REPORT TO THE SERVICES NO. 59

Division 7 (Fire Control)
National Defense Research Committee
Contract NDCrc-83

STATISTICAL METHOD OF PREDICTION IN FIRE CONTROL

In the past it has been customary for antiaircraft predictors to assume straight line flight of the enemy aircraft, and to use various smoothing techniques to handle all departures of the inputs from linear signals.

However, if antiaircraft engineers ever succeed in furnishing a sufficiently deadly solution for the problem of straight-line targets, then this very success would make the problem relatively unimportant; for enemy aircraft would then be forced to fly other than straight courses. Furthermore, aircraft which attempt to fly straight courses do not precisely do so, for a variety of reasons. It is also clear that, whatever the path of the enemy aircraft, the inputs to the predictor consist of the true signals, due to the actual motion, upon which are superposed "noise" due to tracking errors.

Thus it is important to investigate how one can, by various methods, predict other than a pure linear signal. For analytical curves of varying amounts of complexity (horizontal circles, helices with dive or climb, etc.) the prediction procedures can be worked out by simple geometrical methods. But to investigate prediction problems for signals which contain noise due to tracking or to flight errors, or for signals corresponding to various general types of curved flight, that is a much deeper and more difficult matter.

This is, in very general terms, the problem with which Professor Wiener has been concerned. In a previous memoir (Report to Services No. 19) he applied powerful analytical tools to develop a statistical method of predicting. In the present report he indicates, in two papers dated December 1, 1942 and January 15, 1943, the result of applying his method to certain definite cases. That these particular applications did not turn out to be of practical importance does not, in our judgment, mean that the study was not well worthwhile. The general theory will doubtless have other applications; and it was a matter of importance to know just how successful this statistical method would be for the antiaircraft problem.

On page 5 of Professor Wiener's report dated December 1, 1942, he refers to certain charts. On these charts were plotted the actual path of the airplane on Antiaircraft Artillery Board courses 303 and 304, as determined from theodolite data. These charts are very long, and there does not seem to be any practicable way of reproducing them for inclusion in this report.

The initial distribution is listed on the following sheet.

Extrapolation, Interpolation &c.

FINAL REPORT ON SECTION D - 2 PROJECT #6 December 1, 1942
SUBMITTED TO DR. WARREN WEAVER
SECTION D-2, NDRC

INTRODUCTION

The present report concerns the use of statistical methods of prediction, particularly as applied to the control of anti-aircraft fire of large caliber at long range, and even more specially concerning such fire as used against planes flying an approximately level and straight course. This investigation had its inception in certain questions that were put to the author in the fall of 1940 concerning servo-mechanism design. At that time the author was very much interested in what has become a routine matter since, namely, the use of the methods of communication engineering in servo design.

In firing at a moving target, it is necessary that the projectile arrive at the position of the target at some future time and not at the present position of the target. This means that some method of determining the expected future position of the target is necessary. The simplest method would be to determine precisely the present position of the target and its velocity and by multiplying the velocity by the expected time of flight to arrive at the vector expressing the differences between the expected future position and the present position. This is only strictly accurate for perfectly rectilinear flight observed with a perfect instrument. It is well known that our tracking instruments have very appreciable intrinsic and personnel errors and it is further well known that even a careful attempt to make a straight flight through an air subject to all sorts of currents and with machine subject to irregularities of performance both intrinsic and due to the pilot is impossible. It is further clear that errors far below such as would throw a shell effectively off the present position of the target may lead to false estimates of the future position great enough to cause misses. Thus, even for nominally rectilinear flight, the problem of prediction is one which seems to involve a fairly complicated mathematical apparatus, at least for its critical investigation and treatment.

It then occurred to the author that the problem of prediction, so close to problems in servo-mechanism theory, was equivalent to that of designing an operator which should approximate in an appropriate sense the operator $e^{i\omega\lambda}$, which is well known to represent anticipation by the amount λ, but which is not realizable without alteration by any stable circuit. The problem then was to arrive at stable approximations to this; and a priori it occurred that an operator of the form

$$\left[\frac{1 - \frac{\lambda i \omega}{2n}}{1 - \frac{\lambda i \omega}{n}}\right]^{2n}$$

might fulfill the desired purpose. Accordingly an attempt was made, using Dr. Bush's differential analyzer, to study the behavior of such operators on data of various sorts, and in particular on data consisting of a succession of linear segments. This attempt was performed and the author gradually came to be aware of some of the intrinsic differences in the problem. If n was so chosen that the operator gave a really good prediction for a rectilinear part of the course to be followed, this was at an enormous expense of stability when the target plane changes from one rectilinear course to another. At such places there was a simply enormous overshoot, and it became obvious that in any curve not precisely of the shape of a simple sinusoid or straight line, any attempt to use this method of prediction in an extreme form would lead to a failure because of lack of stability,—or what is the same thing, to an excessive response of such a system to high frequencies in the input. This response was so great that the overshoot increased exponentially as the number of meshes in the circuit increased. On the other hand, if the number of meshes used in the circuit were small, or in other words if n were small, the approximation of the operator in question to an operator predicting by a fixed lead became sketchy in the extreme. It was obvious that the problem of prediction was essentially that of a compromise between two effects in diametrically opposite directions. As in all such compromises, the question of determining the optimum sacrifice of accuracy to be made for stability and of stability to be made for accuracy was the cardinal question; and as in all such cases, this led to considerations essentially belonging to the calculus of variations.

In order to settle such a problem mathematically, it was necessary to have a numerical measure of the amount by which a method departs from ideal performance. In the case of prediction for anti-aircraft fire, the fundamental goal of our efforts is to find the design of the predictor which shall produce the maximum number of effective hits. This notion, however, is not well adapted to mathematical treatment; and in particular, not to the calculus of variations. In the first place, it involves a rather conventional definition of what an effective hit is; and in the second place, integral numbers like numbers of hits are in general not good material for minimization. It is therefore expedient at our present stage of development to replace this minimization problem by one more immediately accesible to mathematical treatment.

Of all quantities which lend themselves to an easy minimization, the most natural are those which are inherently positive because they are squares of some somple real expression or sums of such squares. If this be applied to the course of a plane, then a tentative definition of the best prediction is that in which the mean square distance between the actual and predicted position is a minimum, this mean being taken with respect to the time.

I wish to say that I am well aware that this definition has serious faults. In the first place, it puts an over-emphasis on the points where the predicted and the actual positions differ by a large amount. Now in fact after a very limited twilight zone, one miss is as useless as another. If a shell bursts over 100 yards from a plane, it is quite as effectively wasted as if it burst a mile away.* On the other hand, the mean square definition of error gives a considerable weight to small errors occuring with great frequency over a long interval of time. It discriminates between two methods of prediction, in one of which a prediction, let us say, in error by 20 yards, is consistently reduced by five yards over the prediction make by the other method. Now within, let us say, 40 yards of a plane, a shell burst is a hit, and may be regarded as completely destructive to the plane. Thus at both ends of the scale the mean square test is too sensitive, and it is not specifically sensitive to the region of error corresponding to the boundary of the area blasted by the shell. Notwithstanding these considerations, the author has not found any method of direct treatment of the true ballistic problem which is

* NOTE by Div. 7. This statement is some what too strong. For example E.L. Welch has shown (the Chances of Damage to Aircraft from A.A. Shells: A generalization of Previous Methods of Solution. E.B.D. Report No.23, page 21) that when the standard errors in the rectangular components of the errors in the burst point are of the order of 500 feet, then about 19% of the vital rounds are less than 100 ft., from the target, 29% from 100 ft.to 200ft.32% from 200ft.to 400 ft., and 20% over 400 ft. away. These data are for the British 3.7". If the standard error is about 300 feet, then the corresponding percentages are 24%, 35%, 32% and 9%

mathematically manageable. He believes that such a method must be found by the use of mathematically manageable criteria such as that here employed, together with a certain amount of artificial elimination of parts of the plane course which are not important for real military purposes.

Another thing that will be seen is that the method employed is statistical. The criterion of goodness of a prediction procedure is one involving time averages. It is in the theory of time series that the author has sought the basis of his methods. Without going into any detail concerning these methods, it may be said they are based on the two basic ideas of *auto-correlation coefficient and spectrum*. If we have a sequence of numbers a_n, the auto-correlation coefficient ϕ_m of these numbers for the lag m is the mean value, taken over all time, of the product of one of these numbers and its associate m stages later. The spectrum is the function whose Fourier series has as coefficients the auto-correlation coefficients ϕ_m of the original sequence. These are the simplest but not the only statistical parameters of the time series. In order for any statistical prediction method to function, it is necessary for such statistical parameters to have a certain degree of stability or in other words to be much alike over a considerable range of different time series and of different parts of a single time series. Whether the stability exists in particular cases is a question of fact but it is on this question of fact that the possibility of the effective use of methods of prediction, ours or any others, depends.

It is unnecessary here to go into the mathematical detail of the theory arising from these conclusions. It has been presented by the author in a monograph published privately by the NDRC entitled *The Interpolation and Extrapolation of Linear Time Series and Communications Engineering*. It is essentially similar to a method carried out somewhat earlier by Kolmogoroff in Russia but not known to the author until the pamphlet in question was well underway. This method has recently been somewhat extended and projected, and it is the intention of the author to give a subsequent report explaining these extensions and projections in some detail, but they do not concern in any vital manner the questions which he has been specifically asked to answer in the present final report.

In the early winter of 1940, Mr. Julian Bigelow was added as engineer to the personnel of the project, and later Mr. Paul Mooney, as a mechanic. Mr. Bigelow helped the author very considerably in the development of the theory and in the numerical computation needed to test the theory; but his assistance was greatest and indeed indispensable in the design of apparatus, and in particular in the development of the technique of electric circuits of long time constant. By the use of an amplifier of extremely great input impedance and a very accurate voltage-ratio of unity, he was able to show that circuits with any impedance characteristic realizable at all were realizable with the use only of amplifiers, capacities and resistances, completely ignoring the use of inductances. The inductances are well known to be the least satisfactory element of ordinary circuit design. They always have undesirable parasitic resistance, and where the inductance is itself to be at all large, this resistance may be entirely excessive. It was therefore a step of enormous importance to free the technique of circuit design from the use of such objectionable elements. Mr. Bigelow also designed an apparatus for generating curves not unlike the track of an airplane as raw material for testing the statistical method. This apparatus for generating "tracks" was statistically stabilized by giving the operator a task to perform which was strictly beyond his capacity for accurate performance. In the apparatus a guide light was moved across the wall of the room by clock work, and the operator had a control by which he was to move a following light. This control led on the one hand to an apparatus which deliberately falsified the kinesthetic sensations stimulated in controlling it, and on the other, controlled its spot of light in velocity rather than in position. The angular distance between the input spot of light and the output spot was registered through potentiometers as a variable voltage, and this voltage operated two Easterline-Angus recorders. One of these recorders was operated directly, while the other was operated through an adjustable electric circuit of the amplifier capacity inductance type, the constants of this circuit being adjusted as in the authors theory of prediction, on the basis of the auto-correlation of the input recorder for previous runs. With the aid of this apparatus a very significant degree of prediction of the input was obtained, and Mr. Bigelow and the author consulted with members of Section D-2 of NDRC for further instructions. This consultation took place in the early summer of 1942. At the same time a detailed plan of a predicting apparatus was developed in which the method of prediction of our theory was applied to the data of flight of the plane as transformed into intrinsic coordinates or in other words, coordinates determining the direction of flight, the speed of flight in track, and the altitude.

At the end of the summer of 1942, it was decided by Section D-2 that before any definite conclusion should be arrived at concerning carrying our theory into practice we should inform ourselves more specificallly as to the actual statistical character of airplane flight and of the tracking of such flight. In accordance with this, Mr. Bigelow and myself made an extended trip in which we visited Mr. Flood's project at Princeton, where we obtained a considerable amount of information concerning long time and short time errors in tracking, and concerning the possible use of methods for the precise determination of plane position; the Anti-Aircraft Board at Camp Davis, by which courses 303 and 304 as here presented were put into our hands, and by the courtesy of which we were given a much more extensive acquaintance than we previously had had of the techniques and difficulties of anti-aircraft fire control; the authorities at Langley Field, who put at our disposal considerable information concerning the regularities and irregularities of different types of flight; the Ordnance Bureau of the Navy Department at Washington, who then and on a later occasion informed us very fully of what naval procedure and anti-aircraft prediction was; the authorities at Aberdeen Proving Ground; Frankford Arsenal at Philadelphia; Dr. Fernberger at Philadelphia; the Foxboro Instrument Company; and finally the work being done on tracking at Tufts College. In this way we learned a great deal concerning tracking accuracies and techniques, and the matter of airplane flight from the standpoint of the anti-aircraft gunner. We also obtained by the courtesy of the Army Anti-Aircraft Board a considerable amount of numerical material which was to serve as the basis of an estimate of the value of our method.

PROCEDURE

Runs 303 and 304 were made at Camp Davis. Only angular tracking was performed, the altitude of the plane being assumed constant. We took the $R_0 \cos \Theta$ and $R_0 \sin \Theta$ coordinates of run 303 at second intervals and obtained their autocorrelation coefficients. The same work is under way on 304, but is not completed.

The on-course and cross-course autocorrelation coefficients a_n and b_n, respectively of the consecutive differences for n and for n-1 seconds of the components of 303 were used to obtain spectra. It was found that in the spectra both of on-course and cross-course components, there was a marked minimum at period about 13.5 seconds. This minimum was taken to indicate the approximate boundary between components due to the track and components due to the tracking. It is scarcely to be expected that a gap should not exist between frequencies primarily in the true course and frequencies primarily in the tracking. It may be observed that we already know that the spectrum of tracking errors tends to have a peak at a period of about 10 seconds and fall off rapidly for longer periods. The autocorrelation coefficients a_n and b_n were separated at this frequency into high and low frequency terms, α_n and β_n being the low frequency parts. The system of equations

$$x_0 a_{-\mu} + \cdots + y_q a_{q-\mu} = \sum_{\nu=-\infty}^{20+\mu+1} \alpha_\nu - \sum_{\nu=-\infty}^{\mu+1} a_\nu \qquad (\mu = 0, \ldots, q)$$

is solved by a method due to Dr. Crout of M.I.T. The quantities are then used to give rise to the weighting factors

$$Y_0 = 1 + x_0$$

$$Y_1 = x_1 - x_0$$

$$Y_2 = x_2 - x_1$$

$$\vdots$$

$$Y_9 = x_9 - x_8$$

$$Y_{10} = -x_9$$

If then the sequence of values of $R_0 \cos \phi$ at second intervals is given as X_n, we obtain the best mean square approach to $X_n + 20$, in terms of X_n, X_{n-1} ....X_{n-10} as

$$X'_{n+20} = \sum_{k=0}^{10} X_{n-k} Y_k .$$

A similar method is then used on the values of $R_0 \sin \Phi$, independently. We plot on the same graph X_n, a smoothed X_n, X_n', and thepredicted X_n'' obtained on the basis of a 10-second memory point method by taking

$$X''_n = 3X_{n-20} - 2X_{n-10},$$

plotting the pairs ($R_0 \cos \phi$, $R_0 \sin \phi$) and their predictions and smoothings as actual points in the plane. Furthermore, the predictions X' and its corresponding predicted $R_0 \sin \phi$ are made for #304 on the basis of coefficients computed from # 303. On the # 303 chart, certain other smoothing methods and prediction methods are also shown. (see accompanying charts.)

RESULTS

It is amply clear in #303 that the mean square error of prediction is less for X' points than for the X'' points. The very great errors at the beginning of #303 are reduced by not inconsiderable factors. It also appears that the X' points are closer to the X points than are the X'' points 90 times out of 134, or 67% of the time. On the other hand, allowing an error of 40 yards for hits to be considered as effective, the X' points produce only 43 hits as to 41 by the X'' method, a relative improvement of only 4%. This is scarcely enough to justify the construction of new apparatus.

For a prediction method to justify itself, it must be transferable to other courses than those on the basis of which it is designed. When applied to #304, the mean square error still seems to be reduced, though slightly, but the other factors favor the X'' method. X'' yields 53 hits against 48 for X', and is better 57% of the time. Taking both runs together, X' is better 54% of the time, but X'' yields 3% more hits.

Let it be noted that a least-square method is dominated by its bad points. In #303 the early predictions are much worse than the later predictions, and much worse than any of the predictions of #304. This suggests that it would be desirable to re-examine the two methods in the light of autocorrelation coefficients obtained from the later points of # 303, or from #304. This I intend to do in the near future.

In all such work, the difficult and expensive part is the computation of the autocorrelation coefficients, although this will be much cheapened and speeded by a new computing machine, invented by Mr. Bigelow for another government project. Once these are obtained, it would be a shame not to use them to the full. I propose to submit a supplementary report in a few weeks, containing predictions for #303 and #304 for leads of 0, 5, 10, 15, 25, 30, and 35 seconds, based on autocorrelation coefficients obtained from either:

(a) the corresponding coordinates of #303

(b) the corresponding coordinates of #304

(c) compromises between the $R_0 \cos \phi$ and $R_0 \sin \phi$ coordinates to produce apparatus independent of the angular orientation of the apparatus with respect to the general direction of flight of the airplane. I also propose to include some practical examples of altitude prediction.

Let it be noted that the assumptions that the altitude of the plane is constant in #303 and #304 mean that we have been predicting, not true horizontal coordinates, but mixtures of these with the vertical coordinates. This affects the usefulness of any instrument based on our computations, but not the general question of the feasibility of such instruments.

CONCLUSIONS

Apart from the possibility that the use of predictions based on #304, or the latter part of #303, may improve matters much more substantially than I now expect. It appears that my methods do not lead to any practical improvement over the "memory-point" method. This latter method can be realized either by taking as one terminus of a rectilinear segment to be prolonged a really fixed mechanical point, as in the Bode method, or a short time average, as in the Lovell method. In either case, the results as applied to #303 and #304 will not be substantially better than those actually obtained by the use of Sperry or of USMC apparatus, as shown in the accompanying official charts.

It is almost certain that the mathematical advantage of my method will really appear for considerably shorter times of flight than 20 seconds. Here, however, the large-caliber AA weapons, to which I have been asked to confine my report, find as heavy competitors rapid-firing automatic weapons of smaller caliber, which fire traces, and hence act simultaneously as weapons, observing instruments and ballistic cams. With light portable weapons, the advantage of an elaborate, not too easily portable instrument becomes quite questionable.

To what extent the negative result of this investigation is due to bad tracking, to what extent to the restriction of the useable past to 10 seconds, and to what extent to the fact that the enemy plane has a very considerable chance to change its flight pattern, whether voluntary or involuntary, in the twenty seconds of projectile flight, is not yet fully clear. I intend to study these matters with somewhat greater thoroughness, although I am convinced that the last named effect is preponderant. This is indicated by the general similarity of the X' and X'' curve, which differ much more in their long-time swings than in their general short-time character.

The work done on #303 is optimum from the mean square point of view. The fact that this optimum work is not significantly more effective from the practical point of view than the memory-point method, nor than the overall results of present predicting apparatus as realized in practice, indicates that no great consistent improvement in long range AA fire is to be expected. This is all the more obviously true in that the actual predictions used for these tracks employ times of flight varying from around 20 seconds up to about 35 seconds. In particular, it is now clear that the unremovable errors of these predictions are not smaller than those errors due to oscillations in the time-of-flight feed-back cycle, etc. Present field results are much closer to the best obtainable by any imaginable apparatus than I had expected, and I presume, than most workers in the field had expected. Accordingly, there is less scope for further work in this field than we had believed to be the case.

RECOMMENDATIONS

The author finds that an optimum mean square prediction method based on a 10 second past and with a lead of 20 seconds does not give substantial improvement over a memory-point method, nor over existing practice. He proposes to check this result with a similar method in which the more unpredictable parts of the course are discarded, and to discover the maximum lead for which his method yields an effective improvement in prediction. He also wishes to examine the theoretical effect of better tracking. He considers that if these investigations do not yield a much more favorable result than those already carried out, and he does not anticipate that they will, he will have established that new developments in the design of long-time predictors have already reached the point of diminishing returns, and that all efforts in this field should be concentrated on features leading to more rapid production and simplicity of use in the field.

January 15, 1943

Dr. Warren Weaver
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Dear Dr. Weaver:

The present report is intended to cover all remaining issues on project 5980. As a basis for this report, I refer back to your letter to Major General Williams and others dated October 6, 1942. In that letter you distinguish between four types of curved flight. These are:

(1) The curved flight of deliberate-evasive action
(2) The rather steady longer radius flights of a plane making a wide sweeping change in direction
(3) The very much longer radius curve, presumably followed by a plane whose pilot is attempting to fly in approximately a straight course
(4) The irregular deviations of a plane whose pilot is attempting to fly a straight course due to bumpiness of air, etc.

You make a distinction between two types of flight predictors, namely, an analytical flight predictor depending on the assumption that the course has certain analytical properties, and a wander predictor, the latter being statistical in character.

You ask

(1) Of the four types of curved path referred to above, how important tactically do I consider it to be to develop an anti-aircraft predictor that might furnish better predictions?
(2) Do I think an analytical curved flight predictor is important in connection with type three?
(3) Do I think that type (2) occurs frequently enough to make it important tactically to shoot effectively during this type of flight?

These questions were addressed to various army officers as well as to myself. I have seen the answers of a good many of these army officers, and I have in addition had the opportunity to make a thorough statistical study of flights 303 and 304, furnished me by the Anti Aircraft Board at Camp Davis, North Carolina. I have also had the opportunity of looking at a good many of the other courses from a less detailedly critical point of view, and of talking over with airforce authorities the mechanisms of straight line flight and the causes and extent of errors in such flights. I have come to the following conclusions.

(1) The curved flight of deliberate evasive action, in particular when the plane is flying at a height of 10,000 feet or more, is much too curved over the interval between which the shell leaves the gun and that at which it arrives at its destination to make any effective prediction for such flight worth while. This conclusion might conceivably have to be changed for very short range, small calibre anti-aircraft weapons, but it is extremely doubtful in such a case whether any mathematical treatment of the matter can begin to compare with the use of simple and foolproof devices for predicting, plus the building up of a battery throwing a simply overwhelming weight of metal.

(2) The evidence from all military sources that has been made available to me is that the rather steady, longer radius curved flights of a plane making a wide, sweeping change in direction, occur so seldom during actual combat that it is scarcely worth while to design fire control apparatus to meet them.

(3) The very much longer radius curve followed by a plane whose pilot is attempting to fly in approximately a straight course can not be too completely distinguished from

(4) The irregular deviations due to bumpiness of air , etc. Between (3) and (4), we find a quite appreciable deviation of the plane from a straight course, and(if we use any elementary method of prediction) a much more appreciable deviation of the predicted position of the plane from its actual future position. These deviations are large in comparison with the deviation of the burst of a fuse shell from its root-mean-square position. Any reduction in this deviation will cause a more econimical use of ammunition and an appriciable improvement in the number of hits with a given amount of ammunition.

The author has made several attempts to introduce an improvement in the prediction of the future position of the shell in such a manner as to conserve as much as possible some existing type of predicting apparatus, and also to lead to as stable and consistent a theory and design as possible. It is well to notice that in an ideal arrangement the proper coordinates to introduce would be intrinsic coordinates, as we have indicated in an earlier report. The chief value of introducing intrinsic coordinates is that they make a sharp distinction between the on-course and cross-course coordinates, which have a sharply distinct statistical behavior. Another way of producing what is to a first order the same effect is to introduce one of the geometrical methods of prediction first, such as, that of Bode,* and then to apply statistical prediction to the errors of the geometrical method.

*Note by Division7, the"Bode" method to which Wiener refers is a memory point system which was originated by Bode, although unknown to him, it was developed considerably earlier by Mr. Duncan J. Stewart, Member of Division 7.(Barber-Colman Co., Rockford, Ill.)

I may remark parenthetically here that I do not consider the distinction between geometrical and statistical prediction as absolute. Any geometrical prediction which involves too great a uniformity in the geometrical characteristics of the curves to be predicted is to be thrown out because it is not applicable on the average to the predictor problem which we meet in practice. This simply says that it is to be thrown out because it is statistically inadmissable. Similarly any statistical method of prediction is based on the existence of certain uniformities between the various curves which are to be predicted, and these uniformities are expressable in more or less geometrical language.

To return to the Bode prediction, Bode's method is to fix a certain memory-point in the past of the motion of the plane; and to extrapolate linearily along this straight line. Any such method will of course have errors, as the motion is not in fact perfectly rectilinear. These errors may be observed and recorded, and a crude method of improving the Bode prediction would be to correct the future position predicted for the plane by cancelling out the present observed error. This method would involve a negative feed-back. It can be made much more effective by replacing the present error of position of the plane computed statisticclly from this. The Bode method has a great advantage in that it is easy both theoretically and mechanically to treat departures from uniform rectilinear motion across the general path of the plane on a different basis from those along the path of the plane. It is therefore both from the theoretical side and from the practical side a method extremely adaptable to improvements such as those here suggested.

It will be noted that the memory point in the Bode method need not be at the beginning of the trajectory. In fact, the Bode apparatus is so constructed as to allow for a change in the memory point used in the middle of the flight. A method of prediction closely related to this would be one in which the memory point is set a fixed distance back in time, beyond the running present point. This result may be achieved to a considerable degree of precision by taking average past coordinates of the point, as read through an electrical lag system, and comparing them with observed present values. Such a method differs in description, but not in actual function, from one in which the derivatives of the coordinates of a plane are taken, not instantaneously, but by means of an integrating, smoothing device, and used as the basis of a linear extrapolation of the coordinates of the plane. Such electrically smoothed derivatives are used in a device invented by Mr. Lovell, and in the improvement of the Sperry predictor by the United Shoe Machinery Corporation.* In the latter case, the smoothing covers and interval of some three seconds.

* Note by Division 7. A rate smoothing and matching device, designed to be added to the Sperry director in the field, and following the general scheme suggested by the United Shoe Machinery Co., has also been produced under Division 7 auspices, and is now under service production.

On the basis of courses 303 and 304, predictions were made by several tentative methods, of which the significant ones appeared to be: (1) the Bode method, using the initial point as a memory point; (2) the modified Bode method, using a point 10 seconds back as the memory point; (3) the Bode method, with the cross-course error of the Bode method corrected by a statistical method of prediction based on a conjoint use of the statistical data of 303 and 304. Over a period of time of 165 seconds for 303 and about the same time for 304, and with an assumed radius of destruction of 40 yards for a burst, this yielded

Track	(1) Bode	(2) 10 Sec. Bode	(3) Statistical
303	6 hits	22 hits	23 hits
304	35 hits	55 hits	49 hits

In course 303, the curvature of the course is very conspicuous on the map, and it is perhaps not surprising that the fixed memory point is disadvantageous. On the other hand, 304 is a much straighter course, and the result is really noteworthy.

It appeared that an improvement of (3) by the application of a statistical method to the on-course coordinate as well as the cross-course coordinate would increase the number of hits, but not greatly, and would certainly not bring out any clear superiority to method (2). It also appeared that the merits of (2) would not be greatly changed by reducing the amount of past used to 6 seconds, nor by increasing it to 20 seconds. It was easy to get statistical methods which should slightly but appreciably improve the number of hits for 303 or 304 separately, but not simultaneously.

To come to practical conclusions, the Bode method is certainly not to be used with fixed memory point, and it seems that to put the memory point under the voluntary control of an operator is giving him too much freedom of judgement in practice. The Bode apparatus can be brought to a high efficiency by a supplementary statistical 1+network. Even at this level, however, it does not seem from present evidence that it will be out of the class of a somewhat improved U.S.M.C. apparatus, and similar simple smoothing devices.

In the computation of method (3), the full force of my statistical method was used, without any restriction, as before, to a ten-second base. As far as 303 and 304 are concerned, the work may be considered substantially final. However, these flight paths are to a certain extent not completely accurate trajectories. They have been made on the assumption that the height of the plane above the ground is constant and only the azimuth and angle of elevation of the plane have been measured. That is any departure of the altitude of the plane from constancy has been registered as a fictitious change of azimuth or angle of elevation. This means that in any true mechanization of an improved predictor, there will be three qualities that we have to take care of, not two, and these three will be on-course; distance, cross-course distance, and altitude. It is important to design the constants of any predictor on the basis of the history of the actual data submitted to the predictor, and if any further use is to be made of the suggestions presented in this paper, it will be necessary to carry out further experiments in this direction.

Furthermore, two courses made under similar conditions with a similar plan are not enough to serve as a basis of design, no matter how much they may be suggestive that improvements may be obtained. Before any attempt to realize method (3) can be carried out, a program of examination of courses run certainly up into the10's and probably into the 100's will be needed. I hesitate to make any such recommendation because I frankly do not know to what extent work of this sort may tie up present working sources of the country, and because the present expectation of great improvement is too distant to be significant in the present war. I most definitely do recommend that such study be made within some part of the long time program of our armed services.

Respectfully submitted

NORBERT WIENER

NW/lb