Operational Aspects of Instrument Design

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Of wonders of science and feats of design
Has many a scribe writ the praise;
And if I now mention the subject again
It's distinctly a relative phase
For while science and gadgets are fine in their ways
One worries at times 'bout their clutch,
Especially when science, design, and math'matics
Combine to get us in Dutch.

THE AMAZING RELIANCE today on implements and machines, particularly on those we term *instruments,* and our unquestioning trust in what they do or tell us were well described a few years ago by Hugh L. Dryden, then associate director of the National Bureau of Standards (now director of aeronautical research, National Advisory Committee for Aeronautics) in an address before the Instrument Society of America (*2*):

> A significant, astonishing, and typical phenomenon of modern times is the general confidence placed in the readings of any well-constructed instrument. The automobile driver swears by his speedometer; the housewife looks at the thermometer to see whether she ought to feel too warm or too cold; the handsomely ornamented dials in the lobby of the skyscraper convincingly prove that the wind is blowing 60 miles per hour from the northwest on the roof. Even engineers who should be more skeptical have contracted the habit of relying on the readings of instruments. This naive faith has in general been justified by the care which the instrument industry has exercised in the control of the accuracy of its products. Nevertheless, this great faith in the readings of instruments should continually remind us of the responsibility resting upon us to justify that faith, to make more accurate instruments, which hold their calibration under severe conditions of service, and which are most nearly foolproof in operation. We have an obligation to promote the correct use of instruments in a manner to secure uniformity and mutual understanding.

Dr. Dryden justly stresses our indebtedness to the instrument industry for the care it "has exercised in the control of the accuracy of its products." He does not mention, however, either in the passage quoted or elsewhere in his address, the enormous influence exerted upon the instrument industry by the mass experience of the many users of instruments produced in quantity and employed under a great variety of conditions—a mass vote that expresses experience so vast that even the improbable has plenty of chance to occur and usually does. If the design of an instrument released today is faulty in even the slightest degree, you hear about it tomorrow, or at most, the day after tomorrow. It is this combination of mass vote registering a vast experience and a demonstrated manufacturer's responsibility that justifies our faith in these instruments.

In the case of instruments of which only a few are produced, and of instruments (e.g., military contrivances) produced in quantity for use under extraordinary conditions that are difficult or impossible to simulate, our trust in the instruments rests on less firm foundations—it rests largely on extrapolation from commonplace experience bolstered by faith in the manufacturer's ability to design and produce an instrument that will do the job intended under the conditions of actual use. When the designer and producer of the instrument are far removed from the ultimate user not only geographically but also in experience and perspective, too often the case and sometimes unavoidable, then our faith may be really unwarranted.

The more complicated the instrument, the severer or stranger the conditions of use; the less we know about the eventual operator of the instrument, the more acute are the problems of instrumentation. While these problems do occur in the design of laboratory instruments, they are more frequently critical in the design of control devices—instruments which measure the values of a number of variables (with or without the intervention of a human operator) and which react in a predetermined manner to establish a prediction for control purposes.

An excellent example of the difficulties which the Operational Analysis Groups of the Air Forces encountered was the first group of automatic sights of World War II, designed for the defense of bombers against fighters. From the strictly instrumental point of view, which necessarily precluded field studies, these computing sights were clever mechanisms for determining the amount of deflection required (a "lag," not a "lead," when the fighter was following a pursuit-curve course toward the bomber). The computation was based on the observed angular velocity of the fighter relative to a coordinate system based on fixed directions within the sight's mount, which was firmly fastened to the bomber's frame and not gyrostabilized. The component of this relative motion pertinent to the deflection problem is the motion of the fighter relative to a coordinate system determined by the velocity vector of the bomber, the vertical plane (i.e., the plane through the zenith and through the bomber's velocity vector), and the plane through the bomber's velocity vector perpendicular to the vertical plane (which will be horizontal if and only if the bomber's velocity vector lies in the horizontal plane). Unfortunately, as a result of the pitching, rolling, and yawing of the bomber, there is generally considerable motion of the sight-mount coordinate system relative to the coordinate system determined by the bomber's velocity vector. This motion is, of course, irrelevant to the deflection problem. Consequently, in the absence of gyrostabilization of the sight mount or an adequate "smoother" within the computer to iron out the irrelevant components of motion, these sights merrily computed deflections with comparatively great exactitude but on the basis of largely irrelevant data. It didn't take experienced gunners long to discover this fact in actual practice. As a consequence these computing sights were torn out of the bombers in the combat theaters, and for a time the whole program of development of automatic computing sights for aerial warfare was seriously discredited.

Here there were remedies available to the instrument designer—gyrostabilization of the sight mount, a built-in smoother, or both—but cases do exist where uncontrollable external conditions limit the realizable precision of a high precision instrument to a far greater extent than do the more evident limitations on the design parameters involved. A case in point is the optical range finder (*2*). In effect, this instrument measures the angular parallax, α, of the target when viewed from the two ends of a base of length b contained within the instrument, the scale of the instrument is graduated to read range, r, directly, and the graduations are based on the equation $r = b/\alpha$, where r and b are in the same units (e.g., feet, yards) and α is in radians. An error of $\Delta\alpha$ in the measurement of the parallactic angle produces, therefore, an error of $-(r^2/b)\Delta\alpha$ in the measured range. Range finders have been built with base lengths as small as 20 inches and as great as 100 feet. Turret range finders on battleships have base lengths of 50 or 60 feet. A typical American instrument for use against airplanes has a base length of 13½ feet. The ratio of the range to be measured to the base length is generally so large that satisfactory accuracy cannot be obtained unless the parallactic angle can be measured with an error not greater than two or three seconds of arc. To lessen the errors due to the limited acuity and resolving power of the eye, range finders are usually equipped with an eyepiece of magnification M, the effect of which is to transform α into $M\alpha$ for purposes of measurement. Thus an error of δ in the apparent angle so measured becomes an error of δ/M in α. From this error relation and the basic one given previously relating errors in α to errors in r, it would seem, and it has often been tacitly assumed, that an increase of magnification by a given factor, or an increase in base length by the same factor, would be equally effective in increasing the precision of the instrument as a *range* finder; and that by sufficiently increasing the magnification, or the base length, or both, any desired precision in terms of *range* could be achieved. Unfortunately, convection currents in the air between the range finder and the target—there is always some quiver and sometimes a veritable "boil"—invalidate these conclusions. From studies conducted under the auspices of the OSRD during World War II and some more recent investigations carried out by F. E. Washer and his associates at the National Bureau of Standards, it now appears (*2*) that any single measurement of the parallactic angle, α, has a standard deviation component, perhaps between 0.80 and 1.20 second (1 second = 0.000005 radian), that originates in the air path between the range finder and the target, and consequently is beyond the control of the instrument designer. By substitution in the basic error relation given earlier, the corresponding standard deviation component of a range estimate of a given range using a particular base length can be determined. Thus, taking 1 second as a typical value of the standard deviation component (due to shimmer) of a measurement of α, the corresponding range standard deviation will be 1 percent if the range is 2,000 times the base length and 5 percent if it is 10,000 times the base length; and this amount of uncertainty will be present even if the range finder and the observer contribute no additional components of error (a purely hypothetical situation!). The instrument designer can offset this component of error only by making the base long enough to achieve the precision required at a given range, if practical considerations will allow him to do so.

Up to this point we have considered only cases where "external conditions" in themselves prevent full realization of the built-in accuracy and precision of the instrument. Let us now consider some instances where the human operator of the instrument is the stumbling block. These human troubles can be of two kinds: *physiological* and *psychological*. In the case of the optical range finder the limited resolving power and limited acuity of the observer's eye are physiological limitations. The limitations of coincidence judgment produce a standard deviation component in the *apparent* field, perhaps of the order of 3 seconds. In terms of the measurement of the parallactic angle, α, this becomes a standard deviation component of $3/M$ second. Combining this with an air-path component of about 1 second, as just considered, the contribution of these two sources of error to the standard deviation of measurement of the parallactic angle, α, is $\sqrt{1+(3/M)^2}$, which for commonly used magnifications of 8, 12, and 24 diameters equals 1.068, 1.031, and 1.008 seconds, respectively. From these figures it is evident that the air-path component completely dominates the situation, and that by using readily accessible magnifications the contribution resulting from the aforementioned physiological limitations of the observer can be rendered negligible. Unfortunately, this is not the whole story, for use of high magnifications introduces a psychological difficulty in the form of observer frustration resulting from the shimmy of the target image—the quiver of the target (as seen through the air path from the range finder to the target) *magnified M times* by the lens of the eyepiece. Indeed, if the magnification involved is 20 or 24 diameters and the air path is "boiling" vigorously, which is the situation on hot days, the observer may be unable to "observe" at all, that is, unable to bring the target image and the reticle of a stereoscopic range finder (the two target images of a coincidence range finder) into "coincidence."

A strictly psychological difficulty arises in connection with the *stereoscopic* range finder. In making a setting with a stereoscopic range finder the reticle and target are brought into coincidence by turning the range knob. "If . . . the target is extended or against a background so that there is no free space on either side of the target, then when the reticle is projected beyond the surface of the solid object the observer is loath to accept the apparent penetration of the reticle into the solid target, and hence a bias is introduced which may correspond to a large error in the indicated range. Similarly, when one attempts to set a crisp, sharply defined image of the reticle in stereoscopic coincidence with a target not sharply defined and obscured by blue haze, the pronounced difference in atmospheric perspective leads to an erroneous setting. These errors resulting from biased stereoscopic judgment may amount to as much as five or six seconds [in the measurement of the parallactic angle, α]. If one attempts to check the graduations on the scale of a range finder by reading ranges on a selected series of terrestrial targets at known distances, the stereoscopic bias for the different targets will, in general, be different for the different targets, giving a jagged calibration curve which suggests that individual graduations on the scale are displaced from their correct positions in a nonuniform manner. The falsity of this conclusion can be shown by testing the range finder with a better planned test in which stereoscopic bias is eliminated or equalized for the different targets" (*3*). The seriousness of this psychological source of errors is readily appreciated when it is recalled that satisfactory range measurement generally requires measurement of the parallactic angle with an error not greater than two or three seconds. Fortunately, there is a design trick (*2*) used in the German R40 range finder whereby bias from this source can be averaged out by making two settings on the target, and this may be used for range reading on stationary or slowly moving targets.

The automatic computing sights mentioned earlier were plagued also with operator errors of psychological, or psychophysical, character arising from the interaction of the operator and the instrument. In the first place, the several motions the operator had to perform were somewhat unfamiliar and awkward, and cultivation of adequate skill by intensive training was necessary. With one type of these devices the operator moved the gun mount (upon which the sighting mechanism was firmly fixed) in azimuth and elevation by means of a handle-bar arrangement, one handle of which contained a pistol-grip trigger for activating the gun(s), while rotation of the other handle adjusted the range setting of the sight. It was difficult enough to learn how to perform all of these motions correctly to begin with. To be called upon to perform them correctly in the excitement of an attack, lasting 5 seconds or less, was asking too much! (How many persons can *pat* their heads, *rub* their stomachs, and *scratch* a stomach itch all at once, without getting mixed up? The gunner's problem was even worse!) To make matters still worse, the reticle of the sight was controlled by the computing mechanism and was not under direct control of the gunner—the most amazing changes of position of the sight reticle would result at times from a simple movement of the hands.

There exists today, largely as a result of such wartime experiences, a branch of applied science known as *human engineering* (called by the inner circle "psychophysical systems research") that is actively concerned with "man as a worker of machines, and with machines as things that man must work." This aspect of instrumentation deserves close attention from both designers and users of instruments.

Although the instrument in hand may be able to perform its intended function with great accuracy and precision, this built-in accuracy and precision may be highly illusory in terms of the uses to which its end product are to be put (e.g. measurements for direct use as such in scientific research, engineering tests, and so forth; or measurements constituting the stimuli activating an automatic control device). Thus high speed punch card equipment is fine for processing statistical data to obtain accurate summary figures with ease and rapidity. But the trustworthiness of these summary figures for purposes of administrative action or scientific inference depends upon the trustworthiness of the raw data themselves for these purposes, which in turn hinges upon the extent to which the method of obtaining these data effectively produces random samples from the population to which the conclusions are to apply. The capabilities of these machines, which bring the processing of very large amounts of data within the realm of practicability, carry with them the danger of diverting attention from the need for considering carefully in the planning stage of an investigation (a) exactly *what kinds* of data are required for the purpose at hand, (b) *how* they are to be collected, so as to assure validity for this purpose, and (c) in *what quantity* they will be needed in order to achieve adequate precision. The approach "Let's record everything in sight and the machine will do the rest" is not to be recommended, for when the data are in and are partially or fully analyzed it is too often found that "everything in sight" failed to take account of factors critically affecting the value of whatever conclusions might be reached.

Finally, the great practical value of many instruments stems in the last analysis not so much from the high precision of individual observations thereby attainable as from the fact that they make it possible to obtain broad coverage with observations of ample precision. For example, consider the core-boring instruments and associated techniques developed by the Bureau of Customs and others for evaluating the clean-wool content of a shipment of baled wool. In the case of many carpet wools the relation between the between-bale and within-bale components of variation of the clean-wool content is such that accurate evaluation of the (average) clean-wool content of the entire shipment requires the sampling of many bales, taking adequate amounts of material from each bale sampled. This is made possible by the core-boring tool. Without it, the cost of obtaining the same intensity and coverage of sampling by breaking open the bales and drawing samples by hand would be prohibitive and would necessitate relaxing accuracy requirements below levels considered desirable. As another illustration, consider the Magne-gage designed at the National Bureau of Standards to measure the thickness of nonmagnetic coatings on a magnetic metal object. By its use the total thickness of composite coatings ranging from 0.0005 to 0.003 inch can be determined rapidly and nondestructively to within about 10 percent; and the thickness of each component layer of similar coatings, to within about 15 percent. The important feature is that the determination is nondestructive *and* rapid, so that many different parts of the coated object may be examined when the quality of a coating or the effectiveness of a coating technique is being evaluated.

To sum up: From prehistoric times man has been a user and developer of tools. Man now depends more than ever on implements and machines—some crude, many elaborate; some delicate, others sturdy; all the servants, and some at times the masters, of man. Thomas Carlyle said: "Man is a Tool-using Animal. . . . Nowhere do you find him without Tools: without Tools he is nothing, with Tools he is all" (*1*).

But if our instruments are to serve their purposes fully—and this is especially true of modern, complex control devices—both the designer and user must never forget (1) the conditions of operation, (2) the nature of the measured quantities, (3) the relation of these quantities to the end action, and (4) the psychological and physical characteristics of the operator. With these elements in mind, instrumentation will progress, providing man with ever-increasing extension of his senses and control over his environment.

References

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