious. I don't know how far we will be able to go with it, and I wouldn't want to make any bets on it, but that's one thing we're playing with at this stage.

Now, to change the subject slightly. When you get to dealing with this kind of material you have a lot of control of contrast within the display, but by no means as complete control as you might want for some purposes. Any material on reflecting surfaces, of course, has very definite limits to the kind of contrast that you can get. Particularly on this facsimile paper, the limits are much narrower than they otherwise might be. So, we were interested in developing a system which would give us more complete and independent control of the contrast of figure and ground areas and Mr. Hanson's going to describe that.

TWO OPTICAL SYSTEMS FOR CONTROLLING CHROMATIC AND ACHROMATIC CONTRAST IN FORMS RESEARCH

John A. Hanson

I have a couple of optical systems to describe. The reason for one of them, as Dr. Crook mentioned, was to provide higher contrast than we could achieve with the reflective materials such as we had with the facsimile equipment. The types of systems that we have developed are shown in the hand-out sheets. One of them was designed for contrast studies of form materials; the other one has been designed for the presentation of acuity targets using different colors of target and background. When attempting to present colored targets on different colored backgrounds, using optical systems, you are usually up against several limitations. A common method is to superpose the target on a background, but when you're using colors you then get an additive color mixture. For some problems there is the additional requirement that the brightness, saturation or wavelength of both the target and the background be independently variable. As now worked out in both systems, brightness of both figure and ground is continuously variable.

In Exhibit A, up in the top of Figure 1 we have a form, say, a circle on a light ground. With these optical systems the brightnesses can be varied continuously so that if we call the first figure on the left a positive they can be continuously varied until you have arrived at a negative. This is done without changing the stimulus slide.

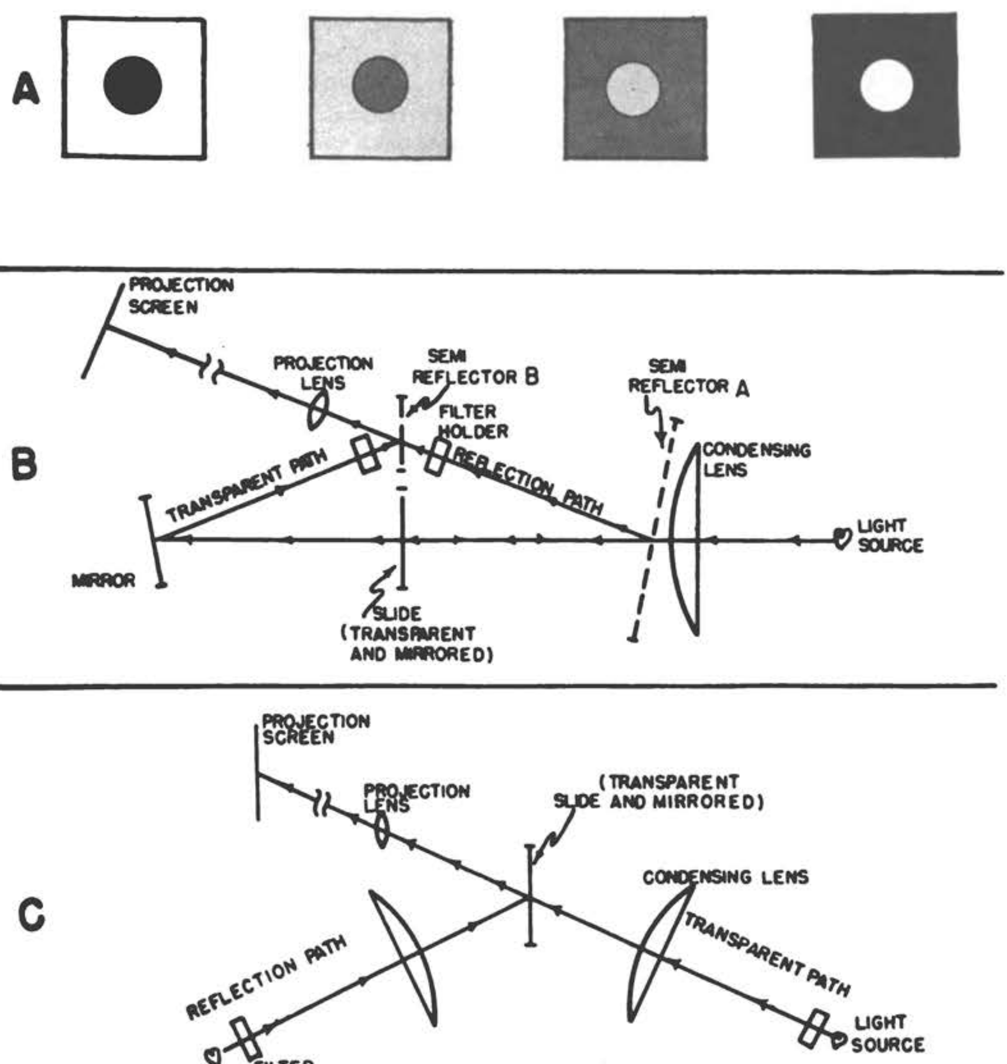


Fig. 1.

The main gimmick in both of these optical systems is a different type of slide. The slides that we use differ from regular slides, say a regular lantern slide, in that a regular slide is in general transparent and opaque. We'll forget about gray scales for this discussion. In the case of a high contrast slide, portions of the slide are either fully transparent or totally opaque. The slides we used are transparent and mirrored. I'm sure you can't tell that this is a mirror from any distance but I have a few samples up here that people can look at if they are interested.

The method of getting forms in this mirrored state is one we have developed by borrowing some of the tricks of the photolithographic plate maker. That is, we put an emulsion on glass, then expose the emulsion through a film positive using ultra-violet light. When this is put through what is akin to a developer the part that has been exposed to ultra-violet light does not dissolve and the part that has been shielded does dissolve. Where we want the form we have clear glass with an emulsion mask around the form. This is then sent out to be commercially silvered. We get it back and dissolve away the mask, leaving just a mirrored form on clear glass. Actually we're able to get resolution that is quite good and we can handle fine-line forms.

Going now to the diagrams themselves: the first system we developed was the one for controlling contrast. We hadn't been thinking of it in terms of the color problem at that time. It is the optical system which is labelled "B". I don't know whether the lines and arrows are particularly helpful or confusing. Starting with the light source the light is collected by the condensing lens and as the light comes out (following now from right to left) it goes through a semi-reflector, which we've labelled "A", and then comes to the slide. Now, the light that hits the transparent part of the slide will continue on through and is reflected from the mirror on the left. It goes through a filter holder, or variable polaroid, or neutral density wedge or what have you as an intensity control. The light rays then reflect off the semi-reflector "B" to the projection lens and then to the screen. Going back now, the light that is coming towards the slide which hits the mirrored portion of the slide is reflected back to the semi-reflector "A". The rays reflected from semi-reflector "A" then pass through the intensity control and semi-reflector "B" to the projection lens. If all four of these components, that is the semi-reflector "A", the slide, the mirror, and semi-reflector "B", are in proper registration you can control the figure and ground brightnesses independently. Once the slide holder is suitably built and you have your mirrors lined up, it doesn't require a registration with each slide so long as each slide goes into the same plane. The images will be in registration when they hit the projection screen.

The second system is labelled "C". It was designed in relation to our color and acuity problem. It could be used for form discrimination or what have you. It differs from the previously described system in that it was an attempt to try to prevent some of the light loss that is in the other system, by getting away from semi-reflectors. It was also an attempt to get away from the registration problem of the four components in the other system. There's nothing between the slide and the projection lens. Therefore, the registration just takes place automatically at the slide. In the diagram, going first to what is labelled the reflection path, which is the one on the left, the light goes through a filter holder, which would consist of color filters, and a neutral density wedge, and then, of course, through the condensing lens. The light which hits the mirrored part of the slide would then be reflected to the projection lens. The light that hits the transparent part of the slide would go through and just be a light loss. In the transparent path the light which hits the mirror is just reflected to the side and lost and that which passes through the transparent part of the slide proceeds to the projection lens and then to the screen. Actually, of course, just plain glass does have a reflection factor and therefore when used with a glass slide the two paths are not quite independent in that you will get, say, 10 percent reflection off a glass surface. Actually, for the acuity project, where the targets are fairly standard geometrical forms, we actually have the targets as little mirrors not mounted on glass; that is, the transparent part is just air.

DISCUSSION

RAPPAPORT: I found Dr. Crook's paper very interesting, especially the fact that he is investigating complex figures under different noise conditions. I think many of us can see the relation between this problem and some other problems we are trying to solve for the military. I wonder, Dr. Crook, if you would say something about the discriminability of some of your complex figures under different noise conditions and also about how different types of noise interact with certain types of discriminable figures.

CROOK: With regard to the first question: about as far as we have got with differentiating different types of complex figures is to establish that you can get data curves which are more or less systematic and more or less reproducible. We can differentiate, to some extent, among degrees of complexity as I indicated. At least at this stage, the data seem to show that you have kind of an optimun of complexity, as measured in this simple way, in terms of the number of sides. The only other aspect of the actual complex forms we have attempted to vary systematically is the matter of degrees of similarity as determined by the business of slipping points. Again, you get systematic relations between the form characteristics as defined in this way and the recognizability. This is a kind of objective scale of similarity. Just what is possible to do with it, I don't know, but it has at least a superficial relevance to various kinds of operational situations where you have to match one kind of form with another.

RAPPAPORT: Is it true that in the case of four or six-sided figures, the one with more sides is more easily discriminable in noise than the one with the fewer sides?

CROOK: Well now, all the discrimination data that I've been mentioning are under noise. What we can say about the particular set of forms that we've tested is that the eight-sided figures are more discriminable than the four or the sixteen-sided, but that's as far as it goes. I want to do this with a broader sampling of the complexity dimension and on a larger variety of actual forms.

FRY: I'd like to ask a question about the inherent pattern in the noise; that is, if you take an ordinary photograph, say a transparency, and enlarge it so that you get the dots visible you can see under these circumstances a very definite pattern inherent in the statistical distribution of dots such as you get in a photographic scale. I take it that the same thing occurs in any kind of a statistical variation of dots such as you have. Now, in your picture arrangement, as I understand it, you have the lines scanned so that you have a regular pattern in one dimension and an irregular pattern in the other. I wonder if you run into any particular problem here? It looks to me as if you'd have a kind of astigmatism introduced.

CROOK: You do. This is related, of course, to the definition of the system. The particular facsimile equipment that we were using was originally set up with a definition of 104 lines per inch. It was modified to 52 for the purpose of the job we were on. The dots aren't quite square, they're little parallelograms in effect. When you have contours that make it possible to get a clean dot, you get a little parallelogram but characteristically you don't exactly get a clean dot. Lines that run parallel to the scanning line may look fairly sharp. Lines that run diagonal to the scanning lines may have jagged edges. When you're down at a level where this limit of definition is relevant, then there is definitely an astigmatism effect.

FRY: It is not so much in that direction but in the other direction that you get . . .

CROOK: You mean vertical, as opposed to . . .

FRY: Vertical as opposed to parallel to the scanning line. There should be some inherent variables and patterns here which would interfere with the visibility. In other words, if you have a bordered image of an object that you're trying to recognize, and superimpose upon a distribution of dots which you have on a matrix pattern, then these two patterns would be competing with each other.

In discussing Dr. Crook's paper I mentioned the pattern inherent in a statistical distribution of points such as is supposed to represent developed grains in a photograph of a uniform surface. I was inquiring if anybody had studied possible confusion of this inherent pattern with non-uniformities representing real objects. *

The type of inherent pattern which I refer to is illustrated best by a quilt pattern (see Figure 1) made by putting together a number of squares which are duplicates of each other but arranged so that their edges match.

*This and the following paragraph and figure were added by Dr. Fry after the symposium to clarify the discussion.

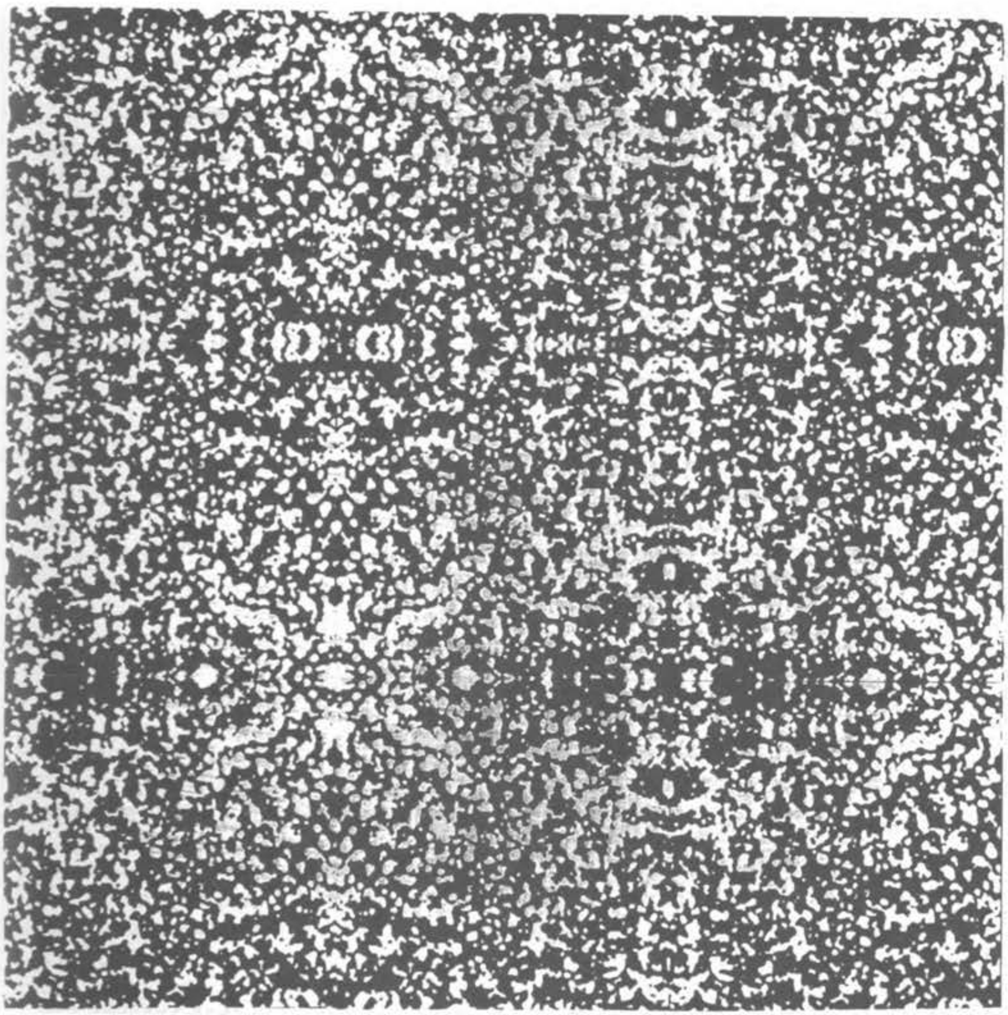


Fig. 1.

Essentially the same kind of thing can be done with a kaleidoscope. Each square in Figure 1 is an enlargement of a section of developed photographic film which has been uniformly exposed. The inherent pattern is quite obvious when viewed in this way.

CROOK: Now, I don't know whether I can answer this question technically or not. If you look at copy that has no noise superposed, the vertical edges look cleaner than the diagonal edges. They may not look quite as clean as the horizontal edges, but the resolution in the two directions is generally comparable. It's particularly in one diagonal direction, to a somewhat less extent in the other diagonal direction, that you get this impairment resulting from this so-called astigmatism effect. As soon as you begin to put on a little bit of noise, the disturbance from the noise pretty soon overrides anything that the printing system itself does.

WEISZ: I think part of the answer here is that the frequency response of the system going along a line is roughly comparable to the definition vertically. Something like a fiftieth of an inch.

CROOK: It's limit of resolution is supposed to be the same as that in the two directions.

HARCUM: I wonder if Dr. Weisz or Dr. Crook could explain how this random series of dots that you used for your mask differed from, let's say, looking at the same targets through a neutral density filter or in some other manner decreasing the illumination in the surrounds.

WEISZ: Are you talking about the artificial noise?

HARCUM: Yes.

WEISZ: We introduced the speckling that I think you cannot get simply with a neutral density filter.

HARCUM: What order of size?

WEISZ: Well these are supra-threshold, certainly.

BOYNTON: Certainly the individual effect, though, of introducing this masking noise is very much the same as that of adding a veiling stimulus to the field. It looks to me like reduced contrast, in some sense.

CROOK: It looks like more than reduced contrast with at least some sizes and kinds of cut objects. You get the contours chopped up much before contrast reaches the stage where it alone will impair the recognizability.

HARCUM: Excuse me. In what sense are the dots that are not contiguous to your figure relevant to the recognition of the form of your figure?

CROOK: They introduce a contrast effect. There's no implication that that's not relevant. I think two important things happen: reducing contrast and degradation of the edge gradients.

TANNER: I'd like to know how many different masks you have for each noise level.

WEISZ: Essentially one, but you can fiddle the components relative to one another so that it's not always static, you see. It's not just one mask.

BOYNTON: I'd like to ask Mr. Hanson a question. It's a very tricky system I think you have there, making mirrors out of your figures. Is it good enough to get rid of any visible edges between the figure and the and the background completely, assuming that you have equal luminance and no color difference?

HANSON: In the system labelled "B", it is difficult to get registration good enough so that you can't see a contour, even when you have the main body of the figure and ground of equal luminance. In the other system, you can much more closely approximate this, although I wouldn't say that you can completely get rid of it. Part of the difference might be that you might get an impression of a gradient or edge just because of a slightly different color component due to the slight selectivity of the mirror.

BLACKWELL: No! It's impossible there because of the size of the source. We did this about 15 years ago and instead of using the mirrored flat object, we made it three-dimensional and stuck it up like a mushroom. We made a stellite cone so that the object form was at the end of the little cone. I grant that this is not a real irregular form but then you can do it because you can avoid the penumbra effect which comes about because of the lack of a point source. I'm convinced that you cannot lick this if you're using conditions where the eye can discriminate well. You can't ever lose the boundary for a number of reasons but the stellite tubes will do it.

GILMORE: Depends on the quality of your collimator, doesn't it?

BLACKWELL: No, the source will always be unfortunately not a point and no collimator can collimate other than one point at a time.

BOYNTON: Well, theoretically you should be able to get close enough to a point source so that the difference would be so slight as not to be visually detectable.

FRY: I have two comments on this. First of all you have a problem in this kind of a system with lack of coherence in the two divisions: the same kind of a problem you run into with a bar prism in a photometer in which your mirror gets rid of the dividing line. The problem here is that you have a non-coherent source and you get the gradient at the edge, so that the intensity of the edge or illuminance at the edge would be one fourth of the maximum. So you're always going to get little black lines or dots down the center of the field. You have exactly the same problem in connection with this system.

The other comment I want to make is in the form of a question. I wonder what kind of a problem this system would be applied to? Many years ago I think it was agreed that in this kind of experiment it is much better to have a uniform field and then superimpose upon this the ΔB. Then you know what the ΔB is, always, because you can control that independently of the backgrounds upon which you are superimposing. Then, when you want to get to, say, dark on white you use two projectors one for the black object on the white screen and one on the white screen to superimpose on the form. You always have perfect control then of the ΔB and I just wonder what kind of a special application you have because you have two projectors anyhow. Why not just put them straight on the screen and save them running through a mirror?

HANSON: If I understand your question completely, for just the monochromatic situation you can achieve the same result by having two projectors and have control of background and your target. However, the chief situation where this complete separation is desirable is where you are working with color, so you're not faced with the situation of having an additive color mixture.

WEISZ: Let me just add to what Mason Crook had to say on the technique of taking a standard figure and varying it in steps of 25 percent variation of the points that constitute a figure through varying degrees of difference. One of the reasons for using this as a technique was to beat the problem of the framework of discrimination that has been mentioned here a couple of times previously. You can set it up, you see, so that you instruct your subject that 50 percent of your figures, your comparison figures, will be the same, 50 percent will be different. Then it is a question of requiring him to make a same-different discrimination without any specific group of comparison figures. He needs no knowledge of his alternatives. What you're testing, rather, is the discriminability of the two comparison figures without a well-specified class of comparison figures. This is, I think, a help to make the data that come out of any particular experimental situation a little more generalizable.

GILMORE: It seems to me that one of the interesting applications of this facsimile equipment you have would be to project noise having different electrical characteristics on to the same stimuli, perhaps in the electrical paths in the system to see what effect this has on legibility or recognizability. This has direct application to a number of military problems and I wonder if you have done anything with this kind of form?

CROOK: The project in which we were originally using this started out with Gaussian noise and that constituted the bulk of the experimental variable that we were manipulating in that respect. At a later stage the question came up, would it be worth while, since this was a radio problem, to introduce multipaths, or multipath interference. A variation of this general system was set up, not in our laboratory, but in the laboratory of the company we were subcontracting from, and a multipath simulator was developed which introduced into copy a more streaky effect, definitely not the kind of a picture you get from the Gaussian noise. So far as I was able to get any impression of it, it was a pretty good simulation and probably had some value in producing results that were applicable to the actual operational situation. That's the only variation of that kind we've had any experience with.

WEISZ: The only point that can be added here is that there were some variations, I guess of the band pass filters in the system, that varied the granularity of the noise over a four to one ratio with very little difference in legibility as far as we could see in very informal tests.

GILMORE: This was a low pass filter... ?

WEISZ: You've lost me, but I think so.

BOYNTON: I think we all owe a big vote of thanks to our nine speakers of this afternoon for a very interesting program.

SESSION III

THE RELATION OF FORM TO VISUAL DETECTION

Chairman: John H. Taylor

TAYLOR: One of the really big problems, it seems to me, that has confronted those of us who've been working to apply basic visual data to practical problems has been the kind of thing Dr. Blackwell and others have mentioned already today. The fact is that we seem to have a great deal of very good quantitative data for relatively simple situations. I'm thinking, of course, of the data typified by the Tiffany results, where one knows, by dint of much hard work on the part of Dr. Blackwell and others, a great deal about how one can be expected to behave when confronted with simple circular targets on uniform backgrounds. But, there is a big stumbling block immediately. If one tries to predict from these data to more complex situations, even a little bit more complex, it becomes a problem. Dr. Blackwell has said many times that the Tiffany results are just dandy for predicting visibility of flying saucers, but that's about the end of it. Fortunately, recently Dr. Kristofferson of Michigan has made some of these targets a little less circular and we'd like to hear very much a sort of a status report on what Dr. Kristofferson found out about non-circular targets. Kris, will you take it from there?

VISUAL DETECTION AS INFLUENCED BY TARGET FORM*

Alfred B. Kristofferson**

The ability of the human observer to detect the existence of a visual target is well known to depend strongly on a multitude of variables. One broad classification of the relevant variables places them in three groups. Briefly, these are: (1) characteristics of the target, that is, the entire energy change with which the detection response is correlated, (2) characteristics of the background stimulation, and (3) the state of the observer, determined by past stimulations, instructions concerning the target, etc. Most of the data presented here are relevant to the first category, target characteristics; however, some belong in groups 2 and 3. As a result,

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*Supported by Department of the Army Contract DA-36-039-SC52634 with the Engineering Research Institute of the University of Michigan, operating under a Tri-Service Charter, administered by the U. S. Signal Corps.

**With H. Richard Blackwell and Stanley W. Smith.

the data will be quite representative of our recent research efforts in visual detection theory.

Among the variables which have been explored most thoroughly are target form, duration, and luminance, and background luminance. Target form, which refers to size, shape, orientation, and position, forms the body of this report, although some effects of the other variables will be discussed also.

These investigations of target form have been guided by a theory of spatial summation which has been discussed before the Optical Society on two occasions and which is soon to be submitted for publication. The theory is designated an "element contribution theory of spatial summation" and, although conceived in neural terms, its mathematical expression is in psychophysical terms and, as such, it incorporates all sources of spatial interaction, those present in the formation of the retinal image as well as those contributed by the central nervous system. The separation of these two levels of "interaction" awaits development of convenient methods for specifying retinal illumination distributions produced by target objects.

The element contribution theory* asserts that the detection of a target occurs whenever the amount of excitation at any point in the neural excitation pattern produced by the target exceeds a critical value. Each point within the neural pattern receives excitation from every retinal point stimulated by the target. To each retinal point there corresponds a neural locus. The amount of excitation contribution to any neural locus from any retinal point is a function of the distance between the retinal point and the retinal point corresponding to the neural locus. This contribution as a function of retinal distance is the element contribution function. Targets which are symmetric and presented with their center at the point of fixation will be detected whenever the amount of excitation at the center of their neural pattern, which is modal excitation for a symmetric target, exceeds the criterion amount. Most of the data to be discussed concern symmetric targets having single, centrally situated, excitation modes. These are called uni-modal targets. Also, all targets are restricted to the relatively homogeneous central one-degree of the visual field.

It is a consequence of the above theory that the target luminance required for detection decreases as target size increases. The theory also demands that, when area is held constant, the shape which maximizes detectibility is a circle; when detectibility in terms of threshold contrast is plotted against target area, all non-circular targets should fall above circular targets on the threshold contrast axis.

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* A description of this theory is being prepared for publication under the title "A Neural Formulation of the Effects of Target Size and Shape upon Visual Detection" by Kincaid, W. M., Blackwell, H. R., and Kristofferson, A. B. This theory represents an extension of the theory of retinal summation proposed some years ago by Graham, C. H., Brown, R. H., and Mote, F. A., Jr. (J. Exp. Psychol. 24, 1939).

As already mentioned, the theory is developed quantitatively. The procedure requires the determination of the shape and relative magnitude of the element contribution function from one set of detectibility data homogeneous with respect to shape. The simplest derivation is in the case of circular targets. Thus, $\phi(R)$, the contribution function, is determined from the area function for circular targets and used to predict contrast thresholds for non-circular targets.

Certain invariant experimental procedures employed in the following experiments should be mentioned. Contrast thresholds for each target are routinely determined by measuring detection probabilities at each of five target luminance levels by the temporal forced-choice method. Threshold contrast, $\dot{C}$, for any target is defined as

$$\dot{C} = \frac{\Delta \dot{B}}{B} \quad (1)$$

in which $\Delta\dot{B}$ is the target luminance which must be added to the background luminance, B, to produce a detection probability of .50.

The observers view a large white plastic screen which is uniform over the central 25° of visual angle except for four dim, one minute diameter points of light which direct fixation. In foveal experiments, the target is presented as a positive luminance increment through the screen in the center of the diamond pattern formed by the orientation lights. Details of the automatic presentation apparatus and the devices for recording and scoring responses have appeared in the literature. In all of the experiments to be reported here the targets are of uniform internal luminance, although some data have been obtained on targets containing internal luminance gradients.

White light at 2600°K is used throughout and, in all experiments, viewing was done binocularly with natural pupils. The question of interaction between the eyes in detection behavior, which is not directly relevant here, is another currently receiving attention in the Vision Research Laboratories. Unless noted otherwise, observers always have full knowledge of target form.

The basic data are those on circular targets.

Customarily, the area function for circular targets is plotted as log area versus log threshold contrast. Figure 1 shows an interpretation of such data in terms of the element contribution theory with the relative contribution to detection per unit area plotted against distance from the center of fixation. Within some small radius, all elements of area contribute equally to detectibility, their relative contribution being 100 percent. This is the range of Ricco's law. As elements of area are added beyond this radius, their relative contribution falls off rapidly at first, then more slowly, a measurable contribution being made out to the limits of the rod-free fovea. The two sets of data were obtained under identical experimental conditions except for background luminance, the dashed curve being derived from data at zero background, the solid curve, ten foot-lamberts.

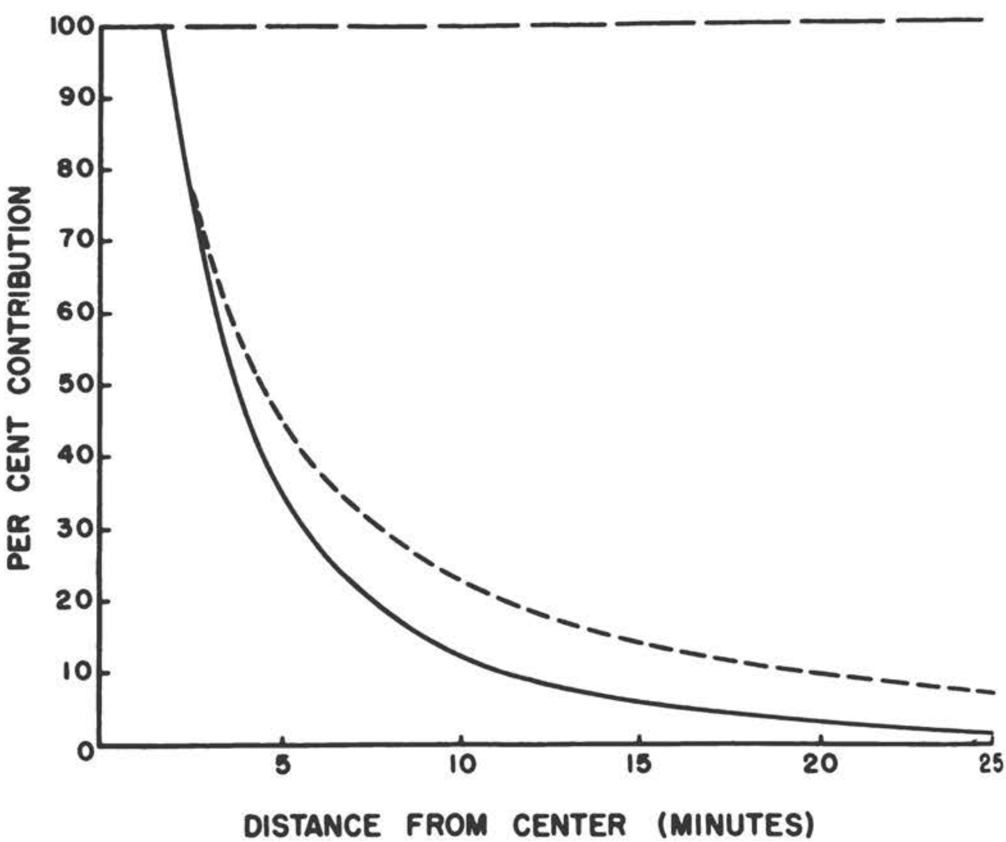


Fig. 1. Element contribution functions, zero and 10 foot-lamberts.

It is especially noteworthy that even in the central fovea the extent of spatial summation is much greater at zero background, implying that even in this retinal area, where projection is usually assumed to be point-to-point, all blurring is not due to blur in the retinal image.

Figure 2 shows some of the rectangular targets which have been used, the particular sample displayed here being of constant width, one-minute, and increasing length. In some studies a much more complete sample of rectangular targets has been used, including those made up of all combinations of the seven dimensions shown in this figure.

In order to bring out the effects of target shape and to test the theory as extremely as possible, targets as markedly non-circular as possible within the limits of symmetry and uni-modality have been employed. Some of the most extreme forms are the multi-legged targets shown in Figure 3. Those in the upper row are crosses, and those below are referred to as "spokes". The effects of the number of rectangular units, the angle between them and the over-all linear dimensions of the target have been explored.

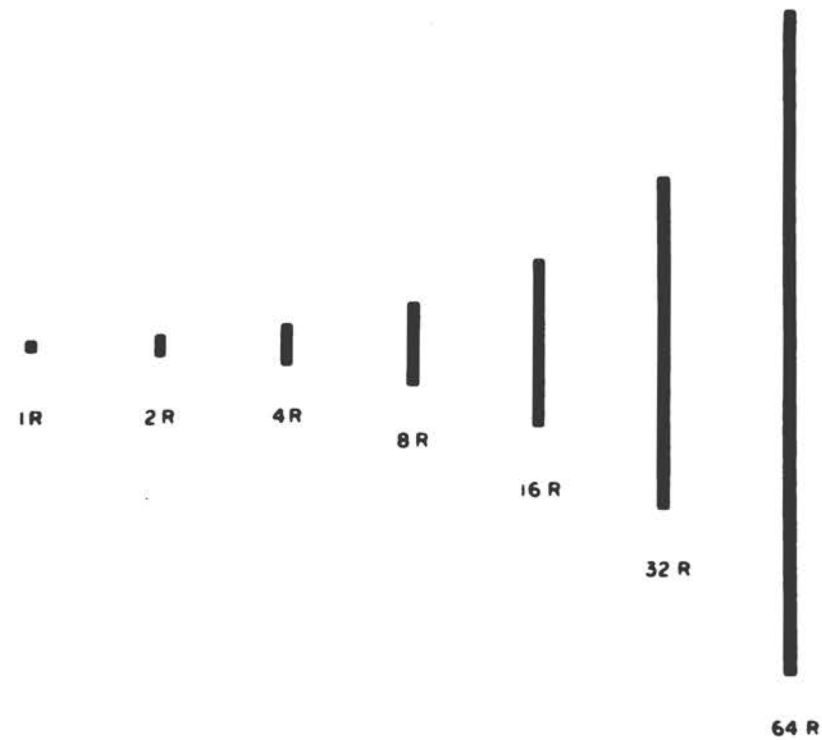


Fig. 2. Sample of rectangular targets.

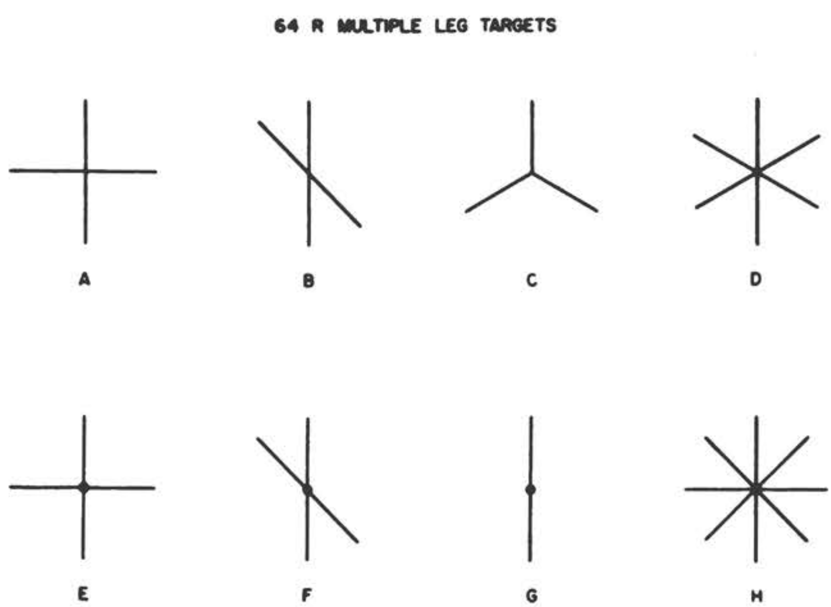


Fig. 3. Sample of multiple-leg targets.

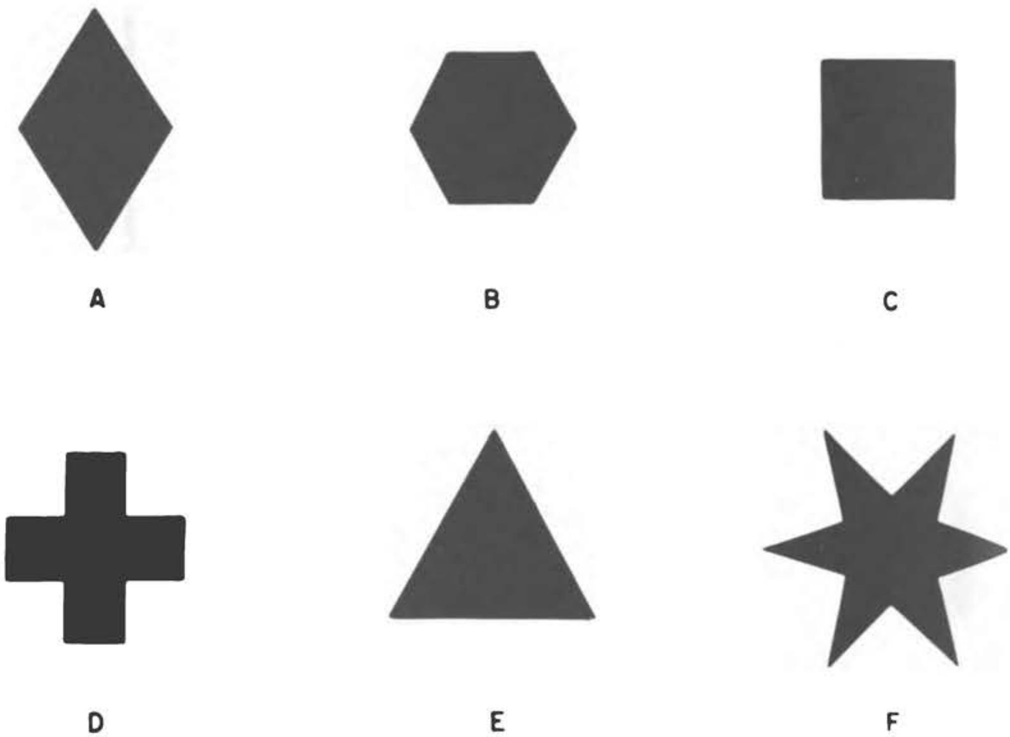


Fig. 4. Geometrical form targets.

Several simple geometrical forms, shown in Figure 4 have been used. These were of one size only, each being of the same area as a 32-minute diameter circular target.

A summary of the data for all targets run at a background of 10.0 foot-lamberts and an exposure duration of .010 seconds is displayed in Figure 5(1). The quantity ϵ_c is the log unit difference between threshold contrast for the target and threshold contrast for a circular target of the same area. The horizontal line represents no departure from the equal area circle. Note that nearly all departures are positive which means that, as the theory requires, non-circular targets have higher thresholds than circular targets of the same area. The multiple-legged targets depart much more than the rectangular targets. The geometrical forms differ very slightly, the one high point being the star. Although it cannot be inferred from the figure, the departures for rectangles are closely related to their dimensionality or length-to-width ratio as predicted by the theory. The "more rectangular" the target, the less efficiently utilized is the energy it contains. The highest point in the figure for rectangular targets is for the 1 x 64 rectangle. For all targets, the mean log unit decrease in detectibility due to shape is 0.17. This number, of course, depends strongly on the exact sample of targets selected.

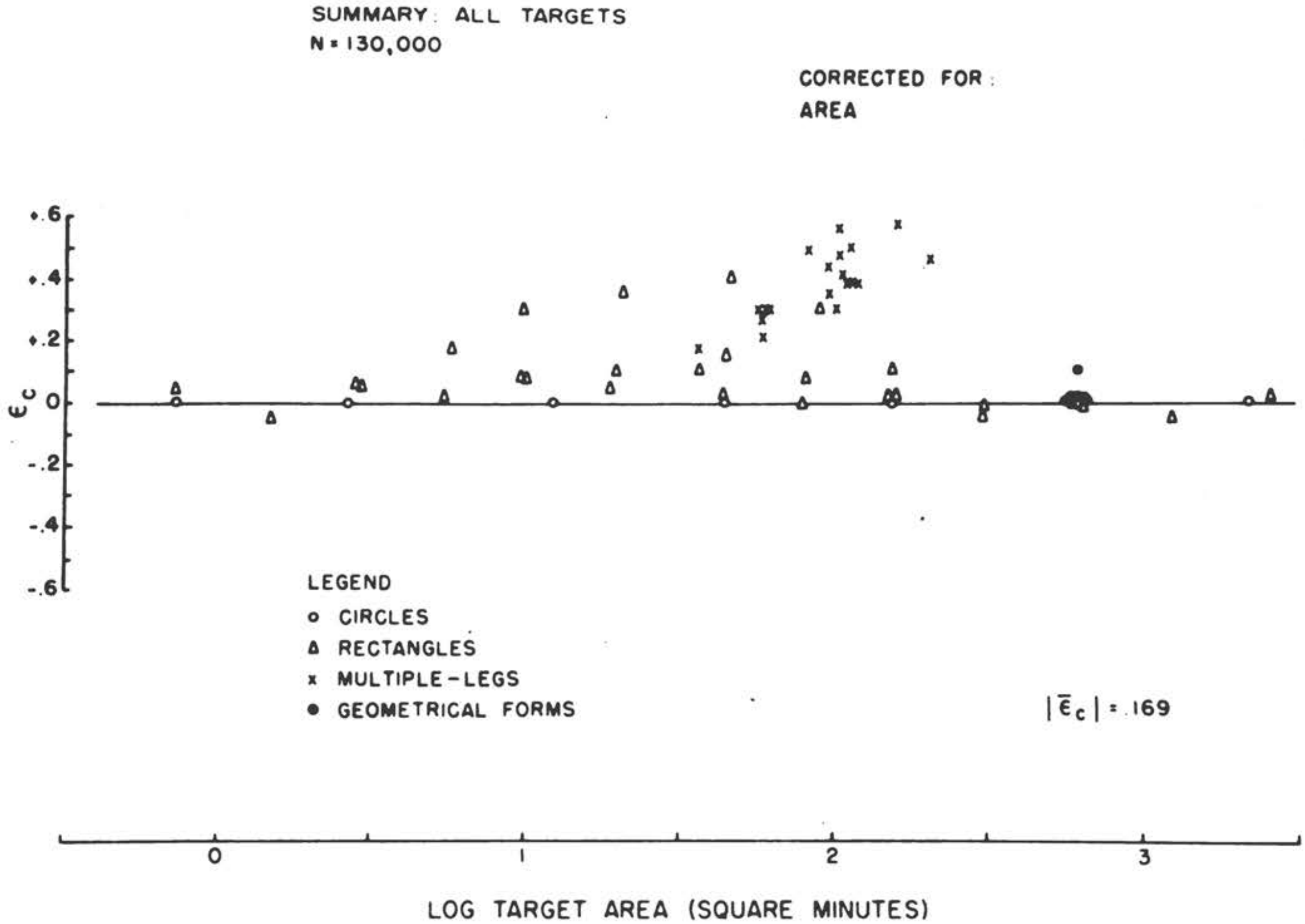


Fig. 5. Deviations from equal area circle, all targets, 10 ft.-L.

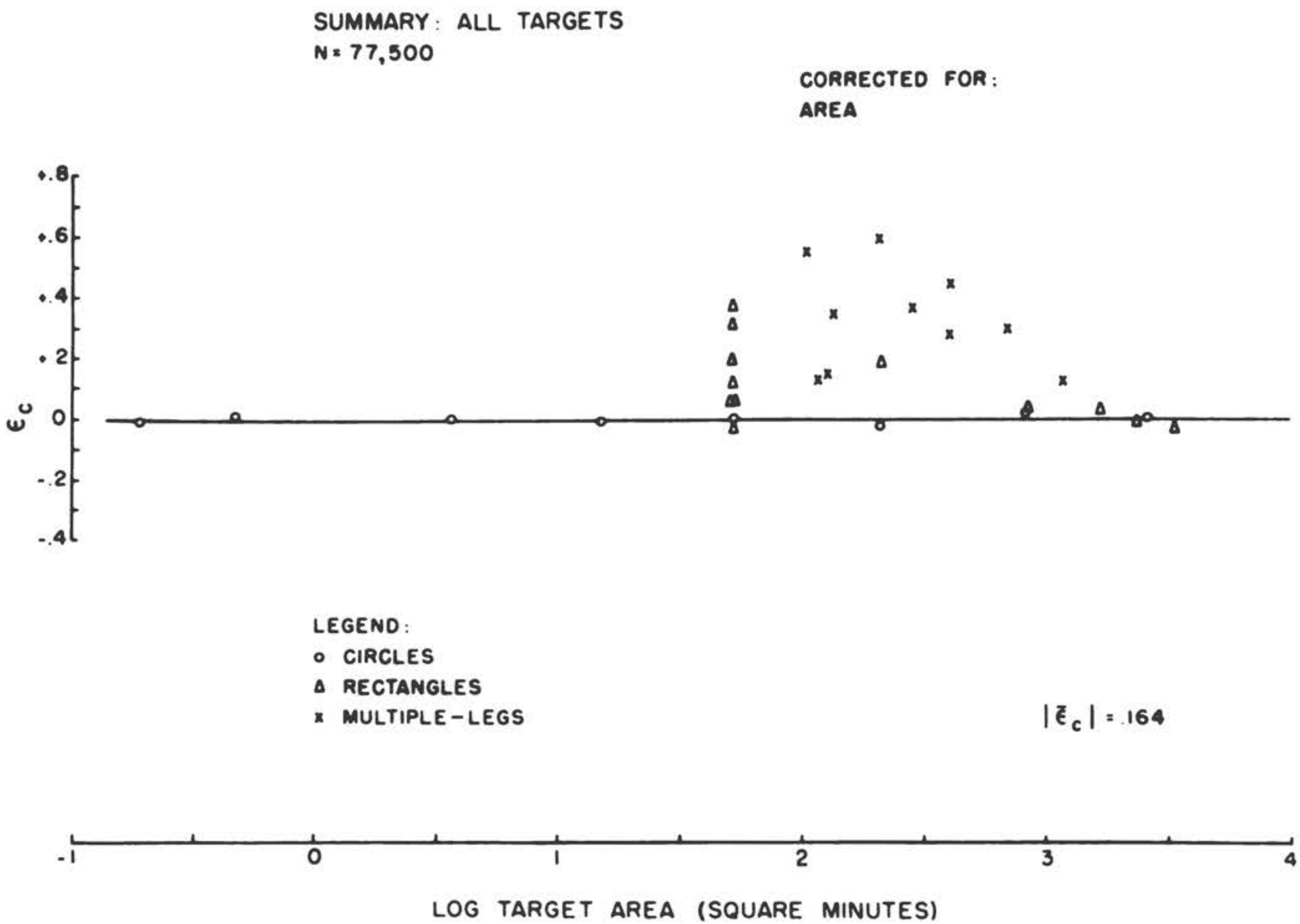


Fig. 6. Deviations from equal area circle, all targets, 0 ft.-L.

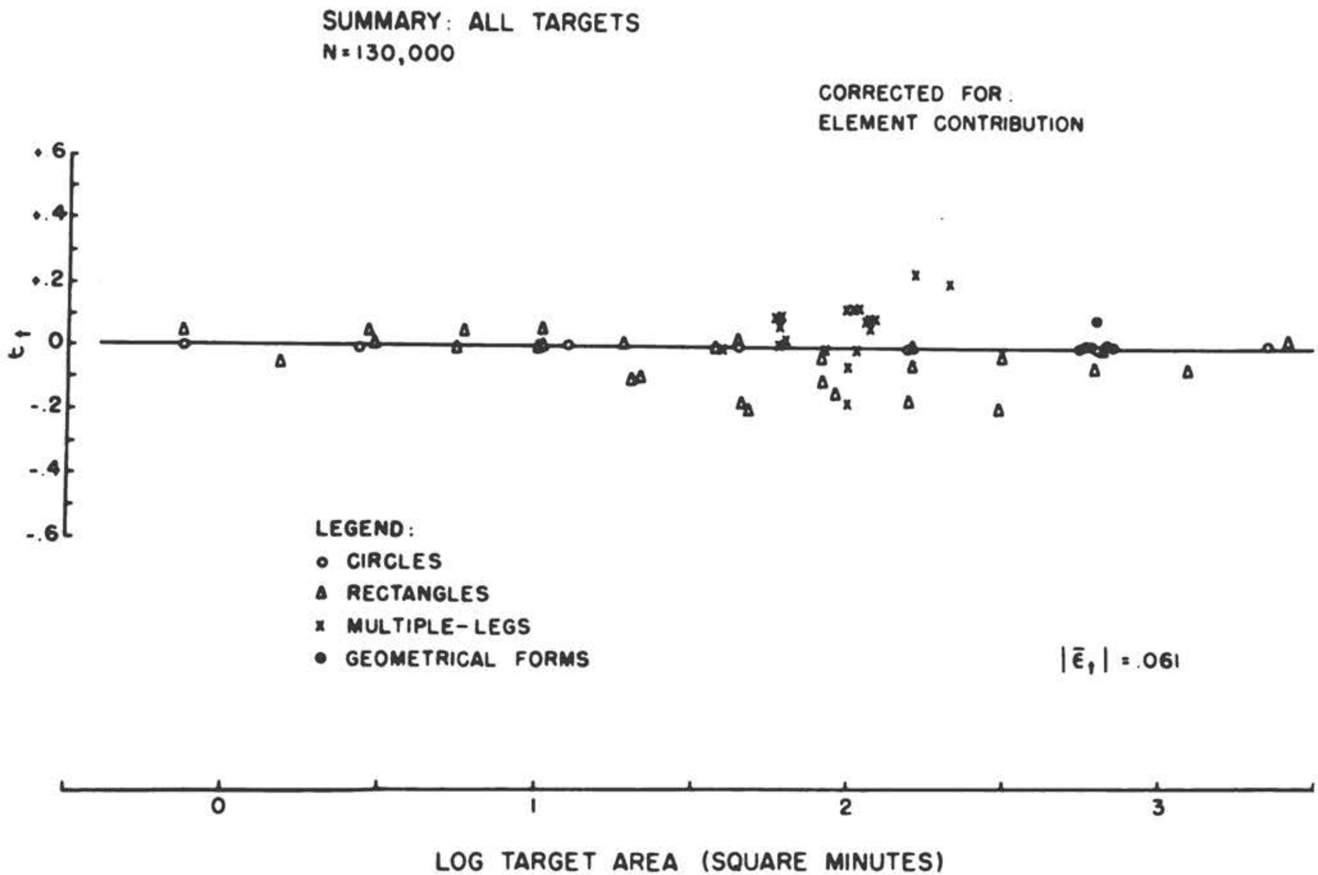


Fig. 7. Deviations from theoretical prediction, all targets, 10 foot-lamberts.

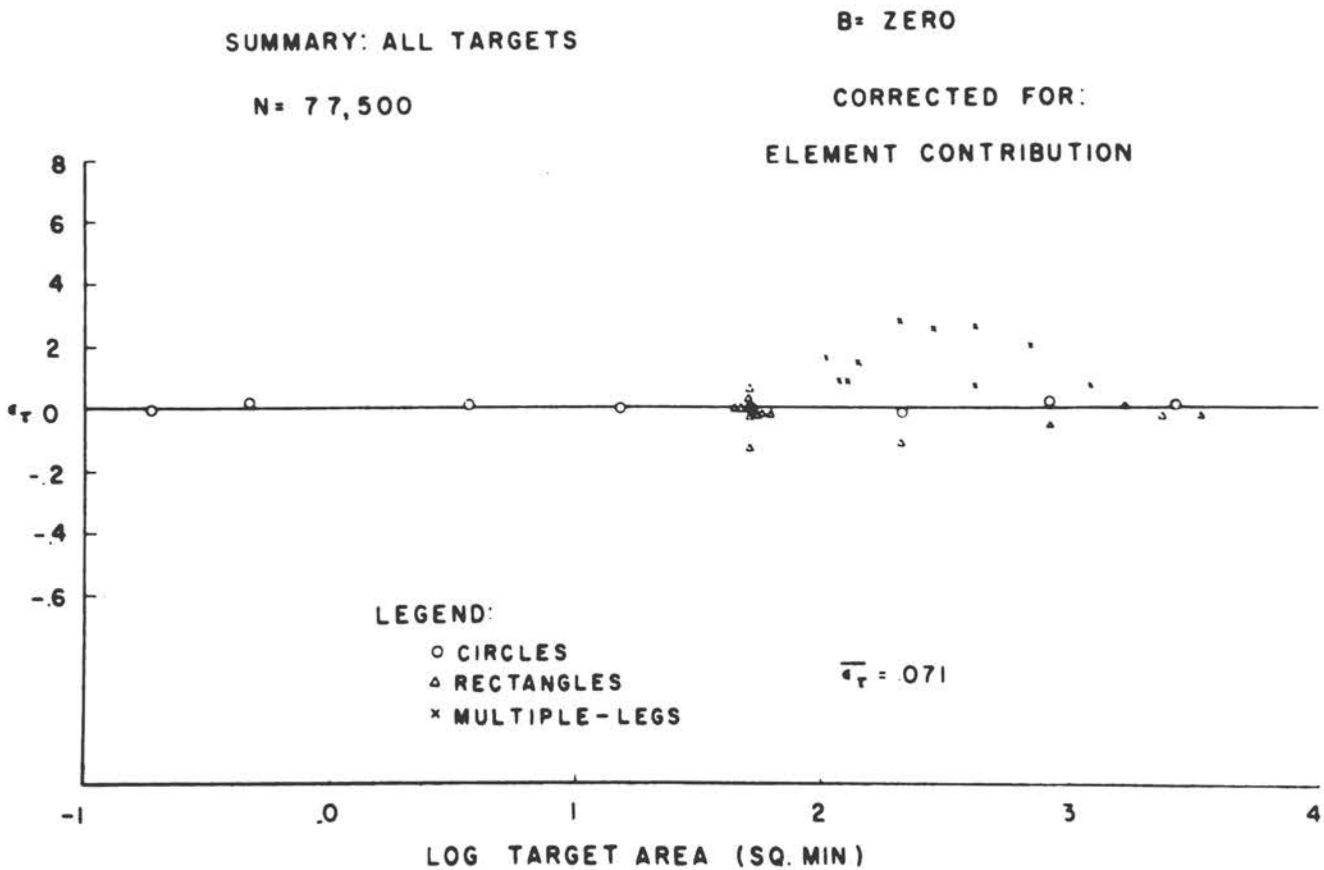


Fig. 8. Deviations from theoretical prediction, all targets, 0 foot-lamberts.

The data shown in Figure 6 come from experiments similar to those in the preceding figure but are for zero background(2). The targets utilized represent the most critical targets from the more extensive series run at 10.0 foot-lamberts, the mean deviation being .16 log units, nearly the same as before.

Part of the analysis with respect to the element contribution theory is shown in Figure 7. The vertical axis on this figure is the log unit difference between obtained threshold contrast and the threshold contrast predicted by the element contribution theory. These are the same data at 10.0 foot-lamberts background presented in Figure 5. Over-all, the theory reduces the variance in the data considerably, the algebraic average of the log unit deviations being 0.007. The absolute average deviation, .061, represents a reduction in variance of 64 percent. Note, however, that there is a trend for rectangular targets, denoted by triangles, to be more detectible than predicted by the theory while the reverse is true, on the whole, for the multiple-legged targets.

Figure 8 shows the zero background data treated in the same way. There is less of a tendency for rectangular targets to deviate from predictions. The multiple-legged targets appear about the same as in the 10.0 foot-lambert case. The efficacy of the theory is about the same as before.

Extensive analysis of these data has revealed certain systematic trends related to some aspects of target geometry. This empirical analysis has been performed on both of the quantities under discussion, the deviations from equal area circles and the deviations from theoretical prediction. There will be presented here very briefly the results of this analysis of the deviations from theoretical prediction.

Two aspects of target geometry are relevant here. The first is simply the quantity (β-α), the difference between the length and the width of the target as indicated in Figure 9. The second is the quantity termed "utilization". α' and β' are the length and width of the rectangular box which just encloses the target; A is the target area. Hence, the percent utilization is the percent of the area of the box occupied by the target. In the analysis under discussion, these two quantities order the residual errors of prediction best of various empirical analyses we have tried.

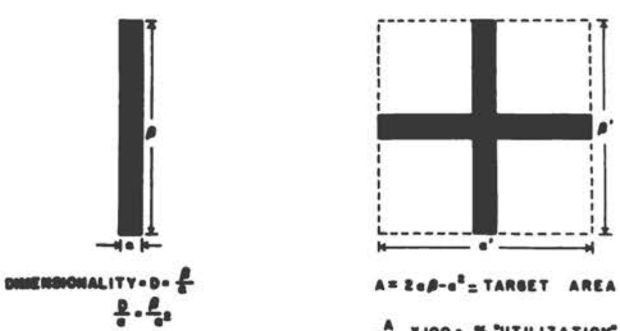


Fig. 9. Relevant target dimensions.

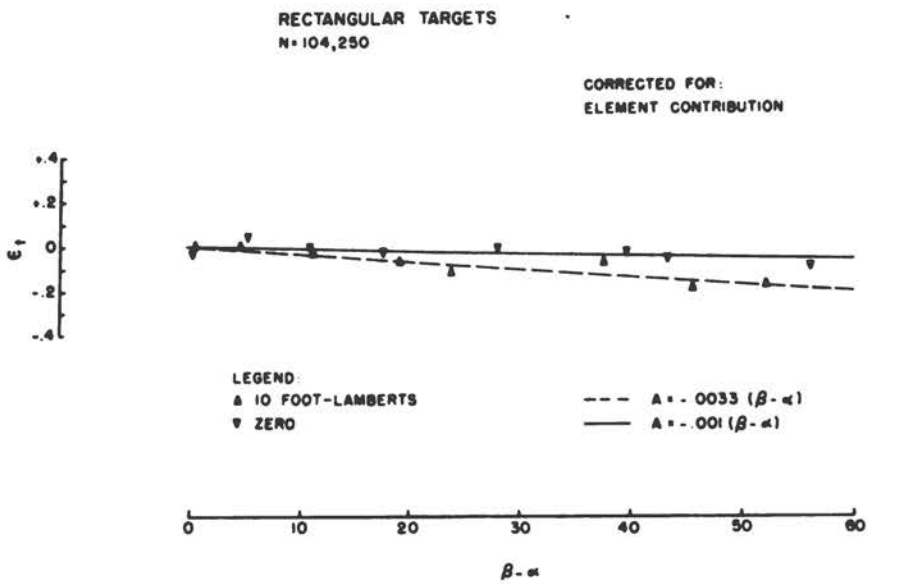


Fig. 10. Deviations from theoretical prediction for rectangular targets versus (β-α).

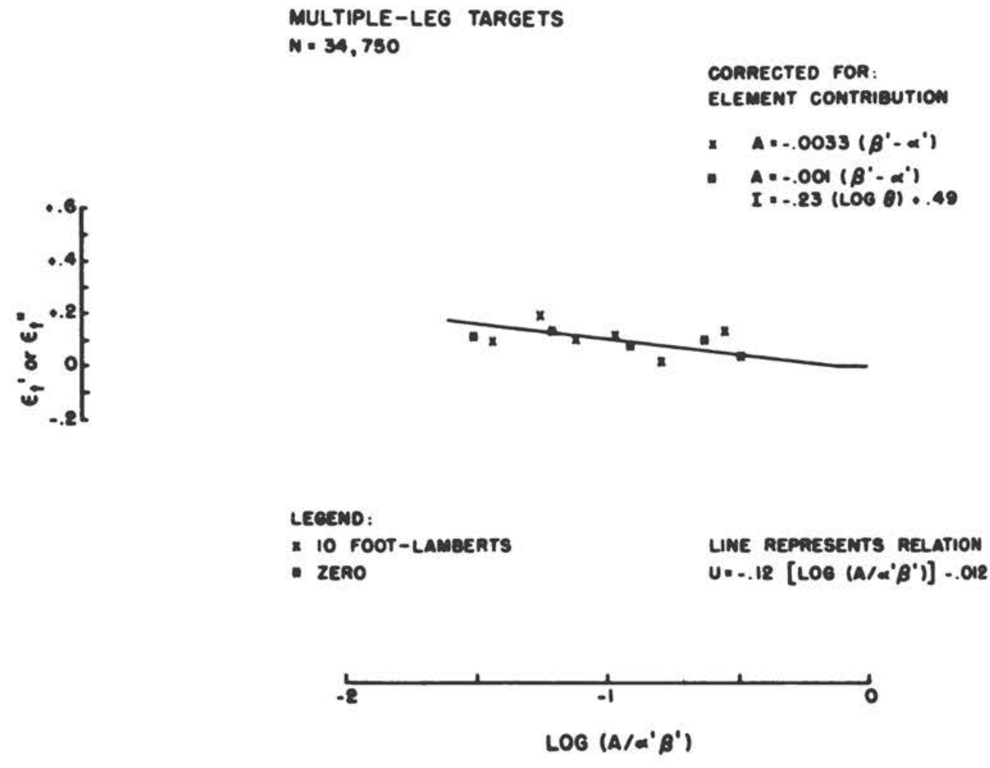


Fig. 11. Deviations from theoretical prediction for multiple-leg targets versus utilization.

As noted previously, the deviation from prediction for rectangular targets is greater at 10.0 foot-lamberts than at zero background. This is clearer in Figure 10 which also shows that the deviations from prediction are ordered in terms of (β-α) in both cases. The longer a rectangular target is in relation to its width, the more does its detectibility exceed that expected theoretically. At 10.0 foot-lamberts the maximum deviation is about .18 log units.

After adjusting the log unit deviations of the multiple-legged targets for (β-α), the residuals are found to be ordered in terms of the utilization variable, as shown in Figure 11. There is no difference between background levels.

Thus it is clear that the element contribution theory, while being successful to a marked extent, fails to account for all of the data when targets of extreme form are considered. Long, narrow, rectangular targets tend to be more detectible than the theory requires. The reverse is true for multiple-legged targets consisting of overlapping combinations of long, narrow rectangles.

Several additional experiments which throw some light on the relations discussed above have been done. In Figure 12 is a series of rectangular and cross-shaped targets having a constant width of about .8 of a minute. It includes a series of rectangles of increasing length, a series of crosses with the horizontal cross-bar of increasing length, and two smaller crosses which have the same width as the other targets but are of an over-all length of about 8 and 16 minutes of arc. A relevant target measurement is ℓ'; the total linear extent of the target.

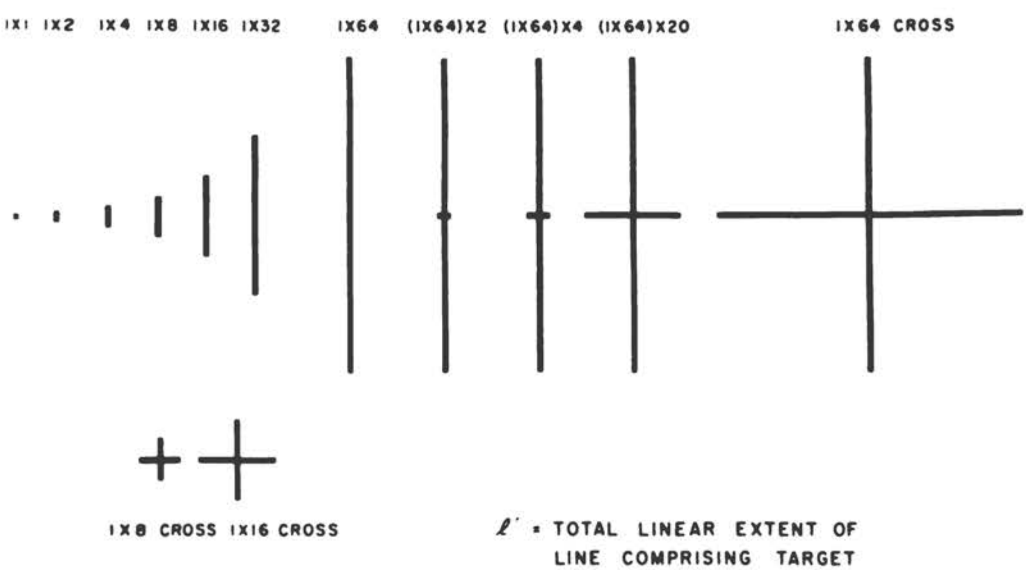


Fig. 12. Rectangle-cross series of targets.

A set of similar data on a second observer agrees in detail with that shown in Figure 13. Threshold contrast decreases as the length of the rectangular target increases, out to about 50 minutes of arc. There is no added increment to detectibility as ℓ' increases further; the 1 x 64 crosses do not differ from the 1 x 64 rectangle, even though they are of nearly twice the area. However, note that the 1 x 8 and 1 x 16 crosses fall on the function, implying that the lack of an increase in detectibility in the case of the 1 x 64 cross cannot be attributed to the area of intersection of the cross.

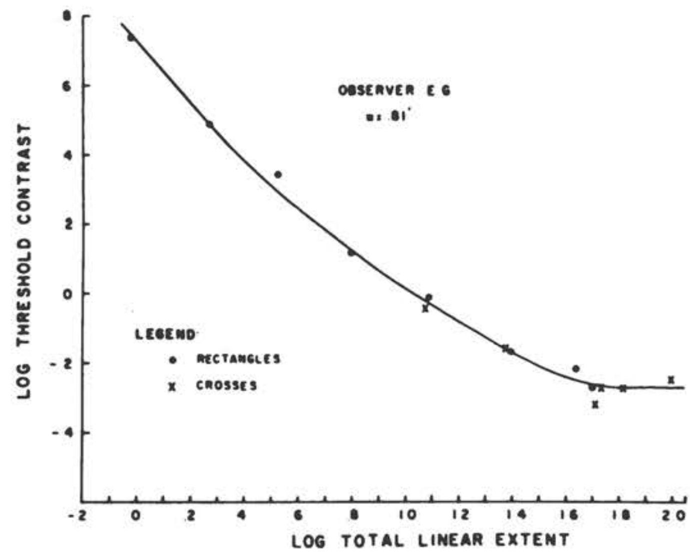


Fig. 13. Log threshold contrast versus log total linear extent, rectangles and crosses.

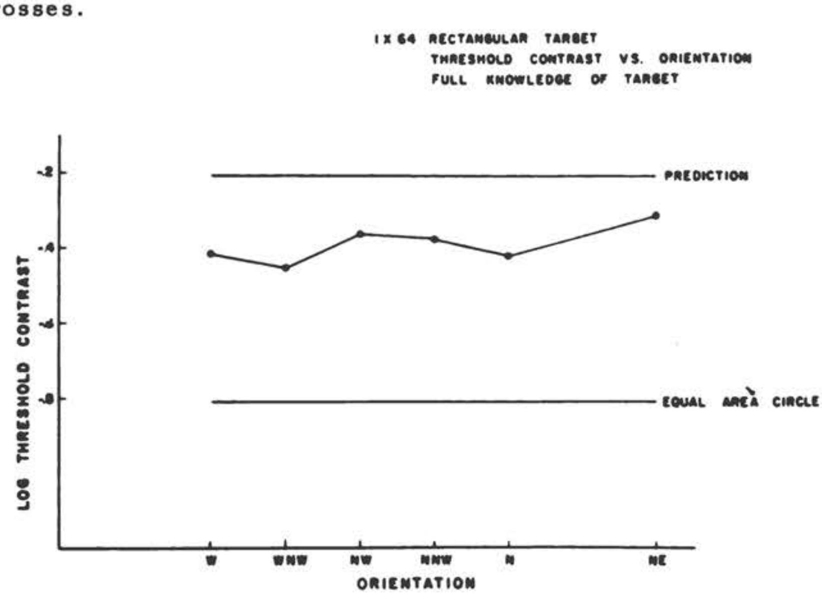


Fig. 14. Meridional differences in summation.

It has been suggested that part of the inadequacy of the element contribution theory might be due to $\phi(R)$, the contribution function, being different along different meridians. To test this, thresholds were determined for the 1 x 64 rectangle at a number of orientations other than vertical and horizontal. Obviously, the data in Figure 14 require concluding that orientation is of minor importance and the rectangular target is more detectible than predicted at all orientations.

Another hypothesis which has been entertained to account for both the rectangle and multiple-legged target results assumes that information is accepted only from a relatively narrow, long band of receptors. This "receptive band", if it exists, must be manipulable in direction since the orientation of a rectangle is irrelevant. The data in Figure 15 are threshold contrasts for the 1 x 64 rectangle in the vertical and horizontal orientations, when the observers have full knowledge of orientation, which has been the case in all experiments up to this point, and when the target is changed in orientation at random from trial to trial. Information concerning target orientation is irrelevant; with or without knowledge, threshold is lower than predicted by the element contribution theory.

Some other theoretical formulations of spatial summation have stressed the statistical nature of visual detection, usually assuming that small areas across the visual system fluctuate in psychophysical sensitivity over time. In addition, it is often assumed, and there are some data to support the assumption, that two small areas separated enough to eliminate optical and neurophysiological summation vary temporally in sensitivity independently of each other. This leads to a third source of areal summation which can be called probability summation.

THRESHOLD CONTRASTS FOR

1X64 RECTANGULAR TARGET

FULL VERSUS PARTIAL

KNOWLEDGE OF ORIENTATION

	HORIZONTAL	VERTICAL
FULL KNOWLEDGE	.53	.47
RANDOM PRESENTATION	.54	.50

PREDICTED THRESHOLD CONTRAST (ELEMENT CONTRIBUTION THEORY) = .81

Fig. 15. Effect of knowledge of orientation of rectangular target.

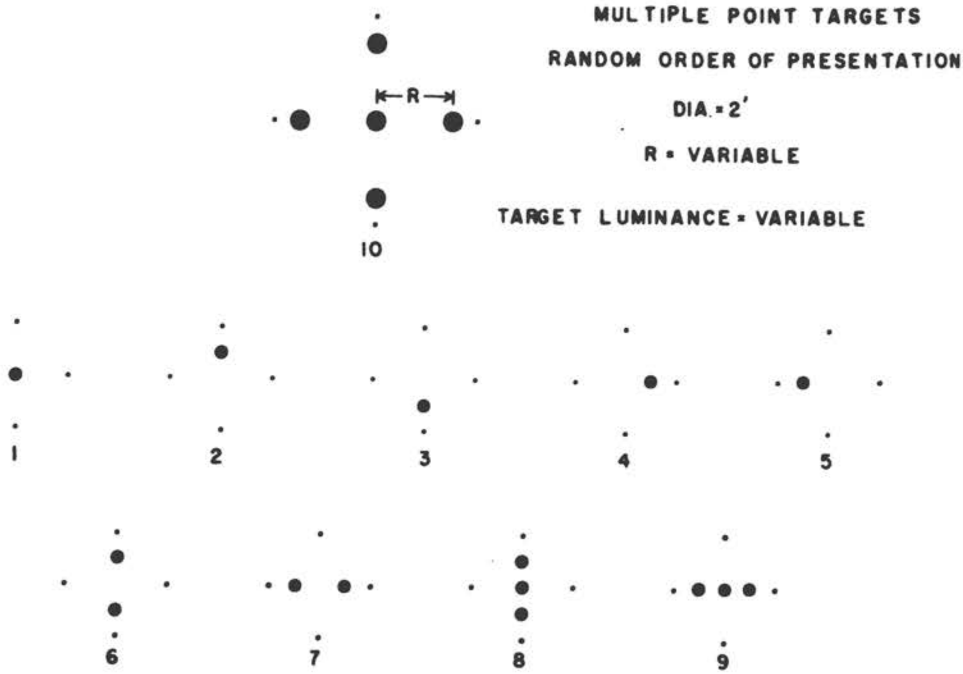


Fig. 16. Multiple-element targets.

A study of spatial summation has been started using targets, similar to those shown in Figure 16, which are directly amenable to probability interaction analysis, and which can be analyzed from the element contribution point of view, also. In a single experiment, the ten targets shown in the figure, for a single value of R, are presented in a random order at the usual five levels of target luminance. Detection probabilities are determined by temporal forced-choice.

This method is an exceedingly insensitive way to determine the element contribution function and the data so far are inadequate for that purpose. However, a probability analysis can be performed on presently available data, as in Figure 17.

The index gamma is defined at the top of Figure 17. P is the obtained detection probability for a multiple-element target, $\overline{P}$ is the probability calculated from the assumption of statistical independence, and $\hat{P}$ is the measured probability associated with the most detectible element of those comprising the compound target. Thus, when the multiple-element target is no more detectible than the most detectible element within it, gamma is unity, labelled "no gain" in the diagram. When the obtained probability for a multiple-element target is the same as predicted by probability summation, gamma is zero. For summation in excess of probability summation gamma is less than zero. These data were collected at 10 footlamberts background with a target duration of .010 second. The curve is

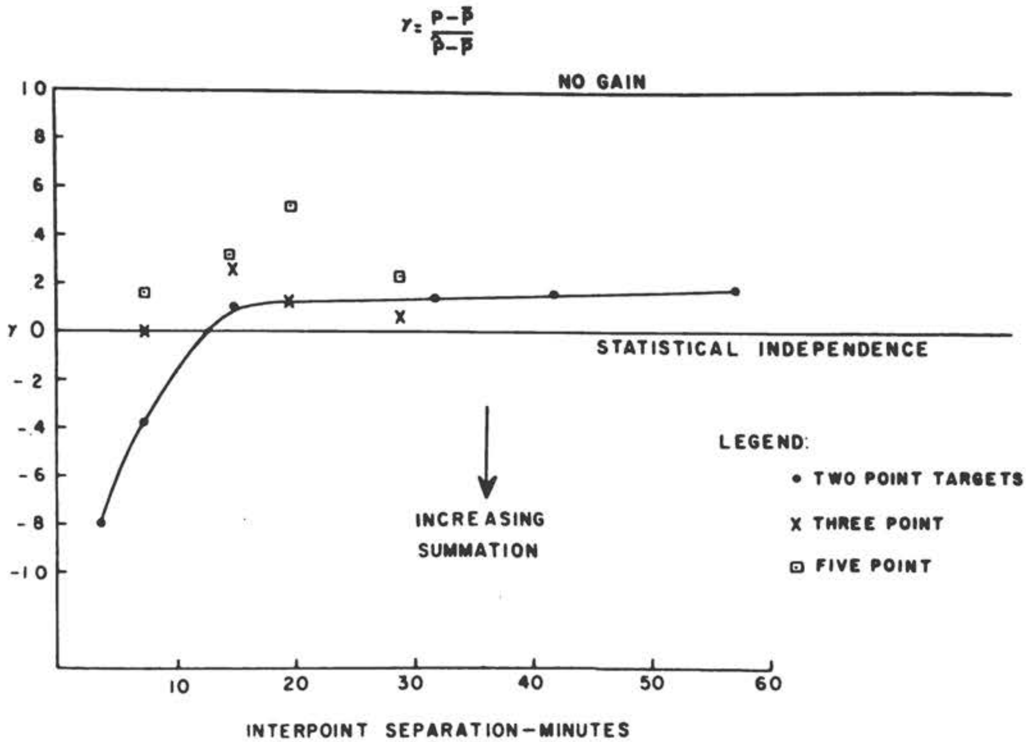
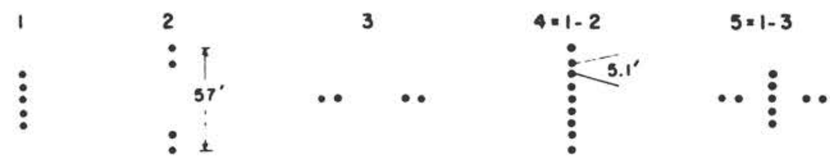


Fig. 17. Probability analysis of multiple-element target data.



	1	2	3	4 = 1 - 2	5 = 1 - 3
DETECTION PROBABILITY (N = 150)	.95	.75	.86	.99 (.99)	.98 (.99)
	.69	.29	.32	.92 (.78)	.68 (.79)
	.55	.24	.19	.72 (.66)	.52 (.63)
LOG THRESHOLD CONTRAST	-.11	+.10	+.08	-.16	-.10

Fig. 18. Configural effects in multiple-element targets.

drawn free-hand through the data for two-element targets. Summation exceeds probability summation when the smallest distance between elements is of the order of eight minutes of arc or less. At large separations the obtained probabilities are definitely less than expected probability-wise. Further, the function appears to have a slight positive slope, all the way out to fifty-seven minutes, a fact difficult to cope with in probability terms.

As the number of target elements increases, the failure of the probability summation hypothesis becomes more evident. It must be stressed that these results are incomplete; it is anticipated that much more will be done in this direction.

The multiple-point targets have been utilized in an attempt to clarify some of the configurational effects brought out by the previous data.

The five targets diagrammed at the top of Figure 18 were run in a further test of the hypothesis that a linear array of information is more efficient than an orthogonal array. The probabilities in parentheses are those predicted assuming independence between targets 1 and 2, in the case of target 4, and between 1 and 3 for target 5. The three rows of probability values differ in target luminance. It is apparent that the linear array is superior, both when compared to the probability summation prediction and in terms of gain over target 1. In fact, there is no indication that target 5 is more detectible than target 1.

These results suggest that configural effects are significant for targets of this type. However, the differences observed can probably be attributed to differences in interpoint distances for targets 4 and 5. The indicated distance, 5.1 minutes, is well within the zone of high summation shown in the previous figure. This conclusion is supported by the results of a recently completed experiment, similar to this one but with a minimum interpoint separation of 16 minutes in the case of target 4. Both the rectangular and orthogonal arrays showed small gains over target 1; neither target gained as much as predicted by probability summation, and there was little difference between targets 4 and 5.

The foregoing is a representative sample of the work which has been done with target form at the Vision Research Laboratories. It is quite apparent that much remains to be done in the area of target form and detection. However, it is planned to extend this work to include more complex behaviors, such as pattern discrimination, visual acuity, and pattern recognition. Such problems will be approached in ways which will maximize the application to them of theories and data derived from the study of detection. This attitude, of course, assumes that pattern discrimination, for example, can be studied more profitably if visual input information can be adequately described. The element contribution theory, or some variant of it, shows promise of performing this function. It may be that in the simplest cases pattern discrimination can be shown to be fully attributable to dimensions of sensory input which can be defined by detection data.

For example, some data have been obtained which suggest that, within limits, the detection probability for a target is equal to the probability that the spatial location of the target can be correctly identified. From this it is a relatively easy step to defining a class of targets for which the probability of discrimination between members is equal to the probability of detection of those target elements which are not in common between them. In the present state of knowledge concerning detection, it would probably be most efficient to begin with targets similar to the multiple-element targets described previously, in which spatial interaction can be minimized and the probabilities of discrimination between patterns reasonably computed from the detection probabilities of the elements.

In more complicated cases which involve interactions within targets more must be known about the interactions. It is as reasonable to expect such knowledge to come out of investigations of pattern discrimination as out of further detection experiments.

Thus, pattern discrimination, which should include visual acuity, can, and should, be studied in concert with detection beginning with the general assumption that detection limits the more complex discriminations. It should be unnecessary to add that other variables, such as knowledge of the target, can be expected to play increasingly important roles.

THE ROLE OF FORM DISCRIMINATION IN THE PERCEPTION OF MOTION

Robert M. Gottsdanker

To start with, there must be some inhomogeneity of the visual field -- by appropriate or inappropriate stimulation -- if anything is to be seen, let alone be seen as moving. But right off we encounter the characteristic reciprocal relationship between form and motion: while rudimentary form is basic to form motion, motion is an important means for establishing form. An object may be invisible in peripheral vision because of insufficient contrast until it is moved. For more usual circumstances, the Gestalt psychologists have demonstrated how the similar motion of scattered parts of the field will unite them into more inclusive units. This has been termed "common fate". This situation often comes about because of motion parallax. More general is the motion perspective described by Gibson, whereby objects are not only defined but also located, modeled and moved in three-dimensional space by two-dimensional patterns of flow and expansion. Closely related are such phenomena as the kinetic depth effect in which the shadow of a rotating figure acquires the appearance of the solid figure itself and those reported by Johansson in which the shadows from a number of moving objects are combined perceptually into the complex motion of a single object. A further example of how motion is instrumental in providing perceived forms is that described by Katz in which both the shape and the components of motion are derived from just a few points on a rolling wheel. There must be scores of other examples.

Returning to the question of how much articulation there must be of the visual field to allow the perception of motion, we all know that the existence of a single small discriminable form is insufficient because of the autokinetic effect. Or rather, we get a perception of motion which cannot be related to physical changes in the outer world. Even adding another form does not always provide enough information. As Duncker has shown, when the frame around an object is moved (and nothing else can be seen), it usually is the enclosed object which seems to move. This is induced motion. Here it might be protested that the two objects really do allow an accurate perception of motion, as the amount seen is invariant even though the wrong thing is seen as moving. But it certainly doesn't allow for a veridical sorting out of the moving and stationary parts of the environment. It should quickly be pointed out that this misidentification can occur in highly structured fields. We see the leering leading man stationarily rush past an unconvincingly moving sofa toward an escaping Marilyn Monroe, who nevertheless is accommodatingly sliding back toward him. This, of course, provides the retinal replication of ocular pursuit. We take this effect for granted but cannot refer to any coherent fund of experimental evidence on the structural relationships which make for compelling induction. This is a problem I hope to explore before too long.

What else can be said about how form discrimination operates in the perception of motion? It has long been established that thresholds of motion are much higher when background objects are absent than when they are present. Also, there is the observation by Hamann in 1911 that when a rider is seen entering a wood after having been seen against an ordinary field, his apparent velocity increases markedly. Both of these and the finding that apparent velocity is lower in peripheral than in central vision strongly suggest the importance of contoured forms.

One suggestion -- and it is no more than that -- is that the breakdown of contour with motion across the retina and other kinds of retinal smearing may operate as useful cues of the motion. De Silva gives a whole array of effects which occur at different angular velocities ranging from slightly blurred outlines at 10^{o}/sec. to a stationary sheet of light above 116^{o}/sec. Furthermore, the effects are asymmetrical so that direction may be identified. Of course, none of the effects could be obtained without contoured objects to begin with. A common observation seems to fit in here. We like a sequence we have taken for home movies and decide to enlarge a single frame. It turns out to be blurred although the figures in action appeared sharp. Perhaps the smearing contributes to the overall effect of an object in motion. Some corroboration is given by the more jerky appearance of animated cartoons. For the fixated eye there is a discontinuous trail and no disruption of contours. There is one complication here. Retinal smearing will occur if the eye moves over the scene. In pictures of real action, then, there may be a combination of retinal and emulsional smearing.

If the addition of some structure to the surround reduces thresholds of motion, there should be selective effects of different kinds of background. But who can predict which structure will result in the most accurate perception? Not I. At least I couldn't before I experimented on this problem in my series of studies on the accuracy with which the acceleration of target motion may be identified. The technique of motion picture animation was used for generating the target motion. Judgments were made of whether a target was accelerating either positively or negatively. The same round target moved against a variety of background structures. These included a simple frame, a frame and starting or ending point only, or both points. Also included was a background with two landmarks spaced evenly in the course of motion and one with unevenly spaced internal landmarks. Further, there was a background with fine regular vertical lines which gave a uniform texture for the target to move across and one with fine irregular vertical lines which formed a random texture.

The findings may be given simply. As would be expected, there was poorest identification with only the surrounding frame. In fact, for the amount of acceleration used, the identification was not significantly better than chance with 100 subjects. However, the rest is surprising. There was no difference among any of the other structures, with one exception. The best by far for identifying acceleration was random texture. There are several possible interpretations but the results do appear to argue against the useful computing of rates on a macroscopic time scale as could be used when the target passed the two evenly spaced landmarks. Also, there is the strong suggestion that the regular grid and reticle patterns which are typically employed in visual displays make for rather poor judgments of motion.

FORM AND VISUAL DETECTION*

Julian E. Hockberg

Before a shape can be discriminated from other forms, it must be "registered" in some way. It is generally considered that in order for the form of a given area to have any effect upon behavior, it must be figure, while if the very same area of retinal stimulation acts as ground, its form remains undetectable. I would like to report here on some relevant ideas and current and projected research on certain aspects of this problem.

In an earlier series of investigations of form detection, first undertaken in 1948 with Gleitman and MacBride, the absolute illumination thresholds were determined for the energence of several different forms in otherwise complete darkness. Our original predictions were based on a rather fancy and fanciful physiological model, and the results obtained have

*Although time prohibited reading Dr. Hochberg's paper in his absence from the symposium, it is presented here as it was submitted prior to the symposium.

been "reasonably" consonant with the expectation that such thresholds would vary directly with the ratio of Perimeter to Area of the stimulus figure. (Attneave has since suggested using the ratio of perimeter squared to area.) Although it is in principle possible that we could proceed step by step to build up on such basis a predictive theory of form detection, its first stages are so vastly simplified, and the intervening steps so staggering, that such programs convey a distinct impression of pie-in-the-sky. We do not even know whether similar relationships hold for relative thresholds, i.e., when the surrounding illumination is different from zero (or approximately zero). We do know, as we will see later, that this relationship does not hold when the backgrounds are not homogeneously illuminated. We have very little understanding of the basic problem, and there is little prospect that the situation will be much different in the foreseeable future. I would like to describe a quite different problem before returning with renewed hope to this one.

In a series of investigations first undertaken with McAlister in 1952, stimuli were chosen which would give rise to at least two mutually-exclusive figures; various measures have been obtained of the likelihood of seeing each of the alternative figures, and the attempt was made to find some measurable characteristics of the stimuli which would correspond to these relative response probabilities. Some framework is necessary, and the one within which we have been working is that the likelihood of reporting each alternative is inversely proportional to the amount of "information" the experimenter needs in order to specify it. About two dozen sets of forms have been sampled by now, and the best set of weighted scoring dimensions will account for about 85 percent of the response variance; however, we know that we should be able to do considerably better than this, since intersubjective response correlations frequently run around 0.98, i.e., ideally, we should be able to predict about 96 percent. We are now girding our loins in preparation for the application of factor-analytic techniques to this problem. The point that I wish to make here, however, is not the formula or its efficiency -- we will be bursting out of it pretty soon, now, anyway -- but the basic conceptual difference between this approach and the preceding one: the intention here is not to predict or "understand" the responses made to each and every stimulus in the class being dealt with, nor to search for universal deductive laws, but to build up an inductive formulation to cover as great a proportion of stimuli as we can.

Let us now return to the general problem with which we first started, the brightness contrast threshold for different forms. So far, this had been investigated with single or few forms against homogeneous surrounds. Once we place our stimulus in a more natural and cluttered setting, the picture changes drastically. In one of an unpublished set of studies started with Brooks in 1955, modified Gottschaldt figure-extraction procedures were employed in the following manner: four test shapes were each embedded in a set of four different complex line patterns, so that none of the four test shapes could be detected in any of the total of 16 concealing patterns. The illumination of the concealed test shapes was independently controlled by the experimenter, so that the brightness contrast threshold to detect each form in each pattern could be determined. The resulting data no longer even approximately fit any formula as simple as P/A, P^2/A, etc., nor could even any complex formula for the test shape thresholds be devised, since the differences between the thresholds of any given form embedded in its different patterns, was greater than the differences between forms. This, of course, is a consequence of the different ways in which each embedding pattern competed with the concealed figure, and we should expect this same uniqueness to appear in the detectability of form in our normal environments. Must we await development of "basic theory", and then apply it to the analysis of each form in each set of surroundings? I would like to propose that an alternative program, based on the same sampling attitude displayed in the previous series discussed, may have a greater likelihood of "payoff", both for immediate utility and as the raw material for eventual theory-formation.

There are two major classes of concealment of forms, namely (1) by hiding or occulting portions of the contour, and (2) by the "appropriation" of contour by "good continuation". Each of these involves both intra-organismic factors (hereinafter termed subjective factors, without prejudice), and environmental or ecological factors; both sets of factors seem amenable to statistical sampling procedures. Let us see briefly what information we would need, and how we would proceed to get it:

(1) The first class was concealment by occultation: How great a proportion of a contoured area must be visible for detection of the members of different classes of forms, at some given level of probability? We need to know here how much of the sides and of the inflection points, for different degrees of curvature, acuteness, etc., are necessary, in order for the full form to be perceived as a figure continuing behind the interruptions; this information could be obtained by progressive widening or increasing the number of holes or slits in a shield behind which each of a wide sampling of shapes was exposed. In addition to this "subjective factor", we need to know something about the environment: what sizes and locations of occlusion and exposure are likely for different classes of environment, i.e., what is the distribution of "overlaps" and "gaps"? We expect some information on this question to result from a survey which will be described below.

(2) The second class of concealment was by contour-appropriation: This is a slightly more subtle form of interference; it depends upon the fact that, with some reservations, "contour belongs only to the figure". Consequently, if the contour of one object lines up within some limits with the line or edge of some other object, and the retinal area adjacent to that of the object becomes the figure, the object may become unformed ground. The same effect may be obtained if some in-line or surface-color on the object becomes appropriated by some other line or edge, so that part of the object is incorporated into a new figure, part becomes ground, and its own shape cannot be detected. This, of course, is the principle employed in the design of camouflage, and, as with camouflage, the characteristics of both the human observer and of the environment in which the object is likely to appear, must be taken into account.

The "subjective factor" here concerns the as-yet inadequately-sampled question of "figural preferences"; that is, consider a given stretch of contour, say, concave to the left: what are the populational probabilities of the left rather than the right adjacent areas becoming formed figure? How do these vary with different degrees of curvature? in combination with different segments of contour? There is a large number of Gestalt demonstrations here which can provide good hunches, and a growing body of careful experimentation; I believe that it only requires the self-conscious desire to accumulate what Brunswik would have called a more representative sampling of stimuli as well as of subjects, to amass quite usable stimulus-response distributions.

In addition, there are two purely stimulus factors to be considered. The first concerns what I call geometric compatibility, the number of kinds of contours which, formally speaking, can continue or incorporate the contour of a given class of shape. Thus, there is no way in which a circle or arc of radius *x* could be concealed in a gridwork graph in which the lengths of graph units are of the order of magnitude of *x*, whereas a square of side *x* could be completely concealed in a large number of different positions, as a hexagon would be, even more so, on a honeycomb grid.

The second stimulus factor is what Brunswik might call an ecological survey of the microcharacteristics of different kinds of environments. The purpose of this is to determine the frequencies of occurrence of different line densities, intersection angles, numbers and degrees of curves, etc. Some preliminary sampling of this sort has already been undertaken, using stratified samples of sub-areas taken from aerial and landscape photographs of cities, deserts, ocean scenes, and countrysides, taken under diffuse, overhead and glancing lighting. Some kinds of environments are reliable (such as cities), with high correlations from part to part and from photograph to photograph, while others (notably deserts) so far appear unsystematic. This proves to be a rather pleasant if painstaking survey to make, and we hope to have available soon a set of tables of environmental reliabilities, and of frequency distributions of the microcharacteristics. It is to be hoped that the more reliable of these figures, when taken in conjunction with the information of the sort we have discussed previously, would permit form-detection to be predicted with a known probability of error.

I would like to summarize briefly, and then offer rebuttal to three sets of objections which occur to me. I have proposed that, in view of the complexity of findings and the absence of an adequate deductive theory of form detection, we face the problem of figure-formation in an actuarial fashion, that we seek to make probability and range statements, rather than seek "full understanding" at this time. (Incidentally, I include under this deprecation of the present theories of form, any to which in the past I may have given support.) Such information will almost certainly prove useful, and it may provide a sounder basis for theoretical speculations than we have at present. I can think of three major objections to this approach:

(1) The various distributions involved may not permit useful prediction; however, this doesn't seem to be the case in terms of what we do

know and can predict "intuitively", and we won't know any better until we try.

(2) Such static factors as we have considered may be quite irrelevant to the normal world of moving observer and of optimal, determinate space perception. I consider this proposition doubtful, for three reasons: (a) we don't know how much motion parallax is actually involved in the normal inspection of the world, and there is some reason to believe that our normal spatial percepts are frequently quite far from optimal or determinate; (b) there is ample experimental evidence that, under viewing conditions which involve moving observers, but which are non-optimal in other respects (e.g., involving monocular vision), the static factors are very important indeed; (c) the frequent success of protective coloration, and the traditional but non-quantitative examples in which real objects are in plain sight of a searching observer under optimal viewing conditions, and yet are rendered undetectable by lining up their edges with those of other objects.

(3) It has been held by some that we cannot hope to predict responses from stimulation alone, without taking into account the underlying physiological organizations, interactions, learning processes, etc. I do not see the force of these arguments as long as we are concerned with the problem of predicting behavior, since if stimulus and response are randomly related to each other in a given class of situations, no theory of underlying processes will enable us to predict one from the other; if we do have a correlation or psychophysical correspondence, a theory can then only help us to understand, and perhaps to direct our gaze to other fruitful ares. If however it is upon the interactive or configurational nature of the assumed underlying processes that such objection is based, it is well to remember we can always approximate any function as closely as we wish, by a polynomial in sufficient terms. It is true that knowing the "true function" could probably make matters a lot easier and simpler, but such knowledge is quite out of sight, and refusing to settle for anything short of it may help keep it so.

DISCUSSION

HOOD: Perhaps you can answer a question about the blurring worked in with animated cartoons, Walt Disney has used it in the last few years to some extent and I think that a perfect example of when he wasn't using it was "Snow White and the Seven Dwarfs", one of his earlier pictures, in which the motion is jerky. I think most of the recent cartoons which Hollywood features use this blurring principle to a considerable extent. Do you know what laws they apply in blurring their figures?

GOTTSDANKER: It's news to me.

TAYLOR: I can make one brief comment on that. Some years ago -- Colonel Gillespie will remember this experience and also Dr. Blackwell -- we attempted to make some films for use in an aerial reconnaissance simulator. For this purpose we decided that we wanted a sharp image, so we closed down the sector on the shutter of a Mitchell camera and we anticipated the problem a little bit by attempting to run the camera at a 128 frames per second. Even though the projection luminance was quite low, these things phenomenally were extremely jerky. In the presence of sharp images you just won't accept this as smooth motion, even at low screen brightnesses.

GRAHAM: I wonder if I might ask Dr. Kristofferson if he sees any points of similarity between his elementary contribution theory and the account of it that Mote and I and Brown put out in 1939(1).

KRISTOFFERSON: Yes. The similarity is extremely great.

FRY: I'd like to try to say again what I think is the basic objection to the means of assessing the elements of contribution. I agree fully with the general idea of this contribution function. I subscribe to the idea that it represents both an optical type of summation and also a neural type of summation. I think as far as my thinking is concerned it's in perfect agreement. The only problem is that of method of assessment. I did an experiment a number of years ago which involved this arrangement: instead of using rectangles which have ordinarily been used in connection with the evaluation of the effect of area upon visibility of such targets, I took off from the point of view that it's not the area, but in one case it's the curvature of the border and, in another case, the length of the border. One can show similar effects of length of border in the one case and curvature of the border in the other which are similar to these things. Now, if we apply this information to this assessment, the thing that is basically wrong with it, I think, is the use of the disk for making the assessment, because you can't partial out the effect of kinks and curvature of the border as you increase the diameter of the disk from the effect of area. Now, if you go over to, say, rectangles as usually have been used, we can keep the length of the rectangle constant and we can vary the width and get a pure effect. If you want to go back to an assessment in terms of a point or in terms of a disk, you can do it mathematically, but you get experimentally here a means of getting the contribution function directly. Furthermore, this kind of target brings out the other factor in the situation that you're dealing with more than just a contribution function. I think you people have come to recognize that this is a point that goes beyond the contribution which I think was made by Dr. Graham and his collaborators. You get an effect of length of line that can't tie up with this contribution function. It is related, I think, to some kind of a process associated with border formation in the retina which you can't describe in terms of area or just a straight summation effect. It'd have to be described in some kind of a synchronizing or exciting mechanism which goes lengthwise along the line to make it more visible; that is, the longer one more visible than the shorter one.

BLACKWELL: May I try to unscramble these variables as a co-author of this paper? This is not a new discussion for many of us. I am sorry that some of you may have heard this before. Basically, there are two different points of view here. Dr. Graham and our group are much closer together than we both are to Dr. Fry. Dr. Graham's idea was that there was a kind of interaction across space. He didn't say at what point in the nervous system, but it doesn't really matter. He assumed that this is a power function and set about deriving the power from experimental data. That compares with Dr. Kristofferson's and my elementary contribution functions very closely. The main difference is that, rather than assume a power function, we derived the entire form of the equation from the data. That's perhaps the most crucial difference between Graham and Brown's most excellent paper and the theory we've been developing in recent years. There are minor differences, but this is the main thing.

Now Dr. Fry's idea, which goes back to 1947 and before, really is different, I think, in a very large way. Dr. Fry is concentrating upon the border and the effect of the border. The Graham, Brown and Mote theory and the statement we have made concentrate, rather, on the modal point in the distribution of the hypothetical excitation pattern. As such it is a sort of anti-border theory stating that pattern does not matter. All that matters is the amount that you get in this mapping process at a particular point. Now, Dr. Fry has good evidence, as he pointed out in his jagged chalk patterns, for the suggestion that border is very important. We have always felt that the difference in experimental conditions here is crucial. Dr. Fry uses long exposure time. He uses a method of adjustment in psychophysics where the target is rather visible and in fact the subject may be responding to the visibility of its border. His experimental conditions lead to this. We have deliberately gone to the other extreme, using a 1/100 of a second pulse and working at 50 percent detection threshold where there is little question that the border is ever seen. Under these two conditions one would expect to get different results. We have never regarded the border theory as either descriptive -- and this is no time to defend this statement -- or reasonable for our data. Nor do we regard our theory as reasonable or descriptive of Dr. Fry's data, the difference being, "are we talking about detection?" We are! Or,"are we talking about recognition of border?" He is!

In a sense, Dr. Fry's remarks are more relevant to this conference than mine, but we got into this bind, let's try to get out of it. Now, it's perfectly true that we have had to acknowledge the imperfectness of the element contribution theory, be it ours or Graham, Brown and Mote's. The difference here is a fine one. If we go to rectangular forms, as Dr. Kristofferson pointed out, the darn things are more visible than they should be by theory, but note, less visible than circles of equal area. This was the main reason we got into this business to settle an old argument that I had with Hecht back in 1947. I think we've convinced ourselves that we were right in that argument for we do consistently find circles more visible than rectangles of equal area. If you take the border-quantum theory of Hecht, Lamar, Hendley, etc., you do not get the prediction of the result that we have actually gotten in fact. Now, a further complication is that the cross figures are less visible than they should be in terms of theory. So, we've had to develop little second and third order correction factors to make up for these two facts which the theory does not predict. In a sense these are minor points. The theory works rather well, in our opinion, considering the wide range of targets we use for detection, alone. Now, what do we call this? One theory or another is of very minor importance. An element contribution type theory of Graham, Brown and Mote or ours seems to work very well for detection and works not at all for the recognition of borders.

GRAHAM: I think that some of you may have wondered why, in 1939-1940, after we put out this paper we didn't put out some more. We did put out three papers; one in connection with intensity discrimination. Brown and Neven(2) put out a very interesting paper, I think, on the length of line. This was an interesting paper because what it did was to get rid of one integral in the double integral and in that case it did simplify things. But it became very clear, particularly in view of Brown and Neven's experiments, that one could play with constants and in reality the value which should be chosen as a constant was one half of the one got in a plot of log log I/I_0, as a function of aerial radius, and then that would fit intensity with discrimination data and so on. Further than that, it became very clear to us that we couldn't extend this formulation to contrast, which was of course something of major importance from our point of view; so rather than go on in this way we decided, "let's see what we can get from experiments". Some of you may have noticed recently at Columbia some experiments on contrast have been done. There was one essential difficulty, I thought, with this theory as we left it pretty much in 1940-41. It may be that the Michigan group can improve on it. They certainly have, clearly, in one regard. They now integrate points all over the surface, which is one thing we didn't do. What we should like to do then, or what should be done it seems to me, is to find out what the influence of inhibition is on the effects of excitation over a surface. It is not going to be understood simply in terms of the plus factor or the excitation factor. We have to know the inhibition factor and we have to know the function. I'm sure we have to know it. I think that there would be different empirical values obtained from having played with this thought, but not explanatory concepts and not completely satisfactory.

SESSION IV

FORM DISCRIMINATION IN PROBLEMS OF RECOGNITION AND IDENTIFICATION

Chairman: Leonard C. Mead

MEAD: Let us launch into the fourth session which is "Form Discrimination in Problems of Recognition and Identification".

STUDIES OF RECOGNITION BY MEANS OF CLOSURE

Craig M. Mooney

Closure is the recognition of an object or event which is not completely or immediately represented. It is a sudden and apparently fortuitous insight arising out of a process of contemplation rather than logical analysis. It is an aspect of every day perception, and exemplifies its essential nature -- the drawing of conceptual conclusions from sensory premises. It is very simply illustrated in the type of closure pictures employed by Street(1) and Mooney(2). A few bits and pieces, meaningless in themselves, represent some commonplace object which the observer is to recognize. Recognition, when it occurs, is the result of a kind of spontaneous integration of the parts whereby the whole object is revealed. It is a striking perceptual phenomenon that not only invites attention to a number of basic questions in perceptual theory but also affords the experimental means of seeking answers to some of them.

My original interest in closure, like that of a number of other investigators, had to do with the question of individual differences in the ability to effect closures(2, 3). Such early studies have revealed a number of diverse facts about closure. There are marked individual differences in ability to effect closures. Once a closure has been made it is difficult to lose it and to see the stimulus in its original state. There is little tendency on the part of subjects to try to guess the answers or to resort to thematic projections; those who are poor at closure do not guess at all; but those who are proficient are more inclined to try to guess the answers to items they cannot certainly see. Performance on closure tests is not correlated with intelligence, education, age (except for the extremely young and old), or sex. Closure tests have been included in batteries embodying practically every other kind of standard psychological test and have shown little correlation with any of them. There is no evidence that ability in doing closure can be learned or that it improves with practice. Evidently the capacity has been well developed by the age of ten or twelve years. Efforts to find a use for closure ability -- in the selection of airforce pilots and navigators, for example -- have not been successful. Such evidence persuaded me that there would be little profit in trying to correlate closure with other abilities and that it was premature to try evolving an explanation for the wide individual differences in this kind of perceptual capacity. It seemed wiser to learn more about what happens in the individual instance of closure. Accordingly, I embarked on a series of studies designed to reveal factors that facilitate or inhibit the occurrence of closure.

The first obvious question has been that of the role of viewing time and eye-movements in the perception of familiar objects. This question inevitably arises when one watches people attempting to effect closure. Given a series of closure presentations, subjects perceive some of them immediately. Those that elude them they strive to perceive in a number of ways -- by scrutinizing them intently, blinking and moving their eyes, shaking their heads, glancing away and looking back, and the like. This excessive motor behaviour is just about as striking in adults as in children. Subjects are manifestly working at the pictures, and one wonders whether this labour of looking contributes to the subsequent closures. Is some kind of perceptual work being done, really, or do the closures occur, in fact, independently of the eye-movements and the periods of time used in looking? Most of us would believe off-hand that looking hard enables us to see things better or to solve visual enigmas. Moreover a number of our perceptual theorists -- for example Hebb(4), Gibson(5), Lashley(6) -- emphasize the importance of eye-movements for the clear perception of objects and forms -- especially for the original learning of these. Postulated processes of neural interaction, whether at the level of the retina or the visual cortex or at higher cerebral centers, usually embody the idea of sequential associative processes whereby the bits and pieces of the stimulus-complex summate into superordinate wholes.

One can reason as follows. If viewing time and scanning eye-movements have a contributory role in the perception of familiar objects, this fact should become especially evident whenever perceptual arrests occur because of the incomplete or partial representation of these objects. If the elements of the incomplete configural representation are to be integrated at a retinal or cortical receptor level, or if high-level cerebral associations are to be facilitated through prosecutory eye-movements, then it might be supposed that perceptual arrests and delays would be more frequently overcome under conditions where viewing times and eye-movements were permitted than where they were prohibited. One can put the onus on viewing time and eye-movements to prove themselves. Or, experimentally, one can proceed with the null hypothesis that viewing time and eye-movements do not contribute to perceptual closure.

Experimental test of this proposition logically calls for three different ways of eliciting closure: one, where ample time is afforded for looking at a test item and scrutinizing it; two, where time is very short and there is opportunity for but a single glance; three, where time is ample but only a single fixed point of regard is permitted. The first method entails only direct presentation for unrestrained inspection. The second method can, of course, be achieved by tachistoscopic presentation. The third method has been wanting -- how to have a subject inspect or look at a test item for an ample period of time with but a single fixed point of regard. My first problem was to discover such a method.

It occurred to me that the required third method might be realized through the method of after-images. If I could get a kind of test item that was incomprehensible when presented as, say, black on white (like a photographic negative, for example) but was perceptible when viewed as white on black (photographic positive) then I could use the phenomenon of negative after-images; the item, in the former state, (as a photographic negative) could be burned into the retina for thirty seconds, and then induced as a negative after-image on a gray surface, so that it could be viewed in its original or positive state but only from the single fixed point of regard with which it had been burned in.

Representations of the human head or face seemed to be admirably suited for this purpose. These could be turned into closure pictures by taking strongly lighted photographs of various kinds of people and copying off in solid blacks and whites only the shadows and highlights. The result was an incomplete picture which, when recognized, remarkably resembled the original from which it was derived. When these were converted into their photographically negative state they were incomprehensible. If one used these photographic negatives to burn in an image on the retina and then induced the negative after-image, one would be getting back -- by virtue of the double negative -- the original picture which would be amenable to closure, but only from the fixed point of regard with which it had been originally burned in. Representations of the human head and face presented themselves as test items on these further grounds. Earlier closure tests were composed of pictures of various kinds of objects with which subjects had a variable experience. By using a single class of items, such as human heads and faces, with which all subjects could be presumed to be highly familiar, the incidental variance attributable to differences in background experience would be considerably reduced.

The negative after-images of these closure items, when induced in the usual way, proved rather unsatisfactory. They were slow in appearing, were gray and poorly defined, and tended to drift off the viewing screen. It was desirable to find some way of securing clear, sharply defined, and sustained negative after-images. This was accomplished by introducing a flickering light during the viewing stage. The image was burned in with a bright steady light, and was then induced under a light flickering at about 3 to 4 cycles per second. The negative after-image was strikingly enhanced. It came on at once, remained constant, was bright and clear, and was less likely to drift off the viewing screen. A preliminary series of experiments(7) ascertained that subjects could effect closures with negative after-images under flickering light, and that such closures were not a function of the original burning-in process, nor of flicker itself. The experiments left no doubt about the facilitatory value of flickering light, compared to steady light, in effecting closures with the negative after-images. Only half as much time was required to effect the closures, and fifty percent more closures were accomplished.

In this way the requisite third method was developed whereby ample time could be afforded for viewing a closure test item while the eyes were held to a single fixed point of regard. With the three necessary methods in hand, the principal experimental questions could be tackled.

Fifty incomplete black and white drawings of the heads and faces of miscellaneous persons were prepared and put on 35 mm. slides in both positive and negative states. The projecting apparatus was a 500 watt slide projector with a variable speed light interrupter in front of it having a light dark ratio of 50/50. For tachistoscopic presentation a second light interrupter was placed in front of this and so geared that five out of six of the light flashes delivered by the first interrupter could be blocked. The subject faced two screens some four feet distant, viewing projected test items approximately 28 x 30 inches in size. The first screen, in front of the projector, was used to display the items. The subject, seated to the right of the projector, viewed these at an angle of about 20 degrees (with a visual angle of about 18 degrees). The second screen, directly in front of the subject, was used for inducing the negative after-images. Factorial designs were used, involving large numbers of subjects, and results were dealt with by analysis of variance. A dozen experiments were conducted utilizing some 250 subjects. These are being separately reported(8, 9), and the details need not detain us here. The principal experimental issue was, of course, whether the method of direct inspection -- where ample time (30 seconds) was provided and there was opportunity for multiple visual fixations -- resulted in superior closure performance compared with the other two methods -- tachistoscopic presentation (1/12 of a second exposure), or presentation by negative after-images (30 seconds observation with one fixed point of regard). The factorial designs employed permitted other questions to be dealt with in addition to these main questions -- such as the brightness of the presentation, clear definition of the stimulus parts, central fixations compared with peripheral fixations, treatment of false items when introduced into the series in conjunction with alternative expectancies on the part of subjects. The nature of this experimental programme can be inferred by looking at the following experimental findings. When time was ample, the perceptual accomplishment (about 70 percent effective) was the same whether multiple visual fixations were permitted or observation was limited to a single point of regard. When observation was limited to a single fixed point of regard, the perceptual performance was equally effective whether ample time was afforded or but a fraction of a second. When only brief observations were permitted the perceptual performance was not improved by a succession of these; nor did it matter whether fixation points were prescribed or not, or whether fixations were central or peripheral. The method of direct inspection proved superior on only one count: when false items were introduced, significantly fewer of these were mistakenly seen than by the other two methods. Incidental variables such as stimulus brightness and clarity had no general effect whatsoever.

The very general conclusion from these studies was that these graphic representations gained nothing as perceptual stimuli by being inspected; they operated as maximally valent wholes from the outset; the central, incorporative happenings which may have been involved in the perceptual events proceeded solely from these total givens and were not further facilitated by scanning eye-movements. The method of direct inspection proved superior only as a check against misperception of false items. The findings do not deny a supplementary role for eye-movements in the clarification and identification of the particular elements of complex presentations (as in proofreading, for example, or the discovery of contextual anomalies); but it is not evident that the perception of familiar objects entails or requires such visual elucidation. When all redundant material is removed from these presentations there is no role for scanning eye-movements; they are not involved in the perceptual events nor do they contribute to them; single glances suffice; and it would appear that, for this, the elements of the stimulus-complex subscribe together and at once to each perception through their formal congruence with the implied whole object which they partially represent. So there is this point: How shall we account, in our theories of central, neural processes, for instantaneous perceptual occurrences which do not depend upon, and are not facilitated by, sequences of eye-movements?

We may agree that we do come to perceive familiar objects all at once in a single swift glance; and realize that if we did not we should hardly be able to go about our daily affairs. But this facility may only come -- as Hebb(4), with excellent reasons, points out -- after a period of prolonged learning. It is difficult to theorize about the nature of this kind of learning without utilizing the idea of incorporative neural sequences which -- in vision -- seem to call, in turn, for the kind of conjugative function that might be performed by scanning eye-movements. In this area of perceptual learning, as well as in the area of the perceptual event, it may be a useful stimulant to our thinking and research to advance the null hypothesis that increments of viewing time and scanning eye-movements are not essential for learning novel forms. In my future work in this area I shall try, of course, to utilize some of the closure techniques that have been evolved for these present studies.

TARGET IDENTIFICATION IN RADAR PRESENTATIONS

Anthony Debons

Before going into the business of problems that are the immediate concern of this symposium, I would like to take just a few minutes to comment on several notions that come from our working experience at the Wright Air Development Center. As you all know, WADC is among other things a place of development engineers and researchers. The "Human Engineer" in this environment attempts to fit the findings from psychological science to the development of equipment and weapons necessary in the national defense. This working alliance is a happy and productive one, but it is also one that is forever harassed by the factor of time. By this I refer to the difficulties which emerge when development exceeds research. One

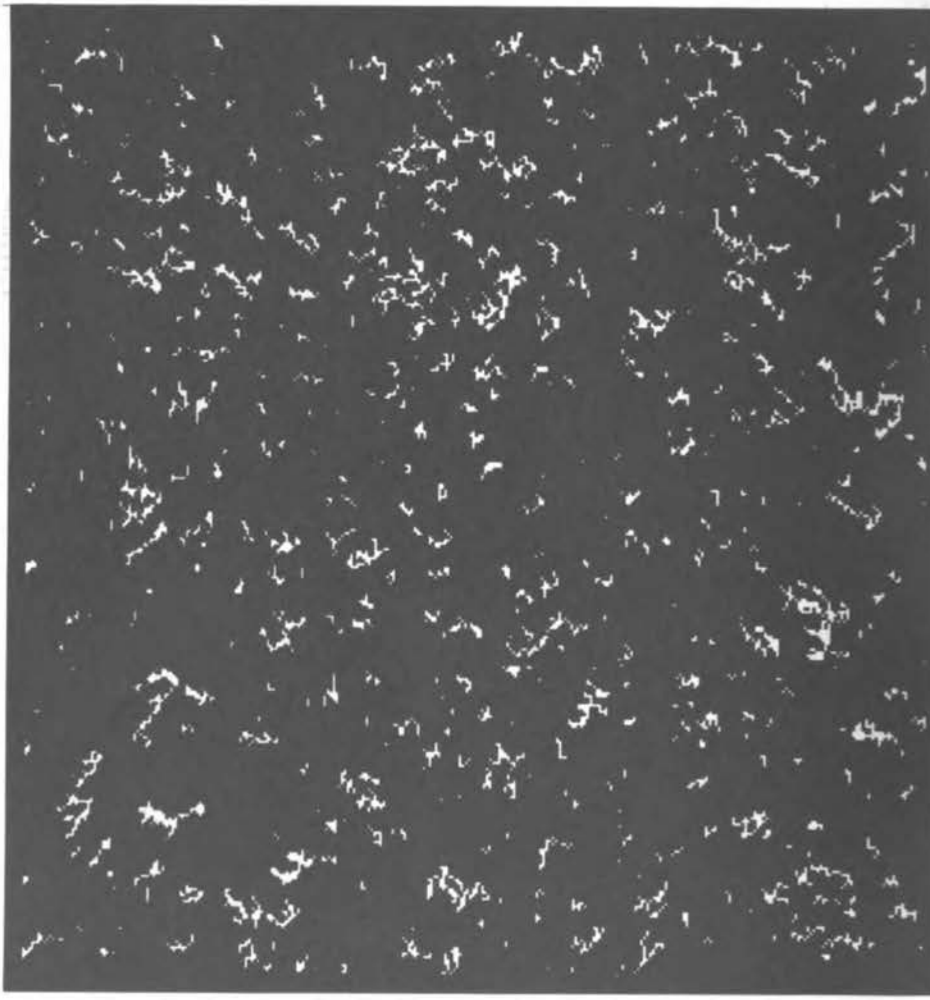


Fig. 1.

finds oneself facing problems today which should have been studied earlier. This continuous race for human factors knowledge to meet rapid strides in technological development can be intellectually energizing but also frustrating. I believe that this is demonstrated in the present status of the radar problem. The early development of radar was a question of combat expediency. It was later that precision bombing necessitated its refinement. It is presently that we are confronted with the problem of how much refinement. But this question of refinement is being posed at a time when both aeronautical and electronic research and development are continuously and rapidly changing equipment capabilities. The reality of the matter is that while we may be studying human factors problems very germane to the configurations now presented on existing radar screens, the questions presented by weapon system specialists relate to configurations of a different magnitude of complexity. In this respect, day in and day out the human

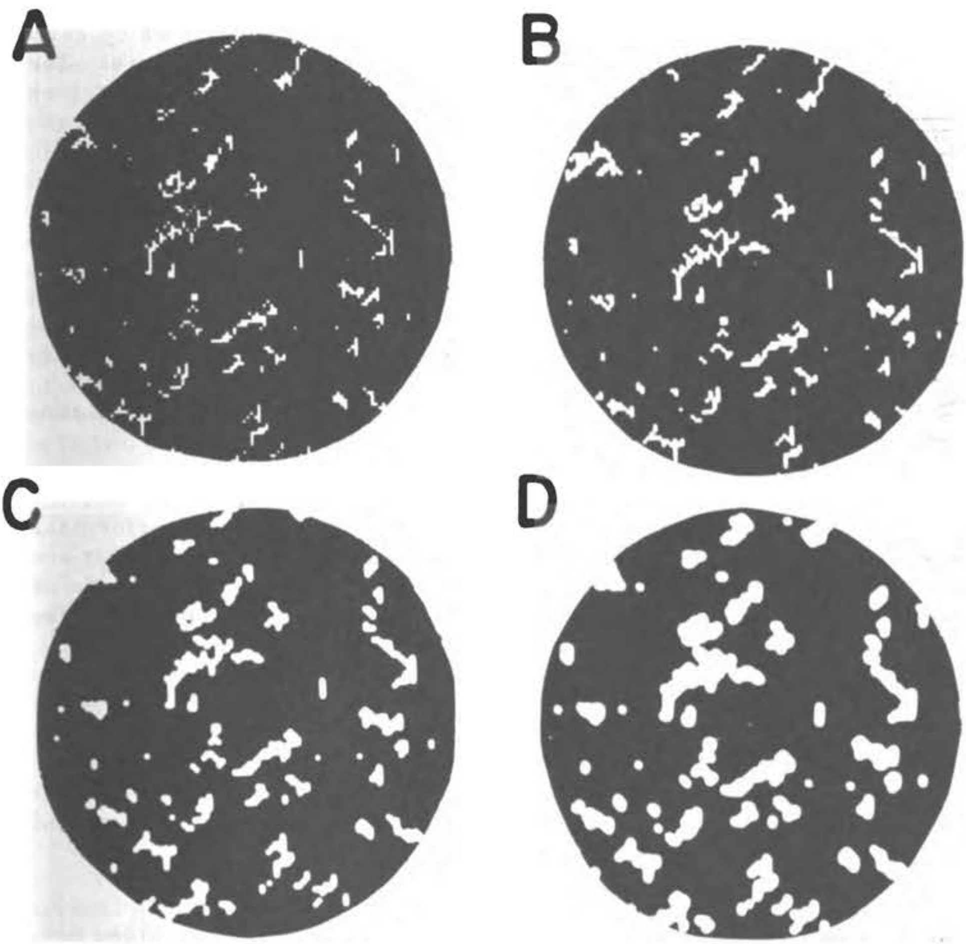


Fig. 2.

engineers at the Aero Medical Laboratory are consulted on problems involving the human mechanics of perception. Often our science can provide a good reference for some intelligent estimations concerning these problems. But often there is an empty void. It is this void and its significance in terms of maximizing our human resources utilization in the event of war that is the most compelling reason for an active research program in this area.

It is within this climate that Chuck Baker, Don Morris, Aaron Hyman and myself got together last year to outline a program of investigation on radar problems that were being submitted to us by contractors, engineers and other interested personnel at WADC. Realizing the difficulty of studying the problems of radar without adequate test materials that could be described in quantitative ways, the first problem was the construction of test forms. We came up with a system which gave us a complex matrix of forms as shown in Figure 1.

Figure 1 shows a matrix consisting of 90,000 cells of which certain cells were selected to be "return" cells by a statistical procedure. The complexity and frequency of the various resultant forms on the matrices can be varied by varying the statistical parameters used in the generation of the matrices.

The matrices are being used as displays from which experimental subjects must locate specific target forms. The stimulus displays can be degraded by various means such as adding random or systematic noise patterns and deresolving or decreasing display definition. Figure 2 illustrates how a portion of the matrix appears for various degrees of display resolution. At the present time a series of studies is underway investigating the performance for target recognition as a function of the type of briefing material, degrees of display resolution, search area size, target complexity, and other variables.

The objective of this research is aimed to determine how much degradation a scope can suffer without impairment to human performance in terms of target identification and bombing error. It is hoped that soon the Aero Medical Laboratory can present to the Vision Committee the findings from the Aero Medical Laboratory research program on radar displays.

THE EFFECT OF IRRELEVANT INFORMATION UPON COMPLEX VISUAL DISCRIMINATION

Richard H. Henneman

A problem of considerable practical significance in complex perceptual discrimination concerns the detrimental influence of irrelevant data upon the speed and accuracy of the perceptual responses. Most types of visual displays present to the observer a considerable amount of extraneous information, that is, information not directly related to the operator's immediate task. On radar scopes such irrelevant signals may appear as visual "clutter" or non-critical targets; on other types of displays (e.g., instrument panels), various kinds and numbers of stimulus dimensions may present irrelevant data to the observer. The operator's task thus frequently requires him to "filter" relevant from irrelevant stimulus data before deciding on the appropriate response. It is, therefore, of considerable importance to discover the degree of impairment of operator performance produced by the presence of such extraneous stimulus information.

This problem was one of those specified under the broader area of the consolidation of information from single and multiple sources, included in a survey on human engineering research support for the Signal Corps Engineering Laboratories by Dunlap and Associates in 1954. Investigation of this problem was undertaken at the University of Virginia at the request of the Surgeon General of the Army, with whom the University already had a research contract in the general area of complex task performance and decision-making behavior.

It seemed advisable to begin the investigation of this problem at a relatively basic level. Rather than attempting to replicate any specific operational task, the emphasis was placed on the psychological characteristics believed to be common to a large number of such tasks. It was found that an attack on this problem had already been begun in an experiment by Archer reported in 1954. Archer's subjects were required to classify oscilloscope patterns with respect to four dimensions, some of which were constant and contributed no information, while other dimensions conveyed the information necessary for the classification responses. In the experimental design the amount of relevant information varied from one to four stimulus dimensions, and the amounts of irrelevant information varied from zero to two dimensions. Archer found that response time increased as a linear function of the amount of relevant information, but surprisingly, was independent of the amount of irrelevant information.

The initial experiment at the University of Virginia sought first of all to verify the Archer findings that extraneous information that is never relevant does not have a detrimental effect on perceptual responses. In the second place there was an attempt to test the hypothesis that when the irrelevant information is at other times relevant information, level of response proficiency will be lowered. This may be contrasted with the conditions of the Archer experiment in which the irrelevant information was never relevant. Finally, it was questioned whether the difficulty of the perceptual discrimination required might interact with the possible influence of the irrelevant stimulus information.

The subjects were required to classify geometrical figures in terms of several dimensions (Figure 1). Subjects had first to judge whether the central key figure was a circle or an ellipse (form dimension), then whether it was large or small (size dimension). These dimensions, present in every figure, and basic to the subsequent judgments, were termed primary dimensions. The subjects had further to discriminate among the stimulus figures on the basis of one or the other of two additional pairs of dimensions: (1) the kind and number of markings present inside of the key figure, and (2) the shape and filling of the surrounding figure or border of the key figure. These last four dimensions, not necessarily present in every stimulus figure, were considered as secondary dimensions. (In Figure 1, the primary dimensions concern whether the central figure is a circle or an ellipse and whether large or small. The secondary dimensions are the two crosses inside the central figure, and the diamond-shaped surrounding figure or border.) All dimensions, both primary and secondary, had two values. In terms of various combinations of these dimensions, the subject was required, upon each stimulus presentation, to make one of 16 responses. He did this by pulling forward or pushing backward one of eight response keys. In deciding upon the correct response, the subject first discriminated between the primary dimensions (i.e., as to the size and shape of the central key figure). This discrimination reduces the number of response alternatives to four. The subject then chooses among these four responses

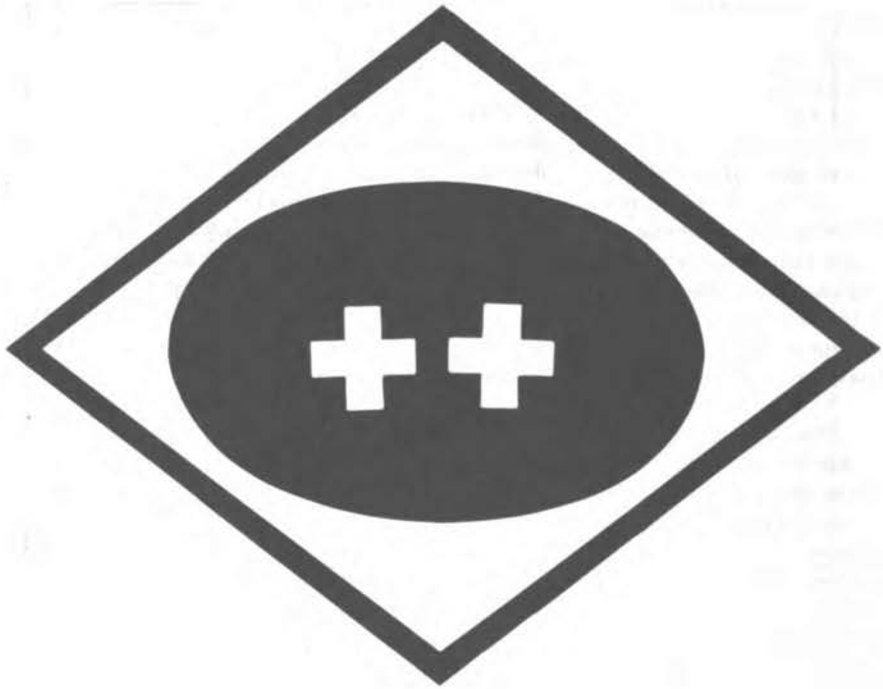


Fig. 1. Sample stimulus figure used in the experiment, illustrating primary and secondard stimulus dimensions (see text).

on the basis of the secondary dimensions (i.e., markings or border). Performance level was measured in terms of errors and response times.

Under the simplest experimental condition only those stimulus properties necessary to determine the appropriate response were present in the stimulus pattern, i.e., the two primary dimensions and the two secondary dimensions that were relevant for that particular identification. This was called the Zero Irrelevant condition.

Figure 2 shows a sample stimulus figure in which the border dimensions (rectangle, unfilled) are the relevant secondary dimensions. You will note that no irrelevant dimensions are present in the figure.

Under two other experimental conditions irrelevant dimensions were added to the stimulus figures. In the first of these conditions, the Never Relevant condition, two secondary stimulus dimensions that were never

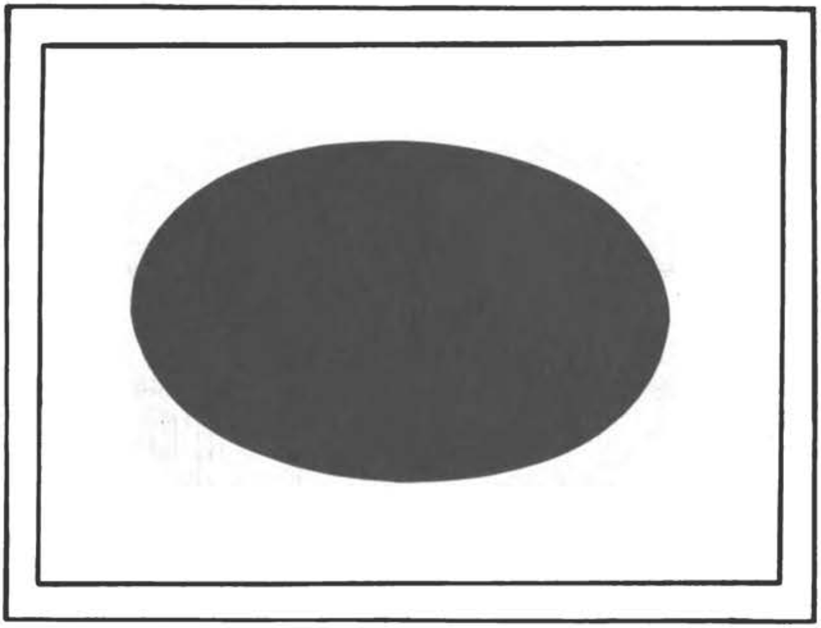


Fig. 2. Sample stimulus figure illustrating two relevant secondary stimulus dimensions. This figure has no irrelevant dimensions.

involved in the identification of the stimulus patterns were added. For example in Figure 1, markings are the relevant dimensions, border dimensions are irrelevant. For this group of subjects the border dimensions were never relevant. (Actually for another group of subjects the border dimensions were always relevant and marking dimensions were never relevant.)

The third experimental condition was identified as the Sometimes-Relevant condition. In a given stimulus pattern two of the secondary dimensions were relevant (i.e., essential for the correct identification of the figure), and two were irrelevant. Whether a specific pair of secondary dimensions was relevant or irrelevant was varied systematically from one stimulus pattern to another. For example, in a given stimulus presentation the border in Figure 1 might have no bearing on the correct identification, since markings would be the relevant dimension. In another stimulus presentation, the same border characteristics might be essential for identification, with markings being irrelevant.

From previous instructions the subject was able to determine from the characteristics of the primary dimensions, which of the secondary dimensions were relevant, and which, irrelevant. In Figure 1, the central figure is a large circle, consequently the markings (two crosses) are the relevant dimensions and the border characteristics (diamond-shaped, with heavy border) are irrelevant. Had the central figure been a small circle, the border characteristics would have been the relevant dimensions, and the internal markings would have been irrelevant.

Difficulty of perceptual discrimination was also varied under all three of the experimental conditions. This was accomplished by varying the shapes of the stimulus figures from a circle to an ellipse through graded steps. There were eight figures in all, half to be responded to as a circle and the other half as an ellipse. There were then four levels of discrimination difficulty as well as three conditions of irrelevant stimuli in the experimental design. Subjects viewed a total of 128 figures on each trial.

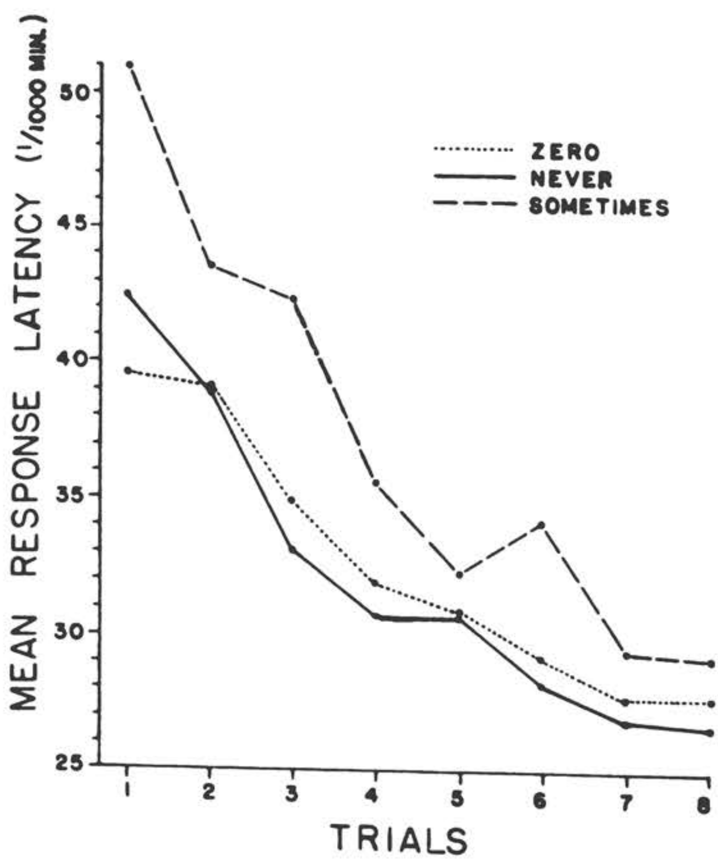


Fig. 3. Response times for subjects under three experimental conditions (Zero Irrelevant, Never Relevant, and Sometimes Relevant Information).

Curves for the response times for the three experimental groups are shown in Figure 3. It will be noted that the curves for the Zero and Never groups fall on top of one another, the data thus corroborating Archer's findings for these conditions. The time scores for the Sometimes group are higher than for the other two groups. This difference approaches significance, but is not significant. The apparent reason for this lack of significance is the relatively small amount of irrelevant information used in the present experiment (i.e., never more than one pair of stimulus dimensions).

These results led to the planning of a second experiment, designed to test the hypothesis that adding greater amounts of irrelevant stimulus data in the Sometimes Relevant situation would result in significantly poorer levels of performance. The design called for three levels or amounts of irrelevant information, and four degrees of difficulty of discrimination. The procedure and stimulus materials were essentially the same as those of the Sometimes Relevant condition of Experiment I. Complex geometrical patterns were again employed as stimulus figures, with two additional pairs of secondary dimensions being added: (1) direction and stroke-width of hatching inside the figures, and (2) color and brightness of the overall figure. Hatching was either vertical or horizontal, and stroke-width either wide or narrow. Half of the figures were red and half were blue; these colors were either bright or dim (desaturated). A different pair of secondary dimensions was associated with each combination of the primary dimensions. The subject was thus able on a given stimulus presentation to determine which pair of secondary dimensions was relevant, in terms of the primary dimensions (i.e., large circle, small circle, large ellipse, or small ellipse). In all of the stimulus figures used in this experiment there was some relevant and some irrelevant information, the determination of relevancy being dependent, as said above, upon the characteristics of the primary stimulus dimensions.

Three groups of subjects (10 in each group) were used, one for each of the three amounts of irrelevant information presented. In the simplest condition one pair of irrelevant secondary dimensions was added to the basic dimensions of size and shape, and the pair of relevant secondary dimensions. In the second condition a second pair of irrelevant secondary dimensions was added. In the third condition still another pair of secondary dimensions (now eight in all) was added to the stimulus figures.

Figure 4 shows a sample figure for the Group II subjects. Besides the primary dimensions of size and shape, and the pair of relevant secondary dimensions (internal markings), there are present, two pairs of irrelevant dimensions -- shape of the inside figure (diamond with filled border), and hatching (horizontal with narrow stroke-width). Since this figure should be discriminated as a large circle, the subject should respond in terms of the marking dimensions, ignoring the others, so the correct response would be based upon the two dashes.

Figure 5 shows the error scores for the three groups of subjects.

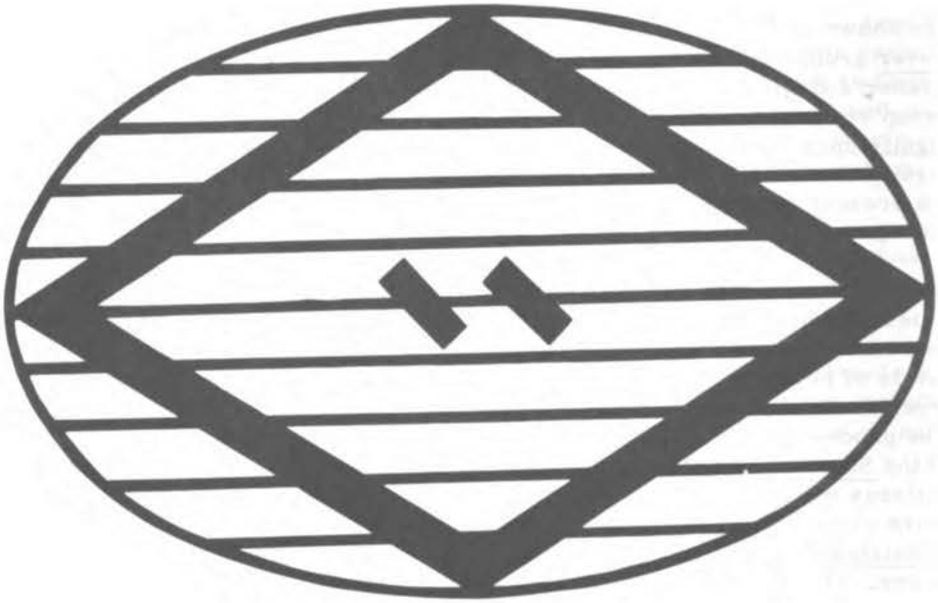


Fig. 4. Sample stimulus figure seen by subjects under the second experimental condition (two relevant secondary dimensions and four irrelevant secondary dimensions).

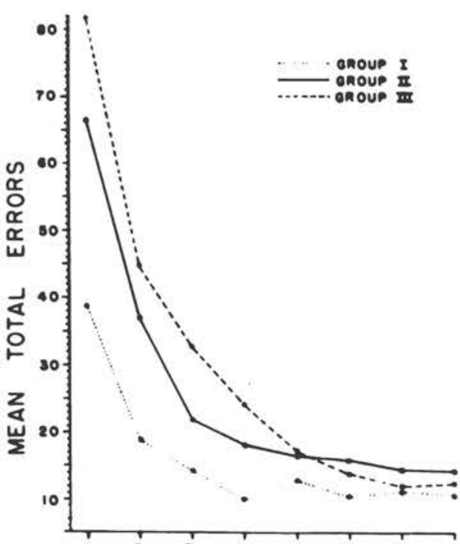


Fig. 5. Error scores for subjects under three experimental conditions (two, four, or six irrelevant stimulus dimensions).

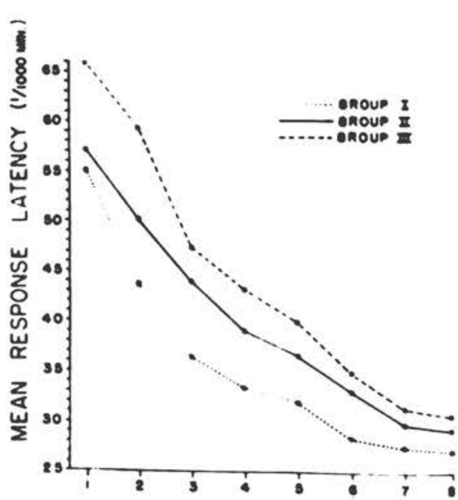


Fig. 6. Response times for subjects under the same three experimental conditions as those of Figure 5.

While there are differences among the groups on the early trials, these differences have disappeared by trial 5 or 6. (Non-parametric test used to show significance of amount of irrelevant data.) It is interesting to note in Figure 6 that for the response time scores, differences among the three groups were maintained over the eight trials. An analysis of variance applied to these data indicates that the amount of irrelevant information is a significant variable beyond the 2 percent level of confidence.

The indication is that there is some reduction in the differences in the groups with practice. This is reflected by the significant interaction between practice and the amount of irrelevant information.

Discrimination difficulty was also found to be a significant variable. It did not, however, interact significantly with amount of irrelevant information. This was probably due to the manner in which this variable was introduced into the stimulus figures (i.e., only the primary dimensions varied in difficulty of discrimination and the subjects never had any trouble in discriminating relevant from irrelevant stimulus dimensions).

DISCUSSION

GOTTSDANKER: One question for Dr. Mooney. When people had more time to see the ambiguous figures, didn't that increase the probability that they would see it and so give a higher percentage? I would expect that because sometimes these things emerge after a while.

MOONEY: That I did check. About 80 or 90 percent of the accomplishment of the group was done in the first fraction of a second. Out of two or three hundred subjects there were perhaps only two or three cases where continued time beyond, say, thirty seconds resulted in a closure. Very, very rare. In fact, so rare that we didn't know what to make of it.

ANDERSON: Did your subjects respond immediately or did you find any relationship with the time it took them to respond? Maybe I should ask the question another way. What was their response and how was it given?

MOONEY: Well, what they did was press a little light, or signal, signalling to me that they had got the closure. This was so I could note this and I did, in fact, take time in reading it. The picture was removed thirty seconds after it was put out for inspection.

ANDERSON: Did you find any relation between closure and time of pushing the light?

MOONEY: Very little. They all piled up at the first second. I've wished that I had a faster apparatus so that I could have got into their minds quicker because they just instantly came up with this.

HENNEMAN: May I ask Dr. Mooney one question? I believe some of your figures are distorted by omitting parts of the pattern. In others you had what I would call irrelevant data. I wonder if the figures where you had irrelevant material were more difficult than those where you had just omissions. Would you know offhand?

MOONEY: I would not know because I have not done such an item analysis as would take me down to each item. I used forty items all told. I used to stir them up and randomly sort into, say, four groups of ten for my analysis of variance procedures but I just wouldn't know the answer. I don't think earlier work with other closure materials gives it either. You see, in this last work I reported I used only the human head and face but in earlier studies I used various kinds of objects depicted such as Thurstone had used, and there I mixed up. I used two types of closure items: those where the graphic material was all relevant to the portrayed object and others where I deliberately put in irrelevant information, because you recall that Thurstone had got two closure factors in his factorial study of perception. One factor was the speed of closure and the other was one he called flexibility or ability to get the thing out when it was sort of concealed. This, it seems to me, was an odd logical position. How did the subject know what's relevant and irrelevant. It is purely the experimenter's convention. My feeling of that work was that the subject was innocent of knowing what was relevant and irrelevant and simply, in a simple-minded way, went on seeing.

GERATHEWOHL: Mr. Mooney did you use any other method for checking eye position during the exposure? Did you photograph eye movement?

MOONEY: No, no. It is not possible for us to do this in our laboratory, but now we have a machine which will pick up the muscle potentials from the muscles and gives us sine curves and other representations of eye movements and I now shall do this. I'll have subjects do these closure exercises and take such photographs.

GERATHEWOHL: I was thinking of the ophthalmograph.

MOONEY: We tried using that and the machine was so complicated it kept breaking down on us and nearly sent us frantic. We gave it up.

GERATHEWOHL: I used it for years ...

MOONEY: Well, we were new. We didn't learn to master it, I'm afraid, in the time this was going on.

SOME NEGLECTED ASPECTS OF RESEARCH
ON RECOGNITION AND IDENTIFICATION OF FORMS

Mason N. Crook

I want to make only three points more or less of a free-association nature, and one digression. The reason for the digression will be apparent a little later.

My first point is kind of a reminder that in a good many practical situations forms are transmitted through instruments or equipment which distort or degrade or do other things to the forms. The characteristics of the equipment, the effect of the equipment, I should say, can sometimes be measured. Information of this kind tells you something about some of the relevant limits of the situation. It might tell you something about how to design the equipment. Now, the reason for bringing this in again this morning, after our discussion yesterday of this kind of situation, is to give me a chance to show a couple of sets of data.

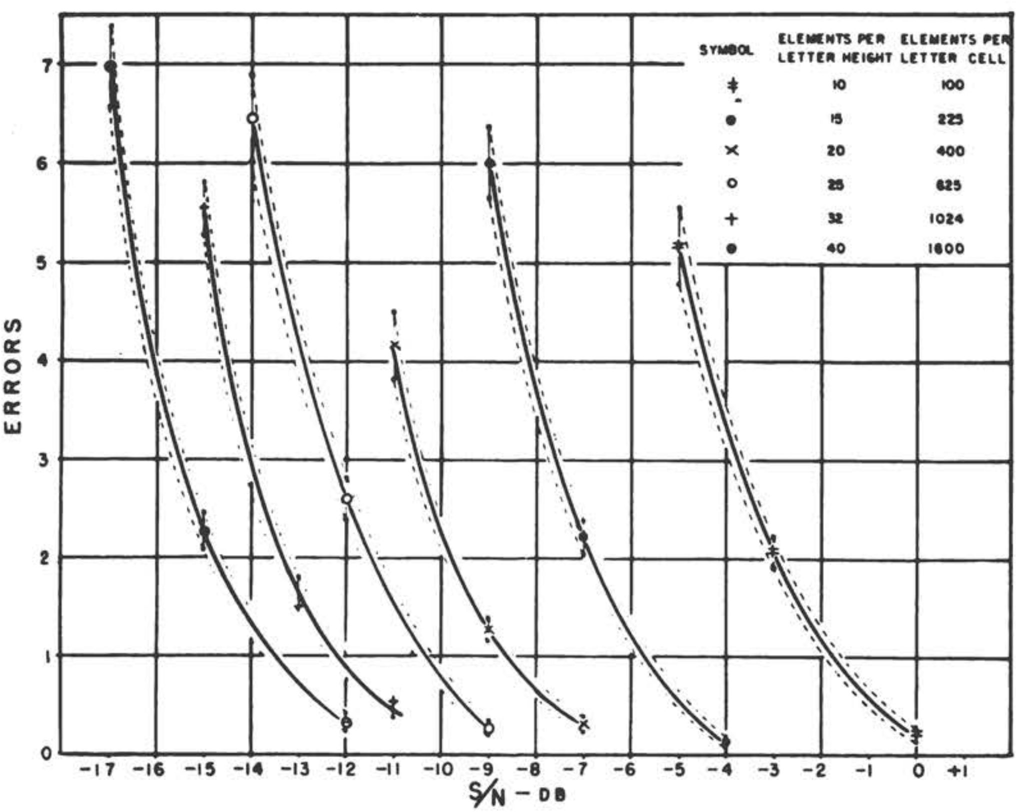
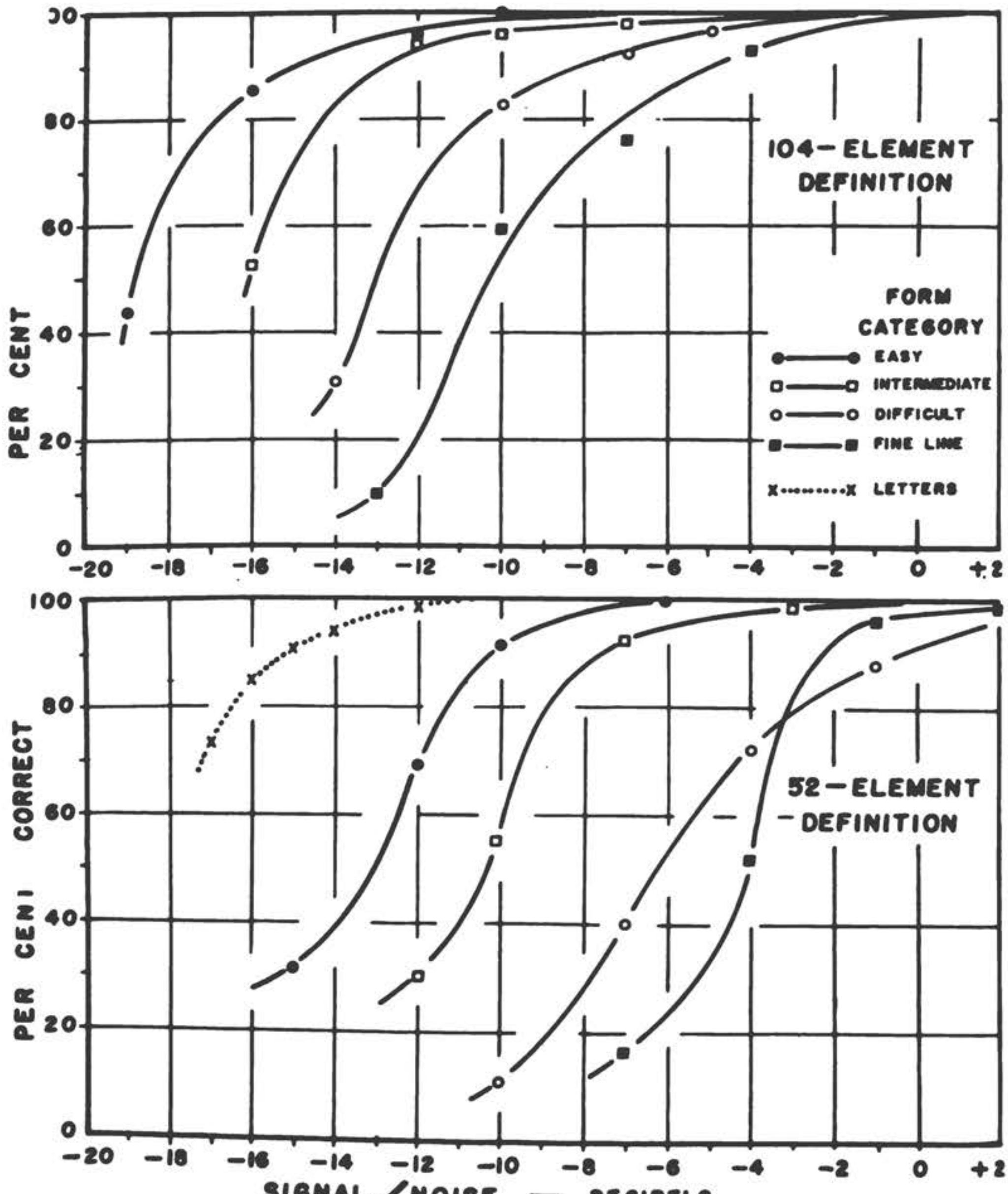


Fig. 1.



104— ELEMENT DEFINITION
FORM CATEGORY
EASY
INTERMEDIATE
DIFFICULT
FINE LINE
LETTERS
52— ELEMENT DEFINITION
PER CENT CORRECT
SIGNAL / NOISE — DECIBELS

Fig. 2.

Figure 1 is a set of scores on Infomax copy which shows errors in sets of 26 scrambled letters as a function of signal noise for letters of various definitions. You probably can't read the lettering and numbering but it doesn't matter. The higher signal noise values are at the right, the lower at the left, of course. The lower definitions, that is, relatively smaller numbers of printing elements in a single letter are at the right. As the curves progress to the left the definition improves. It shows quite clearly the interaction between definition and signal noise in this particular kind of situation. The little vertical marks are standard errors of data points.

Now, Figure 2 is an analogous set of data on those familiar forms that I showed yesterday.* This is plotted differently. The scores here are in terms of percent correct instead of errors. The top graph shows one definition; the lower graph shows another definition for the same sets of forms. The upper graph is the better definition and the two scales on the abscissa are comparable so that you can see that the curves fall farther to the left when the definition is better. The four curves in each of the two graphs represent the four categories of forms that appeared in the figure that we showed yesterday. The one to the far left is the so-called "easy" forms; the next, the "intermediate" forms; the next, "difficult" forms; and the last, the "fine-line" forms. In the lower graph the small segment of dotted curve to the extreme left is a bit of Infomax data comparable to, and I believe taken directly from, the data that entered into the preceding figure. It gives you an idea of the difference in the level of scores for the different kinds of test material. The two curves in the lower graph on the right cross near the top. Now, I can't say statistically that that's a reliable effect but I rather suspect there's a real effect here of some kind because these different kinds of forms interact in rather interesting ways with definition and with noise. So, that is an illustration of some of the kinds of things that might possibly come out when you examine all these variables in relation to each other. Well, that ends my first point.

Now, my second point carries back to some comments that were made yesterday about the time factor in perceptual responses, and I want to concur in the implication of those comments that time if often a neglected factor in measurements of perception and one that might well be given more attention. If you know how long it takes a person to make a perceptual response, you have much information about what's going on psychologically. There are of course applied situations in which this is an important kind of information to have. We have time scores in several sets of experiments: not as many as I would like because it has often happened that, by the time we got an experiment organized so as to do all the other things we wanted it to do, time scores piled on top of that would have made the experiment so cumbersome that we wouldn't have been able to do it at all. But I might introduce one example and here's where the digression comes in. We have been using in a recent project incomplete Street-type forms and we have been getting results which apparently differ in some details from Dr. Mooney's. I think it's unlikely that these ostensible discrepancies are such that they couldn't be resolved if we compared notes in detail, but I don't think this is the time to get involved to that extent. It may well be, and I

*See p. 91.

suspect it is true, that the particular set of incomplete forms you use may determine results in relation to the other variables such as Dr. Mooney talked about and such as we were concerned with. We were using some of the original Street forms and some modifications as they have appeared in the more recent literature. (Well, some of it not very recent -- the article by Leeper, for example, and the one by Verville and associates.) I don't know for sure yet whether Dr. Mooney used any of the same forms we did because I can't see these things myself. I'm perceptually an imbecile on this kind of a test. But the main problem we were concerned with here was the effect of age on perception over the age range 20 to 50 years. We were not doing this in any particular theoretical frame of reference. We used quite an empirical battery of tests such as might be considered to measure some aspect of perception. Incomplete forms of this general type were included in the battery. Now, when we present these forms at 60-second exposure time we get some indication of an age difference over this range. Not a very conclusive one. I think between two subject groups, one group did show an age difference and the other one did not. So, that is kind of marginal. When we present these forms or similar forms at a one-second exposure interval there is a substantial age difference. As we increase the exposure interval to five seconds and ten seconds the age difference tends to decrease. Here, apparently, we differ with Dr. Mooney in two respects: we get an age difference and he doesn't, and, we get a time effect and he doesn't. It's very clear in our data and in the way our results come out, that the subjects were not getting closure on our forms in a flash. They were getting it frequently delayed for some seconds. This suggests immediately that the particular forms used may have been the critical thing in differentiating the two experiments. Well, that is simply an illustration of the relevance of the time factor as an aspect of the perceptual response.

Now, my third point -- that ends the digression incidentally, also -- my third point is a still more impressionistic one and one that may seem rather simple minded. I, myself, and I suppose other people, often get so involved in trying to control stimuli and to measure and quantify responses that from time to time I lose sight of the fact that it may be as helpful to know how the subject is going about the particular operation that he is engaged in as it is to know how well he is doing it. If you stop to think about this you recognize that whenever you put people in a situation complex enough for there to be any freedom at all, and let them get worked into it, they are likely to come to a way of using the cues which is different from the way that was contemplated when the system was set up. This happens to both individuals and groups. For example, if I were working on a somewhat comprehensive level with a problem of efficiency of radar operation, I would want to know a good deal more about how the operator goes about the job of making the discriminations and the judgments he does than I do now. When I try to put myself in this situation -- when I try to see just exactly what is happening there -- I find myself at a loss. Knowing how the subject is operating may give you not only psychological insight about the process but it may give you information that's useful in structuring the situation. With regard to how you go about getting this kind of information, there are several ways. The first one might be considered just simply a formalized program of setting up a sufficiently varied and complicated set of experiments that the performance data themselves enable you to make the analysis. You would eventually tease out exactly what the subject is doing as well as how well he is doing it. This is the most formal as well as the most formidable of the ways you can proceed.

The next thing that you can do is to ask the subjects what they are doing. When Behaviorism was flourishing this was not considered respectable and a good many people more or less took the point of view that even though they were using subjects that could talk it was proper to treat them as rats anyway. The emphasis has shifted away from that point of view for various reasons, but I think it is still true that our laboratory habits are often such as to tend to make us forget that what the subject can tell us is sometimes a useful additional source of information about what's going on.

The third thing you can do is to put yourself in the situation of the subject; observe the test material yourself. I think it would probably be a good working policy for every experimenter to go completely through the experimental routine as a subject. I don't do this myself systematically but only because I don't always practice what I preach. The kinds of things that you can observe here I might illustrate in an instance that is so simple that you might think nothing could be learned from it. When you look at letters and numbers under near threshold conditions all kinds of interesting things happen. One impression I get from doing this is that, in a fair number of cases, when what we might think of as the critical detail is definitely below the acuity threshold, you can use other kinds of cues as a basis for judgment. Now, most of these other kinds of cues, I think, could be subsumed under the general conception of distribution of mass. One letter looks like a different kind of a mass from another letter; not in amount, but in spatial arrangement. It doesn't seem like you're discriminating detail. It seems like you're making a judgment in some other terms and I think this definitely contributes to the scores in some instances. Another example of it: consider the letter "I" or the digit "1" being tested along with other characters, in light strokes on a dark ground, by a threshold recognition method. Below the recognition threshold, any character looks like a little blob, but under standard exposure conditions you will get less light from the "I" than from the other characters because of its smaller area. This, itself, is likely to become a cue. The subject is likely to learn that this very faint character is an "I" and report on it on that basis before he can discriminate it sharply enough to be able to judge on the basis of its outline. One other illustration here: in the Standard Aeronautical Digits you have an interesting asymmetry of confusion. The "4" is confused

4 6

with a "6", is mistaken for a "6", much more often than a "6" is mistaken for a "4". Now, on the face of it this didn't make any sense to me at all when I just looked at the data. When I got to looking at the characters themselves under near threshold conditions, I think I understood what happened. Let's start with the "6". The characteristic thing about the "6" is the tail sloping up to the right. That's what differentiates it more clearly from other digits in the set than any other thing about it. Under near threshold conditions you get the perceptual effect of this diagonal stroke. Now turning to the "4", the thing that happens near threshold, though it might not have occurred to you looking at these characters in clear copy, is an impression of the diagonal stroke which is not dissimilar to what you get with the "6" -- that is, it suggests our generalized conception of a "6". This occurs because of the design of this particular "4". Looking at the "6", on the other hand, it never produces an impression that is similar to our generalized conception of a "4". This, I think, is as a good example of a kind of thing that you could not have inferred, at least without being kind of a genius in inferring these things, from looking at the data alone.

COMPLEXITY AND HETEROGENEITY IN THE VISUAL RECOGNITION OF TWO-DIMENSIONAL FORMS

Benjamin W. White

Last year James Deese of The Johns Hopkins University reported in an experiment on the effect of complexity of contour on the recognition of visual form that subjects could more easily recognize complex forms than simple ones. The forms employed in this experiment were similar to those shown in Figure 1. To quote from Deese's report: "The forms were drawn by introducing certain arbitrary restrictions into the chance selection of numbers determining the direction and length of segment of successive segments of the contour. There was no attempt to regulate similarity or any other relationship between forms; some of the forms probably resemble one another more than other forms, but such resemblances were dictated by the chance selection of segments of the contour and not by design."

Subjects were presented with a single form for 10 secs and then, after various delays, with a set of five forms from which they were instructed to pick the one which was identical with the single form first presented. There were four delay conditions varying from zero to 60 seconds. In all delay periods, there were more errors of identification made to the simple forms than to the complex.

These results are contrary to intuitive expectations, and we wondered if they might be due to the fact that the complex figures formed a more heterogeneous set than the simple ones. As Deese indicates, there was no attempt to control this factor, and it seems highly probable with his method of generating figures that the more complex set was indeed more heterogeneous.

To test this supposition, an experiment was performed with four sets of figures similar to those used by Deese. These sets were constructed

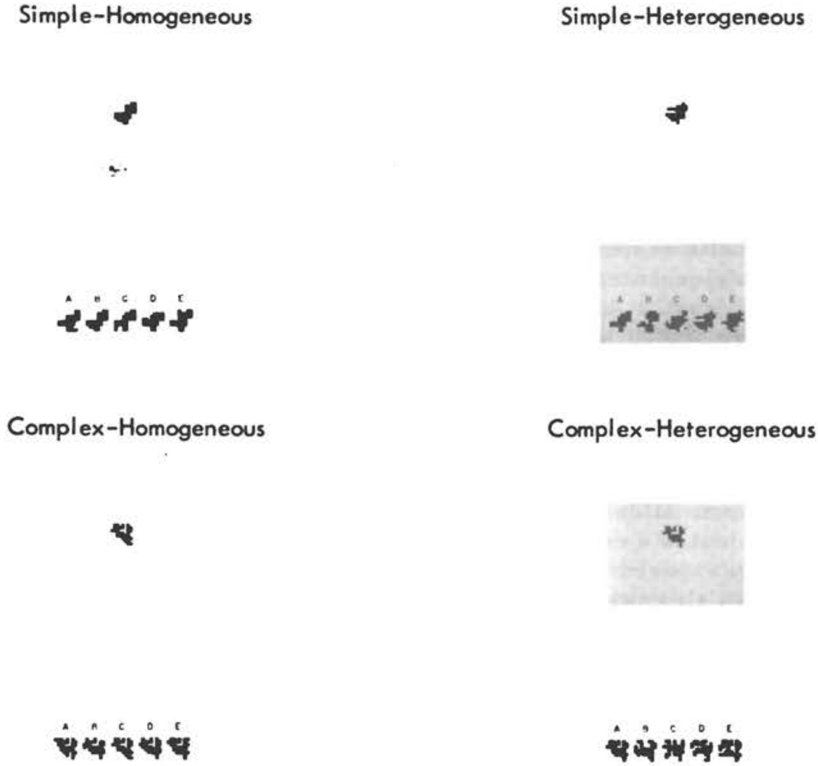


Fig. 1. Sample slides from four sets of 25 differing in complexity and heterogeneity. Top slide in each group is sample target slide. Subject was instructed to find in the five-form slide beneath.

so as to control set heterogeneity as well as complexity. The prediction was that with set heterogeneity controlled, the complex figure would no longer be easier to recognize than the simple.

A brief description of the method of constructing the stimulus materials is in order. On an 8 by 8 matrix 32 cells were blackened to form a "simple" figure. This had 16 right angles about its contour. From this figure, 24 variations were constructed by moving two blackened cells to new locations in the matrix. This was done in random fashion by making up two sets of numbers corresponding to the black cells which could be moved, and of the unfilled cells to which they might go. Drawing two numbers from each population thus specified each variation of the basic simple figure. The 25 figures so constructed were then grouped arbitrarily into groups of five and all possible paired comparisons were made within each of these groups to determine amount of overlap between superimposed forms. Though all the forms differed from the basic simple form by two cells, some of the variations differed from each other by as much as four cells. Average overlap was 28.5 cells. This formed the "simple-homogeneous" set. In a similar fashion, the same basic simple form was changed by moving five filled cells to unfilled positions to create 24 "heterogeneous" variations. The overlap of the pairs in this set varied from 22 to 29 cells, with a mean of 24.6 cells.

Using the same technique, a "complex" figure was now drawn in the matrix. This had 30 right angles around its contour. A homogeneous and a heterogeneous set was created exactly as for the simple figures by shifting 2 and 5 cells respectively, thus forming the "complex-homogeneous" and the "complex-heterogeneous" sets of 25 forms each. Average overlap of these sets was 28.4 and 23.6 cells respectively. The forms so constructed were photographed and the negatives made into 2 x 2 slides. Other slides such as the ones you have just seen were constructed showing sets of five forms.

Using two slide projectors equipped with shutters, a single-form slide was exposed from one projector for 1/50 sec, followed immediately by the five-form slide which contained the single form. This was exposed until the subject had responded. The subject was instructed merely to look at the briefly exposed form and then to specify the letter of the form on the multiple form slide which was identical with the single form. The forms in each set were given in a block in the same order to each subject. The order of sets was counterbalanced from subject to subject. The size of the image of a form as projected on the screen was approximately 3 inches by 3 inches. The subject was seated nine feet from the screen in a semi-darkened room. He responded to each form by marking a previously prepared multiple response sheet. Each set of 25 forms was presented without interruption and there were short rest periods between sets. Subjects were tested individually and a session took approximately 20 minutes. There were 12 subjects in all.

Results

The percentage of correct identifications in each of the four sets of forms is as follows.

	Simple	Complex	Mean
Homogeneous	74	59	67
Heterogeneous	89	84	86
Mean	81	71	

The homogeneous sets were significantly more difficult than the heterogeneous and the complex sets significantly more difficult than the simple. The difference between the homogeneous and heterogeneous sets holds for both simple and complex forms. The difference between the simple and complex sets, however, holds only under the homogeneous condition.

These results lend support to the assumption that Deese's complex figures were more accurately recognized only because they were more heterogeneous than his set of simple figures. Since heterogeneity was uncontrolled and unmeasured in his experiment, this conclusion must remain tentative though his method of form construction made greater heterogeneity of the complex set almost inevitable.

The technique used in this experiment to measure the homogeneity of a set of forms appears to be a promising one. The process of constructing stimulus materials in this manner is laborious, however. A parametric study to relate degree of homogeneity to recognition responses would present a formidable task of stimulus construction. There is no reason, however, that the generation of such forms could not be programmed for a digital computer equipped with an appropriate scope output and a camera. As you see, we at Lincoln are rather addicted to this sort of thing.

The method employed in this study for assessing the homogeneity of a set of forms is most appropriate when all the forms are of equal area, when they are generated by introducing controlled variations on a single parent form, and when all the forms are presented in a single orientation. Even under these restricted conditions, many may feel that this is not an appropriate measure of similarity. This single experiment hardly establishes its utility. It does, however, show that it is important to have some such measure in order to interpret obtained relationships between recognition behavior and other stimulus dimensions such as complexity. It is also important that the measure of homogeneity be truly independent and objective, and not based on judged similarity lest recognition research find itself going in laborious circles.

REDUNDANCY AS A STIMULUS PARAMETER IN FORM DISCRIMINATION

Maurice Rappaport

I find this meeting very frustrating and rewarding at the same time, for two reasons: (1) it enlarges my conception of the universe of things you can do to investigate form perception and brings my universe down to its proper humble perspective and (2) I wish I had read Dr. White's paper before I sat down because apparently we have some interesting things in common, which, at this moment, I won't be able to resolve. But, be that as it may, I think one thing we've recognized here is that there are various ways of approaching this problem of form perception. In a sense we use different templates. The templates might be physical, mathematical, or other types of models. Quite frequently we recognize that the templates don't exactly fit over behavior as we observe it, but the whole purpose of experimentation is to refashion the template so as to take out the little kinks where behavior doesn't conform to the template.

I had a few comments to make about how we find out where to refashion the templates but, in order to conserve time, I think I'll skip that and go on to point out the template we did use at Ohio State University where much of the work I'm going to discuss was originally initiated under Dr. Paul Fitts. In the particular series of studies that we did there we emphasized the stimulus parameters. As Dr. Graham pointed out yesterday and as Dr. Arnoult also mentioned, one can't just look at the stimulus parameters and hold constant the observer variables. I think it was very encouraging to hear Dr. Stilson's remark that he did an experiment where 70 percent of the variance was accounted for by physical factors associated with the stimulus. That leaves 30 percent for the neuro-physiological approach to account for. As I say, we didn't look at such things as attitude, motivation, and the state, the psychological state, of the individual at the time. We were aware of such factors as McGinnis's perceptual defense mechanism and how this can influence what you perceive, and so on. But, we chose to ignore this because we did have our little miniature model. As you all seem to be doing, we were pursuing this model.

Now we did two things. Part of these things were mentioned yesterday by Dr. Anderson. We looked at the effect of amount of information as complexity, defined by amount of information. This is what makes me so frustrated: because of lack of time, I decided not to talk about this after hearing what Stilson said about complexity, that is, form discrimination being inversely related to amount of complexity. We found a similar result and it would be fun to discuss this for we do it through different concepts. Unfortunately, I have to skip this now. This is why I'm frustrated.

So, I'm going to go on and just talk about the concept of redundancy as used in communication theory and show how we applied this to the problem of form perception to see what sort of insight this gives us as to how people perceive different forms. Now, what is redundancy? I think most of us probably know. It was defined technically, yesterday, as one thing, but in a symposium like this the rate of information processing that people can absorb here, I think, dictates that one does give this information out. Let me just say that an informal definition of redundancy is this: that there are more symbols present than are needed, theoretically, to convey a message. For example, if a pilot in an aircraft calls up the control tower he may say "this is 2134, this is 2134." He said it twice. That is redundancy. He repeated the symbols. Really, if it were a perfectly clear channel he need only have said it once. So, redundancy simply says there are more symbols present, more events present, than are needed in a certain situation to interpret what is going on. Now, according to communication theory, the role of redundancy apparently will only come into effect when noise if present. That is, it has the facilitating effect of reducing the distortions introduced by noise in any particular system. I think, theoretically, they say that no matter how noisy the system is, if you make the message sufficiently redundant, you can get it through. Of course, this might take an infinite length of time, but you can do it. The effectiveness of redundancy has been demonstrated with respect to speech problems and auditory communication problems. At Ohio State University we applied the concept to visual discrimination problems to see if it might not have some relevance there. We set up the hypothesis, which goes along with communication theory, that redundancy would facilitate the recognition of visual forms when noise was present. We don't know what we mean when we say "noise" with respect to the human observer. Of course, if we throw blotches on a stimulus we can recognize that as external noise, but there can be noise within the system. You might insert a lens between the subject and the stimulus he's looking at which blurs it or distorts it. That's a form of noise. You might ask him to wear blinders, you might be talking to him at the same time or he might not be feeling well: all this might be noise. Well, we thought we'd look first at the effect of redundancy in a relatively noise-free situation, that is, in a situation where the stimulus wasn't distorted.

Figure 1 shows representations of the stimuli we used here. The top row going across represents 8 figures selected at random from a population of 24 possible figures. Each column shows the same figures made more and more redundant. Consider the first figure on the left. You can see that the figure under it is the same figure made redundant simply by making it symmetrical, flipping it over, getting a mirror image. Other redundancy is introduced in it by increasing the cyclic patterns. For example, if you can consider the bars as having four widths, 1, 2, 3, or 4, you'll notice that the pattern on the first one extending to the right is 2, 1, 4, 3. Going down a level, you have 2, 1, 4, 3 made redundant. Going down farther you have 2, 1, 4, 3; 2, 1, 4, 3. Going down farther you have the same pattern repeated three or four times. In other words, we have more symbols here than are needed, theoretically. All you really need to see is the first four patterns to discriminate the figure and all the rest is redundant. These represent levels of redundancy of 43%, at the top; 71%, when it's symmetrical; 86% and 91%, and so on.

The question is: How would this work out? We all have our templates. We all have our theories. Take just one second to predict what sort of curve you would expect if you had amount of redundancy along your abscissa and response time along your ordinate. We don't have time to get as assessment, but I sure would like to see what people would predict according to their own theories. I'll show you what we got right now in Figure 2. Essentially we found that with increase in redundancy there was an increase in sorting time, in this particular case, which says that the hypothesis wasn't supported. In fact, the opposite was true. Apparently, here in a relatively noise-free situation redundancy doesn't have much of a facilitating effect at all. It's irrelevant information. As an afterthought this seems reasonable. As a further afterthought, taking what Dr. Crook said, when you look at the figures you can see other reasons why this should be so. We weren't discouraged. We said, "Let's see what happens now when we really introduce noise into the situation. Let's introduce noise, like black splotches, by taking a certain amount of the cells in the matrix in which these figures were generated and filling them with black splotches." We arbitrarily chose 17% noise. Why did we choose 17%? Because it made the figures sufficiently difficult to discriminate: more difficult to discriminate than without noise, but not so difficult that the subject couldn't do the job. There are several things you can do with noise

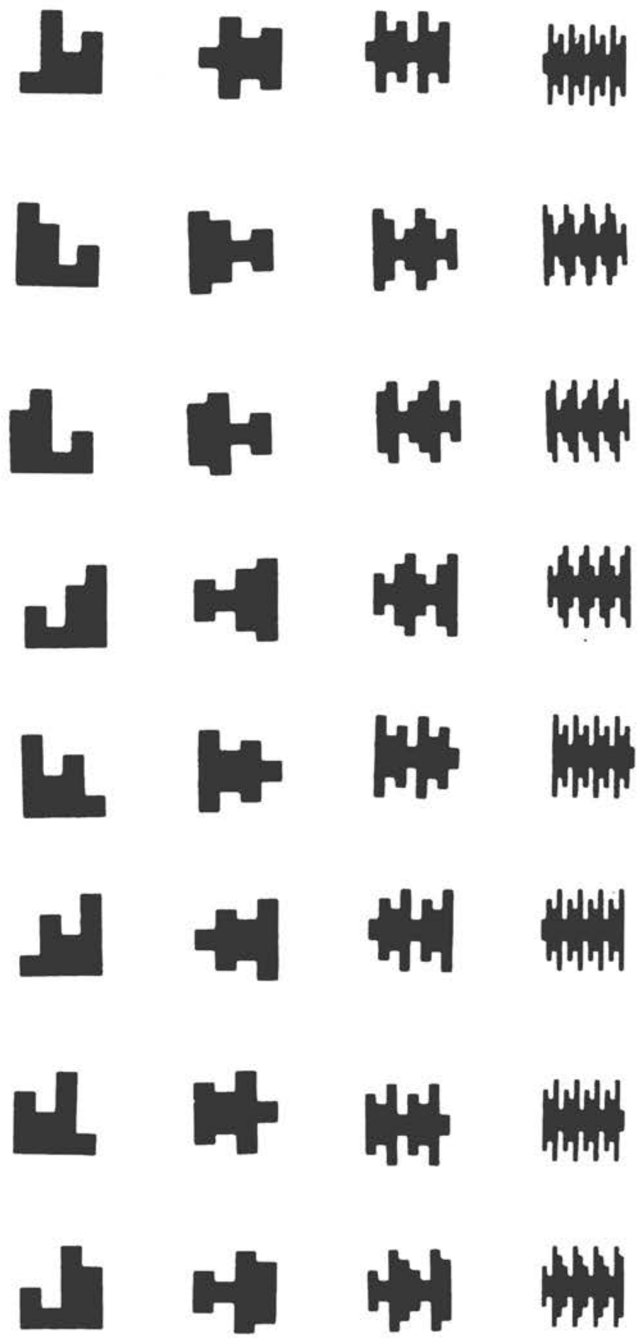


Fig. 1. Four steps of redundancy without noise.

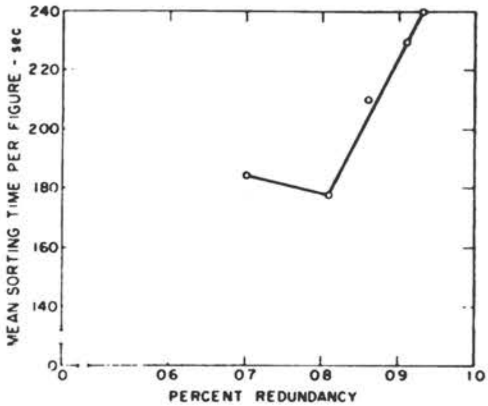


Fig. 2. Noise free.

and I'll bring up this point a little bit later. You can subtract from the figure, you can add to the figure, you can change the kind of noise you use and so on. In this particular experiment, we simply chose to add noise, which meant that we added black spots like the figure. When we did this the figures looked like those in Figure 3. These are the same figures with noise added. They are slightly distorted. In the experiment, the subject saw a noise-free sample. Then he had to go down and pick out the figure that was like it. He had to do this several times, of course. According to your own theory, what would you predict? I'm working on a communication theory model here. That is, as the level of redundancy increases if you have noise, how would you expect the curve to go? Decision making cannot be done at this rate, but at least you have a chance to think about it. In Figure 4 you'll see what happened. Actually, it took significantly less time to sort the figures that were more redundant than the figure that was least redundant. It's a funny looking curve but all those points are below the first dot on the left. The first dot on the left is the least redundant figure. Here we see that apparently we substantiate the hypothesis that even in a visual communication channel redundancy does have a facilitating effect when external noise is present. Apparently, it plays the same role here as it does in communication theory. Under conditions where noise is present redundancy is helpful. Where noise is not present redundancy is superfluous and inefficient.

I couldn't help but reflect on the applicability of these results to some of the things people said yesterday, so I drew on some of the things that were shown. For instance, in Dr. Green's figures he showed patterns that were bars that some of us couldn't see. But, I dare say, the fact that some of us could see them under noise distortion was simply because there were several of the bars going across. If the bar were the signal, it was redundancy introduced in the pattern that allowed us to see it. If there were one bar in the figure it would be less redundant and we would have a more difficult time seeing it. We might be able to use the redundancy concept to offer some explanation here.

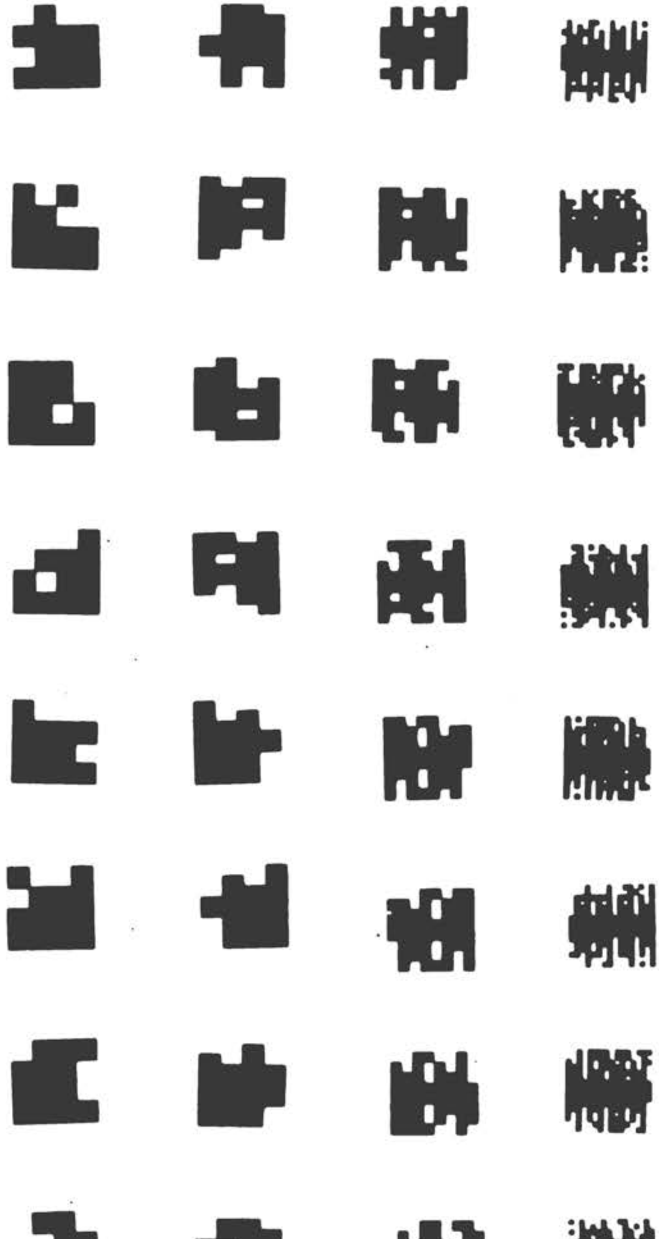


Fig. 3. Four steps of redundancy with noise.

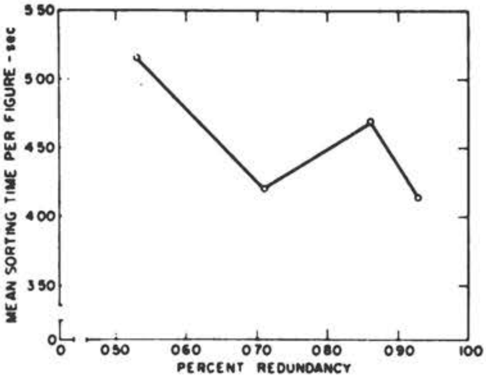


Fig. 4. Noise added.

In the figures Dr. Crook showed, for example, I think he had a picture of a spoon and the spoon was of different sizes. This brings up a point. One can consider size as a form of redundancy and that increase in size is increase in redundancy. As he pointed out, the little dots of noise on his figure did a heck of a lot more damage in disrupting the spoon pattern when it was a small size than when it was blown up to a large size. This points up a thing I hinted at earlier.

Maybe what we need now, or maybe the way this points, is to do some research to investigate the relation between types of redundancy and types of noise: there are all different types of redundancy one can play with and all different types of noise. One suggestion I have here is in the form of a hypothesis I think there's reason to expect would be substantiated. I think noise that is least compatible with the figure -- let me put it the other way -- the figure that is least compatible with the noise is the one that will be more easily discriminated. This has a bearing on radar displays. If you can classify somehow the types of noise that are most likely introduced into your system, then you ought to try to get displays on the radar that are least compatible with those types of noise.

What do I mean by "compatible"? If you recall Dr. Crook's figure* yesterday, and I'm just working from memory, he had his eight-sided figure distorted by noise. In one case the noise was round circular dots, as I recall, and in the other case it was sort of triangular, jagged -- triangular noise, I guess. It would strike me that since the figure was jagged in its shape the triangular noise being more compatible and more similar to the figure would have a greater disrupting effect on the contour pattern of the figure and would make it more difficult for people to discriminate than if the noise were round and the figure were jagged. This is what I mean, perhaps, by compatibility or similarity between noise and figure. Maybe this is a whole area of investigation that ought to be pursued.

Since this symposium is supposed to be in relation to military problems I think it's evident what bearing this has on countermeasures: How to confuse the operator most appropriately or, on the other hand, how to make

*See p. 84

it more possible for the guy who is doing the detecting to see through the noise by giving him the proper information?

You can see that what we've done is to take our approach from the communication theory angle and we've looked at old problems in a new way. I think that this whole concept, while it might not be the explanation of all things in perception, certainly is fruitful and productive of research and ideas and probably ought to be explored. Maybe we'll get a Helmholtz later on who can pull all our templates together into one big theory.

VISUAL DISCRIMINATION AS A FUNCTION OF STIMULUS SIZE, SHAPE AND EDGE-GRADIENT

Wyatt R. Fox

The basic response of the visual mechanism is the discrimination of changes in luminous intensity in the visual world. This discrimination consists of (1) detecting the presence of a signal, and (2) assigning this signal to a category which has definitive class properties (recognition).

Detection. In the attempt to establish the characteristics of the stimulus for the detection response, amount of area and amount of edge of the object have been advanced as determining factors. One group of investigators propose that as area is increased, the detection threshold will decrease systematically and that this response is independent of the shape of the object. These investigators have favored a view that as stimulus size increases a larger number of retinal receptors are fired and accordingly the detection threshold is reduced.

On the other hand, other investigators hold that the detection response is dependent upon the amount of edge or perimeter of the object. These investigators, however, are divided into two theoretical groups. One group proposes that the contrast disparity at the edge activates the post-retinal neural mechanism and that differential stimulation of a row of receptors will account for brightness discrimination. The other group has advanced the theory that brightness discrimination results from the retinal scanning associated with eye movements.

Certain criticisms and gaps in our knowledge prevent the choice of any single theory at this time. For example, the scanning theory is inadequate since studies have shown that the detection response can occur at temporal intervals so small that the frequency of scanning movement is essentially zero. Similarly, the area explanation is deficient in that detection thresholds differ for stimuli equal in area but varying in shape. Finally, the lack of information on the relationship between changes in the edge-gradient and the detection threshold leaves the adequacy of the second theory undetermined.

Recognition. Gestalt theory would predict that the threshold for form will increase as the ratio of perimeter to area increases, providing area is constant. Accordingly, the circle is regarded as the simplest figure and should have the lowest detection and recognition thresholds.

In contrast to this approach, the data from other studies indicate that the role of shape is not clear. For example, other figures, such as the square and the triangle, have been discriminated more readily than the circle. Furthermore, any theory of shape discrimination must specify the role of shape size in recognition. In this respect little systematic empirical information is available.

Whatever the theoretical position investigators have adopted, all agree that the contour of a figure is the "cue" or "information" carrier for shape identification. However, variations in the contour-gradient have not been systematically studied and at the present time the precise relationship between contour and recognition is uncertain.

Relation of Detection to Recognition. The relationship between recognition and detection has received little experimental attention. Nevertheless, it is apparent that a comprehensive account of form recognition must include some statement of the relationship of recognition to detection. For one thing, what is the relative contribution of such variables as area and edge-gradient to recognition as opposed to detection? Furthermore, can detection measures be utilized to predict recognition performance?

Problem. The investigation was designed to determine the effects of size, shape and edge-gradient on detection and recognition. In addition, this study sought to determine whether recognition performance could be predicted from detection thresholds.

Method. A modified Method of Single Stimuli was used to determine the brightness contrast thresholds for figures varying in this perimeter-to-area ratio. The figures, Circle, Irregular Shape, Square, Triangle, Cross, and Star, were presented over three foveal sizes and three edge-gradient conditions for each size range. The gradients were constructed by photographing the figures through various apertures placed in front of a camera lens. This procedure produced sigmoid-shaped gradients of different slopes across the edge of the figures.

Two young adults, with 20/20 acuity and no astigmatism, served as Ss. Viewing was done under daylight adaptation with ambient illumination controlled by enclosing the S's head and shoulders within a frame covered by a photographer's hood. The stimuli were presented in the center of four small orientation dots on a 21° background field (illuminated at 3 millilamberts). The Ss used their dominant eye and viewed the stimuli through a 2 mm. aperture.

The 54 stimulus combinations of target size, shape and edge-gradient were ordered in a random sequence, and the contrast increments assigned randomly to each condition. Seven preliminary lists were presented to familiarize the S with the experimental task. The Ss were asked (a) to report a change in stimulus conditions and (b) to identify this change without guessing. Daily morning sessions were run, with an occasional afternoon session, until 25 replications for each condition were obtained. A total of 6750 responses was obtained from each S after preliminary training.

Findings.

I. Detection (Figures 1 and 2)

(1) Size. Increase in size of the stimulus decreases the threshold of detection at a decreasing rate.

(2) Shape. The stimulus shape did not affect detection thresholds for the smallest size targets. However, for the two larger sizes, stimulus shape does influence the detection threshold in that the Irregular Shape and the Cross have, in general, higher thresholds.

(3) Edge-gradient. Decreases in the steepness of the edge-gradient were systematically associated with increases in the detection thresholds.

II. Recognition (Figures 3 and 4)

(1) Size. Increase in the stimulus size increases the frequency of correct responses.

(2) Shape. The shape of the stimulus had a significant effect on the recognition threshold. The Irregular Shape and the Cross had higher thresholds in general, than did the Circle, Square, Triangle, and the Star. Few "circle" responses were given to non-circular forms. The latter finding is contrary to predictions from Gestalt theory. Furthermore, predictions based on such physical parameters as area, amount of perimeter, and of perimeter-to-area ratio could not adequately predict the present findings.

(3) Edge-gradient. A decrease in the steepness of the edge-gradient systematically increased the threshold for recognition. The use of incorrect response categories did not increase appreciably as the steepness of the edge-gradient decreased. In fact, Ss stopped responding altogether to the very blurred figures except at the brightest contrast increment.

III. Relation between Detection and Recognition

(1) As size of a stimulus increases, recognition thresholds regress at a negatively decreasing rate on the detection thresholds as a limit.

(2) The recognition thresholds were slightly more affected (higher) by edge-gradient changes than were the detection thresholds.

(3) The threshold ranking for shape was essentially the same

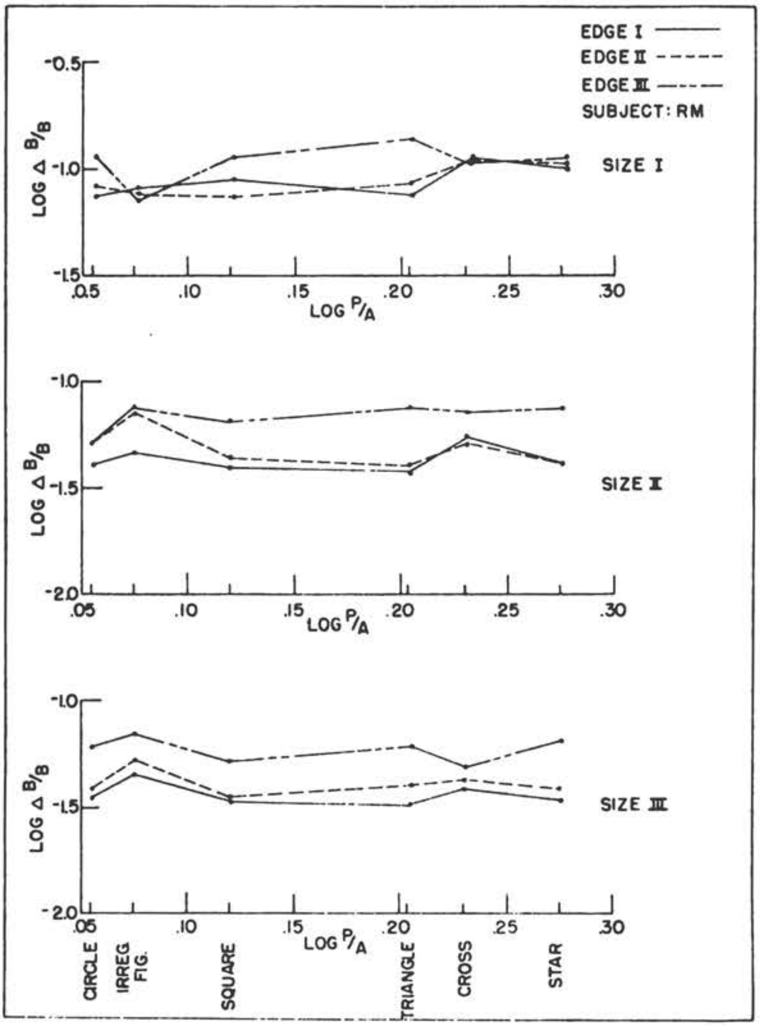


Fig. 1. Fifty percent detection thresholds for subject RM.

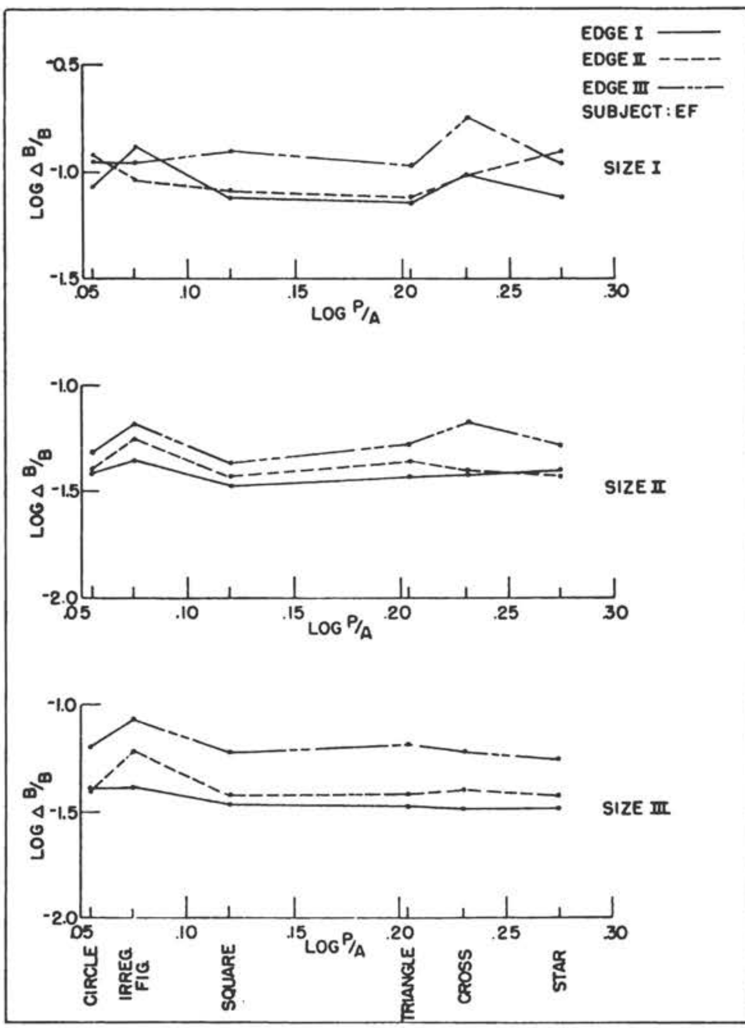


Fig. 2. Fifty percent detection threshold for subject EF.

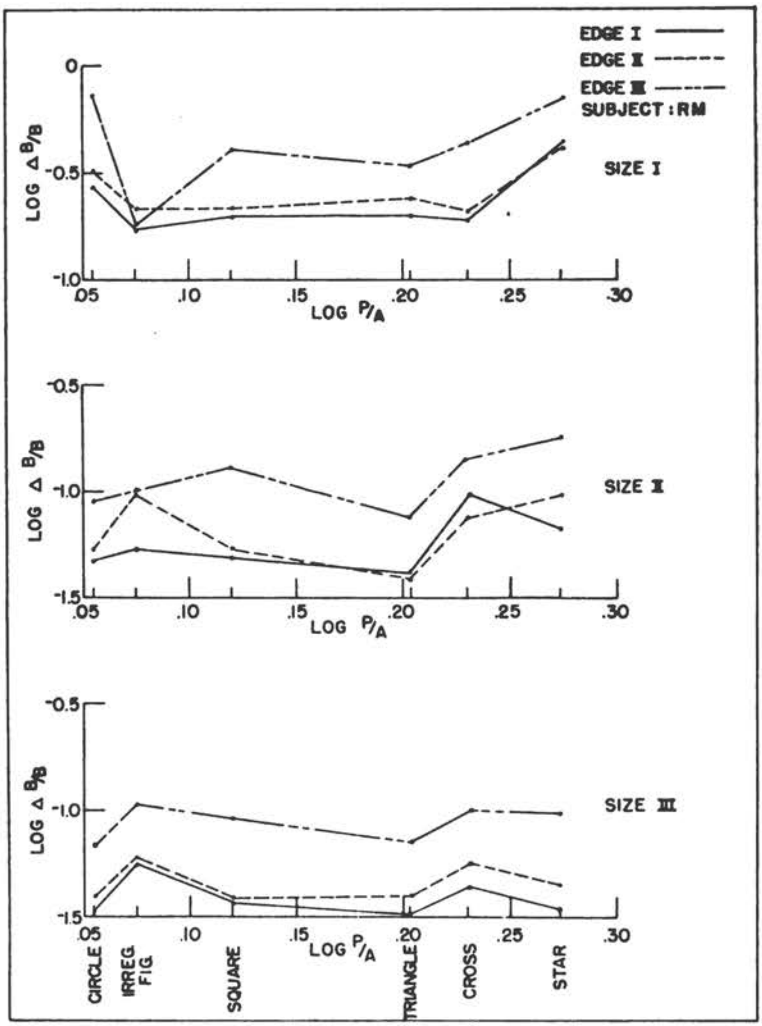


Fig. 3. Fifty percent correct recognition thresholds for subject RM.

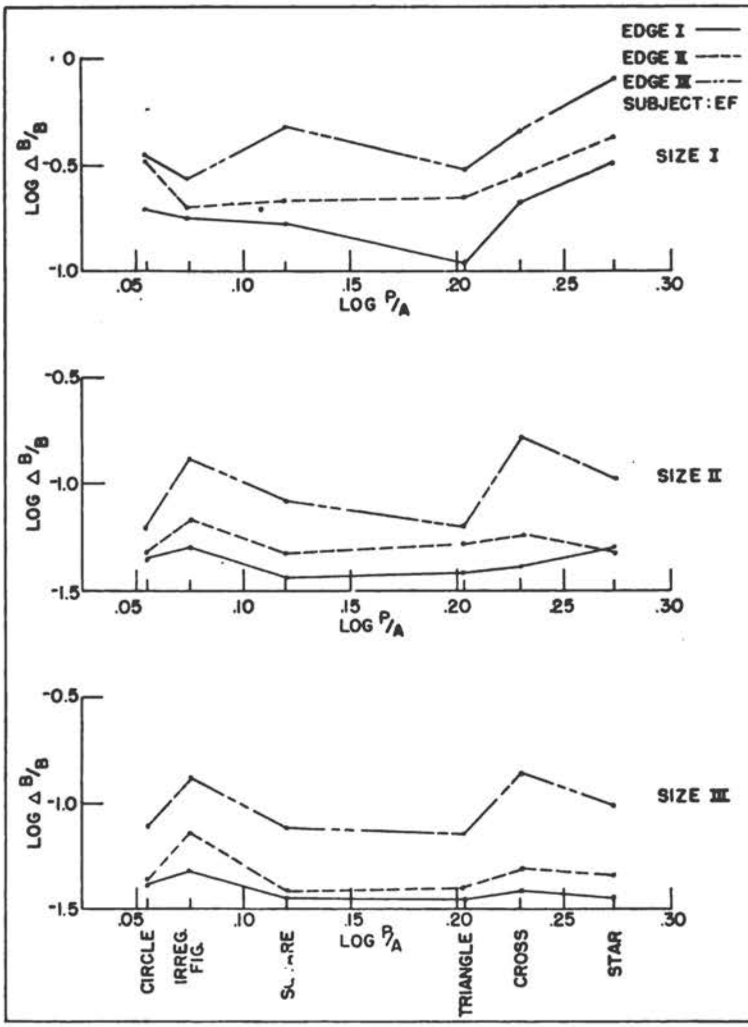


Fig. 4. Fifty percent correct recognition thresholds for subject EF.

for both detection and recognition. Figures with lower detection thresholds had lower recognition thresholds.

(4) The familiarity of stimulus forms appears to affect both detection and recognition functions. Non-familiar forms, e.g., Irregular Shape, had higher thresholds than did figures like the Triangle and Square.

IV. Implications for Further Research

The approach of this study in manipulating the stimulus for contour across size and shape of the target has implications for the following areas of research:

(1) Studies are needed in which the area of the target is systematically impoverished in order to determine if discrimination is based on edge factors or on some edge-area combination.

(2) At the physiological level there is a need to determine the relationship of edge-gradient change to the On/Off mechanism of the retina.

(3) Form discrimination in lower animals has not been systematically studied. The experimental stimuli of the present study would permit comparative investigation which might prove relevant for a comprehensive visual theory.

(4) On the basis of the present results, it would appear that edge-gradient would be critical for any account of the phenomena of figural after-effects. There is a need to determine whether contour is a relevant variable for producing after-effect (Kohler and Wallach).

RECOGNITION OF CRITICAL TARGETS AMONG IRRELEVANT FORMS*

Robert M. Boynton

Before beginning, I would like to acknowledge the very great contribution of Dr. William Bush to this work. As a research associate, Dr. Bush devoted two years of his professional life to this work and deserves a great deal of the credit for whatever we may have been able to accomplish.

We have been working at the University of Rochester for the past

*Research executed under Contract No. AF 33 (616)-2565 between the Aero Medical Laboratory, Wright Air Development Center, and the University of Rochester. Fuller accounts of the work reported here are cited in the References.

two and one half years in an effort to provide the Air Force (Aero-Med Laboratory, WADC) with laboratory data which are relevant to problems of visual air reconnaissance.

As Dr. Bersh pointed out yesterday, there has been little basic work done with complex fields. Studies of detection in homogeneous fields, where the subject knows where to look (and exactly what to expect) just don't apply to the present problem. Our trick, therefore, has been to present our observers with a stimulus array consisting of N forms, curvilinear in shape, among which, on a given exposure, a so-called "critical target" is located -- or, more precisely, may be located, since on many trials there is no critical target.

Our forms are shown in Figure* 1. Dr. Arnoult said in his talk that the failure to specify form means a failure to specify your stimulus. I can specify our forms only by pointing at them -- here they are. You will note that they fall into seven classes, six of which are critical targets (the rectilinear forms). The seventh class, the struniforms, provide the background targets.

Where did these forms come from? They came from a larger population of 240 forms. The latter came out of me. I just sat down one night and drew them. There were implicit ground rules that I followed, but I couldn't say with any certainty what they were.

This set of 240 forms, then, was about as arbitrary as it could be. Arnoult also made the point yesterday that arbitrary forms are to be avoided. I wholeheartedly agree, and yet we had applied research to get on with. And it was obvious to us, just as it should be obvious to anyone here at this conference, that if we had started out to properly select our forms, we would have been spending the last three years on the problems of: (1) Why is a form? (2) How do you measure forms? and (3) How can you generate, objectively, a population of forms of a certain kind? These are important problems, but we did not even attempt to answer them.

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*These forms are not equated for area, perimeter, or any other dimension. A typical target, however, is about 3 cm. in subtense, and it may be helpful for the reader to have the following table in order to evaluate the results.

Simulated Distance	Target Size	Subtense of Figure Array	Subtense of 3-cm. Target
5.16 m.	2	4.50°	20.0'
7.98 m.	1	5.73	12.9
10.32 m.	1	4.50	9.9
20.98 m.	1	2.23	5.0
41.30 m.	1/2	2.23	2.5

Actual distances used may be determined by multiplying simulated distance by target size.

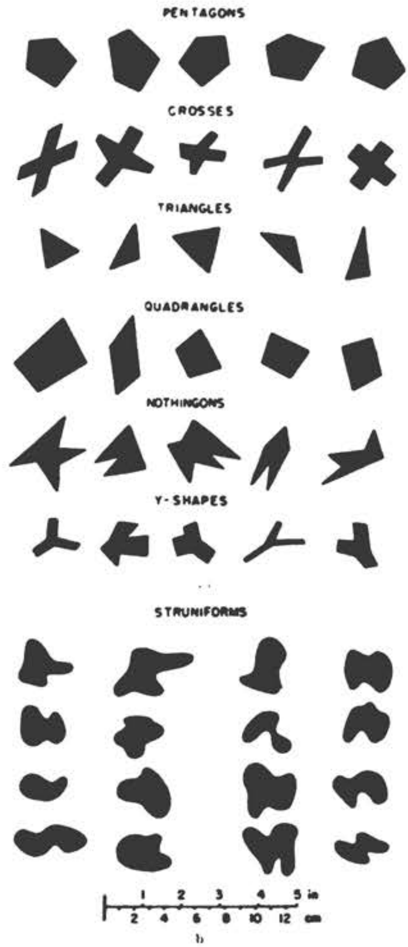

Fig. 1.

From our set of 240 forms, we extracted the 46 that you see here. They were selected on the basis of an experiment in which each of 12 subjects viewed each of the forms many times at several distances and two levels of contrast. Our measure was percent correct recognition for all of the distances combined. The subjects had available the seven response categories shown here, and for the subject to be correct, the proper class name had to be specified. It turned out that about 75% of the responses were responses designating a rectilinear form, which forced us to use some rather messy corrections for chance which we sought to avoid in subsequent experiments.

On the basis of this work, we rejected all but the 46 forms shown here. We threw out those which were too hard, or too easy to recognize, and rejected also those forms which varied a great deal from subject to subject in recognizability.

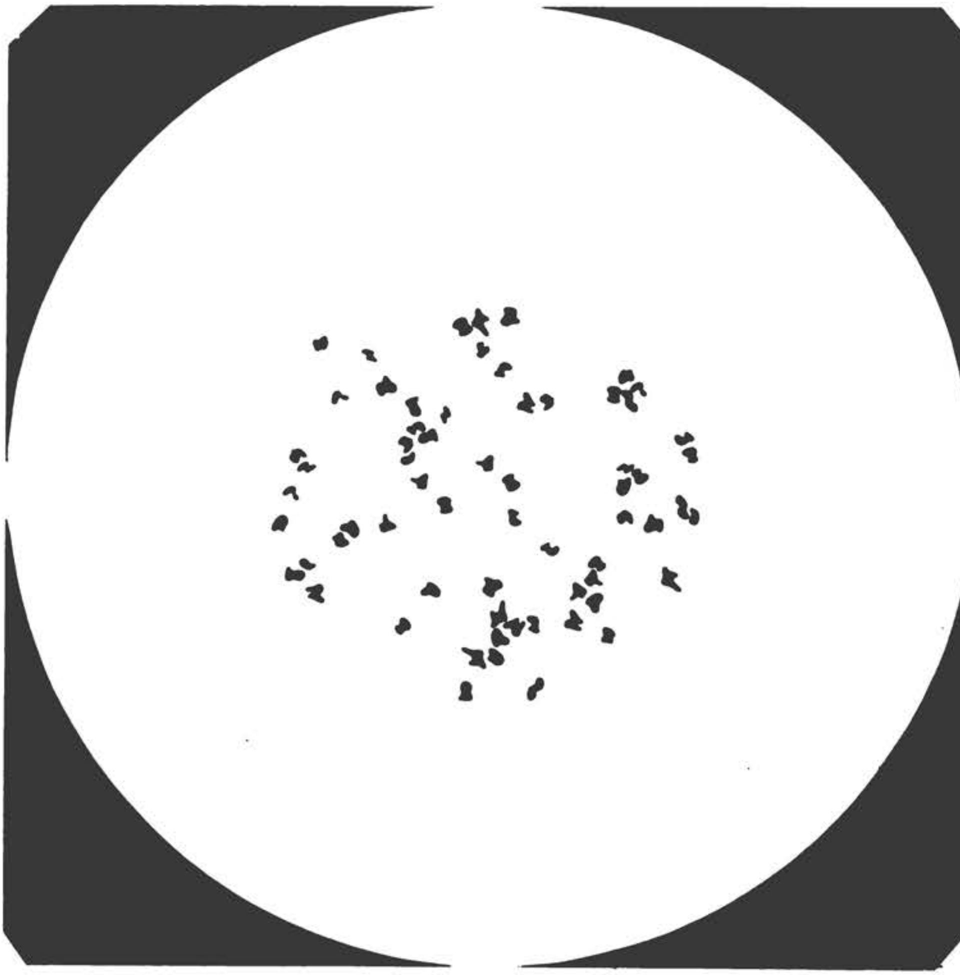

Fig. 2.

Our subsequent experiments have been quite different. We have been exploring the influence of exposure time, target-background contrast, and distance on the recognizability of a critical target which is located among a random array of struniforms. Here, a picture is worth a thousand words and Figure 2 illustrates this. There is a critical target in this figure (a "Y"). At the distance at which those of you in the middle of the room are sitting, our subjects would find this target about 75% of the time with a six-second exposure, which will give you some idea of what training can do in this task. Actually, some degradation of the image has occurred here because of photographic reproduction, whereas in our experiments the targets were viewed directly.

To my way of thinking, one of the most important things we have done is to control the motivation of our subjects on this task, which is hard perceptual work and intrinsically boring. We have done this by means of an aperiodic reinforcement schedule*, where, in advance, 35 to 50% of the trials are randomly selected as "reward trials". If the subject is correct, he gets from 5 to 50 cents. Being correct -- and this is very important -- is defined either as (1) a correct response identifying a critical target (e.g., a response of "pentagon" when a "pentagon" is actually in the array), or (2) a correct response to the effect that no critical target is present. The upshot of it all is that a subject, in order to make money, cannot guess. If he sees no critical target, he admits it. That guessing has been eliminated is indicated by an analysis of the no-target trials, where "error responses" (a critical-target response when none is present) make up less than 5% of the total judgments. We are convinced that this 5% figure represents honest mistakes on the part of the subject.

By manipulating the reward schedule and the ratio of critical-target to no-target trials, we can manipulate the subject's tendency to guess. When we did this in our experimental situation, we got some interesting results. There are shown in Figure 3. The main conclusions are as follows: (1) The percentage of critical targets correctly recognized did not change. (2) There is another class of critical-target responses, which are right to the extent that there is a critical target in the display, but wrong

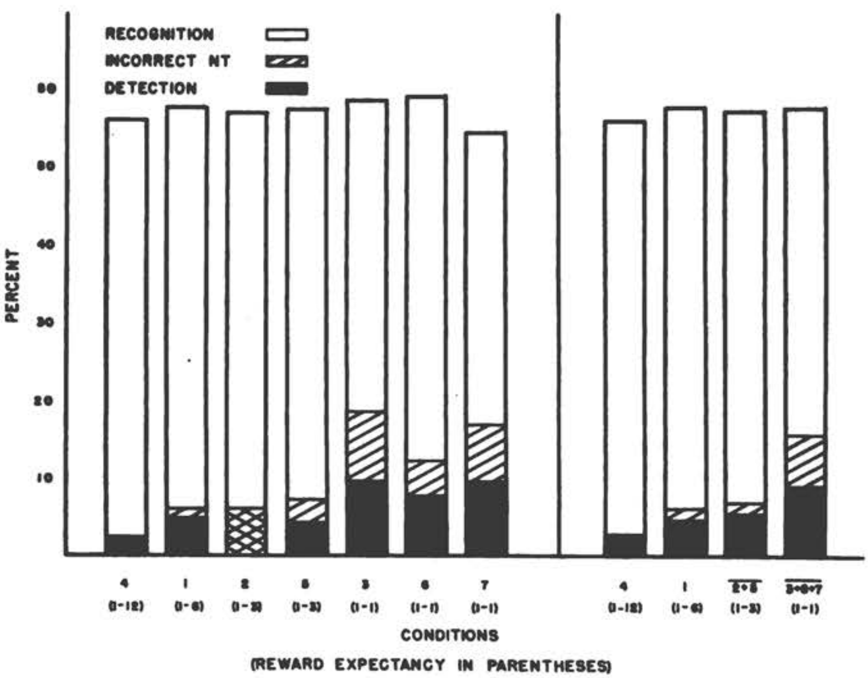

Fig. 3.

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*Credit should be given to Dr. Ogden R. Lindsley, who first made this suggestion in an informal discussion.

with respect to which one is present. This would occur, for example, when a "Y" was presented and the subject responded "triangle", which is, as a matter of fact, a very common confusion. These we call detection-without-recognition responses, or, for short, detection responses. As subjects are motivated to guess more and more, the percentage of detection responses increases, though not as much as we had expected it would. (3) Meanwhile, however, the percentage of error responses (false identifications of non-existent critical targets) increases about 1.5 times as fast. To the extent that these results are generalizable to real-life situations, they strongly suggest that if you encourage your observers to report everything, however small their hunch may be that something of interest is there, you will get (1) no increase in correct recognition, (2) a slight increase in detection-without-recognition, and (3) a larger increase of reports of targets which are not really there. This would seem to be too great a price to pay for a slight increase in the amount of partially-wrong information which can be obtained.*

Our major interest has been to determine the influence of the following four variables upon recognition behavior: (1) Number of figures in the array, (2) Distance between observers and array, (3) Exposure time, and (4) Contrast. We have lots of data concerning these variables, some of which are shown in the following four figures.

Figure 4 shows percent correct recognition as a function of contrast, with distance in meters as a parameter. Empirical equations have been fitted to the data. The chief point to be noted here is that reducing contrast has rather little effect until a level of 40 or 50 percent is reached; below that point, contrast becomes an important variable -- especially for conditions where performance is potentially good under high contrast.

Figure 5 shows how exposure time affects performance. Here there appears to be an approximately linear relationship between log exposure time and percent correct recognition, with the slope varying somewhat depending upon the number of figures in the array.

In Figure 6, we have the relationship between recognition and simulated distance, over a wide range of distances, and for five contrast values. Our range of physically available distances was not great enough to allow us to vary distance directly, so that we had to simulate part of the range by using half- and double-size targets. As often happens, the results for targets of equivalent visual angle but different physical size were not always in accord.

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*This recommendation should be taken with caution, however. Under our conditions, the principal problem which faced the subjects was that of finding the target in the first place. Half the experiment was run under a condition where the subtense of a typical (3 cm.) target was about 8' of arc; the other half involved targets subtending about 5.5' of arc. Especially in the latter case, and to some extent also in the former, there was relatively little doubt about what the target was, provided it was fixated during the exposure.

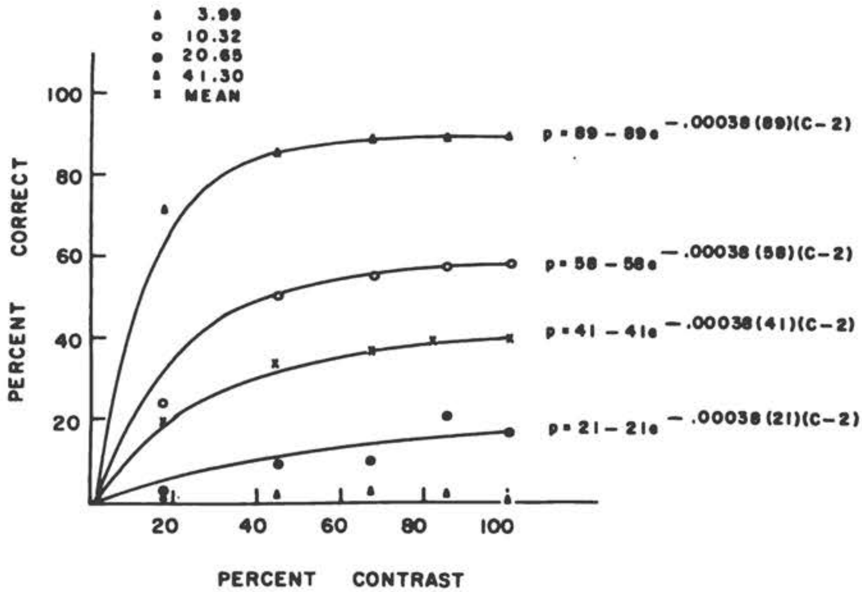

3.99
10.32
20.65
41.30
MEAN
PERCENT CORRECT
PERCENT CONTRAST
100
80
60
40
20
20
40
60
80
100
p = 89 - 89e −.00038(89)(C-2)
p = 58 - 58e −.00038(58)(C-2)
p = 41 - 41e −.00038(41)(C-2)
p = 21 - 21e −.00038(21)(C-2)

Fig. 4.

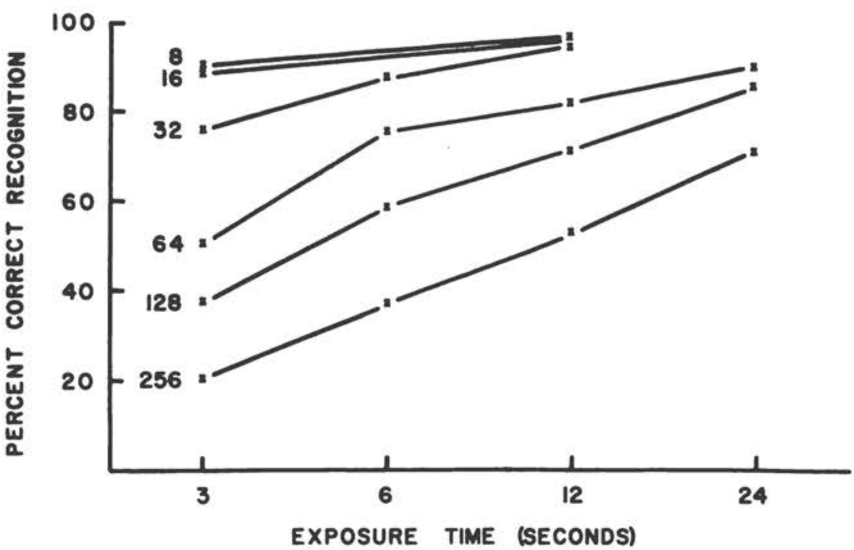

PERCENT CORRECT RECOGNITION
EXPOSURE TIME (SECONDS)
100
80
60
40
20
3
6
12
24
8
16
32
64
128
256

Fig. 5.

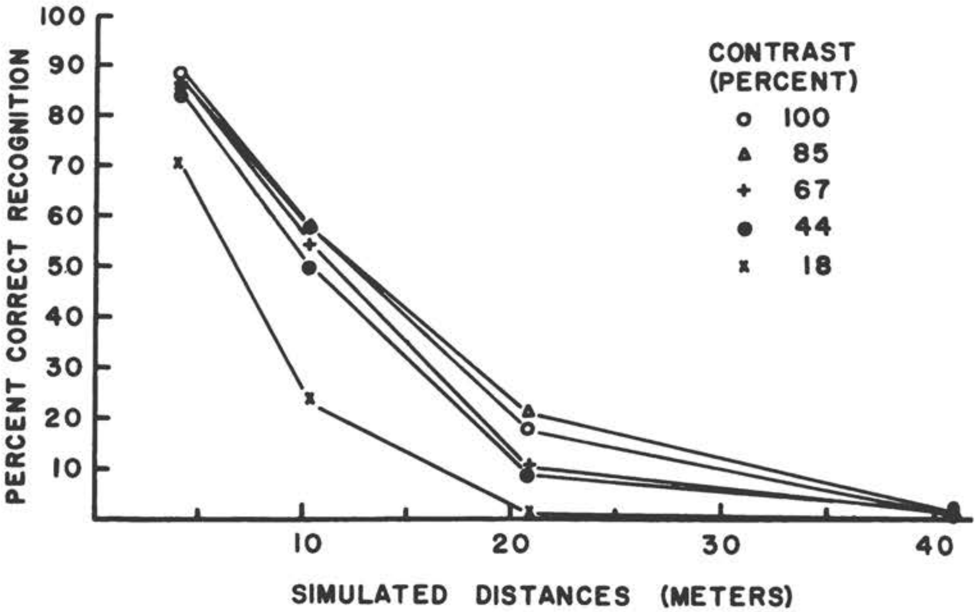

PERCENT CORRECT RECOGNITION
100
90
80
70
60
50
40
30
20
10
CONTRAST (PERCENT)
o 100
▲ 85
+ 67
● 44
x 18
10
20
30
40
SIMULATED DISTANCES (METERS)

Fig. 6.

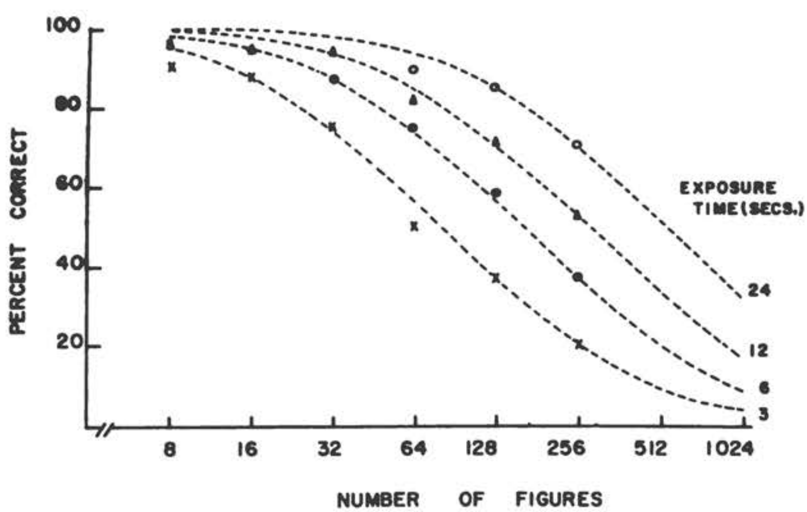

PERCENT CORRECT
100
80
60
40
20
EXPOSURE TIME(SECS.)
24
12
6
3
8
16
32
64
128
256
512
1024
NUMBER OF FIGURES

Fig. 7.

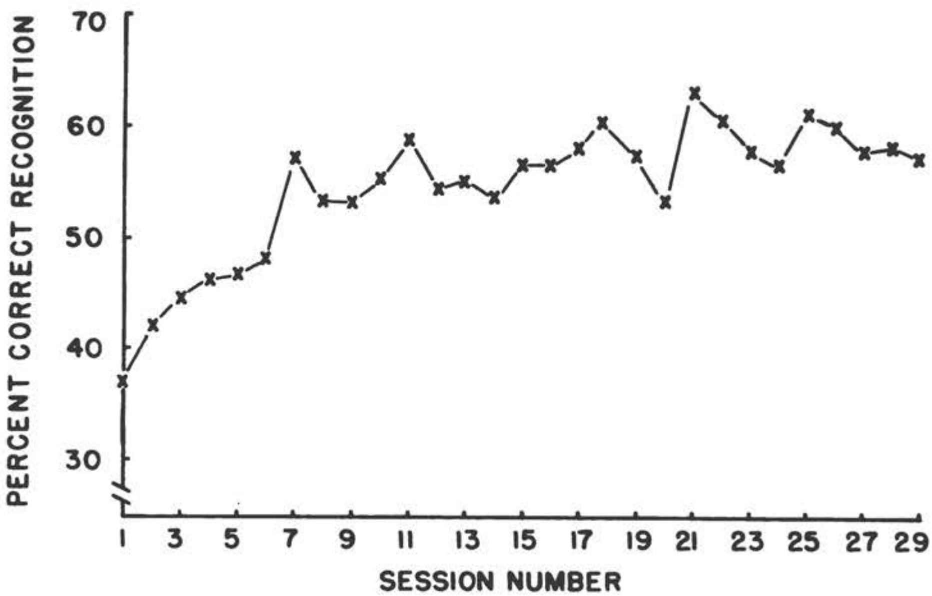


Fig. 8.

Figure 7 shows the relationship between number of figures in the array and percent correct recognition, with the data fitted with ogives. Exposure time is the parameter.

And, for good measure, a learning curve is shown in Figure 8. Actually, there was somewhat more learning than this, because we lost the first couple of sessions because of apparatus difficulties.

In general, these results came out just as you would expect, but the exact nature of the functions and the interactions between the variables is very important. During the past year, we have explored these interactions quite thoroughly. This has been done by finding all of the possible combinations of our four variables, over our working ranges, which would elicit 60% correct response by the subjects. These data are not yet fully analyzed, but we have found, as the most important single outcome, an interchangeability between N (number of figures) and t (exposure time). For example, if a given level, p, of performance obtains with 64 targets in the array and 3 seconds exposure time, and each of these is doubled (giving 128 targets and 6 seconds viewing time), p remains approximately the same. This implies, to me, a very high degree of motivation and search efficiency on the part of our subjects, which, incidentally, we did not get in our first experiment where we found no performance increase between 12 and 24 seconds.

Our task for this summer is to relate these data to flying conditions. The interrelations between the four variables, and altitude, are in themselves complex, for as altitude increases, distance increases (of course), contrast decreases (by an amount which depends upon meteorological conditions), and viewing time increases, for a given aircraft velocity. Our experimental data are good enough so that we can derive equations which fit the data reasonably well for engineering purposes. And here I agree with Dr. Sleight, with whom I took objection yesterday, that if a little violence is done to the data by such an operation, the utility of it is well worth it. Then, with the help of the IBM 650, we expect to derive a great variety of functions showing what should happen to relative recognizability as a function of aircraft velocity, altitude, mode of viewing, and meteorological conditions.

TWO PROPOSED STUDIES ON CONFIGURATION PERCEPTION

Alvin G. Goldstein

Two proposed experiments, dealing with different aspects of two-dimensional form or pattern perception, will be briefly presented. The first study to be discussed is concerned with the general problem of perceptual similarity. More specifically, it is an attempt to investigate an implication of the well-known Gestalt law of similarity as it applies to visual perception. Experimental subjects have not yet been tested but a few preliminary observations will be offered. The second study which I will tell you about is a little more difficult to place in a well-known background. Perhaps the best way to describe it at this point is to say that we are trying to find out whether a subject can report what particular part of a complex shape has been changed after he has discriminated that a change has taken place. This will become clearer to you when I describe the simple procedure that is planned.

Now, for the first experiment. Gestalt psychologists have long held that similar stimuli will form strong perceptual groupings(1). Most of the examples supporting their statements come from visual perception and, typically, the examples are demonstrations. This is not at all surprising since Gestalt psychology is almost synonomous with phenomenology. But the term "similar" has not received much attention even though it is so very important in many areas of psychology(2). With this in mind, we set out to see what kind of relation there is between difference limens and perceptual grouping.

We have used simple two-dimensional geometric forms as stimuli for some part of the experiment. For example, circles of identical outside diameters but with varying contour thickness are presented to each S by means of a slide projector. To be concrete, the original drawings from which slides were made had contour thicknesses of 11.5 mm., (which is used as the standard form) 12.0, 12.5, 13.0, 13.5 mm., etc. These will be shown to the subject in pairs and he will judge whether the two forms

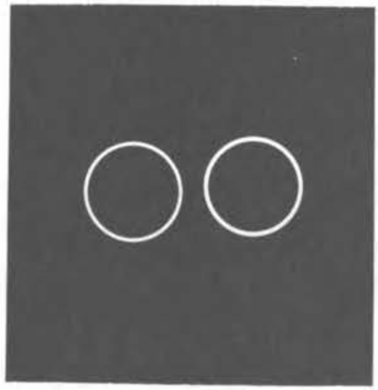


Fig. 1. The difference in contour thickness is above the threshold for this kind of judgment.

are the same or are different (Figure 1). Obviously, this is a simple and rather rough method for getting a DL. In the second part of the experiment the same subject will be presented with the following display. Numerous exact duplications of the standard form (11.5 mm. thickness) have been placed in random positions on a 16 by 16 inch board. We will call these the background elements.

Interspersed among these are many duplicates of one of the variable forms. We will call these the figures elements. These have been arranged so that they form a pattern or figure resembling a circle with a bulge in its contour (Figure 2). The subject is instructed to tell us where that bulge is located (Figure 3). This, of course, resembles the Landolt C method used to measure visual acuity. We have been careful to keep the distance between the contours of the "background" circles equal, on the average, to the distances between "figure" circles. Naturally, several matrices can be made up with the background elements remaining fixed and each of the variable stimuli can be used as elements in the figure. Thus, all the stimuli presented in the first part of the experiment can be presented again for a different kind of threshold judgment. We are considering taking decision times in both phases of the study in addition to correctness of response.

Contour thickness is only one example of the stimulus variables we intend to use. Others are circle size (Figures 4 and 5), ellipse-circle comparison and randomly constructed "families" of irregular forms. Essentially almost any figure can be used if a judgment of same or different can be obtained from a subject.

From a few preliminary observations only one tentative conclusion can be made. There is a suggestion that responses in the simple DL test may not be used directly as predictors of responses in the more complex matrix test. Stating this in another way; the threshold obtained when a standard and a variable figure are judged to be different would lead one to predict that in the matrix these two figures will also be seen as being different and therefore the elements making up the pattern should "stand out"

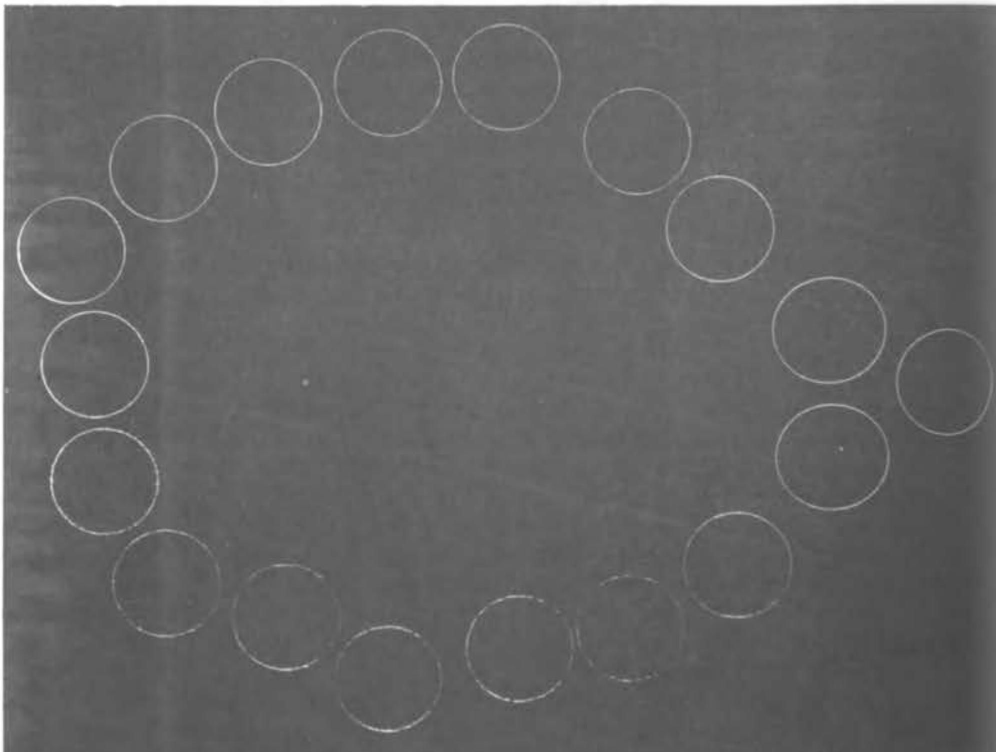


Fig. 2. Basic pattern.

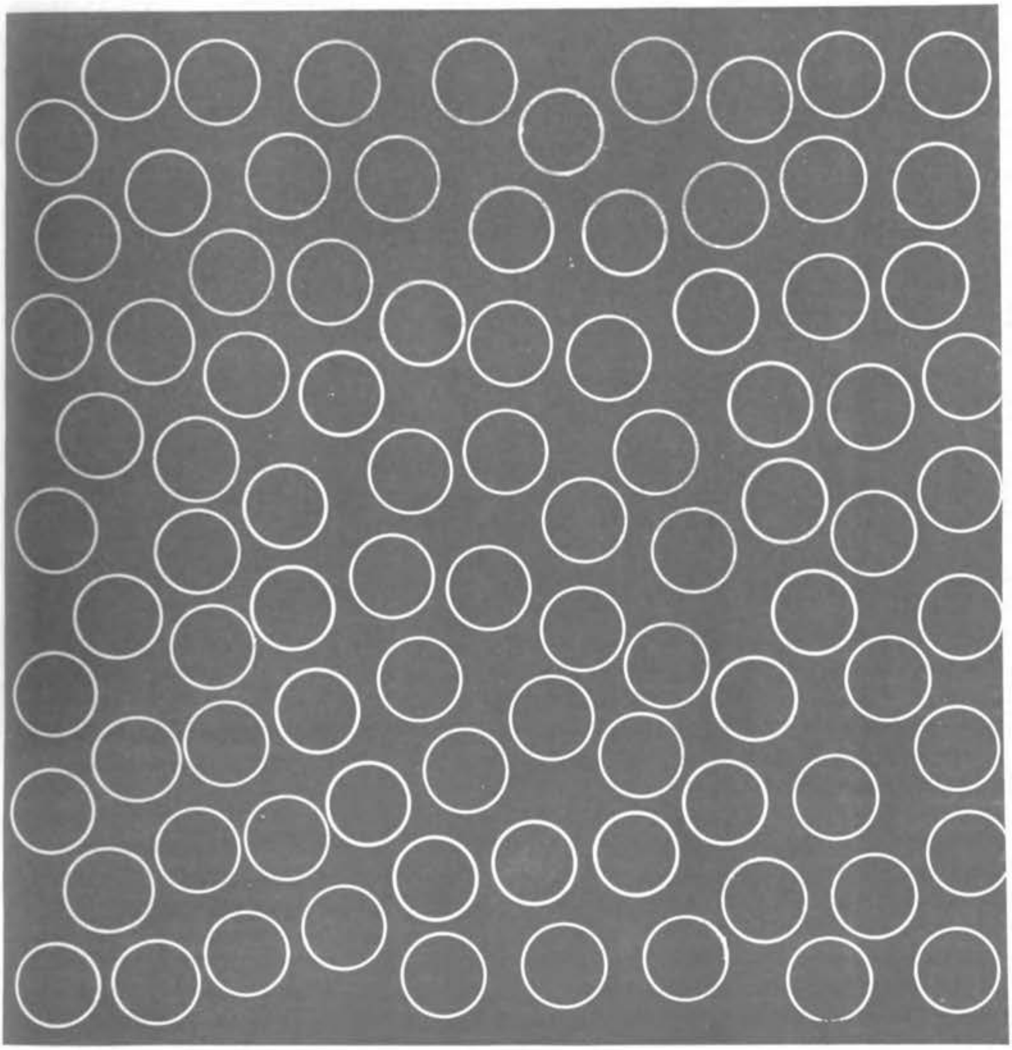


Fig. 3. A matrix composed of replicas of the two circles in Fig. 1. Circles with thinner contours make up the basic figure.

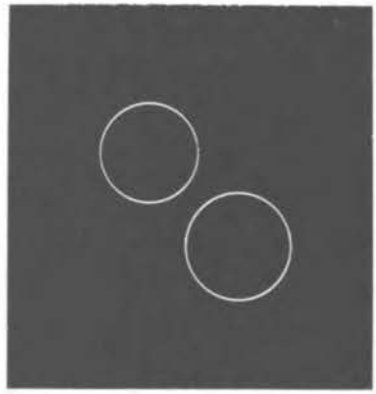


Fig. 4. The difference in size between these two circles is substantially above threshold.

from the elements making up the background. This may not be the case, if the early observations are supported by future data.

Let us now go on to the second study. With my first remarks in the introduction to this paper I have almost described the complete experiment. Only a few more words are needed. This study is an excellent example of the "let's-see-what-happens" type of experiment. Therefore, there will be no need for a theoretical prologue.

The stimuli we intend to use are "families" of randomly generated two-dimensional forms developed from a prototype form in accordance with any one of the numerous methods recently reported in several articles (e.g., (3)). The subject will be presented first with the prototype (or standard) form and then the first or least distorted variable form of the "family". If no discrimination can be made then the standard and the second distorted form will be presented and so on until the subject gives his first "different" response. He will then be shown isolated parts of the standard and variable forms and asked to select that part which is common to both and also the one part which is not common to both forms. Presumably, if he has been able to discriminate between two forms when they are not simultaneously present, then he should be able to point out the part of the form that has undergone change. If it is discovered that one can discriminate between two slightly different random forms but not, in a sense, verbalize the difference, it will not be something new in psychology. But we believe it will be interesting to see if it occurs under these conditions.

There is at least one obvious difficulty with this method which I would like to mention. Typically when the shape of a prototype figure is changed by means of any of the usual methods the area and most likely the area-perimeter ratio is also changed. Obviously, this is not good if it can be detected by the subject. We may use the following procedure -- or a variation of this procedure -- to minimize the possibility that a subject

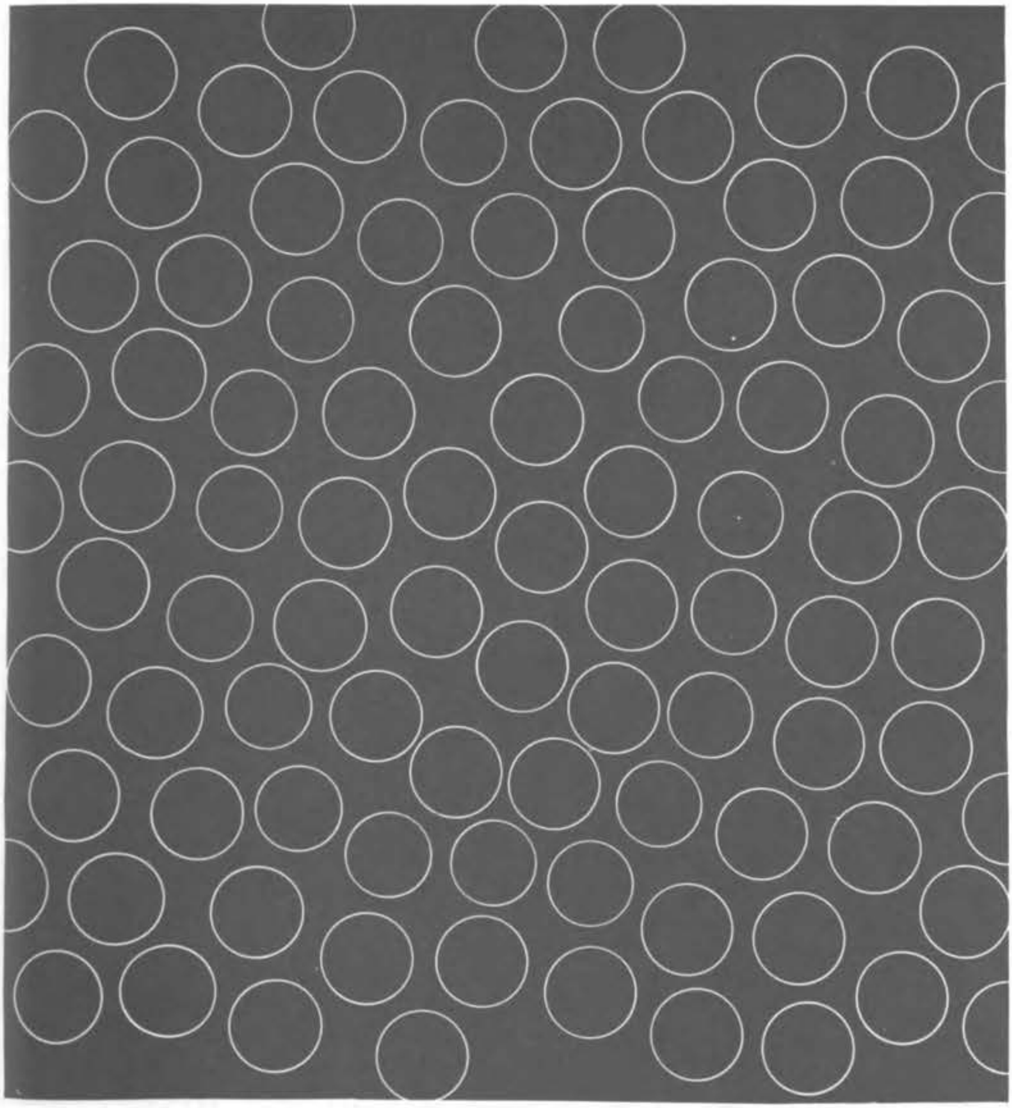


Fig. 5. The matrix is composed of replicas of the two circles shown in Fig. 4. The smaller of the two circles make up the basic patterns.

could respond to areal changes in addition to form changes. The absolute size of the figures or the part figures could be varied randomly; this would almost force the subject to respond to changes in contour.

DISCUSSION

BLUFORD: I'd like to direct a question to Dr. Boynton. Do you plan to put in a limited number of larger areas of varying contrast along with the group of forms?

BOYNTON: I'm not sure that I understand what you mean by "larger areas of different contrast".

BLUFORD: All right. Your observers of the form know that there's always to be a uniform background. Let us say that you divide this uniform background into at least three, four, or five irregularly shaped parts and use a different luminance level for each one of them.

BOYNTON: I think that would be very interesting to do. We don't have any intentions of doing the particular experiment. We thought about doing things of this sort but we simply had to do what we thought was the most important thing first. It's possible we might eventually get involved with something like this.

BLUFORD: I see. Does this research tie in very closely with aerial reconnaissance work, that is, photointerpretation?

BOYNTON: No, but I'd be happy to talk with you about it further, later.

VANDERPLAS: Dr. Boynton, would you please say again what your finding was with respect to the number of incorrect selections as a function of motivation? Did I understand you to say your subjects tended to increase the number?

BOYNTON: As we encouraged the subjects to guess at the presence of critical targets, the percent correct recognition responses as I defined it did not change. The percent of correct detection without recognition responses increased somewhat, as indicated on one of the figures. However, the percent of error responses -- these are responses that a critical target is there when really it isn't -- increased about one and a half times as fast as we increased their tendency to guess. This means that all that we got as a return for encouraging them to be more reckless about responding was a little bit of partially wrong information and quite a bit of completely wrong information.

VANDERPLAS: One interesting thing that we found in a study in which we've been attempting to train subjects to recognize random shapes was that, following a paired associates test in which the subjects learn to label the shape with a nonsense syllable, their performance on the recognition test later when the prototype was imbedded in a group of variations of the random shapes is such that they tend to make more selections correct or incorrect of the variations as a function of the number of times that the response label was reinforced during learning. This tends in another context or study to support your notion that reinforcement of this behavior apparently leads to a greater frequency of selections, correct or incorrect.

BOYNTON: I think that's very interesting. I would like to re-emphasize that our subjects are tremendously trained and the data we got would be different if we used untrained subjects.

BLACKWELL: I want to address a question to Dr. Boynton, too. I certainly agree with him that detection data such as those we have collected have nothing to do with aerial reconnaissance. I've said many times that it's a seal on an ice cap -- and I'm sorry if I conclude it from your data, too -- it's a seal on an ice cap that we can tell you about, but nothing else.

So, I'm rather concerned that Dr. Boynton's task doesn't get at the problem either. I've flown and tried to pick up things on a battlefield and the only circumstance that I think Dr. Boynton has given us data to refer to is the very rare case where there might be, for example, 10, 000 dummies and one tank, or perhaps 10, 000 tree shadows and one tank where the tree shadows resemble the tank very, very closely in size and contrast and approximate shape. I think the question from the Signal Corps was very appropriate. It seems to me the problem, as I've seen it from the air, is that most of the time you see a glut of information -- large areas of varying contrast and a few small things that might be objects. Rather, as a woman goes from man to man, the eye must go from target to target in your setup. As far as I see it, you sort of look at the whole thing, find an area of interest, latch on to that by a panning movement with the eye and make what I would call a "detectional recognition" at that point. So, it seems to me that you've really gone the other way and I'm not sure how many practical tasks this refers to.

BOYNTON: I don't think we can prove it one way or another yet: our situation involves a fairly restricted visual angle and would be like the search from an aircraft that you would do after having decided upon an area of interest. It would be a kind of a fine search that would be going on within an area of that sort. I will grant that our stimulus array does not look like real life and it wasn't intended to, but I hope that the interactions that we have gotten between our four major variables would apply to other quite different situations. This is a statement of faith on my part. Whether it does or not, I think, remains to be seen.

BLACKWELL: But if you can analyze the act as clearly as it is in this case, an act of moving from object to object, you have evidence which makes it clear that that is what they're doing in the best case. I simply challenge that this has got much to do with aerial reconnaissance. A case where you first must find one or two things to look at, and there are rarely 20 of them, so that you sort of go from one to another and say that's the real thing and those other 10 which are just like it are tree shadows is very rare indeed, I'm sure.

BOYNTON: What you claim our subjects are doing is not necessarily what they're doing. Some of the time they may do that. We don't know exactly what they are doing. We have given them no instructions about how to search. They have developed their own techniques. But, it isn't a simple matter of going from one target to another. I can cite one little bit of evidence in favor of this statement. In most of the experimental work I think all of us have reported here, the subjects knew what exposure period to expect, either 3, 6, 12, or 24 seconds. We ran a couple of sessions one time where the exposures were presented at random. The subject was not able to predict what the exposure period was going to be. The interesting result was that they did better under this condition than they did under the condition where they knew what to expect. This suggests to me, that, not knowing what to expect, they have to expect the three-second exposure because that might be it. There must be something that they do during this first three seconds which is more efficient in some way than what they do later on. I don't know what it is. We would like very much to get eye-movement records from our subjects and give a definitive answer to this question, for I'm quite certain that they are not doing a target-to-target search in quite the way you suggested.

SMITH, O.: It seems to me, that, in addition to the scanning problem and in addition to the problem of recognition of static objects, you also have under the realistic condition the fact that if you're searching for a particular object you have no way of predicting a priori what particular perspective transformation is going to be necessary for the identification of that object. We have an awful lot of evidence indicating that varying degrees of slant and tilt with respect to any set plane very definitely influence the probability of recognition, either during the course of transformation or simply on the basis of a static silhouette viewed more or less in a short time span. The whole problem here becomes very puzzling to me. If you take a whole series of random forms, even though an individual does learn what these forms are in the static sense, what predictive basis is there that he will then recognize these things when they undergo varying degrees of perspective transformation? This is essentially the crucial problem to me. The movement in this whole business is being completely left out. The various problems of identification in practical reality are not really being considered.

TARGET APPEARANCE AND IDENTIFICATION THRESHOLDS ON THE RADAR PPI USING VARIOUS ELECTRICAL PARAMETERS, FILTERS, AND SWEEP CHARACTERISTICS

Siegfried J. Gerathewohl

The experiments described in this paper were designed to measure the effects of several variables such as sweep brightness or brilliance, receiver or video gain, sweep movement, and some of the electrical parameters involved at five levels of target identification. Since video gain is entirely controlled by the receiver gain knob which is attached to the Control

Box C-71B/APQ-13, the effects of turning this knob clockwise become apparent on the Plan Position Indicator as an alteration of target appearance. Even if the identification thresholds and appearance levels of real and artificial targets may differ somewhat from set to set, and tube to tube employed, the over-all characteristics of target appearance were found to be stable enough with various pieces of APQ-13 and APQ-23 radar equipment. As a matter of fact, the video gain knob settings as described below must be considered as the optimum of a well-tuned radar training set; and they may serve as a kind of standardization and calibration scale for further research as well as for operational conditions.

In 1946, Hopkinson(1) made experiments on the visibility of cathode-ray tube traces in radar display using four criteria of visibility. However, his classification does not encompass the main characteristics of the various electrical variables in present day radar. For this reason, a new scheme was developed, which was based upon psychophysiological criteria as well as on the main electrical parameters of our system. Our previous experiments on form perception yielded three distinct psychophysiological appearance levels of the target, namely, the threshold of "just visible" targets or pip detection, the threshold of form perception or target recognition, and the level at which a particular target shows its optimum resolution and definition. It was then planned to investigate how these levels would relate to the video gain control setting of the APQ-13-T1A Radar Flight Simulator, and how variations of our main variables would affect the brightness and contrast of four standard targets.

The GE Luckiesh-Taylor Brightness Meter was used for taking readings on target and background luminance. Since no automatic recording photometer was available that would give reliable data in an experiment with continuously changing brightness, the measurements were made by two observers and an alternate, while the variability of the operators was not known.

The method employed in this experiment has been described elsewhere; thus, only a brief outline will be given here(2). First, four simple targets of regular geometric shape were arranged 90° apart in an eight-inch circular pattern, which brought each target within the 10-mile range radius. The targets were placed at the bottom of the tank of the Radar Flight Simulator; and they appeared on the PPI as a circle, a square, an equilateral triangle, and a cross. Their maximal extension was about $2\ 1/2^{\circ}$ visual angle seen from a distance of about 30 cm. from the scope surface.

Method

Since the experiments were conducted to establish certain standards for target appearance and form recognition, they were made with two basic sweep brightnesses (BSB). The brilliance settings of .03 ft.-L and of .30 ft.-L encompass the practical range of sweep luminance applied in operational radar. A BSB of .03 ft.-L can be defined as the barely visible sweep line just about cut-off point. With a BSB of .30 ft.-L, on the other hand, the sweep appears as a bright radiant line when seen at rest, and when it strikes the target. BSB was measured with the sweep at rest at 12 o'clock position after the set was warmed up for about 10 minutes, yielding a basic gain voltage (BGV) of 3.9 to 4.5 volt AC. Normally, a sweep brightness of .03 ft.-L was associated with a voltage of 45 to 47 volts DC at the cathode; and a BSB of .30 ft.-L was associated with a lower voltage averaging from 40 to 45 volts DC. These potentials increased later in the experiments, and this was attributed to the electric warm-up of the set independent of that of the phosphor.

When the receiver gain control knob was slowly rotated from zero to about 115° on our experimental scale at the BSB of .03 ft.-L, the gain center blip appeared on the scope. If a BSB of .30 ft.-L was applied, the center blip became visible at a video gain setting of about 100°. The gain voltage (plate voltage at pin #5) at that point was about 6.0 volts AC.

Normally, the first target pip would appear at different video gain settings depending upon the BSB. This was true also for the higher appearance levels of the target. Using the .03 ft.-L BSB first, a target pip usually became visible on the velvet-like dark and noise-free background at a receiver control knob setting of about 120°. The continuously rotating sweep had already disappeared before this setting was reached. With a BSB of .30 ft.-L on the other hand, the first pip appeared on the scope at a gain setting of about 100°. At this level, only parts of the target could be detected, as for instance the base line of the triangle, of the square, the innermost leg of the cross or the inner section of the circle. This was also true when the sector scan was employed.

At a video setting of about 130°, and a gain voltage of 6.8 volts AC, all four targets became usually visible with either of the two BSBs applied. This occurred independently of pip position and pip size, the latter not being identical all the time. With the low BSB, the continuously rotating sweep was invisible; with the high BSB, the sweep line was just above cut-off.

When the lower BSB was used and the receiver control knob further rotated clockwise, three of the four targets were generally recognized as to their form characteristics at a setting of about 185°. With the high BSB, three targets could be recognized at about 175°; and 180° was thus accepted as average for this level. At that setting, a difference was found in gain voltage depending upon the position of the sweep; and from here on, measurements of sweep brightness and voltage were made either "on target" or "off target". The "on target" gain voltage averaged about 7.25 volts AC, the "off target" voltage about 7.0 volts. No rotating sweep line was visible at either BSB on the velvet-like grey scope background.

At this level, the targets usually appeared as more or less defined silhouettes, better outlined and filled with bright spots at the higher BSB. In virtually all cases, the components closer to the center and perpendicular to the sweep line were more conspicuous and appeared brighter than the other parts of the targets. During sectoring at the level of gain setting, the target stood out clearer and more compact than during continuous sweep rotation.

At a receiver gain setting of about 190° to 200°, the four targets appeared as filled bright forms on the dark background, the latter slightly spreckled with noise pips caused by static or some air bubbles on the bottom of the tank. Although the form of the targets was slightly changed because of the "polar perspective" on the scope and loss of definition, it was so clear and unambiguous that no errors occurred as to form discrimination. With the lower BSB, the "on target" voltage averaged 7.4 volts AC; with the higher BSB, the "off target" voltage was 7.15 volts AC. At this level of highest target definition the continuously rotating sweep was not visible, and its position could only be detected by the fine glitter of static background noise during sweep passage.

Then, the receiver gain control knob was further rotated until the video gain voltage reached its maximum at approximately 235° on our scale. At that point, a maximum gain voltage of 10.7 volts AC was read with the high BSB and sweep "on target", and 9.6 volts AC "off target". With the low BSB, "on target" gain voltage read 10.2 and "off target" voltage 9.7 volts AC. The targets appeared now somewhat fuzzy but compact, well defined at the lower BSB, but less defined at the higher BSB on the bright and noise covered background. With either BSB used, the rotating sweep was visible as a bright line on a fairly bright background which showed afterglow at sweep passage. At the lower BSB, sector scanning yielded a bright target on a fairly uniform scope background.

Finally, the gain control was opened to maximum position. This was attained at an angle of 310° dial setting. With the .03 ft.-L BSB, the targets appeared still bright but blended almost into the surrounding background. "On target" gain voltage was about 8.0 volts AC; "off target" voltage was measured as 7.7 volts AC. In general, the screen looked now rather bright, covered with heavy noise, and showed a very pronounced center ring around the midpoint because of clipping the signal to eliminate the bright center blip.

With the high BSB the sweep was visible as a bright line during both kinds of scanning. "On target" gain voltage was 7.8 AC, "off target" gain voltage 7.4 volts AC. The targets appeared to be weaker than at the lower gain settings; and this effect was very pronounced at the lower BSB during sector scanning. While the shape of the target did not seem to be affected, its apparent brightness was considerably reduced. As to form recognition it appeared that the more complicated configuration of the cross seemed to be somewhat affected by the increasing noise level in that it lost its sharp definition at its far side.

From the observed relationship between video gain and target appearance it occurred that five levels of form perception could be established, which were based on perceptual criteria as well as on electrical parameters. They were previously defined as the following appearance thresholds:

(1) Target detection

(2) Target recognition

(3) Optimum target definition

(4) Maximum gain voltage

(5) Maximum gain control or maximum video gain

All four configurations were used for measuring target brightness. The background was measured close to the target because we assumed that the area immediately adjacent to the signal was the effective background of the target. In most cases (i.e., when signal and background characteristics were appropriate), one observer measured the background luminance at the one side of the target, and the other operator at the opposite side. Since background brightness was not uniform due to noise interference especially at the higher video gain settings, all measurements must be considered as approximations within the practical range of luminance and the variability of set characteristics including such variables like tubes and electronic equipment.

All brightness measurements were made with the continuously rotating sweep at a rotation rate of about 20 r.p.m., and with the sector scan covering an area of about 50° at a sweep speed of 50 to 60 scans per minute, clockwise. With sector scan, sweep gain voltage was always lower than with continuously rotating sweep because the gain is effective only in one sweep direction and, furthermore, has no time to build up to its maximum value but drops to zero in the very reversed direction. On the other hand, the sector sweep passes more frequently over the scanned area, thus keeping the phosphor at a continuous level of excitation. Because of the high voltage fluctuation during sector scanning, gain voltage was measured during continuous rotation of the sweep at the two low levels of target identification (video gain) or with the sweep at rest in "on target" and "off target" position, and only the brightness readings were taken during sector scanning.

Another reason for using the continuously rotating sweep for the adjustment of the target identification thresholds at which brightness was to be measured was that in all cases the four targets were controllable as to their appearance; and fine adjustments could be made so that at least three of them showed the characteristics defined before. This means that the observer rotated the receiver gain control knob clockwise until the desired identification level seemed to be reached, and then controlled his setting on the dial and on the voltmeter, making the necessary adjustments in case of differences between psychophysiological and objective criteria. With the sector scan operating, however, only one of the four targets can be observed at a time, and in this case the two objective controls, namely, video gain setting and voltage, served as indicators of the identification level. The latter two criteria also indicated maximum gain voltage and maximum gain CW levels when the continuously rotating sweep was employed.

An additional modification was made concerning the ID-41. The purpose of this modification was to allow the use of radius lines of polaroid filters for covering the sweep on the radar PPI. Filter densities varied through various degrees of angles of two layers of polaroid material in an effort to determine the best effect of a covered sweep, as was suggested in an earlier publication.

Cover No. I consisted of a strip of solid black tape 4.0 millimeters wide as previously proposed by H. W. Rose, School of Aviation Medicine, USAF, Randolph Field, Texas. The strip was mounted directly over the sweep so that the sweep line was completely covered. However, with this type of shielding the sweep now appeared as a black line on scope, especially when large bright target areas and higher background brightnesses were used.

Cover No. II was a single polaroid filter strip 6.0 millimeters wide having a transmission of approximately 30 percent. The filter was placed over the sweep so that it covered the sweep line and about 4 millimeters of the region behind it.

Cover No. III consisted of two polaroid filter strips 5.0 millimeters and 2.5 millimeters wide, respectively, mounted one on top of the other on both sides of the Plexiglass disk so that one-half of the covered area had a transmission of 1.5 percent only, while the other half had a transmission of about 30 percent. The increase in density was obtained by a 45 degree rotation of the axes of the polaroid material. The filter was placed over the sweep with the denser area covering the sweep line, while the wider part of the single filter covered a 2.5-millimeter area behind the sweep.

Cover No. IV consisted of two polaroid filter strips constructed and mounted like Cover No. III. The axes of the two filters were parallel, however, yielding a transmission of the overlapping area on top of the sweep of about 12.5 percent.

With all our variables previously specified, the following readings were taken at the five identification levels:

(a) Maximum target brightness

(b) Minimum target brightness

(c) Maximum background brightness

(d) Minimum background brightness

The sequence of these four measurements was randomized. After all data had been collected, the brightness contrasts were computed. The means thus obtained stand for the average of 10 readings of each observer and are indicative of:

(1) The brightness range of the target. This range encompasses the means of the highest target brightness during sweep passage and the means of the lowest target brightness just before sweep passage.

(2) The brightness range of the background. This range encompasses the means of the highest background brightness during sweep passage and the means of the lowest background brightness just before sweep passage.

(3) Maximum brightness contrast. This value represents the brightness contrast between maximum target and background brightness during sweep passage.

(4) Minimum brightness contrast. This value represents the brightness contrast between minimum target and background brightness just before sweep passage.

(5) Contrast range. This range encompasses the means of maximum and minimum brightness contrast during one excitation cycle. The greater the values of this range, the greater are the fluctuations in target-background brightness on the radar scope.

Results

The following factors were treated as variables in the statistical evaluation of our measurements:

A. I. D. Threshold and Receiver Gain Control Knob Setting

(1) Target Detection	110°
(2) Target Recognition	180°
(3) Target Optimum Definition	200°
(4) Maximum Gain Voltage	235°
(5) Maximum Gain Clockwise	310°

B. Basic Sweep Brightness

(1) Low BSB-level	.03 ft.-L
(2) High BSB-level	.30 ft.-L

C. Sweep Movement

(1) Continuous Rotating Sweep	360° at 20 r.p.m.
(2) Sector Scan	50° at 55 r.p.m.

D. Sweep Cover

(1) Uncovered

(2) Cover No. I (black)

(3) Cover No. II (approximately 30% transmission)

(4) Cover No. III (approximately 1.5% transmission of sweep brightness)

(5) Cover No. IV (approximately 12.5% transmission of sweep brightness)

E. Observer

(1) Gerathewohl

(2) Cork (most of the time) and Kroczka

For each of the listed factors, brightness readings and contrast values were averaged over the other factors to get the main effect of each factor on each of the brightness and contrast variables. These means are shown in the following tables and graphs.

From studies of replication of forty sets of conditions it is estimated that the identification threshold means and the sweep cover means are accurate at least within ± 11.3%, and that the basic sweep brightness, sweep movement, and observer means are accurate at least within ± 7%. Comparisons of these means yield to inferences regarding the general effect of a factor although under specific combinations of factors a particularly noted effect might be absent or reversed.

Identification Threshold. The mean brightness and contrast values at each I. D. Threshold or receiver gain setting are shown in Table I and Figure 1. Under maximum target brightness conditions, the recognizable target was about 10 times as bright as when it appeared as a pip, and at its optimum definition level was more than 45 times brighter. Brightness decreased when video gain was further increased: The decrement was about

Variable	(110°)	(180°)	(200°)	(235°)	(310°)
Maximum target brightness	.37	3.74	17.62	10.37	2.66
Minimum target brightness	.08	.39	1.20	.96	.48
Maximum background brightness	.02	.04	.20	.39	.34
Minimum background brightness	.02	.02	.08	.16	.15
Maximum contrast	18.07	113.36	94.66	32.04	9.72
Minimum contrast	4.11	19.93	23.74	7.39	3.30

Table I. Arithmetic means of target and background brightness (ft. -L) and brightness contrast at each identification threshold or receiver gain setting.

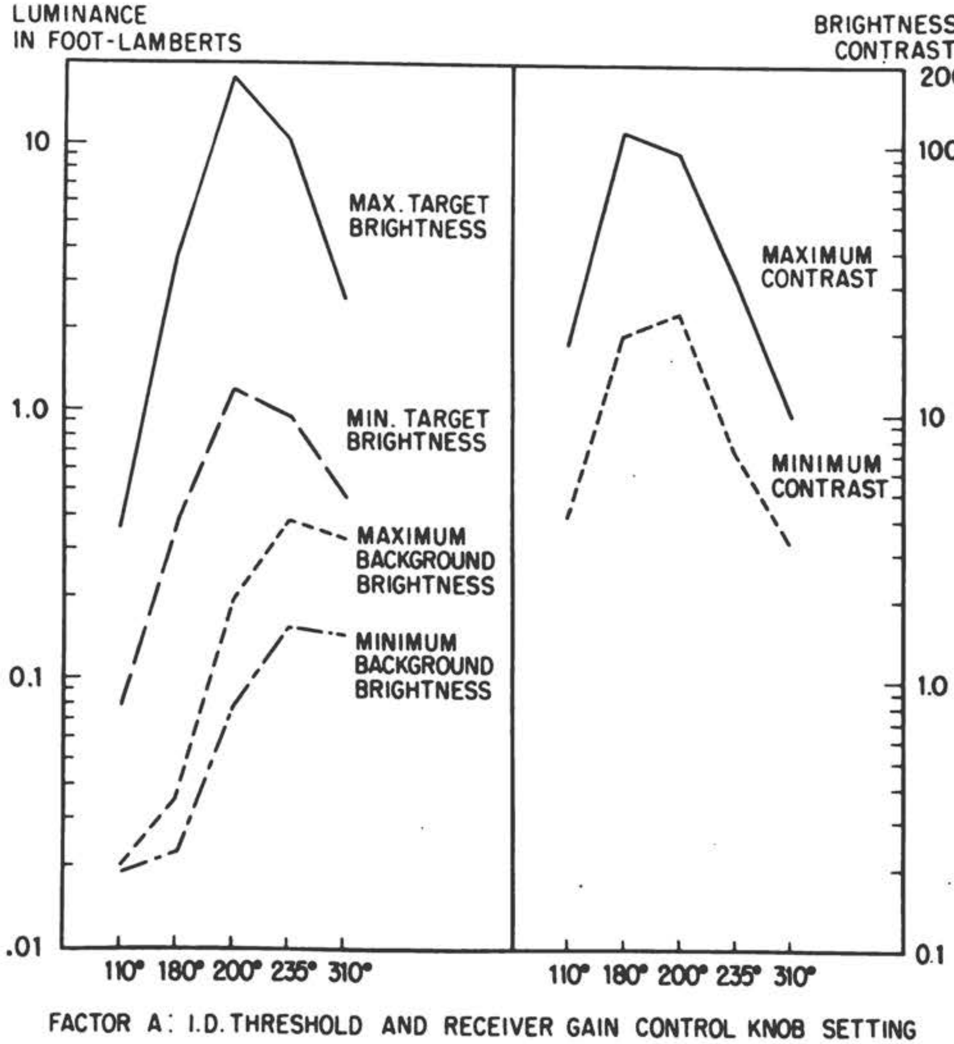


Fig. 1.

40 percent from level 3 to level 4, and about 80 percent from level 3 to level 5. Minimum target brightness was at the recognition level about 5 times, and at optimum definition level about 15 times, brighter than the pip. Highest background brightness was in general obtained at the maximum gain voltage setting, and the luminance dropped but slightly when the receiver gain knob was rotated clockwise to the end of the scale. On the other hand, highest target brightness decreased markedly after optimum definition setting.

The steepest increment in both target and background luminance occurred in the relatively small range between 180 and 200 degrees

Variable	.03 ft.-L	.30 ft.-L
Maximum target brightness	5.76	8.13
Minimum target brightness	.56	.69
Maximum background brightness	.15	.25
Minimum background brightness	.07	.10
Maximum contrast	59.68	47.45
Minimum contrast	11.88	11.51

Table II. Arithmetic means of target and background brightness (ft.-L) and brightness contrast at the two levels of basic sweep brightness.

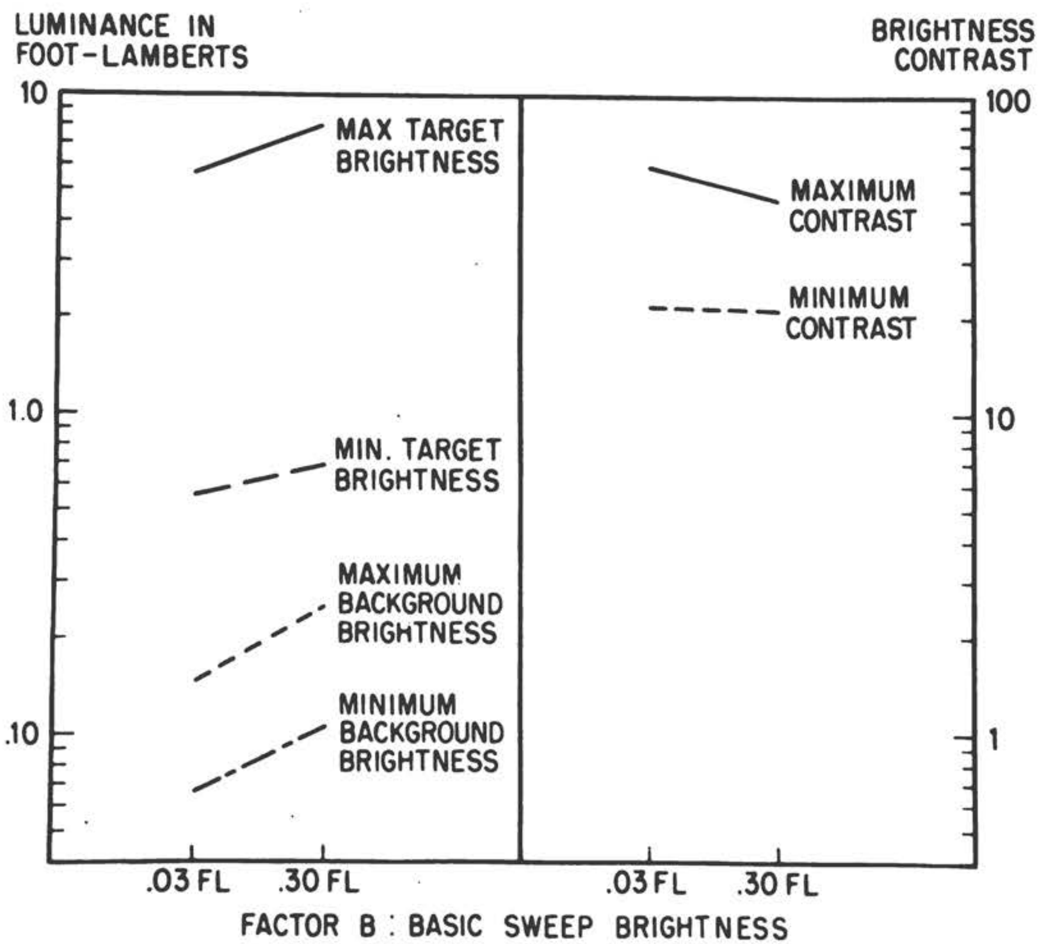


Fig. 2.

receiver gain setting. While average brightness of the medium sized target (about 2 1/2° visual angle) ranged from .1 to 20 ft.-L, peak brightnesses of about 30 ft.-L were measured. Thus it is possible for a radar target to appear more than 200 times brighter when viewed under favorable conditions than at its threshold of detection.

As was expected, the highest contrast occurred at the second and third identification thresholds and dropped sharply from there on, reading its lowest value at the extreme end of the scale.

Basic Sweep Brightness. The arithmetic means of maximum and minimum brightness of target and background, and of brightness contrast at the two levels of basic sweep brightness are shown in Table II and Figure 2. As was expected, the higher BSB yielded higher brightness values but the increase was not proportional: 1 log unit increase produced an increment of about 50 percent in the brightness means, with more effect on background than on target. The latter difference was sufficient to induce a loss of about 20 percent in maximum contrast at the higher BSB although there was essentially no effect of BSB on the minimum contrast.

Sweep Movement. The mean brightness and contrast values for the two types of sweep movement are shown in Table III and Figure 3. Sector scanning had little net effect on maximum target brightness but approximately doubled the minimum target brightness and increased the luminance of the background less than 50 percent. The maximum contrast was similarly unaffected while the minimum approximately tripled.

Sweep Cover. The mean brightness and contrast values for each sweep cover condition are shown in Table IV and Figure 4. Not much difference seems to exist between the effects of the two lighter filters on target and background brightness. While the effects upon luminance are rather inconclusive, the contrast presents a steady picture of high values for no cover, low values (60 percent drop) for black cover, and filter values in between although not proportionately. An interesting phenomenon was observed during the experiments with the overlays. While the visible sweep appeared as a bright line on the circular test field of the brightness meter and could be timed easily, the exact moment of passage of the covered sweep could not be determined accurately. In this latter case, only a darkening of the entire area can be observed when the sweep passes through the field; and there is not much difference between the dark shadow of the cover and the appearance of the background, particularly at low target and background brightness. In case of high background luminance, on the other hand, the covered sweep may pass the test field as a dark band. Naturally, this has some effect on the results of the brightness measurements, and especially on the apparent background luminance: Since the scope background now appears brighter than the sweep, and background brightness is already read before the dark phase starts (i.e., too early), this may lead to higher readings of background brightness when the sweep is covered as compared to readings obtained with the uncovered sweep. Moreover, the accuracy of measurement was also affected at the low I.D. thresholds because sweep cover and dark phase blended into each other and did not permit

Variable	CRS	SS
Maximum target brightness	6.79	7.11
Minimum target brightness	.39	.86
Maximum background brightness	.16	.23
Minimum background brightness	.07	.10
Maximum contrast	53.09	54.05
Minimum contrast	5.91	17.48

Table III. Arithmetic means of target and background brightness (ft.-L) and brightness contrast for the two types of sweep movement.

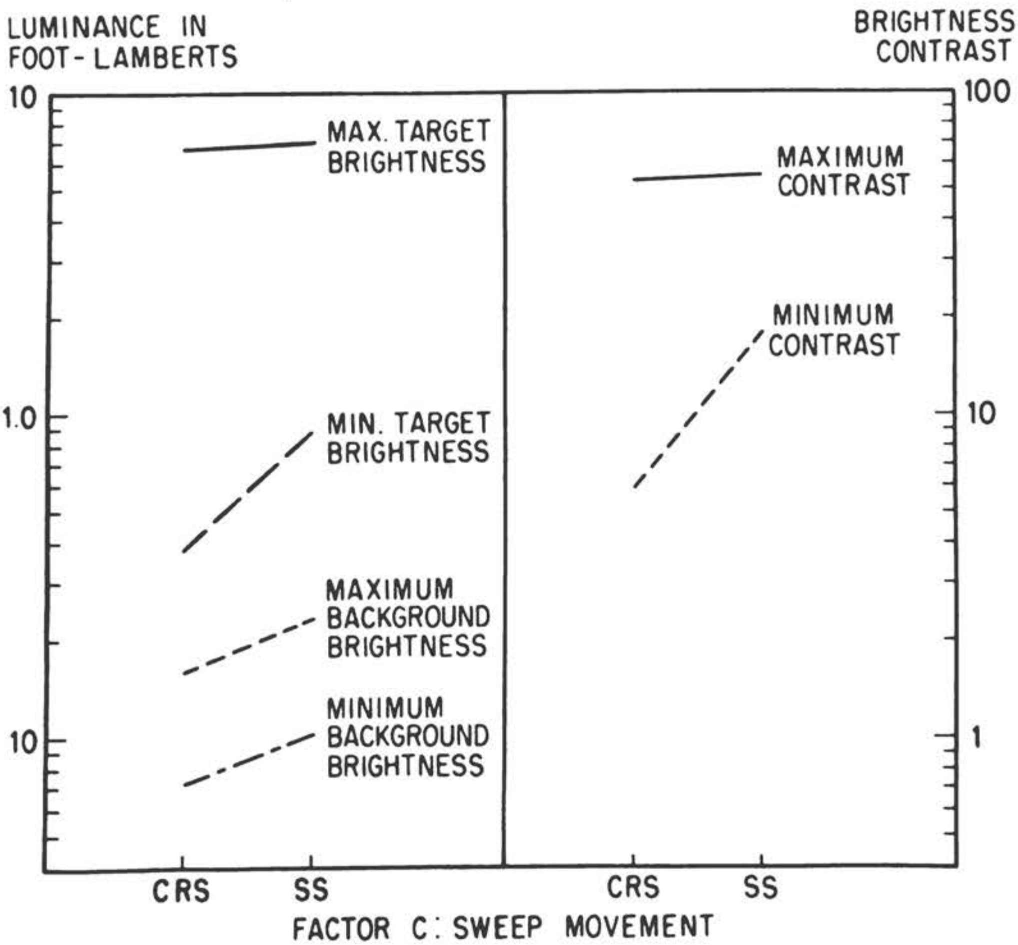


Fig. 3.

Variable	1	2	3	4	5
Maximum target brightness	11.58	4.69	6.84	4.85	6.79
Minimum target brightness	.65	.45	.67	.56	.79
Maximum background brightness	.23	.20	.20	.17	.19
Minimum background brightness	.06	.10	.09	.07	.10
Maximum contrast	84.56	33.98	59.30	47.17	42.83
Minimum contrast	14.76	5.67	13.53	11.86	12.65

Table IV. Arithmetic means of target and background brightness (ft.-L) and brightness contrast for each sweep cover condition.

the exact timing of the reading. This may account for some of the inconsistent results on target and background luminance as shown in Table IV.

Observer. The mean brightness and contrast values for each observer are shown in Table V and Figure 5. The observed differences are within the estimated limits of maximum error.

Summary and Conclusions

A study of brightness and brightness contrast was made with regard to the main variables affecting the appearance of targets on the intensity modulated radar scope. The purpose of the experiments was to gather information about target and background luminance and their contrast during radar operations, and to investigate the parameters which determine form discrimination. The major independent variables under investigation were basic sweep brightness, video gain, sweep movement, and various types of sweep covers; and their effects on target and background brightness and brightness contrast were studied at five appearance levels of the target. The electrical parameters were controlled by two voltmeters connected with the cathode and the grid of the two types of cathode ray tubes used as radar screens. A Radar Flight Simulator AN/APS13-T1A, in conjunction with an APQ-13 radar training set, was employed in the experiments.

Brightness and contrast differences of varying degree were found between the five levels of target appearance or video gain settings, the two degrees of basic sweep brightness, the two kinds of scanning, and the five sweep cover conditions. The meaning of the main effects observed was discussed with regard to PPI scope operation. In addition, the following conclusions can be drawn:

(1) Radar scopes of the APQ-13/23 type are rather poor purveyors of visual information because of relatively poor resolution and high fluctuations in brightness and contrast. Even under optimal operating conditions they fall short of perfect visual requirements.

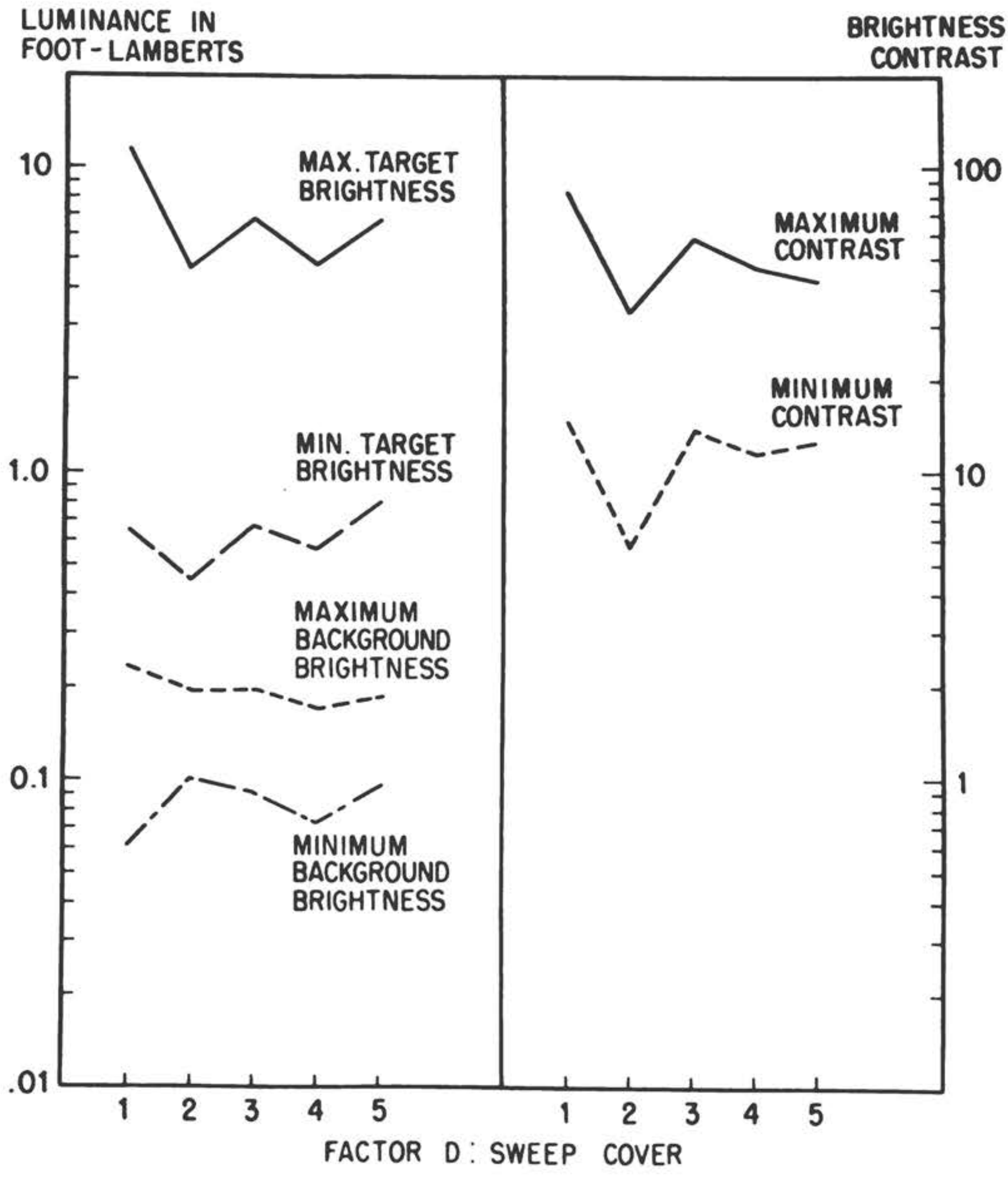


Fig. 4.

(2) Calibration and standardization of radar PPIs are extremely difficult because of the delicate electronic components, the lack of constancy of performance, and the unpredictability of the output of equipment and display. This concerns the electrical variables as well as the mechanical factors and the chemicals involved.

(3) To calibrate the APQ-13 radar set, the following perceptual requirements should be met:

Variable	1	2
Maximum target brightness	6.36	7.53
Minimum target brightness	.64	.61
Maximum background brightness	.19	.20
Minimum background brightness	.08	.09
Maximum contrast	52.02	55.12
Minimum contrast	12.31	11.08

Table V. Arithmetic means of target and background brightness (ft.-L) and brightness contrast for each observer.

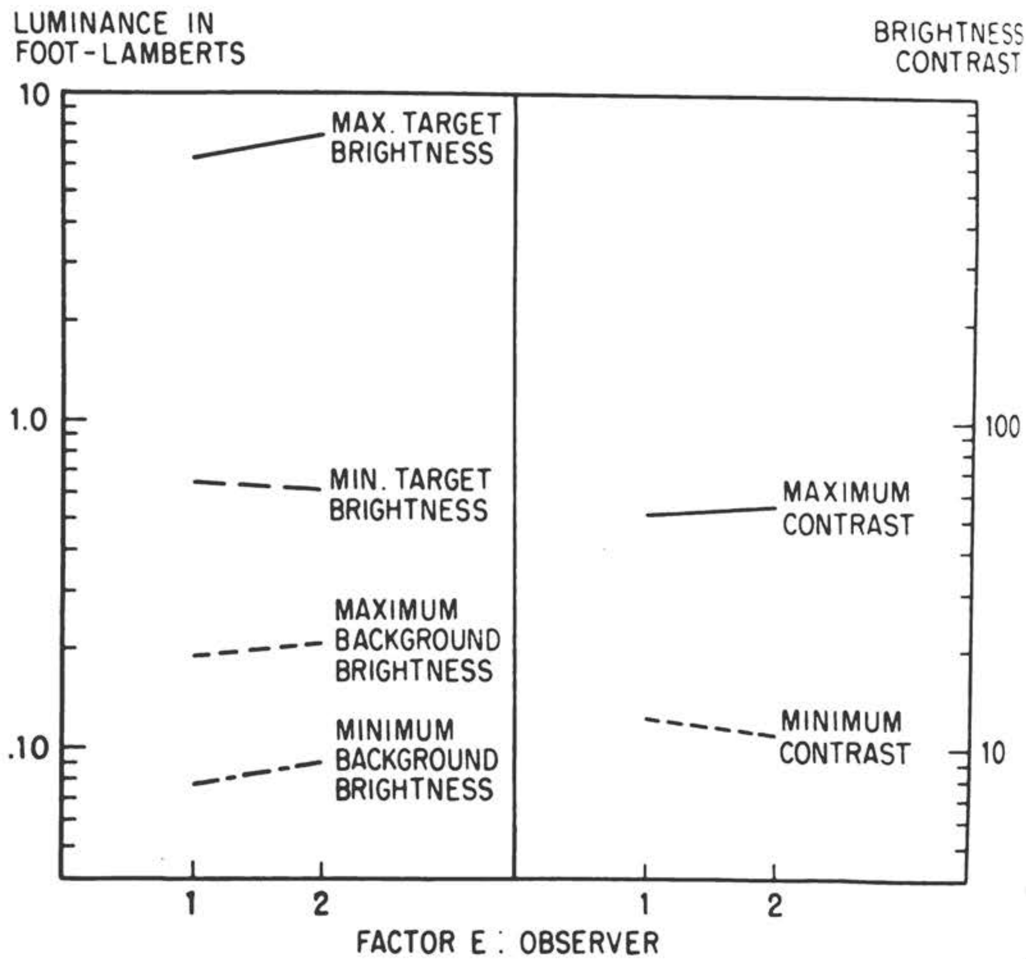


Fig. 5.

(a) The sweep -- either continuously rotating or sector scanning -- should be operated at a constant speed. Variations of the r.p.m. produce fluctuations in brightness and contrast.

(b) The sweep should be operated at the lowest brightness possible. High sweep brightness produces higher scope brightness but adds little to brightness contrast.

(c) The sensitivity of the amplifier should be so that the range between the first three appearance levels is as large as possible. This permits finer adjustment of the video gain and yields optimum target definition. The range used in our experiments seems to be adequate under the present design limitations.

(d) The sector scan provides viewing conditions better than the circularly rotating sweep with regard to maximum contrast, and far superior with regard to minimum contrast. Thus, the difference between maximum and minimum contrast is reduced, and the high fluctuations in luminance are eliminated. For this reason, the sector sweep should be used operationally whenever it seems possible.

(4) The location of the brilliance and the receiver gain control knobs is very poor. Both knobs should be located at the front part of the indicator or at any other convenient place for more accurate adjustment of brilliance and video gain.

(5) Brilliance and gain settings should be facilitated and made more accurate and constant by attaching dials or scales to control knobs. Moreover, provisions should be made for voltage control of the two variables.

(6) The 6AK5 amplifier is rather insensitive and acts after about one-third of the entire range has been used. This causes delayed action and also jamming of the practical range for clear information in the middle of the scale. Maximum target and background brightness and optimal contrast were found at scale settings in between 180° and 200°. For more accurate adjustment and tuning, gain voltage and scope brightness should reach their maximum at about the end of the scale, thus providing for an almost linear increase.

(7) If the sweep line must be retained on the surface of intensity modulated radar scopes, the use of overlays should be seriously considered. Adjustable filters eliminate the bright sweep line, increase the constancy of the visual variables, and thus may help to increase the efficiency of the observer.

LABORATORY RESEARCH CONCERNING DISPLAY PARAMETERS INVOLVED IN VISUAL RECOGNITION

William R. Bush

Introduction

Sensors utilized in a man-machine system require the presentation of a return on a display screen. This display must be evaluated by a human operator who will make a decision based upon his interpretation. The problem of optimizing the display in such a way as to expedite a rapid and effective decision by the human is of concern to many man-machine systems, but becomes paramount under the military requirements of sensor bombing (e.g., radar, IR, TV).

It can be shown experimentally that different situations require different techniques for optimizing the human's perceptual abilities. For example, the ability to recognize and locate a gross target area in a display may be enhanced by a reduction in image detail (Figure 1A). Selection of a specific target within the gross area, however, may require maximum image detail (Figure 1B). This apparent conflict in requirements could be resolved by providing several modes of operation, each tailored to the needs of the situation.

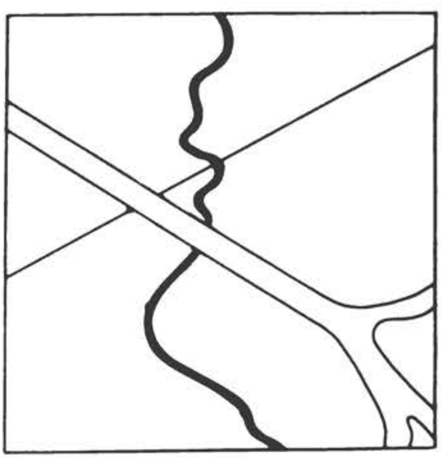


Fig. 1A.

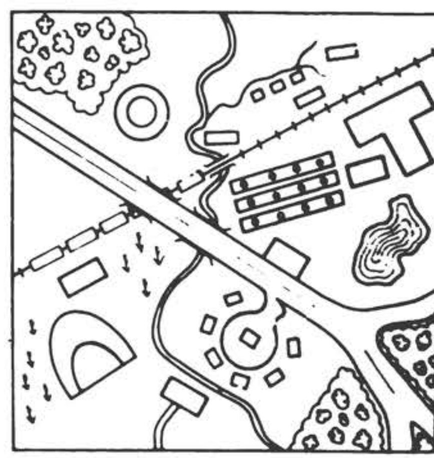


Fig. 1B.

An experimental program is presently being planned at RCA to evaluate the parameters important in a visual display. The experimental hypotheses, outlined above, will be empirically tested in a quantifiable manner under conditions simulating a sensor display. The method by which this will be accomplished is discussed below.

Experimental Program

The stimulus materials will be generated with several restrictions maintained:

(a) The stimulus materials must be quantifiable in nature. In this regard, it is considered desirable to study perceptual processes according to accepted psychophysical techniques. In order to accomplish this purpose, it is necessary to utilize stimulus materials which are generated by certain mathematical expressions (as described below) rather than utilizing radar returns, aerial photographs, etc.

(b) The materials will not be "natural" phenomena, but rather will consist of artificially produced forms comprised of dark figures on a white background. As such, they will not appear meaningful, and therefore, influenced by the past experience of the subjects. In addition, utilization of abstract forms will afford greater freedom in the application of the research data to a large number of practical situations.

(c) The psychophysical data, so determined, will be expressed in terms of functional equations of the type

$p = f(A, B, C, \ldots, n)$, where p is the performance measure and A, B, C, D, . . . , n are the physical parameters.

(d) The validity of the above expression will be determined by predicting, on the basis of the expression, performance when the observers are presented with "natural" stimuli, e.g., radar returns or aerial photographs. These predictions will be empirically tested, and deviations from the theoretical expression evaluated.

The procedure employed to accomplish these requirements has been to utilize certain parameters of probability theory in controlling the appearance of forms within a series of matrices which systematically vary in size. In addition certain contingency controls will establish the amount of "grouping" or "clustering" by any particular set of "elements" which comprise a "form".

By this means, research can be initiated which will indicate the effects of the following parameters on performance:

(1) Number of elements per unit area (indicated by the differences occurring as a result of matrix-size),

(2) Effects of degree of correspondence between two simultaneously-presented displays,

(3) Effects of degree of form-complexity,

(4) Resolution differences between two simultaneously-presented

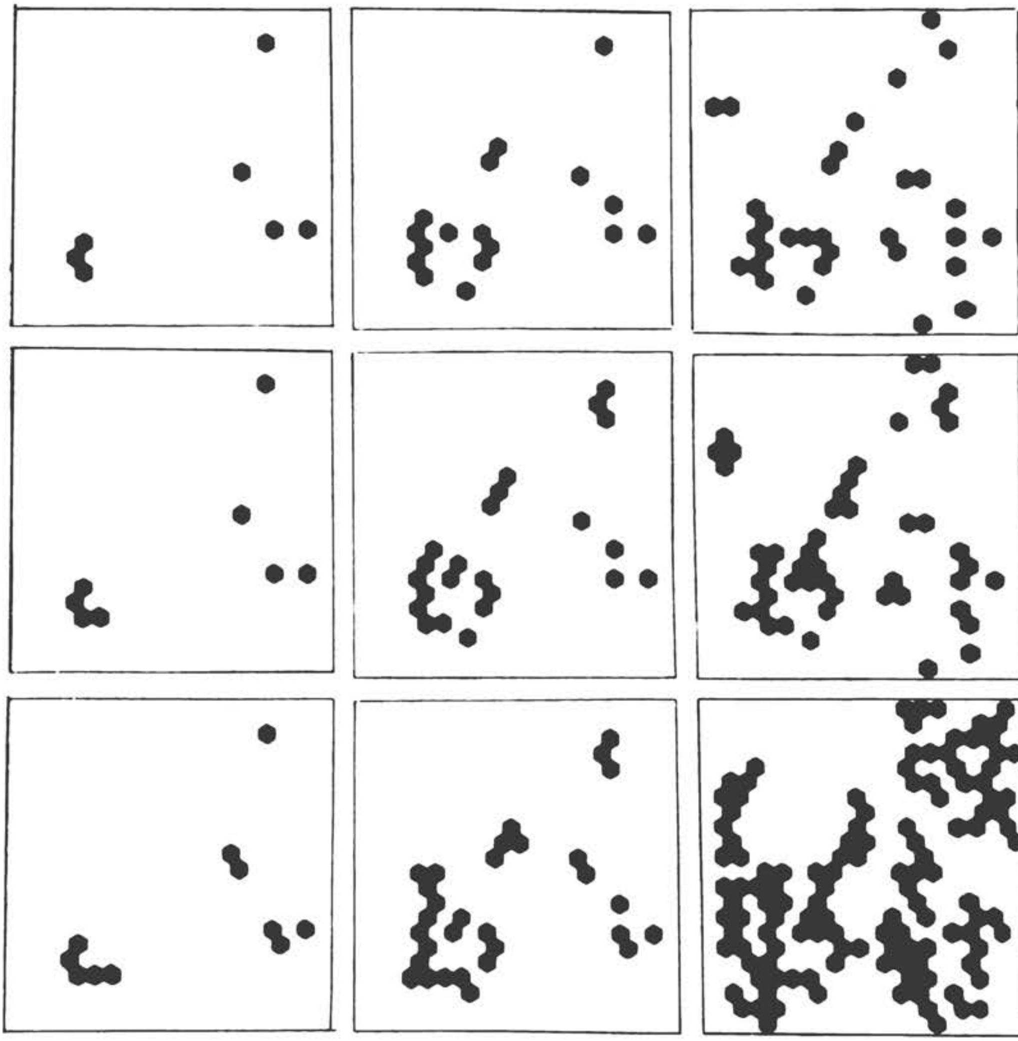


Fig. 2.

displays. This can be obtained by optically de-focusing the displays by known quantities,

(5) The degree of overlap obtainable with two displays that must be matched. This is obtained by generating stimulus materials in such a fashion that only certain quantifiable portions of their area can be matched. For example, the left half of one of the displays may contain forms which can only be matched with the right half of the other display,

(6) The utilization of flicker and/or color differences.

Although it is probably premature -- if not dangerous -- to consider theoretical interpretations of research not even initiated, certain general considerations are at least plausible at the present time. Chief among these would be the probable emergence of certain parameters of form which may be of importance in form perception.

Based upon the design of this study, and the means by which the stimulus materials are to be generated, it can be concluded that only a limited number of forms per se are possible in any one of the matrices. For example, the probability of generating a form which contains 10 or more elements in a 20 x 20 matrix with a low-cluster-level is so small as to discount its likelihood of occurrence completely. To be more specific, it should be possible to compute the probability of the occurrence of any number of elements in a grouping (a "form") for each of the display systems. With this information, it should next be possible to a priori generate all possible forms for a given display system.

This procedure would be most useful in attempting to correlate performance with form itself. Various measurements have been made in the past (Casperson, 1950; Bitterman and Krauskopf, 1953; Boynton and Bush, 1955), but these studies have suffered from utilization of familiar forms and/or too limited a number of forms. It would appear that a more fruitful approach would be to employ a large number of meaningless forms which are produced along quantifiable dimensions (e.g., the number of elements comprising same). It would then be fruitful to utilize certain additional physical parameters (perimeter, area, angle of curvature, points of discontinuity, etc.) as possible correlates with performance.

THEORETICAL MEDIATORS OF FORM RECOGNITION FOLLOWING VERBAL TRAINING*

James M. Vanderplas

The form, or shape, of an object is an important determinant which influences its recognition and identification, as well as its discrimination. An important question arises almost immediately in the consideration of recognition, and that is, "can recognition be learned, and if so, how?"

No one would seriously question an affirmative answer to the first

*This research was supported in part by the United States Air Force under Contract No. AF 41 (657)-47, monitored by the Operator Laboratory, Air Force Personnel and Training Research Center, Lackland Air Force Base, San Antonio, Texas. Permission is granted for reproduction, translation, publication, use, and disposal in whole and in part by or for the United States Government.

part of this question, but I think we would agree that the second part presents difficulties. The results of many studies have shown that recognition training in various forms can influence and improve performance. But despite the extent and variety of training methods successfully used in practice, precise specification and agreement on the underlying factors which influence improvement of recognition are lacking. One important reason for this lack is that systematic concepts, with which to organize our efforts in this area, have been slow to develop. Another reason is that studies of form perception have been largely of a demonstrational or qualitative variety, and little progress has been made in the quantitative specification of either (1) variables which determine differences among forms or (2) variables which determine the observer's ability to learn to recognize form. Notable exceptions to the first statement above appear in the work of Drs. Attneave and Arnoult, at the Air Force Personnel and Training Research Center(1) and in the work of Dr. Fitts and his associates at Ohio State University(2). Both of these groups have made important progress toward determination of quantitative characteristics of form and patterns which may be specified and which may mediate both discrimination and recognition.

Variables which are related to the observer, however, have gone largely unexplored, except in a relative, or qualitative, way. Psychophysical studies have, of course, resulted in detailed knowledge of the limits of discriminability of objects in terms of the contrast and detail of the object. This work is well known. But variables which influence higher-order performance such as recognition and identification, when contrast and detail are well above threshold, have not yielded to quantitative approaches. Variables of the observer, such as past experience, amount of training, and training methods, as they affect improvement in form recognition, are in need of more precise specification and quantification if further progress is to result.

For somewhat over a year we have been engaged at Washington University in research aimed at finding principles for improving form recognition through training. As part of this effort, we have explored the possibility of adapting principles from classical learning theory and applying them to the problem of training observers to recognize form. We have reviewed a substantial number of studies, which have appeared in recent years, in the areas of verbal and motor response learning. Many of these studies indicate that learning to respond with verbal labels, to stimuli in a preliminary task, facilitates the acquisition of new verbal or motor responses to the same stimuli used in a second task. The positive transfer to the second task, in the form of more rapid acquisition of the appropriate responses, has been accounted for in a number of ways. Some writers have suggested that the stimuli become more "distinctive" as a result of the addition of cues related to the first-task verbal response(3). Others have suggested that the stimulus generalization gradient becomes steeper as a result of differential reinforcement during practice(4). Still others have suggested that "mediation," in the form of associations made to the verbal response(5, 6, 7, 8), is an important factor.

For our purposes, if these hypotheses could be verified and extended, they would have an important relation to studies of training methods for improvement of form recognition. These hypotheses not only imply that recognition of forms can be improved, but they also represent attempts to specify the underlying factors leading to the improvement. I would therefore like to discuss briefly several of these hypotheses, since they represent possible aids to a programmatic approach to the specification of the observer variables that I mentioned earlier*.

The first of these is the hypothesis of Dr. Eleanor J. Gibson, which I have represented schematically in Figure 1. The hypothesis states that practice in responding differentially to two stimuli (S_a and S_b on the left of

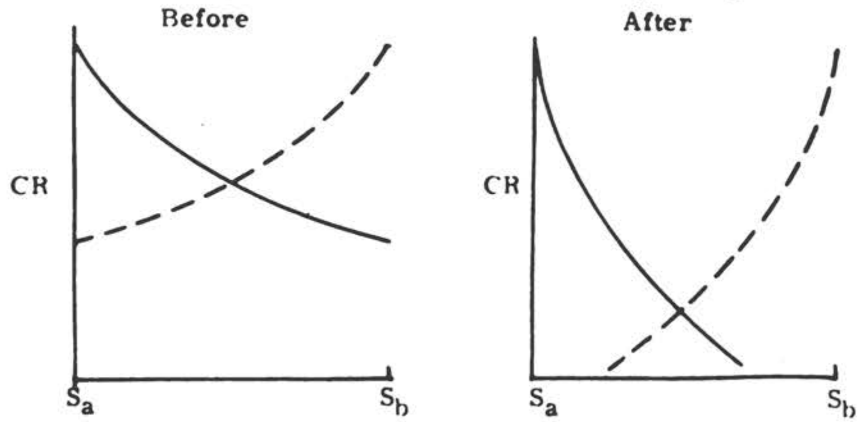


Fig. 1. Generalization gradient before and after differential reinforcement of responses to two stimuli (interpretation of Gibson's postulates).

Figure 1), by reason of differential reinforcement, results in an increase in the slope of the gradient of stimulus generalization. This increase in slope reduces the probability that a response, learned to S_a, will generalize to S_b. The resulting new gradient is shown on the right of Figure 1. The hypothesis is, of course, supported by the literature of classical conditioning. To the extent that the gradient of stimulus generalization can be manipulated, within the limits of discriminability of the stimuli, improvement of recognition by such manipulation is possible, through training. Our extension of this kind of hypothesis, of course, would be that forms which are more discriminable are less readily confused, and thus they are more readily recognized, since fewer errors, or false recognition, would be expected.

A somewhat similar hypothesis, based upon the notion that training in verbal labeling results in greater distinctiveness or discriminability of the stimuli, is provided by Goss(3). I have represented this hypothesis in Figure 2. Goss's hypothesis implies that two stimuli (S_1 and S_2 on the left

*A summary of theoretical mediators of transfer of training, in terms of relations similar to those cited here, is presented by Goss(3).

$$S_1 \qquad S_1 \longrightarrow R_1 \dashrightarrow S_{R_1} \qquad \left[S_1 + S_{R_1}\right]$$

$$S_2 \qquad S_2 \longrightarrow R_2 \dashrightarrow S_{R_2} \qquad \left[S_2 + S_{R_2}\right]$$

Fig. 2. Stimulus complex (right) for two stimuli resulting from addition of response-produced stimuli (center) to original stimuli (left) (interpretation from Goss).

of Figure 2) become more "distinctive" as a result of the attachment of additional stimuli to the original ones. These additional stimuli are assumed by Goss to be response-produced (as shown in the center of Figure 2) and to become attached to the original stimuli (as shown on the right of Figure 2). The resulting stimulus-complex is said to be more discriminable than the original by reason of the added dimension of variation. This hypothesis seems a reasonable one, and is supported by a number of studies cited by Goss(3). Recognition of form, according to this view, would be predicted to improve from training of most types of response, either verbal or motor, since they might all result in added cues and thus to increased discriminability, with the effects noted earlier.

A third formulation, of the role of verbal responses, in transfer of training, also appears applicable to the study of form recognition training. This formulation is provided by Birge(5). It also makes use of the notion of response-produced stimuli, but in a manner somewhat different from that of Goss. The relations are sketched in Figure 3. The verbal response (R_x)

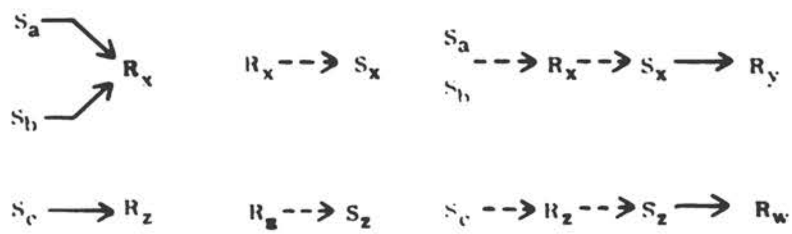


Fig. 3. Transfer of instrumental response from one stimulus to another following association of response-produced stimulus (right) to original pair of stimuli (left) and to a third stimulus (S_c on left) (interpretation from Birge).

is assumed to be conditioned (or otherwise associated) to two stimuli S_a and S_b (on the left of Figure 3), while a second response (R_z) is conditioned (or associated) to a single stimulus S_c. The responses R_x and R_z are assumed to give rise to stimuli S_x and S_z (in the center of Figure 3). These stimuli, S_x and S_z, then are assumed to be conditioned or associated to instrumental responses R_y and R_w. We may note that if R_y and R_w are different recognition or identification responses, we would expect to find correct recognition of S_a or S_c following association of the verbal responses R_x and R_z. However, if S_b is presented, we will expect R_y to occur, and false recognition will result. From these assumptions, of Birge, we might expect that correct or incorrect recognition would depend on two factors: (a) the degree to which the response-produced stimuli are discriminable (in the case of S_a and S_b, the response-produced stimuli are the same, while for S_a (or S_b) and S_c they are different), and (b) the strength or probability of occurrence of the responses R_y and R_w to the response-produced stimuli S_x and S_z.

A fourth kind of mediator which has appeared in the literature on transfer of training makes use of the notion of existence of associations, formed on the basis of past experience or otherwise. The clearest evidence for the operation of this type of mediator comes from an experiment by Buzzotta(6). Subjects learned to respond with a word (a noun or adjective) to each member of a list of nonsense syllables. They were then required to learn a second list in which the syllables were the same, but the response words were different. Positive transfer was found to be related to the probability of occurrence of an intermediate response word which was never elicited overtly during either task. The first- and second-task words were selected from free-association lists on the basis of variations in the probability of occurrence of the associative response, with controls for the probability of direct association between the responses required in the two tasks. I have sketched the assumed relation in Figure 4. The subject responds overtly to the nonsense syllable (S_1 or S_2 on the left) with a word (R_1 or R_2). To keep our notation more consistent with that of the previous figures, I have assumed that R_1 and R_2 give rise to response-produced stimuli (S_{R1} and S_{R2}) which elicit the intermediate, or mediating responses,

$$S_1 \longrightarrow R_1 \dashrightarrow S_{R_1} \dashrightarrow r_1 \qquad\qquad S_{R1} \dashrightarrow r_1 \dashrightarrow R_a$$

$$S_2 \longrightarrow R_2 \dashrightarrow S_{R_2} \dashrightarrow r_2 \qquad\qquad S_{R2} \dashrightarrow r_2 \dashrightarrow R_b$$

Fig. 4. Transfer of instrumental response (R_a on right) mediated by implicit association to original response (R_1 on left) (interpretation from Buzzotta).

r_1 and r_2. The probability of the occurrence of r_1 or r_2 to S_{R1} or S_{R2} is the important variable which is related to transfer in learning the responses R_a and R_b (on the right of Figure 4). In terms of recognition or identification, we might say that a recognition response would depend upon the probability that it is elicited, by reason not of direct conditioning or association with the response-produced stimulus (S_{R1} or S_{R2}), but by mediation via an associative response (r_1 or r_2) such as a concept word or other intermediate response. The mediating response (r_1 or r_2) may serve to "tie together" S_1 and R_a, the strength of the "tie" being determined by the probability of the intermediate, associative response.

I have attempted to sketch a crude summary of possible mediators of recognition and identification, based upon the foregoing discussion*. The relations are shown in Figure 5. I might point out that all of these relations appear to depend implicitly upon the fundamental assumptions that (a) recognition or identification depends primarily upon either discrimination or differential response, or both, and (b) discrimination and differential responses to discriminated stimuli (form) can be manipulated by training or appropriate selection of responses based on the past experience of the observer.

1. P (S_a discrim. fr. S_b, S_c, etc): Primary generalization

2. P ($S_a \longrightarrow R_a$ and $S_b \longrightarrow R_b$): Established stimulus-response association.

3. P ($R_a \dashrightarrow S_{R_a}$ and $R_b \dashrightarrow S_{R_b}$): Evokation of response-produced stimulus.

4. P (S_{R_1} discrim. fr. S_{R_2}, S_{R_3}, etc): Generalization of response-produced stimuli.

5. P ($S_{R_1} \dashrightarrow R_y$ and $S_{R_2} \dashrightarrow R_z$): Established connection of response-produced stimulus and instrumental response.

Fig. 5. Theoretical mediators of recognition before and after training (combination and adaptation of previous Figures).

Such discrimination would depend, first, upon the characteristics of the form. This dependency is represented in Figure 5 as the probability of discrimination of two stimuli, either before or after training, depending upon the basic characteristics of the generalization gradient, and the degree to which it may be manipulated, perhaps, as postulated by Gibson(4). A second factor would be the probability that an associative response would occur, following presentation of the form during training or on the basis of past experience. This factor is represented as a probability in Figure 5. A third factor, a mediating one, would be the probability of arousal of response-produced stimuli and fourth would be the degree to which these response-produced stimuli are discriminable, as represented as a probability in the Figure. Finally, the strength or probability of occurrence of a differential, instrumental response of recognition or identification, following arousal of these mediating stimuli or responses, would influence overt performance.

*In constructing the summary, I have perhaps overextended some of the hypotheses on which this summary is based, and I have therefore not indicated precisely the basis for each statement. I believe that the basis for each statement will be reasonably clear, but for errors in interpretation of these hypotheses I must accept responsibility.

In closing, I should point out that these constructs may represent merely a set of formal "thinking aids", to encompass what might appear to be fairly common-sense notions, of association and habit. To the extent that they may be explicitly defined, in operational terms, however, they represent variables which may be subjected to experimental test by appropriate selection of materials and method, and they may, thereby, make possible the quantitative treatment of variables of the observer which are intimately related to the recognition of form and which have been only vaguely outlined.

PERCEPTION OF GROUPING IN VISUAL DISPLAYS

Horace H. Corbin*

Some of the phenomena of form discrimination have been described in our literature these many years. The ingenuity of early Gestalt psychologists has left us this heritage. The significant independent variables suggested by this literature are "proximity," "similarity" and "common fate" of the textural components of the visual stimulus array. The perceptual grouping that occurs is obvious to any observer. Dots of closer proximity or of similar form, color or size appear to make up a higher order form. What form? Presumably any of a number supplied by the observer. Rows or columns, circles, triangles or squares, lines or n-tagons. While the phenomena are obvious the quantitative relation of independent and dependent variables and the influence of several parameters are not obvious. In fact, little systematic analysis is available. And such analysis is a must if we are ever to bring this material into useful line.

What is the form which emerges at near threshold stimulus values? Is the perception dependent on the size of the display, the angle of regard, or how close we are to the display? In other words, are the forms composed as a result of grouping retinally or centrally determined? This is the broad area of research in which the Mount Holyoke College group has made a small advance. Our interest and that of Operational Applications Laboratory of Air Force, Cambridge Research Center, stems from the problem of identifying groups of objects in a visual display. These groups presumably will be discriminated out of the array of potential objects because of some property common to the group and no others. What property?

We decided to start with dots of similar shape, color, and size and show them in a series of displays in which some of the dots were converging on a destination or becoming more proximal in a circular pattern. The subjects search this series of displays to find the converging group as quickly as possible.

*With A. Clowes, G. Meuser and E. P. Reese.

Procedure

The basic procedure simulated the constants of a radar display. Our basic materials were a series of circular displays, 12 inches in diameter. Each display of the series had 35 dots about 1/15th inch (2mm) in diameter dispersed over the field at an average density of one every 3.2 square inches of display. The diameter of each display of 12 inches simulated a distance of 300 miles, which could be traversed in 30 minutes by planes moving 600 miles per hour. A new display of 35 dots was shown every 10 seconds. A plane traveling 600 miles per hour would move 1.67 miles in 10 seconds and this distance corresponds to 1/15th inch on our display. The diameter of each dot is 1/15th inch; so on successive displays each dot is displaced on its linear course by its own diameter. Each series consists of 60 displays and takes 10 minutes to show. The total straight line course traveled by each dot is four inches and corresponds to 100 miles. We had 19 such series of 60 displays with 35 dots each. Thus, nearly 40,000 dots were carefully located and drawn.

The courses on which the dots moved were highly stylized. There were eight such courses, representing either direction on the horizontal, vertical and both diagonals, or 0, 45, 90, 135, 180, 225, 270, and 315 degrees. Courses of non-converging dots and their starting points were drawn from a sampling box. Points of convergence varied from problem to problem.

We have results from three experiments:

(1) In the first experiment we varied the number of converging objects, from two through eight. The result was a clear indication that the greater the number the faster the identification (or the less proximal the dots have to be to be identified).

(2) In the next experiment we varied display size, observation distance, and where in the display objects converged. To do it we committed our 19 problems to film (a three-month process) and found a projector to automatically turn the frames, modified the projector to register successive frames in one spot on the screen, varied display size by moving the projector on a track to and from the screen, and coincidently varied focus and contrast by a circular variable density wedge filter.

The results can be seen in Figure 1.

(a) Display size makes no difference. The mean is near 47 all across. Why?

Other results are in accord with this, some are not. Grether and Williams (1947) found no systematic relation between speed of reading dials and dial size one to four inches. Worton found optimal size of CRT's at about six inches, above that not much is gained. But this involved single blip detection which would enhance small area detection.

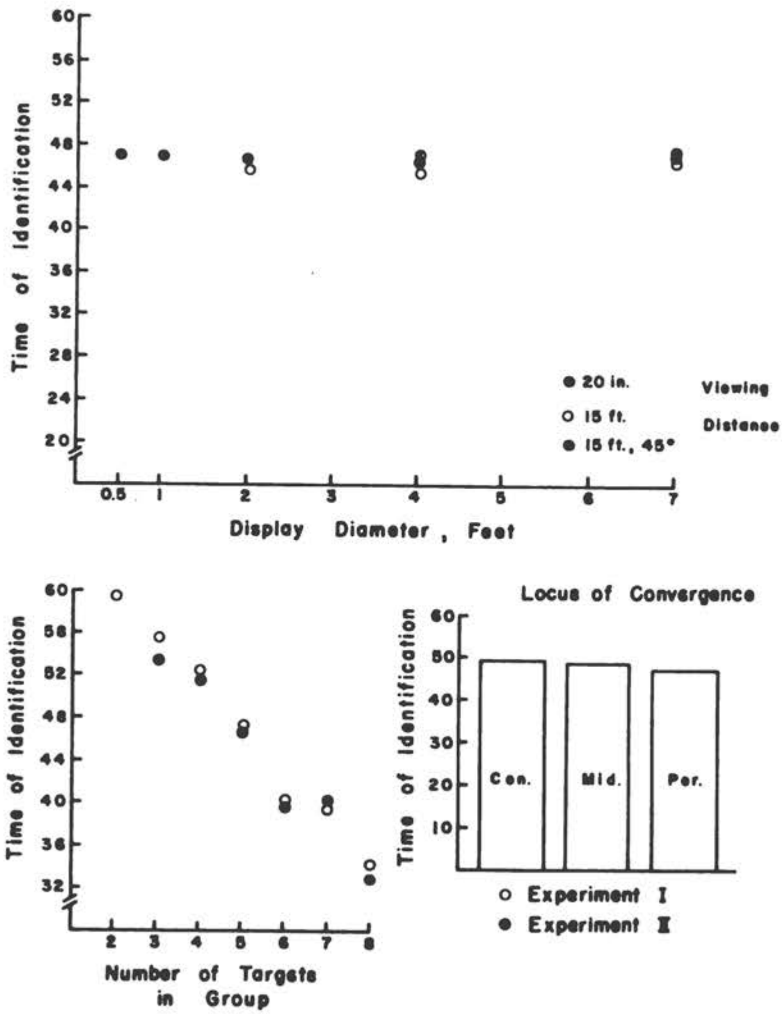


Fig. 1. Time of identification of the converging group of targets (60 represents collision) shown as a function of Display Diameter and Viewing Distance (top); as a function of Number of Targets in Group (bottom left); and as a function of Locus of Convergence (bottom right).

(b) Distance of viewing - no difference, but angles involved are still above acuity.

(c) Angle of regard - no difference at 45° from results in frontal parallel position.

(d) Number of targets converging - clear variation.

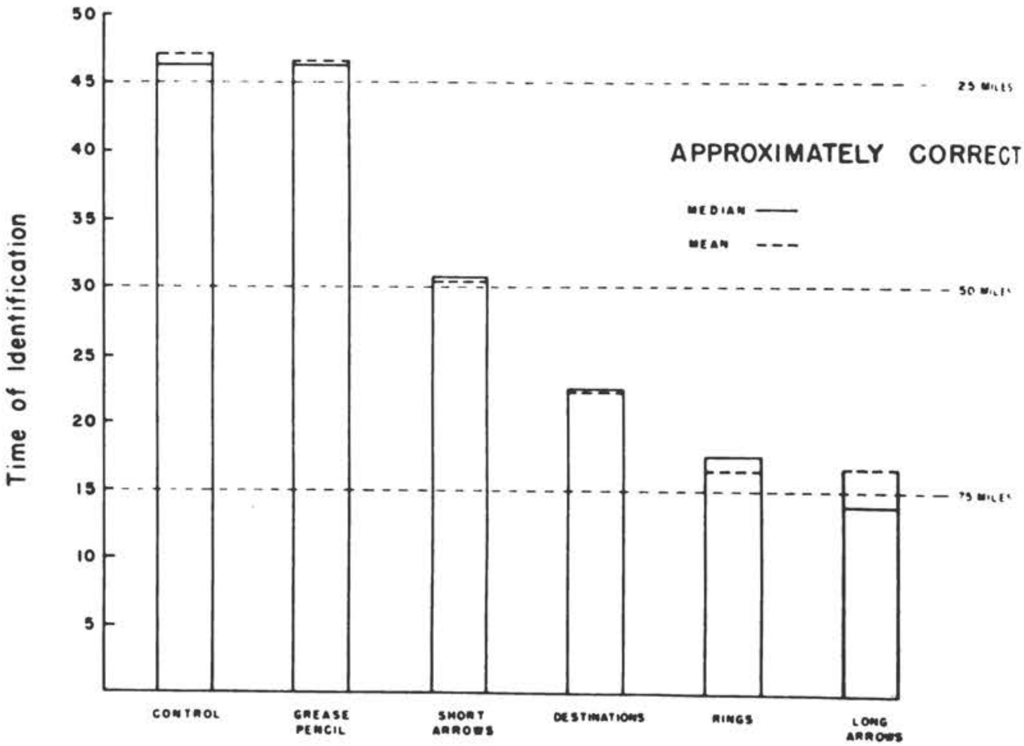


Fig. 2. Time of Identification of the Converging Targets shown as a function of Various Aids Employed on the Display (to a criterion of Approximately Correct -- all but one target had to be identified and the destination had to be approximated).

(e) Location of convergence - probably no difference.

(f) Individual differences - not prominent.

(g) Wrong choices. What do subjects look for?

Of 529 choices:
14% triangles
8% rectangles
20% arcs
11% circles
45% pairs
2.6% irregular figures.

Average separation of choices: 2.3 cm. from convergence.

(3) Aids to the discrimination: the procedure repeated previous ones with the exceptions of starting each problem later in the series and using only three, four, five, and six converging dots in the various problems presented.

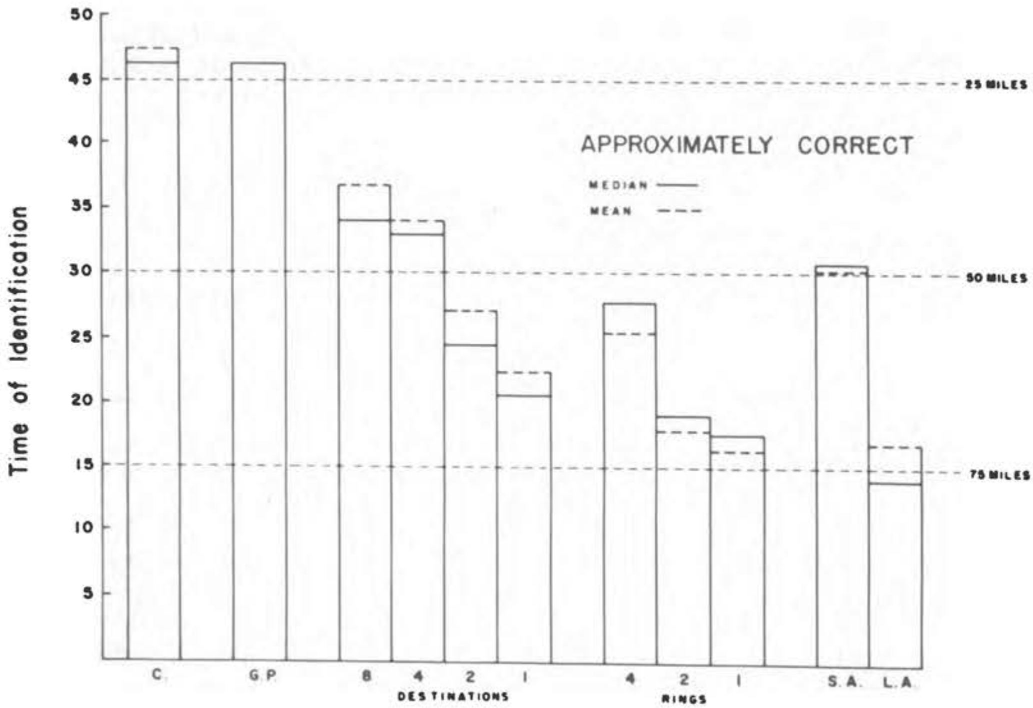


Fig. 3. Time of Identification of the Converging Targets shown as in Figure 2, illustrating the influence of the Number of Destinations and Rings Employed as Aids to the Discrimination.

The aids were:

(a) Arrows, 1 cm. and 3 cm., were tried. They were appended to each dot in the direction of the destination of each. Thus, they tended to make converging dots more proximal to the point of convergence (an obvious aid).

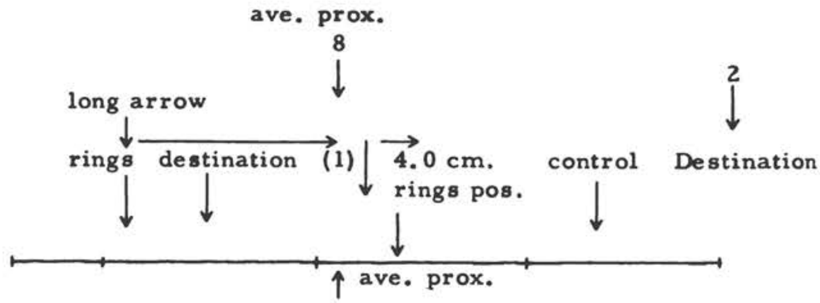
(b) Rings with destinations as the center. They were 4 cm. in radius or about 40 miles from the destinations. One, two, and four rings were shown.

(c) Destinations: 1, 2, 4, and 8 each a red spot 3.5 mm. in diameter.

The median and means displays on the 1 - 60 scale are shown for control (no aid) and various aided series in Figure 2. Arrows, rings, and destinations make a clear difference. Long arrows seem best. Rings are also excellent aids and are less cumbersome in the display.

Figure 3 shows that one destination and one range ring are superior aids to multiple destinations or rings.

Here is a chart which relates the significant values of all performances. This shows the relation of the constants of the display and the variable performances of group perception with each of the aids.



MILES	100.0	75	50	25	0
INCHES	4	3	2	1	0
CM	10.16	7.62	5.08	2.54	0
DISPLAY	1	15	30	45	60

<u>Conclusions</u>

(1) Number of converging objects is a significant variable in this discrimination of form.

(2) Aids such as rings, arrows denoting course, and destinations enhance the perception to varying degrees. Rings ranging from destinations seem to aid the discrimination most.

DISCUSSION

TANNER: I'd like to comment on Dr. Bush's paper where he has split up and thrown away detail in advance, or this sort of notion. Essentially, what I think you were doing is restricting more and more the task that this type of system can perform. When you do this I think you lose one of the big advantages that you can gain from using a human being. I think, also, that we might carry what you were trying to do one step further, if we've going to do it at all. Let's get rid of these people. In other words, let the decisions be made by the system and get rid of the observer completely. The narrower you make it the less you want the human being in there. One of the big gains you get with the human being is due to the fact that he is versatile and can do so many different things.

BUSH: My personal feeling would be that if we can get the observer out of the system and still have the system efficient we should do it. The observer must be a part of the system as we now know it, in order to take advantage of his ability. However, if we do put him in there I would suggest that we try to reduce the amount of work that he has to do, making use of his versatility but not pushing it to any extreme limits.

TANNER: Now I think you get into a very delicate problem. You go to a lot of work to reduce the work he has to do, but maybe he'd be able to do it anyway. When they first put the long plotting rings on the radarscope they found the integration didn't help the observer a bit. They designed the thing and it didn't help because apparently the observer was doing his integration well enough without the aid of a ring. So, at this point we have to make some very careful decisions as to just how he is reducing the job. Perhaps reducing the job for him may not really be helping him.

BUSH: Maybe we can wait for another six or eight months, and then one of us may well be right.

HARCUM: I'd like to ask Dr. Vanderplas a question. I'm not quite sure I understood your "mediating responses" notion. Did you say that it would be a positive transfer situation where the mediating responses were more nearly the same or one where there was more of a multiplicity of mediating responses to the true visual stimuli?

VANDERPLAS: Well, first of all we should ask what we mean by positive transfer. In operational terms what does the hypothesis of Birge imply? She demonstrated this in a study in which children learned the same name for two different forms. The forms were animal forms of vague character which were printed on boxes. The children named the forms with a nonsense syllable. Two forms were named with the same nonsense syllable, while a third was named with a second. A reaching response, in which the child lifted the box under which the child thought a piece of candy was placed, transferred from the first box to the second. It did not transfer to the third box. When the response -- the response word -- in the first part of the experiment was the same for the two boxes, she called it positive transfer. I think we would call it misidentification. The degree to which she found positive transfer was related to the amount of practice and to the extent to which the subject verbalized the response in the first task. It depended not only upon the similarity of the response, which in this case was identical, but also upon the fact of oververbalization versus non-oververbalization. Does that answer your question?

HARCUM: I think so.

BLACKWELL: Yesterday, Dr. Morris tried to take the observer out of the vision conference here and today Dr. Gerathewohl succeeded. If I might, I'd like to go back across our last boundary and ask Dr. Gerathewohl if he carried through his experiment to the point of showing that the contrast variable, which I have respect for, does indeed indicate better visual performance. He showed us physical measurements of luminance and contrast under different conditions of the CRT display. Presumptively, the more contrast the better. Is that actually so? Have you shown it by experiments?

I didn't see any experimental data on vision in that slide at all. Was I wrong?

GERATHEWOHL: No. I think that means I left the observer out? I left the observer out because no experiments have been made to show that you can see the targets at all the contrasts you have. The experiment was only meant to show that the contrast changes when you are manipulating the physical conditions in your system. That tells when you can get the highest contrast and under what conditions. It is not supposed to show that the highest contrast is best. That is an assumption.

BLACKWELL: Of course, presuming no other variables are present. Actually I waited for years for someone to show the extent to which contrast is important in a CRT display. We have had many studies from Johns Hopkins, for example, where such things as CRT bias have been related to so-called visibility and I was struck by the fact that presumably a mediating variable was involved. I suspected that the contrast was reduced at the high CRT biases and, hence, it is for this reason visibility was reduced, but I couldn't prove it. Here you've gone to the trouble to measure the contrast and I was hoping I would see the visual data to back up the relevance of contrast.

GERATHEWOHL: Well, all right, we seem to have a difference of opinion.

BLACKWELL: But in one of your slides where you took away that center scan entirely, I lost orientation. I am sure if you gain contrast at the cost of good orientation on the part of the observer you may have gained less than you have lost from other causes not controlled in the experiment. Even though the contrast may have been enhanced, the visibility may have been reduced.

GERATHEWOHL: I don't see how orientation comes in because with what little orientation . . .

BLACKWELL: I mean where he points his eyeballs, orientation in that sense. Where does he look?

GERATHEWOHL: Oh, I see what you mean. This is not a search experiment.

MEAD: This is not a visual experiment.

GERATHEWOHL: It was not a visual experiment: a visual experiment of the measuring type and nothing else.

BLACKWELL: I can see it is dangerous to infer this proves which is the best of those conditions, for the Tiffany data do not convince me.

SESSION V

SUMMARY AND CONCLUSIONS

Chairman: Colonel Joe G. Gillespie

BALLARD: The purpose of this session is to try to pull together any things on which we may happen to be in unanimity -- apparently there are only a few of those -- and to discuss the rest of them.

DEBONS: I'm not sure whether we can iron out our solutions to one problem. One of the most formidable tasks we have in the Service is to translate academic information into reasonably intelligent information for other disciplines, such as statistics, electronics engineering and such. Although we here who have somewhat of a common language to discuss these problems can benefit highly from such meetings, I was wondering what we could do, as a group, to effect enhancement of communication of our major notions to the other interested parties in this problem area.

TANNER: The type of meeting that we have had suggests some of the efforts to answer the question you just asked. We've been mighty good. We've had, it seems to me, a strange combination of two types of meeting. Part of it has been strictly scientific, leaning very heavily toward theory. Other parts of it have been pretty much what the title of the thing says it was going to be.

I think that both types of meeting should be partly supported by the military. I don't want to see the military supporting one thing when they think they're supporting something else. I would like to see them come out and make the statement that they are going to support a theoretical meeting and then support that. The theory plays a role in that you can collect data and the theory tells you what you think the data are. It's a theory that is formed in the communication between the people who do this work. I think when we get better formulated theory the engineers will have no difficulty picking out the type of thing they want.

DEBONS: Do I gather this correctly now, Mr. Tanner? You are saying that we ought to develop the theories here independent of the people that I'm talking about, the physicists?

TANNER: No. I don't say it has to be independent. What I am saying, really, is that the theory is the important thing. This is the thing that's going to generate the communication.

DEBONS: Most often those people have difficulty in realizing the full importance and implications of the theories. Consequently, instead of their giving us support for your theory or for developing theory, we find them fighting us. It's obvious if they understood more clearly what the implications of the theory are to future developments they would be more inclined toward support.

TANNER: I think that one thing that we've pinned a little bit of difficulty on here is the fact that we've got a theory that doesn't really relate very well to some engineering theory. But, if we have that type of theory, perhaps when people working on systems have to put the observer in the system along with the engineering aspects of it, we will come up with theory which will tie the thing together.

BLACKWELL: At the risk of being anti-intellectual and anti-professorial I would like to refer to the question in the same way Tanner did; that is, to use it as an excuse to get on my feet and say what I want to say -- nothing personal intended to Spike.

I think that Colonel Debons answered his own question in part. I suspect that the only way that the Vision Committee or any symposium like this one can expect to communicate with the practical, line people is through those invaluable people who wear both hats -- people like yourself who have to be both an officer and a scientist. In the past of the Vision Committee this has been the only answer. We've never found a real way to make scientists out of military people or vice versa. It's those few people who provide the liaison like General Byrnes and others we can mention who have provided the answers in the past.

I'd like to go on by saying that I feel a little smug because what I said yesterday morning without hearing the program, I think, bears repeating. I said then that I suspected that by the time we were through we would agree we didn't know enough to solve a single military problem in the field of form discrimination. At the risk of being obnoxious, I'd like to repeat that remark. Now I think I know why and this brings us back to the question that Tanner was raising.

As I survey what has happened here, I've been very much impressed with the vitality and ingenuity of a great many theorists whose work I was not familiar with before. Certainly the information theories and other types of theories of form discrimination are very much alive and work is going ahead. My concern is that it must be directed in some kind of reasonable way at particular classes of problems. I'm not arguing for field experiments nor even simulation experiments, but for experiments that know where they are going.

Now, let me be specific. As I see it, of the things that were reported here, few bear any resemblance, and Dr. Boynton, I'm sorry to be beating on you, too -- few bear any resemblance to the practical problems involving what I might call "direct surveillance" or "reconnaissance" or "reconnaissance of literal visual displays". I think my friends in the PI business have a legitimate reason to say that nothing that has been said here has much to do with the picture. I don't see the relationship between struni-forms and nothing gones, which are perhaps most like photographs, and reality. I certainly don't see the relationship between these complex type figures and the kinds of things that I see in a photograph or from the air. So, my plea is that, insofar as we are trying to solve -- and most of you are, I realize -- problems of PI or problems of direct visual surveillance, we've not doing the right kinds of experiments.

In my opinion it's quite unlikely that information theory is going to be very much help. I just don't believe we're ever going to get anywhere trying to talk about these classes of figures. If you sit back and sort of review in your minds the pictures we've had on the board, you find that by and large, we're talking about matrices, the construction of these by one or another set of rules, the examination of complexity and what have you, as variables in these matrices. I insist that I don't think this has got anything to do with photography, or anything to do with looking out of the window of an aircraft and trying to find the target on the ground.

With respect to visual displays of a non-literal sort I have not so much confidence. It may very well be that the attack that's being made across the country represents a very good attack on problems of visual display of a non-literal sort where one can code and use principles of this sort, if you will, as the basis for coding. I think honestly, and here I am with Tanner with a sort of belligerent honesty, that what we think we're doing is very important.

I don't think we can expect the military to support theoretical research unless they see that it's going in the direction that they're going. Maybe, then, what we need is to make this statement: most of the work we've talked about here hasn't got anything to do with photography and hasn't got anything to do with looking out of the window of an aircraft, but it does have to do with visual displays. In this case, the title of the conference might have been changed to "Visual Displays". I think we need to do something about these other cases which I think are still of importance.

Let me make one last comment which came out of the interesting session we had last night. The photographic people made the remark that the last thing they needed was some more methods of making things harder to see. They can see all kinds of ways to degrade images and make detection-recognition more difficult, but what they wanted to know was how you can improve it. Our argument has to be that once we understand, then we can improve it. My only question is: Is this the most direct way, in all honesty, to improve the problem of PI or of looking out of the window of an aircraft? I think probably it isn't. It seems to me there is a real place for this kind of research, though, in an area like visual displays where we cannot predict what form things are going to have ten years hence, but I have a feeling a tank is going to look like a tank ten years from now. If there's any problem in photography or in direct vision we should attack that one in a more direct manner.

GERATHEWOHL: I am in favor of Colonel Debons's proposition because I think that a lot of very valuable research is being done. All the basic research that's going on in this field is not worthless. On the contrary, I think it has more or less application, but I think there is a necessity for the same people who really pin down what came from the basic research to put it into wording and into practice that the engineer can do something with. There was, if I am not wrong, a research project in the

Air Force with the title "Why are Scientific Publications in the Air Force not Being Utilized" or something like that. If I am not wrong, my friend Dr. Haber who is now up at some aircraft company spent almost a year of research in order to show why the tremendous amount of work that is done in the Air Force is not being utilized by engineers. So, I think something should be done about this problem. I think if the Vision Committee is supposed to have a practical task and a practical purpose one would be that it establish an organization something like a liaison committee, or whatever it is. It should be made up of some experts who have experience with basic research and connections with universities, on the one side, and experts familiar with the practical applications, on the other side. The committee would translate or scrutinize literature to see what is going on and what has direct application for the Air Force. It would bring this out and put it into right terminology and channel it to the people who need it. I think this is about the same thought Colonel Debons had.

DEBONS: I might just add to what Dr. Gerathewohl said. I find that psychologists, themselves, are inclined to say that we don't know what the answer to this or that problem is. But, if they did go back in the literature and competently interpreted some of the findings that are now available bearing on the problems that we are encountering, for example, in radar they might find the answers without any effort, without any research. There isn't enough integration of our past information on the present problem.

SLEIGHT: If people would write and speak in a language which at least their colleagues can understand, it would enhance the probability that non-scientists could understand it, too.

GILLESPIE: Let's move on, now, to Dr. Blackwell's statement.

FRICK: I thought I was going to be argumentative about Blackwell's statement, but then he managed to put in a few qualifying clauses. I would like to question just a little bit some of the discussion that's gone on about applied research and whether we are, in fact, improving radar sets or radar operation. In particular, let's consider one aspect of this PI problem in which I am not an expert. It does seem to me that we've looked a little bit ahead into the future of what the PI problem is going to be. Let's assume Mr. Eisenhower's open sky policy or something of the sort. We now have visions of large numbers of aircraft flying over incredible numbers of miles, taking pictures frantically as they go along. Do a little computing. You start ending up with 27,000 miles of 35 millimeter film or something on your head every 24 hours. What is the difficulty in processing this or the difficulty in handling it? I think the problem is not the one of recognizing the seal on the rock, which you mentioned earlier as being one which you thought we had gotten some place on. The problem is, in fact, pattern recognition or form discrimination. We can and do have machines of one sort or another that will do about as good a job, or even better job, of detection than the human operator under any conditions that I know of; given one thing, that the machines know what to look for. In short, they must know what the shape of the signal is or what is coming in or what went into the printing. We don't quite know how, although some of the more dewy-eyed engineers have gotten, or sort of bought, the notion that what we don't really know how to do is to recognize patterns. We haven't the slightest idea, really, of how to go about this. I would say that a large part of the work that's been discussed here is sort of nibbling at the answers to this problem. I would also say that this is the most practical, "the most close" to applied military interests, of any of the things that have been discussed, including some of the ones related directly to radar sets. Dr. Gerathewohl's discussion this morning of "let us get back to the radar set" is fine, but while we're busy getting back to the radar set the military is busy getting away from the radar set. In the next several years, at least in the radar sets of this country, nobody will see any raw radar data. What they will, in fact, see are random dot patterns of exactly the kind that you have seen here. They look very much like them. There's no particular problem of contrast. There's no particular problem except to make sense; namely, to recognize patterns of these things. This, I would say, is the critical applied problem that much of the work here relates to. It may look superficially as if it were really in the blue sky and the psychologists were, as usual, on pecking behavior in chickens when they were really interested in the education of small children and had gotten off the beam, somehow. I think that is not the case. I would say the closest thing to the real and true military problems has been just exactly some of the pet work that has been going on here in the artificial situations, à la Dr. Boynton, and à la the random, miserable random, dots and nonsensical figures.

RABBEN: I'd like to say first of all that I'm rather surprised to find myself on Dr. Blackwell's side with respect to his last remarks. Second, I am not a psychologist and perhaps, therefore, not even a scientist. But back to PI: one of the things that impressed me most was something that Dr. Crook mentioned in one of his digressions: it's often profitable to consider how the subject gets his results rather than seeing what the result is that's obtained. I would be an awful lot happier if the field of experience of psychologists were generally applied to how a PI does his PI, rather than to what configuration of these random forms, as such, is apt to appear. One instance occurs to me, if we consider what a PI does have to do. He's faced primarily with two problems: in one case, he's looking for specific items; in another case he's looking for an item which may or may not be significant. In the first case, if he's examining a large area, he very quickly eliminates most of that area if he knows what his specific item should be. In the second case, he has to consider the very large area for anything of interest. What I would like to see is a good deal more attention paid to how he goes about doing this. How can he improve it?

STILSON: With respect to this conflict: perhaps the adequate theory of form discrimination would tell us how these things take place; that is, how PI's operate on those dots, make the decisions and observe things. Where does an adequate theory come from? I think we could go right back to the stimulus problem as far as form discrimination is concerned. That is, if we base a theory on a representative or an adequate range of stimuli, presumably the theory would include those applied situations which Dr. Blackwell is proposing to investigate. I think that if we construct the theory on an adequate stimulus range we will have something which will apply to your applied situation as well as to our artificial random dot matrices and so forth.

GERATHEWOHL: I would like to make a remark to Dr. Frick's comments. As far as form discrimination is concerned in radar, of course, your standpoint is right. But, as far as radar is concerned, as you talked about the development of radar sets in the future, I think your standpoint must be very limited. Let's not kid ourselves. The radar purpose is not just to jot down and discriminate some forms. We have air-to-air radar. We have sound set radar. We have air-to-ground radar. Only in the air-to-ground radar does form discrimination come in. This is part of the whole radar problem where detection of targets is very significant. We have to rely on the SAGE system. The whole defense of the United States depends upon just the blip; namely, the enemy aircraft picked up very early. This involves other variables: contrast, brightness, and all the other things, not just form discrimination. Form discrimination is only one conception of one problem in radar in military work.

FRICK: If it is a fact that people are not going to be doing any of this, then it doesn't matter a bit about the contrast of one thing or another because these are all automatic detection schemes. The point I would make about this is that we're increasingly using automatic detection schemes for most of this work and the trend seems to be very much this way. In the experiments we have done, and Lord knows who else has done, the machine is better than the operator on most occasions, given optimal targets, optimal contrast and so on. So, this is the trend and I, as a psychologist, am perfectly happy about it. The pattern business is still with us at a slightly different level. Somebody is going to have to evaluate the air situation no matter how it is done. This is essentially a matter of pattern discrimination.

HARKER: The problem I want to present is closely related to the discussion Dr. Blackwell started and Dr. Frick carried along. I was reminded by the problem presented by Dr. Bluford of the Signal Corps Laboratories of a problem which I believe has been with us in the Navy in the form of sonar; in the Army in the form of mine detectors. It goes, I think, to the core of the form discrimination problem. To wit, we get signals from an object in the environment and these signals are such deciding variables, but we are at a loss to prescribe or delimit the categories or the details of invariance which are amongst the signals.

I'd like to put this in a concrete situation, thinking in terms of a problem of high priority. The source of radiation from a tank is the muffler. Mufflers are carried by all vehicles. Our concern is to discriminate one vehicle from another. Mufflers may appear in all aspects, so at this point you may have a terrific variety of stimulation coming in. It is not any stimulation to which we are accustomed in the optical or normal environment. I'd say it is only a signal that the physicist or the engineer is receiving on his lead sulphide plate. Now, he has no methods at his disposal in terms of the physical sciences to establish the invariance of the incoming signal. If he did, he'd immediately go to detecting. So he turns to the human. He says, "Can you detect it?" He does more than that; he says, "Can I put it in the optimum form for you to detect?" And so he says, "Shall I make it a color display? Shall I make it a temperature display of a known type? Shall I make it an FM display?" It goes on interminably. At this point we can build mockups with various figures which have been stated or we can go into facsimile studies and the like. What I am trying to say is that the real problem is: How does the human establish invariance or delimit the stimuli he is receiving so that he can operate within a set of stimulus categories? For instance, in the discussion here each individual started his study by saying, "I made out my stimulus matrix this way." What did he do? He chopped a hole out of a great big population and then he started talking about forms within those limits. Well, how does the human, faced with the display which finally gets to him, chop a hole out of what has been presented to him? Having chopped a hole, we're back in psychology again. So I think we have missed one of the most significant problems for the Services with respect to the human being. How does he behave to delimit or to pull out of the highly variable stimuli that he receives certain stimulus invariances to delimit the stimuli to which he is going to respond?

ARNOULT: I would like to say in partial reply that I assume, at least in my work and I think in a lot of other people's, that that is really what we are trying to do. We are trying to find out what the significant parameters of all possible shapes are so that we can then talk about all possible shapes and forms. In other words, when we know them we will be in a position to answer the harder questions, but before that we have to start with what we can handle and build up little by little. I wish we could do it all at once. I'm impatient too, but I feel I can only handle a little bit at a time. It's sometimes pretty discouraging even when you take just a little bit. You find that that's frequently more than you can handle and you wonder how you're ever going to handle any more. But I don't know any better way to do it.

BLACKWELL: I'll just be short. Like everything that's been said I agree with you 100 percent. Yesterday in my paper, Dr. Crook, I made the statement that one thing I could do well with vision was identify, better than with the other senses. I think the problem, as I was trying to state it, is that because of the parameters understanding is so difficult that I can't see it in five years or ten years. I'm recommending that we decide what our frame of discrimination is in this particular problem and go at that particular problem. As I was saying, in effect we're spread too thin. We'll never know all about perception in five years. Let's be a little more direct. At least in the area we're in, we're not being very direct. Maybe you are getting the best answers to radar. I don't think we're doing it in aerial photography.

BERSH: Frankly, at this stage of the game I don't know whom I am answering or with whom I'm in agreement or disagreement, but I have a number of points to make primarily with respect to the interpretation problem. Since my familiarity is greatest with photographic interpretations I will restrict myself to that kind of consideration. Let's consider a data processing system such as the intelligence people are interested in. The goal of the system is finishing puzzles. The end product is derived from bits of information which at some stage of the process are simplified or synthesized, rather. The human comes into play in an actual system of this type at several stages of the game; at least he can come into play at several stages of the game. He can come in as a screening element for more skilled individuals. He comes in most importantly as an information extraction element if the material is pictorial. At the moment we must depend upon the human being to extract the necessary information from that pictorial element. Finally, he comes into the picture at the synthesis end, after he has extracted target information. By that I mean that the targets are on the photographs which were taken over such and such an area and sooner or later we must present them to individuals who must decide how they fit together and what picture they present. Now, whether we like it or not, the time we have available for utilizing certain kinds of intelligence is decreasing very rapidly. Certainly when we are talking about strategic warning information the time element is becoming very critical. The system must operate not only accurately but rapidly, very rapidly. Whether we like it or not, the bottleneck in the system is the human.

A machine has been developed which will code, index, sort, store, and retrieve with considerable efficiency and considerable rapidity. But in the middle of this system there is a human being who has to take a certain amount of time to extract vital information and at the end of the system there is a human being who has to take a certain amount of time, perhaps even more than the extractor, to put the information together. We want to do whatever we can to help these individuals. At the same time we want to do whatever we can to reduce the role of the individual and the time bottleneck in the system. We need him. We've got to go along with him. We've got to do what we can to produce his optimum role in the system. This means that we have to be concerned with time factors. Whether we like it or not we have to be concerned with time factors in form discrimination. By time factors I'm referring not only to such factors as how quickly he can recognize and make a single response, but the rates at which he can do work of this type over long periods of time.

This brings up the fatigue problem parenthetically. We must be concerned with these matters. For example, we have to decide how rapidly we can present information to him. Does he need all the time that he takes to extract information from a photograph or can we cut down on his time considerably by forcing him to make his responses more rapidly? Questions of this kind are extremely important. Until we answer questions as to what presentation rates are compatible with the degrees of accuracy we require in the system, we cannot make any wise decisions as to the rate at which we should shove the material through the photographic interpreter.

My remarks are somewhat scrambled, so now we'll take up the screening problem. Why are we interested in the screening problem? We're interested in it because the volumes of data are simply too massive for the number of individuals who are available to extract information from the data. We would like to do the screening process by machine and to some extent perhaps we can. For example, it is quite possible that a machine can separate completely useless pictures from pictures which are useful. Let us suppose that we have pictures which are completely cloud covered or that we've processed several below contrast threshold for form or even for detection. If we knew what values to crank into a machine, I'm sure we could create a device which would do the task for us. But if we can't make a machine, we have to go to humans once again. We may have to go to humans anyway, particularly if the information is from hot areas, because we cannot take the risk of losing some vital piece of information which might mean the difference between adequate warning in time and inadequate warning time. Well, this means that we must be interested in the problem of how the human being can screen, how he can make simple discriminations based primarily upon form recognition. That is, can he tell us, or how can he tell us, when a target is of military significance and when it is not? How quickly can he tell us, if he is forced to assume this role, whether there is anything on the photograph that is likely to be useful or whether the photograph is too degraded for interpretation or perhaps whether it has such a cloud cover that it could be shunted to the meteorological people but not to the photographic interpreter.

There are a number of other points I'd like to make. We are interested in the influence of degradation factors upon form discrimination. Whether we like it or not we are not going to be able to provide people with ideal representation of the picture on the ground. All of the sensors, even the photographic sensors, have limitations. We improve them as much as we can but after we are finished improving them they are still to some extent short of the ideal we would like to present to the photograph interpreter. So, we must be interested in such factors. We must know what the limiting values of the degradation factors are. We must know what kinds of judgments and what types of responses are comparable, given such kinds of degradation. Of course this also contributes to the rate problem. It is obvious that the rate at which you can shove material through the interpreter will vary depending on the quality of the material.

Another point I would like to make in connection with the same kind of problem is that there are many, many practical situations in which the form factor, while basic to all form recognition, is not of primary importance because there is nothing we can do about the character of the targets which are sensed by the sensor. The targets have a certain shape, they have certain configurations, and we hope that our sensors will represent them to us with maximum fidelity. They may not. There are, however, other things that we can do to increase the fidelity with which the sensor represents the information to the interpreter. We can improve the sensing system. We can decide, for example, what kind of camera is appropriate. Once we've gotten the material on the ground we can do certain things to the film material to improve its fidelity as far as the interpretation function is concerned. We can enhance contrast, or contour, or we can screen out detail. We should not, by the way, overlook such factors as working conditions. Such things as illumination and the setting in which she does her work are very important.

For those reasons, I say that while we are very much interested in the form factor in situations of this kind, we could set it to the side very often and concentrate on those variables which can actually be manipulated in an operational setting. This is extremely important!

I make one final point because, frankly, I consider myself a learning man among vision people and I want to thank Dr. Vanderplas and Dr. Boynton for bringing the learning variables into the situation. Obviously the stimulus factors are only half and sometimes less than half the problem. Obviously also, once we have done everything that we can to improve the situation, what we can do over and above that is going to be primarily in the learning area. We must be concerned with the learning factors, with the question of how we train an individual to recognize on the basis of minimal cues, and how we can improve recognition of distorted images. For example, a man in a low flying aircraft has got to learn when he is going to get a distorted image because of his position relative to the terrain. We must also be concerned with such problems as generalization and discrimination learning phenomena not merely as phenomena which set the situation for form discrimination on the stimulus end. Let me give you an illustration of some of the ways in which we can use learning procedures so that the learning considerations possibly help the photographic interpreter. If it is true that the photographic interpreter or the radar interpreter or the infra-red interpreter will have to be concerned with degraded images, perhaps such a thing as a photographic or reference key should not be a key which is an absolutely crystal clear representation of what the thing looks like ideally. Perhaps it should be a degraded representation which simulates what he actually will get in the way of a photograph. Perhaps we should make his task a pure matching task rather than a task of trying to determine which of many degraded pictures represents one that is perfectly crystal clear. Perhaps one of the things we might do in training is to show individuals, actually show them by some sort of motion picture or other procedure that might be applicable, how a clear picture is degraded as the aircraft gets higher in the air and moves faster or the reverse. We might show him how a degraded image develops into a clear image.

WHITE, C.: I understood that one of the things we were looking for here was points of agreement. I've counted three people who said exactly the same things and I think that's a darn good percentage. Dr. Frick, Dr. Bersh, and myself have all pointed to coding. So it leads back to symbology. Here is a situation in which we have complete control of the stimulus. In an area in which we can determine what's going to be given, we can do all of our learning studies in regard to them, and so on. So, I would like to repeat what I said in my beginning remarks: one definite thing that could be done by the Vision Committee would be to set up and establish a vocabulary, a symbology, for radar displays. In my first remarks yesterday, I coined probably a very poor term, "emergent form", and then yesterday afternoon I was very happy to see one of the darn things finally when Dr. Green gave his excellent movie.

ARNOULT: Could I resolve my conflicts this way? Maybe we could sort out the things that have been said here by considering that we have talked simultaneously about at least two fairly widely different kinds of interest. We have people here who are interested in the interpretation of radar displays, visual reconnaissance and photo interpretation. It seems to me these people quite probably should take Dr. Blackwell's advice and gear their research to perhaps a five or ten year time span because, as Dr. Frick pointed out, they're faced with engineering obsolescence. On the other hand, there are others of us here who are interested in form perception. It is our fine hope that the things we do will have meaning for form perception problems of two, three, five, or eight hundred years from now perhaps, or maybe even until such time as the human being is faced with obsolescence. Now, I think maybe a very proper question would be whether or not the Armed Forces should be interested in supporting that kind of research. There perhaps is the question that we could chew on.

TANNER: I want to point out that I'm very much in agreement with what some of you have said about the place of theory. If we have a theory that covers things well enough, we will solve the problem. There are two ways of looking at theory, however. One is that you could develop the theory which defines or refers to a human being, it says here. The other is that you could develop a theory which tells you how you could discriminate forms.

I think in the information theory point of view we have some concepts beginning to grow as to how you should make a choice between sets of alternatives. One of the first things we run into if we try to work on a problem such as this is that you first ask yourself the question: What am I going to try to look for here? You find that it is usually easier to handle if you are going to look only for a finite number of things rather than an infinite number. I am not sure that we need to know what all possible forms are at all. I think that George Miller has some very interesting comments on this, though I'm not sure that I understand them all. The magic number "seven" business that he comes up with shows, I think, that this gets back to Dr. Harker's comments: How do you cut a hole in the person's experience? I think we always cut holes in it. We just wouldn't have time to make a choice between this and the business of the population of all forms.

As far as the military is concerned, I think that one of the things here is that in the training procedures you might show the people how to cut holes. You can get better performance if you do show them how.

The other thing that I'd like to point out is that if you develop an ideal theory, then you can learn how human beings behave by comparing behavior, as you say here, to the ideal theory. You know that if they perform equivalently to the ideal, then they must perform in an equivalent manner to each other. This much you can find out. If they don't perform equivalent to the ideal, by comparing the difference between their performance and the ideal you can draw further conclusions. I think we should be looking at two problems. One is how the human beings discriminate form and the other is how to discriminate form optimally.

GRAHAM: At the risk of seeming irrelevant, I'd like to say a couple of words about Dr. Arnoult's next eight hundred years seen in the context of present psychology and the general context it offers to the discussion we've had in the last few days. I did mention, I think, in my little ten-minute talk yesterday that the field of form perception involves a great number of psychological topics: form perception itself and all of its complexities, space perception as "proprietary" to this area, the area of illusions, figure on ground, grouping of units, minimal cues, structuring, figural after-effects, interaction of the senses. Another one I picked up in the course of the discussion is "concept formation", which I hadn't included yesterday. Well, it turns out now that as I check off these topics in terms of what has been discussed during the session I find that, in fact, everything has been touched on in this discussion: form perception, space perception, illusions, figure on ground, grouping of units, minimal cues, structuring. "Figural after-effect", I guess, is the only one that, so far as I know, has not been discussed at all. "Interaction of senses" has probably not had discussion either, although I suppose that certain problems that have been taken up might be considered by extension to fall within this area. Even concept formation has been touched upon. Running throughout all these topics and the experiments connected with all of them have been the general threads, the dominant threads, of perceptual analysis in terms of emphasis on stimulus factors and emphasis on past history factors, as seen, for example, in the paper by Dr. Vanderplas.

From a sort of meta-theoretical point of view or pre-theoretical point of view, the discussions have been concerned with such attitudinal aspects of scientific analysis as attitudes or the general Einstellung and the empty organism approach as opposed, for example, to the physiological approach or, as opposed from another point of view, to the phenomenological approach.

Under any of these one may consider, too, all the related theoretical sub-aspects. For example, information theory is a representational device. I suppose it can be applied within the general context of the empty organism notion and possibly, if you want to consider events or probability of events, within the physiological context. Then the physiological approach might be related to information theory. It is possible for us to consider other representational devices such as physiological mechanisms or models or mathematical models or what not. All of these we've heard something about in the course of the discussion today.

More practically, we've heard a good deal, too, about techniques for perceptual study, nonsense forms. I've heard a little of these matters previously, but haven't known much about them. Then, of course, there's the device. As Samuel Johnson once said, "Patriotism is the last refuge of a scoundrel", so, I have sometimes felt that scaling is the last refuge of a psychological scoundrel. I don't really mean that. The essential thing, it seems to me, is that sometimes we have to do it. We should always keep in mind what we're doing. What we are doing, of course, is getting a subject's discrimination, taking the critical values that depend on that discrimination and correlating them with some sort of response that we've heard. Now this is, I suppose, a very practical and sometimes useful device, but it is not always a final device. In any case, I certainly believe that at this stage of events scaling forms, nonsense forms and what not, may be as useful as using the physical characteristics of the form to specify the characteristics of stimuli. Again we come back to the old problem of SR relations as opposed to RR relations. In the case of the physical characteristics of forms and the dependence of responses on them we have the well-known SR relation, whereas in the problem of the scaling of forms and the resulting responses dependent on the scale measures, we have the RR relations which need not be discussed further. We have a pretty good notion of all of the implications and the consequences of these procedures.

I do think that one thing might be said about the psychological aspects of the discussion today and yesterday, too. It concerns a matter which is very dear to my heart: that is, the fact that the general term perception is a class term. There is no such thing, I think, from my point of view, as a perception or as the perception. We have to remember, for example, that the kind of correlations we get when we give absolute judgments, when we give verbal quantitative responses, may be entirely different from the kinds of responses and the SR correlations we get when we use a typical psychophysical procedure as, for example, in the case of the method of constant stimuli. My statement that a man is six feet tall isn't the same sort of datum as the measurement of a man against a meter stick. I think it is necessary in the analysis of perceptual data to be very tedious about this matter. It seems to me go through the whole gamut of all of the possible correlations and the general class of correlations connected with each other, directly or indirectly, easily visible or invisible, that fall under the general category of perception. It is so easy to talk about my perception of that man as six feet tall when I say, "What do you mean by your perception?" In one case you had a meter stick. In the other case you haven't had a meter stick. What kind of response does one see? What kind of phenomenon does one see in the dark? Moving spot? The autokinetic. The subject says it moved an inch. Did it move an inch? Well, I don't know. I don't know that that absolute judgment has quantitative significance. Certainly we do have an object here from an infinity of objects and we have to consider for that particular subject, I think, the general context of that population of objects. Well, this kind of problem is something I should like to emphasize as a personal opinion concerning the general study of percepion, but I think that it probably turns out methodologically that many of the studies taken up here and discussed in the last two days do not run into this problem. I think it is so important that probably we can consider it.

A number of specific problems might come up that might be of some interest in this general connection. For example, what is the difference between recognition and identification? Here again we have, I think, the need to specify the general class of correlations involved. In the case of recognition, we have essentially a discrimination as between two possible forms of response; Yes and No; it exists or it doesn't exist; I see him, I don't see him. In the case of identification you have to do more than this. You have to name the object from a population of many names or of many nouns or whatever you want to call them.

Search procedures, at another, different level of discourse, involve all kinds of interactions, it seems to me, between temporally contiguous chains of discriminations and possible interactions among the senses.

I could talk, too, about a number of specific topics. One that I hinted at yesterday is the time factor and the temporal threshold and its systematic importance or relation to so-called sensory problems, perceptual problems, and what not. This I think should be explicated much better. The general topic should be explicated much better than it has in the past, but I am not going to go into it now.

One final practical topic arises in connection with this two-day discussion. It seems in a period of not more than, I guess, six or seven years suddenly the field of form perception has bloomed and those of us who haven't had too close contact with it during this time are, I hope, informed. I feel that I am, and I can assure you that I think I have learned a lot and will learn more when the report of this meeting comes out.

Why this sudden increment of knowledge that has come in such a short time? Well, for one thing, I think the need is apparent: military. It's also apparent to me just from the theoretical point of view. Five or six years ago I wrote a chapter on form perception for a book that has not yet appeared. I hope it will appear some day. That chapter is now completely out of date, of course. Why has it been possible in this period of time to gain such information, valuable possibly not from the military point of view, although I hope so, but certainly valuable to my understanding of theoretical psychology? Well, for one thing, it may be that there are a lot more full-time psychologists working in institutions under circumstances where they don't have to devote time to teaching, administration, and the million and one other things that come up in the university situation. If these people cannot do better in extending the general knowledge of these topics, then I think there is something wrong. As a matter of fact, of course, they do in a much shorter period, now, extend the general boundaries of the topic farther than was ever the case when the general theoretical topics of psychology depended upon the restricted contributions made in university laboratories. I hope the university laboratories will still contribute and guide research to a certain degree at least to the extent of giving certain attitudes to graduate students, but I think that in terms of piling up data and contributing rapidly the full-time research man is certainly in a better position than the university people are now.

GILLESPIE: Dr. Morris do you have anything to say to someone?

MORRIS: I couldn't say a word after that.

GILLESPIE: Some brave individual back here can, I believe.

GREEN: There's one point that you probably all recognize but to my knowledge has not yet been stated at these meetings. Form perception is not basically a visual problem anymore than it is basically an auditory problem or, indeed, a tactual problem. It appears in all of these fields. Pattern recognition occurs in speech recognition in audition and there are form perception problems in the tactual senses, too. I remember an experiment that I had fun with a few years ago in which the subject's problem was to discriminate by feel among a set of switch forms, switch handles. The interesting thing to me, which is kind of obvious once you've thought about it, was their having been able to identify these perfectly well tactually without ever seeing them before. I then showed handles, one at a time, and they could recognize which was which without ever having seen them previously. Essentially the point is an obvious one: form perception has been discussed here more or less as an accident because we are all interested in vision. The data have to come in to one sense or another so it might as well be vision, but from a theoretical point of view form discrimination transcends this particular sense.

SMITH, O.: Briefly I have six points to make. First, I'd like to pat Dr. Bersh on the back, primarily because I think the problem could be stated in quite another way. What are the conditions necessary so that problem-solving can be reduced to a perceptual level? This, posed in quite a different context, also has to do with immediate versus delayed decision making. Now, from the discussions of form which have taken place here, it is clear that this decision has not always been made with respect to the data under consideration. I think this is very important because the use it's being put to specifies the conditions under which these decisions need be made. You have to have temporal limits. Unless you have temporal limits, you can't satisfactorily discriminate, for practice or for theory, what you're dealing with.

Now the second problem has to do with the use of noise. I have a rather biased notion of what may constitute noise. Noise may come in on a channel and to me it's a variable element which does not remain static and does not remain constant. You can give it a mean value over time but it's unfair to assume that it's constant over time. Consequently, there's a big discrepancy between the use of noise as "the classicial information theorists" might conceive of it and the way it's customarily in use in this session. First of all, let us consider any stimulus. It doesn't make any difference what the stimulus is, but if you are involved in a notion of variable noise, then what do you have? You do not have a static situation. You have a condition under which your noise fluctuates at various intensities over the entire range. You have a condition in which your noise intercepts, cuts out, and reveals the stimulus at different times. You don't have a static pattern. You have a problem in motion. This has been a problem which has been significantly omitted in this session. I think that if you're going to get down to brass tacks and deal with theory which takes into account practical behavior, discrimination, identification and recognition, and what have you, you're going to have to change your techniques of investigation because the techniques as I see them in a static situation involve problem-solving in a strict set of circumstances.

Third, Dr. Vanderplas indicated that he is interested in variables of the observer. Now, there's a whole wide area of information which is being neglected by this group. If you're interested in completion types of problems wherein you may want an individual to complete a form, or if you want to deal with Gestalt figures or buried figures, or hidden figures, you're dealing with a type of problem which has been used consistently in psychometric procedures for testing. We have lots of very nice factors. We have community testing for these factors. We have notions on intercorrelations that are not of a high order. Operationally, what does this mean? It means simply that if you scale you'll find some people low on one and high on other. This is the operational definition of a low correlation. If this is the case, is it not likely to be the case in the investigation of these different types of factors? If you're going to make valid conclusions, what are you going to have to do? You're going to do what the sociologists have done for a long time. Stratify your sample, and then go ahead and make your testing to get your results. Now, with this stratified sample, under these conditions, what do you expect? Here is what you should get. You should get a principle of learning which is effective, but differentially so, according to the population which is using it. So far, we've been dealing with two, three, four, five people and not including the totality. If you're interested in the all-over population which are going to use your data, for application that is, then you're going to have to specify the effects which are differential. We have these techniques now and are trying to use them.

Now, let's look at the fourth problem and the thing which is dearest to my heart and also to Jimmy Gibson's heart for the moment. I believe you've neglected the study of motion, that is, motion transformations as a problem in terms of identification of forms. There are an incredible number of variables here which exist for the human operator for recognition, location, identification, and these all stem from the fact that objects are transposed and they give different patterns of projection depending upon the point of view from which they're seen. You may have an object which is easily identifiable by experienced characteristics of its form. But these, in turn, may not be the type of characteristics of form which are represented from all points of view. Now let us go back to the flying situation in which you have a man flying under visual conditions, searching the ground for purposes of orientation, and navigation. Here's primarily a problem of identification and form transformation with which he may or may not be familiar. Until theory and fact have been studied and imbedded into this area, we are certainly going to have trouble with lots of types of problems.

Now, the fifth point: I see no reason why psychologists can't pave the way at the same time as attempting to solve theoretical problems simply by investigating forms which people do have to identify. They need not be nonsense forms. You can have a satisfactory basis for prediction. You can satisfy problems, and, if you're good enough, you ought to be able to get some general situations which would in turn be of theoretical importance to the larger classes of forms to which we want to generalize. There's no immediate necessity for reducing forms to the nonsense level although that may have high practical value in terms of learning and various other problems. So, if you're interested in paying your way as a psychologist I do suggest that you go ahead and investigate some of these forms which people find of practical significance for identification and set up your problems in such a manner that they may prove to be of value to theory as well as fact. This just seems to be a common sense point of view of an industrial psychologist.

I hear the word Brunswik mentioned and I think of my friend Julian Hochberg's paper because much of what he had to say was on this topic.

The sixth problem has to do with the source of the visual input. Now, from my point of view I don't care where it comes from as long as you can make some assumptions about how it's used. We've demonstrated this quite adequately by various kinds of motor responses so that now, with respect to Dr. Bersh's problem as to how much should be included within an image, I dare say that we can specify the conditions of stimulation for which certain classes of acts can be made. Both are of a theoretical scaling nature so that you are saying that "A" is further away than "B", or making a fractionation judgment of broader responses, or other types of inductions which identify objects in space. These can be generated entirely independent of any assumption about where these things come from, as long as we can account for the relationship of the observer to the plane in which these figures are being displayed, and as long as we can control the scale of the projection. The reason the scale of the projection is so important is because if you don't control it, you're going to get responses which are inconsistent with already well-defined learning habits, and then you're going to get an awful lot of interference effects. You should keep your scale consistent with what we know about our ordinary world so that all of our research which has had to do with motor performance with respect to identification immediately transfers. It's a simple setup.

ARNOULT: I think this might be my last remark. It seems to me there's one remaining point that perhaps deserves some clarification. I'm very much afraid that some people here may have perceived part of the discussion that has taken place in the last two days to have been in the form of a controversy between two kinds of people: those who promote neurophysiological explanation and those who promote what might be called SR or empty organism types of approaches. I don't see any such controversy from where I stand, frankly. If there were one, I would be lined up firmly on the side of the neurophysiologist. A few minutes ago Dr. Graham said that scaling techniques seldom provide final answers. I would like to extend that and say that I sincerely trust that we will never have to accept such a final answer. If we want ultimate answers, I'm with the neurophysiologist. However, when I'm dealing with complex processes such as form perception I find myself in the difficult position of not knowing where to start unless I first get some organization of my material in the form of specifying the stimulus-response relationships that I'm going to work with. This is why I'm currently working along this line. Now, I share with Dr. Blackwell the desire to crawl inside my own head and see what's going on there, but I have a difficulty and that is that in the particular problems I'm working on I can't find out any way to start without first putting my foot in my mouth.

BLACKWELL: This is my last remark, too. I agree with Dr. Arnoult that there really isn't any type of controversy and never was one. Apparently we all agree as to what all the misunderstanding will be and we're simply quibbling about what you do first from the present vantage point. Again Dr. Arnoult has said that there are two kinds of people here with respect to their method of starting. Perhaps there are two kinds of people here with respect to what they are going to do next, what their particular problem is and I refer back to my earlier remark that it seems to me that the problem of displays can be controlled and it's a problem that's being well handled. The problems that worry me happen to be a different class -- those where we take what we've got. I thoroughly agree with the remarks everybody has made. I have no one to argue with. It seems to me now quite clear that if we are to solve problems involving real objects which are going to stay like that we have to go at them probably as they are and not convert them into something like forms. My only point was that I think we know enough in certain areas to use some of our "neurophysiological" inferences and my plea yesterday was that we try it in these areas rather than just give it up and resort to the last refuge of scoundrels, as Clancy Graham said of Sam Johnson, in the form of scaling or simple stimulus definitions. It seems to me we have three levels here, RR is the worst, SR is the next worst, PR (physiological response) is the fact, but different areas occur to one and one can use the best one available to him.

TANNER: All this suggests somehow that we thought that we might come out with a valid theory. The one thing I think I'm really convinced of as far as theory is concerned is that the concept of validity is invalid.

FRICK: I think it's too bad to end up as a fine a meeting as this is on quite such a note of sweetness and light. I would like to say that real objects don't stay like that. I think that is what has been meant by discussion about how nice it would be to know more about movement and the time factor and so on.

My other remark is directed essentially at two places; at Dr. Arnoult and the eight hundred years before we know about these things and to reenforce Dr. Green's suggestion that there are other people interested, too, and that these things are not confined to vision. It goes like this, and it all ties in together: about two years ago, two or three mathematicians programmed a computer to recognize simple figures. It didn't do a very good job. At 60,000 operations a second it took 15 minutes to tell the difference between an "O" and an "A". This they knew before, but the interesting thing is that this ties up with the fact that real objects don't really stay like that. This experiment was interesting because there was no stimulus invariance in the input. In short, what I think I'm saying here is that we do not necessarily require the stimulus invariance that has been suggested in order to get at the recognition problem.

My final exhortation on this is that since this is primarily a group that's interested in vision and optics, I would suggest that sometime members of this group who are interested in form perception spend some time looking at the Journal of the Acoustical Society where for the past five years there have been from one to two to three articles on pattern recognition, each year. You may assume that there was not single stimulus invariance around but I think there was! These people have been doing a lot of work on pattern recognition which they call speech recognition, and it looks very much like object recognition. I think that sooner or later the two are going to get married and not in terms of Dr. Arnoult's eight hundred years or maybe one hundred years. I think it's going to happen pretty quickly, and I think it would be a very good idea, if it is going to happen quickly, that we start recognizing the real agreement in interest and problems between acoustic and, what's the word? (somebody supplied the word "visionary") - visionary ones.

WEISZ: Let me add one very short, positive dogmatic pollyanna note here. If you look at the literature in form perception up till very recently you'll be astounded by how much theory there is and how few forms ever got experimented with. I'm not sure that this point has been made yet today. I'll go over it again. Any investigation that deals with forms, investigates classes and varieties of forms and drags out some experimental data rather than theory, cannot help but reflect some of the theoretical framework within which it is done.

SLEIGHT: Bit by bit, I have been getting concerned with the kind of thing that I frequently hear as a summary comment on a meeting such as this. "We're not working on the right problem." It seems to me that there is possibly a scientific approach to selection of the correct problem. Customarily, I believe, the selection is done now through one research project suggesting something interesting and then you go from there to the other approaches. But the feedback is very indirect from the military services who when in the field realize that there is an inadequacy of information and that perhaps the scientists can help them. What I would like to suggest is that there first of all is what we might call a human felt need. This may be self-preservation or it may be comfort. Let's go from there to our scientific planning, and this is what the operations research people are doing to some extent. Let's go to what we might call "evaluation of the felt needs through criteria". When I mention this word "criterion" some people will see different colors, but anyway you always have to have the problem of validating criteria. From validating criteria -- with validative criteria, I mean, you can go to a statement of the problem. If we took an independent poll of all the people here and asked them to list the five problems that they think are most outstanding that someone besides themselves is investigating, I think we would get a terrific scattering of problems. Well, anyway, if we checked we might find some consistent attempt to validate. If we state our problem, we can then go from the definition of the problem to what might be, in action terms, statement of the project and then this would be financed by someone and the work would proceed.

Then, there is still one other aspect on which it would be nice if we could have a consensus, and that would be the appropriate methods to be utilized. Among the primary methods, gross methods, obviously would be utilization of the existing data of the literature. Then there would be experimentation and I don't underestimate the significance of opinion as a kind of experimental information. Thirdly, there would be the approach of mathematical models. All of this now will lead to some kind of joint, valid conclusions that go into action. OK, this sounds like a lot of work for a simple proposition, but I feel that we frequently leave this aspect here as an incidental thing to be dreamt up by people who may or may not be qualified to do it.

BOYNTON: I think I have something to say now. I agree with Dr. Sleight to the extent that some kind of organization needs to be present in structuring the research that people do. But I think he is overlooking one extremely important point and that is the factor of motivation on the part of the people who are doing research. Research can't be ordered the way you order tanks, or food, or belts or other equipment. I think that it would be true that if you went back to the past history of research accomplished not only in psychology but other fields you'd find that the kind of research that is done by an individual or a small group of people where one problem leads to another perhaps has made the most significant contribution over all, to our knowledge of the world about us. I think in order to do good research a man must be awfully involved in it, not be just interested in it. A lot of it has got to come from his own thinking. In short, it's got to be the kind of stuff that keeps you awake nights when you can't go to sleep trying to figure out the answer to some bewildering experimental or theoretical problem. I think that to the extent that one tries to direct, too much, the research that other people have to be doing some of the spark that I think comes out of this kind of ego-involvement goes down the drain.

DEBONS: I have a special request here. Dr. Vanderplas was my boss for about three years and during that time he was a source of inspiration in the attack on this problem. I was wondering if he would say a few words, not in the role he is playing now, but, rather in terms of his past experiences.

VANDERPLAS: I'd rather comment entirely in the role I'm playing now. I've forgotten everything I learned at the Aero Medical Lab. I'd like to make two comments both of which are related to the problem of the development of theory of form recognition. I think it's been made fairly clear already but I'd like to emphasize the fact that the development of theories, as recognition theory, might depend upon two things which should be considered independent. First is the development of theories which specify the parameters of form or shape or pattern which are independent of the representational character of the form; that is, in terms of what I think Dr. Blackwell is calling "literalness of form". Second is the specification of parameters of the literalness of the form, i.e., the meaning independently of the intrinsic character of the form as a stimulus. And I think it's trite to say that we need theories which also develop relationships between these. I asked Dr. Blackwell when he sat down -- and I'm glad he's made his last remark -- I asked him what a tank looks like and he said that it looks like a tank. I think in terms of theory that what one should say is: What are the relevant parameters of the shape of the tank, disregarding the fact that it is a tank, which are related to recognition and what influence does the fact that it is a tank -- and these are observer variables -- have on its recognizability?

Another point that just occurred to me, which I've written down, has to do with the processing of information. One of the things a human can do that a machine cannot is to adopt an erroneous assumption, use faulty logic, and reach a correct conclusion. Physicians do this all the time. Detectives do the same thing in determining who killed him, and I think scientists develop most of their theories on the same basis. They do it on grounds of intuition and inference, and I think we develop theories on bases which most scientists would violently disagree with. They adopt a most likely hypothesis, find the facts and then test it. I think what we need are theories which define the methods of information processing. I think information theory is one of these. There are probably others which no one has even thought about. These may depend on visual detection. They may depend on recognition and identification. There are three hierarchies of level of performance.

I think if the Vision Committee is interested in the total problem we may find it to be visionary and we may be able to call it a "Thinking Committee" rather than just a "Vision Committee".

ENVOI

It is not the intent, nor indeed the proper function of the editors to attempt to summarize or evaluate the symposium. The reader of these proceedings will have, we hope, already formed his own impressions of its worth and drawn his own conclusions regarding the state of the art insofar as it is here represented. From the admittedly privileged vantage point of the editorial chair, and with benefit of hindsight, it may, nonetheless, be permissible to add a few observations of our own.

First, we think it safe to say that we discovered that more research on form discrimination is under way than any of us would have guessed. In addition, it was most encouraging and stimulating to find that such a wide variety of experimental and conceptual approaches had been developed; the lively interchanges between proponents of various approaches fostered, in our opinion, somewhat better cross-disciplinary communication than may have existed before the symposium took place.

Second, in light of the present and anticipated rapid development of more and more effective sensing devices, it would seem that one of the most critical military visual problems is, in fact, form discrimination. Even though considerable research is going on, more will be required.

Third, although it may seem as though most significant aspects of the problem that we presently recognize are under formal study, or at least under informal consideration, some of the most critical aspects are receiving only slight or no attention. As an example: from the point of view of military operational requirements it would seem that considerably more emphasis should be placed upon recognition and identification of forms in dynamic situations -- that is, in situations in which the target or the observer, or both, are moving, with the result that the target is undergoing continuous alteration of form. As a further example, from the general experimental viewpoint, critical problems still seem to lie in the area of stimulus generation and specification -- we need to be able to identify those aspects of form to which observers respond, and which, indeed, differentiate forms from one another.

Fourth, in all areas of research in form discrimination it would be helpful if we had considerably more information than we now have about what the observer is <u>doing</u> when he recognizes or identifies a form. Such information would bear directly upon problems of stimulus generation and specification, experimental design, and data analysis. Little, if any, formal research is being conducted in this area.

Fifth, and correlary to the fourth, operational requirements as well as theoretical interests demand a better understanding than we now have of the significance of the observer's performance in discriminating forms. We think, for example, of the presently obscure relation between accuracy and speed in form discrimination.

Finally, we believe that we can conclude from the symposium that there is a large enough hard core of competent research people already tooled up for forms research, or at least already thinking about it, to make it possible for us to catch up with technological developments and eventually to keep pace with them. With a very few exceptions, however, the people involved and the research being done are in university laboratories. This fact may place some burden on the Committee on Vision; first, to help translate operational requirements into significant problems for research; second, to help stimulate competent people to continue, or to undertake, research on critical forms problems; and, finally, to help communicate the results and to aid in their effective application.

We would like to think that the Committee on Vision's sponsorship of the Symposium on Form Discrimination as Related to Military Problems and especially this publication of its proceedings will have been effective first steps in performing those functions.

JWW

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