extent. During several years of extensive researchwork in the optical laboratories of the WILD plant, Mr. L. Bertele, the well-known designer of a number of highly luminous objectives, has developed a new type of photogrammetric objectives, the "AVIOTAR" lens. This new design invalidates every known formula and all the experiences made heretofore with regard to the resolving power. Thus, WILD Heerbrugg is still leading in the field of photogrammetry, regardless of strong competition.

The AVIOTAR is a so-called normal-angle objective with a field angle of 60°, and its aperture of $f/4.2$ is greater than that of all photogrammetric objectives in existence to date. Therefore, it is possible to use the AVIOTAR under much less favorable light conditions than any other objective. Regardless of the unsurpassed luminosity, the resolving power for poor contrasts on aerial photographs has been improved by about 70%. However, not only the quality of the image, but also the absence of distortion for all practical purposes, made the AVIOTAR the leading photogrammetric objective. The maximum distortion is smaller than 5 micron for a focal length of 170 mm. In other makes of photogrammetric objectives, the distortion varies with the diaphragm aperture; this is not the case with the AVIOTAR. Under favorable conditions, the diaphragm can also be closed somewhat, which brings about an additional improvement of the already very high quality of the picture.

The AVIOTAR is chiefly used with the new fully-automatic plate camera WILD RC7 (focal length 170 mm, plate size 150×150 mm), for pictures requiring a very high degree of accuracy. Upon request, it can also be mounted in the normal cone of the fully automatic camera WILD RC5 for films 180×180 mm. This special AVIOTAR Lens has a focal length of 210 mm. Also on this camera it has shown outstanding results. The excellent quality of the image to the very edges considerably increases the possibilities for enlargement for photographic maps as well as for stereoscopic plotting. As an average, one can figure with an improvement of 70% for enlargements and with an increase of 50% of the plotting accuracy. Therefore, for the same plotting scales and the same requirements with regard to accuracy, it is possible to fly at much higher altitudes, and thus to increase the economy of the pictures to a considerable extent.

The quality of the original negatives appears most advantageous in the Autographs and plotting machines because of the extraordinary amount of reproduced detail and because of the brilliancy unachieved heretofore.

PART II—RESEARCH

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I. INTRODUCTION

In general, the ultimate in any science is a set of nomenclature, theorems, formulas, and instruments that are near universal, near exact, and near certain. There is a general agreement among men doing research that currently truth is simply the preponderance of probability, or that which has the greatest frequency of agreement. Therefore any body of knowledge is no more than the springboard to some newer body of knowledge that will have been found to have a greater preponderance of probability. In many fields of science, for all practical purposes, an approach to the ultimate—that is, standardization and crystallization of nomenclature, theorems, formulas, and equipment—has been attained. But to standardize and crystallize a science prematurely—that is, before all avenues have been explored and investigated—is certainly unscientific in that it closes the door to further inquiry. I think we can agree that the science of photogrammetry is in its infancy; therefore, an attempt at standardization as an end in itself may actually be harmful. There can be no doubt that there are many untouched reservoirs of investigation in the field of photogrammetry. If we can attach any validity to the foregoing statements, then any such fragmentary comment as "This organization or individual is not conducting its or his research along orthodox lines" is immature.

These preliminary remarks are meant to frame the spirit of this paper and to further justify space taken to define the position of an organization devoted largely to photogrammetric research.

II. RESEARCH VERSUS PRACTICAL OPERATIONS

Many of us are prone to view research as purely theoretical and therefore nebulous, as compared with the practical operations of the practical man. A little analysis of the philosophy and mental discipline of the various organizations conducting research in the United States will demonstrate how unfounded this view is. Before embarking on any investigation, we might subject ourselves to the following questionnaire.

1. Is the idea, method, or instrument functional? In greater detail—
   a. Is there a need for this item?
   b. Will related operations be more efficiently executed as a result of the investigation, provided the final test shows successful results?
   c. Will related operations be more efficiently executed as a result of this investigation in lieu of another?
   d. Are any pertinent areas of investigation being neglected as a result of this investigation?
2. Are we qualified to conduct the investigation?
3. Are we duplicating research being done elsewhere?
4. Is the idea based on sound presupposition—that is, does the idea cohere with the existing body of knowledge?
5. Is the termination of the investigation possible within a reasonable time limit? This question requires qualification. Generalized research may involve years, but any generalized research program can be broken into specific finite areas of investigation that can show results immediately. If the general program does not lend itself to fruitful by-product results in a short space of time, the project is by-passed for more productive projects from the chronological point of view. Frequently, a certain amount of preliminary research is done for the express purpose of legalizing further research.
6. Is the item capable of an economical laboratory test? For example, if one develops a solution to the three-space problem, it must be capable of a synthetic solution before we can justify a real empirical solution. The inferences drawn from any phase of the test supersede any conclusions drawn from a previous phase.

It is interesting to note here that the criteria for research enumerated above are essentially the principles on which efficient industries operate. It would appear then that being practical, as opposed to the discipline attending functional research, is just another way of being inefficient.

The difference between the method of research and the method of common sense is concisely expressed by George Santyana: "the world of science or research is merely an extension of common sense. The difference is in quality and not in kind." Stated in another way, the world of research is simply common sense refined with the tools of mathematics and instrumentation. The world of common sense is cruder-science more sophisticated. The method of research is common sense refined to the point where it largely eliminates errors characteristic of common sense.

III. THE DOCTRINE OF RESEARCH

We might take for our code, or doctrine as it were, "no knowledge is infallible." Charles Peirce (1839–1914), a great American philosopher and onetime employee of the U.S. Coast and Geodetic Survey, is the original exponent of this doctrine. To say "no knowledge is infallible" sounds like a paradox insofar as the doctrine itself is infallible. We adopt this doctrine, not because it is infallible, but because by adhering to this doctrine, we have a self-correcting system.

Thus the sum and total of photogrammetric knowledge, or any kind of knowledge for that matter, represents the highest plane of positive reasoning, or the greatest preponderance of probability, and this body of knowledge is subject to revision and correction and is subordinated to a greater preponderance of probability in the light of more conclusive data. To accept as infallible or to believe without reservation is to close the door to further inquiry, and therefore prevent the truth or error from ever being more closely approximated. This doctrine, if accepted, whether it be true or false, will find its own errors.

To prevent any ambiguity in meaning, let us dwell a moment on what we mean by "preponderance of probability." We mean the greatest frequency of agreement and something more—namely, the greatest frequency of truth agreement or what Charles Peirce terms "truth probability."

For instance, many of us agree on propositions that are not true; we have been simply mass-hypnotized as it were. Specifically, it is important to distinguish between the artificial frequency of agreement, or that situation where many of us agree because of the influence of calculated salesmanship, and the frequency of agreement where many people have made many honest measurements and found agreement.

IV. NEGLIGIBLE ERRORS

There has been considerable discussion among photogrammetrists on what constitutes a negligible error. Let us consider errors due to lens aberrations for a moment. It is possible to list the various aberrations and assign a numerical maximum magnitude to each aberration based on laboratory-desired data. Evaluation of these errors in the light of specific precision requirements of a specific camera should reveal which of the aberrations must be considered, and which can be omitted, on the premise that the error is negligible. This, however, has been repeatedly demonstrated not to be a reliable criterion. It has been pointed out that the actual resultant resolution does not coincide exactly with the theoretical resultant resolution—that is, the observed resultant resolution is generally less than that obtained by summing the laboratory-determined resolutions of the lens, negative emulsion, and print emulsion. Similarly the resultant image displacement does not agree exactly with the image displacement computed from a list of lens aberrations which have a numerical magnitude. In the case of camera calibration, we consider an error negligible if the difference between the true subtended angles and the corresponding computed subtended angles, due to the omission of a particular error, is less than the permissible discrepancy. Though we cannot say categorically "a thing is true because it works," neither can we say a thing is true because it is theoretically true. Since the end product of any theorem is its empirical application in real mapping operations, we are forced, for the lack of a more reliable criterion, to take as a criterion of negligible errors an empirical one.

V. ETHICS

We hold scientific truth to be the end and aim of inquiry, and research to be a refined method of conducting this inquiry. Certainly the method of research is not "a thing apart" but a way of life that embodies profound ethical ideals. To pursue the way of research, regardless of how contrary to our wishes or how devious the route, takes an unselfishness that stems from placing scientific truth above any personal gain or ambition.

PART III—COMMENTS ON INSTRUMENT RESEARCH

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INTRODUCTION

THE chairman of the research committee of this society, Mr. McNeil, requested that this article be written for publication, commenting on instrument research in photogrammetry. This opportunity to record a few thoughts related to instrument research as applied to this field is welcome and worthwhile if it proves of interest to those who support instrument research, particularly in photogrammetry.

BASIC RESEARCH

Instrument research is fundamentally dependent on basic research for establishment of basic facts needed for sound results. To evolve new basic facts needed for stimulating useful ideas is essentially the difficult function of basic research groups. But new facts are not produced exclusively by such groups, but by all persons who think carefully and logically. Those who are engaged in basic research continually discover new facts which stimulate ideas of their own and lead them into applied research activities. These ideas can often be directly applied to building an instrument to serve a specific need. This need may be for an instrument to be used by these groups to establish new facts; but often their ideas lead toward an instrument meeting the economic needs of others. This is the point where instrument research begins.

BASIC PROBLEM OF INSTRUMENT RESEARCH

Instrument research in photogrammetry, as well as in other fields of instrumentation, is primarily the searching for and stimulating of dormant or original ideas from many sources, and their development to the point of economic usefulness in the form of an instrument. A dormant idea usually is an idea that has been previously expressed but never advanced to practical economic use. An original idea is one usually conceived when other ideas and facts meet. An original idea may be called a pregnant one being capable of producing a new result, but needing considerable nursing before arriving at mature usefulness.

INSTRUMENT RESEARCH AND INVENTION

One of the chief elements of instrument research is the subject of invention. An original idea alone is not the entire basis of invention, although it is often loosely called an invention. Invention, in part, is an original idea proven capable of being reduced to useful practice. For a person to claim an invention solely because of his earlier expression of an idea is a dubious assertion. This statement is particularly important because in all fields or research there are those who speak and write prolifically to express ideas vaguely on all phases of a particular subject. Often these expressed ideas are in a form so complex or obscure that the idea is barely recognizable. To simplify such ideas, and to combine them with other practical ones, leads toward their reduction to practice. In this reduction to practice of an idea in the form of an instrument lies the essence of invention. Although "invention" is often used as a synonym for "patent," it is