THEMIS SIGNAL ANALYSIS STATISTICS RESEARCH PROGRAM

SOME HISTORICAL REFLECTIONS TRACED THROUGH THE

DEVELOPMENT OF THE USE OF FREQUENCY CURVES

by

E. S. Pearson

Technical Report No. 38
Department of Statistics THEMIS Contract

July 3, 1969

DEPARTMENT OF STATISTICS
Southern Methodist University

FOREWARD

Professor Egon S. Pearson of University College London visited the Department of Statistics of Southern Methodist University on March 28, 1969. He consulted with the faculty and gave a lecture to graduate students, faculty, and invited guests; this report summarizes that lecture. The same lecture was also given at several other universities in the United States.

We record here our great pleasure in having had the opportunity to meet with Professor Pearson. His spirit, humor, and gentleness are an inspiration to all who come in contact with him. The work reported on has many applications which are not yet exploited. It is our desire to make these ideas available to other potential users.

SOME HISTORICAL REFLECTIONS, TRACED THROUGH THE

DEVELOPMENT OF THE USE OF FREQUENCY CURVES*

by

E. S. Pearson
University College London

I have chosen this subject for the theme of my talk because the chang-
ing attitude of statisticians towards frequency curves can be used - how
shall I describe it? - as one of the strands which link together much of
the historical development of mathematical statistics during the 50 years
1890-1940. May-be also you will like to hear something on these changes
first-hand; for I came early enough into the statistical field to share in
some of this development and to recapture a little of the earlier excite-
ment of the 1890's, when the foundations of modern statistical theory were
really being layed.

Let me make two points before I start: one is that when you are
hearing an account of the development of ideas and methods you must not,
with hind-sight, think: "Oh that should have been obvious!" One of the
interesting things that one finds in studying the history of science is
with what difficulty, often by trial and error, the human mind arrives at
something really new.

My second point is a minor one; I shall have to refer a certain
amount to my father, and as it becomes a little embarrassing to be saying
frequently 'Karl Pearson' or 'my father', I shall sometimes call him more
impersonally by his initials, "K.P.", a style commonly used by his con-
temporaries.

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*This 'lecture' is based on talks given at several statistical centres in
 the United States, during March and April 1969.

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I suppose that my serious reading of K.P.'s early papers, mostly published in the Philosophical Transactions of the Royal Society, began some 50 years ago, just after the end of World War I. Undoubtedly I was immensely stimulated by them, as others of an earlier generation have told me that they were. R. A. Fisher, I think, was reading these papers in the years 1912-15; he, also, was influenced and, possibly, drawn into statistics from what he found there and began to believe he could improve upon.
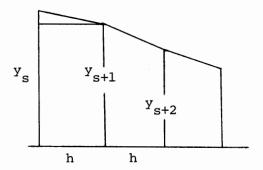
Let me start by running quickly through the early history of the statistician's fundamental distribution, the Normal curve. We know that De Moivre first used it in 1733 to provide a mathematical approximation to the binomial; he did not use it specifically as a probability distribution function of a continuous variable, but the link with the binomial is of interest because of later developments. The curve was also used by Laplace as an approximating function, but in his hands and particularly those of Gauss it formed the centre-point of the Theory of Errors. Much of this work was theoretical and it was possibly Bessel in about 1820 who was the first person actually to compare the curve with a real distribution of errors.

During the 1820's the Belgian, Adolphe Quetelet (1796-1874), who wished to establish an observatory in Brussels, put himself in touch with most of the great European mathematicians and astronomers. Rather later in life his interests turned towards demographic and vital statistics and he was able to show that the error curve also fitted certain physical measurements made on man. He was primarily interested, however, in mean values and propounded the idea of l'homme moyen, the average man who possessed all the mean characters of his race; variations about the average he ascribed to 'accidental causes' which I do not think he attempted to explore.

the symmetrical binomial, he took a binomial with $p = 1 - q \neq q$, and derived a curve $y = f(x)$ by equating the (slope)/(ordinate) ratio, $\frac{1}{y} \frac{dy}{dx}$, to a similar ratio found from the binomial polygon, viz. from

$$\frac{y_{s+1} - y_s}{h} \div \frac{1}{2}(y_s + y_{s+1}) \ .$$

Fig. 2

In this way (in the autumn of 1893) he obtained as a limit the differential equation

$$\frac{1}{y} \frac{dy}{dx} = \frac{-(x - a)}{b_0 + b_1 x} \ , \tag{1}$$

with its solution

$$y = y_0 e^{-x/c} \left(1 + \frac{x}{d}\right)^m \ . \tag{2}$$

Next, he went further than this and applied the same treatment to the hypergeometric distribution, and reached the more general equation

$$\frac{1}{y} \frac{dy}{dx} = \frac{-(x - a)}{b_0 + b_1 x + b_2 x^2} \ . \tag{3}$$

Although he made some attempt, following tradition, to relate the underlying hypergeometric to a system of correlated, elementary components,

I think it may be said that to all intents and purposes he now dropped the basis of the derivation of equation (3), and started afresh from this point.

The solution of (3) (Pearson, 1895, 1901) assumed different forms according to the values of the four parameters, which could be shown to be functions of the first four moments of the distribution, $y = f(x)$. In particular, if the variable x was standardised in the form

$$X = (x - \text{mean})/(\text{standard deviation}) ,$$

the parameters in the solutions of (3) depended only on the standardised 3rd and 4th moments; these he took in the form

$$\beta_1 = \left(\frac{\mu_3}{\sigma^3}\right)^2 , \beta_2 = \frac{\mu_4}{\sigma^4} . \tag{4}$$

Why did he use β_1 , not $\sqrt{\beta_1} = \mu_3/\sigma^3$? For practical convenience in the solution and representation.
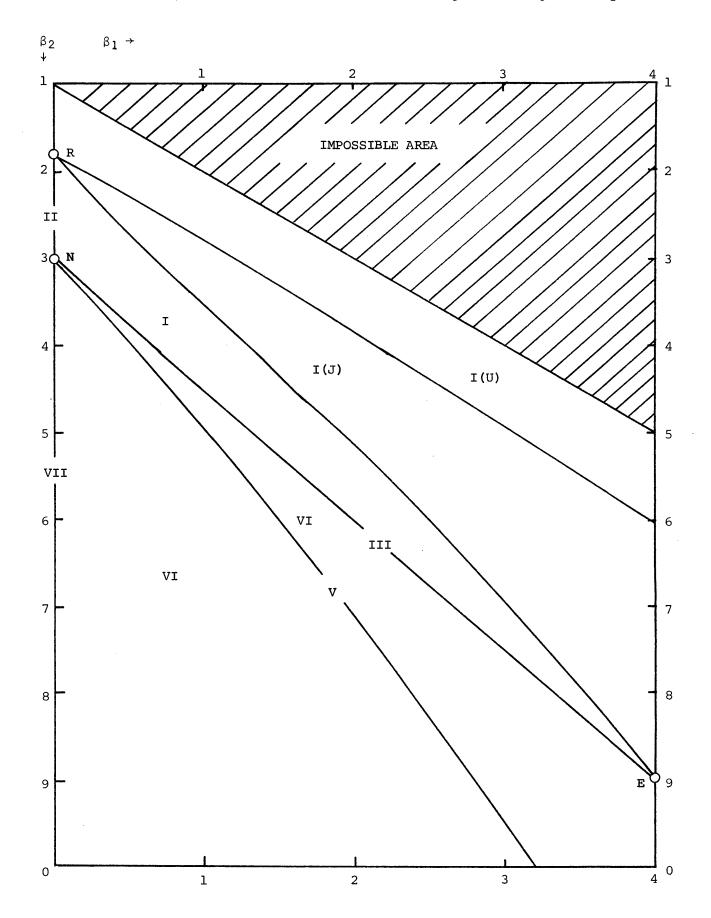
As a result, if rectangular coordinate axes $O\beta_1$, $O\beta_2$ are taken, the field is divided by certain lines into a series of regions (see Fig. 3), the bounding lines and intermediate areas being each associated with a particular form of solution to (3). The seven main types were given numbers I-VII, roughly in the order he discovered them. We might now prefer a clockwise ordering of numbers, but this would now lead to immense confusion. The result was as surprising as it was elegant. It is immaterial

(a)   whether we now choose to take the coordinates as
      $\gamma_1 = \sqrt{\beta_1}$ and $\gamma_2 = \beta_2 - 3$,

(b)   to use a new terminology, e.g., to speak of gamma, beta, inverted beta, t-distributions and so on.

The effectiveness of the system lies in this very comprehensive classification of forms of distribution, in terms of two shape parameters.

FIGURE 3.   PEARSON-TYPE CURVE REGIONS IN β_1 , β_2 PLANE.

Special Distributions: N=Normal; R=Uniform or Rectangular; E=Negative Exponential.

Of course there can be and are other systems. There are the Gram-Charlier or Edgeworth expansions, but if one goes only as far as the 4th moment, the Pearson system has a great advantage. It will only provide a single mode (except in the case of U-shaped distributions) and it will give no negative ordinates.

During the 1890's Pearson fitted his curves to a great variety of data, drawn from biological, medical, economic, meteorological and other sources. On the whole he obtained extremely good fits, although it was not until he developed the χ^2 test in 1900 that goodness of fit could be adequately examined. It was, perhaps, inevitable that during the next 30 to 40 years his students, in their numerical classes, had to go through the hard labour of curve fitting. I am sure there was some advantage in this hard labour, as a training in accurate computing (which meant using a hand calculating machine) and there was certainly an intellectual satisfaction in finally plotting the ordinates of the curve against the histogram of the data.

But I think that it gradually became clear that the occasions when there was any practical advantage in fitting a curve to observed data were less than expected. Later, of course, in 1922, the criticism was raised by R. A. Fisher that the method of fitting by moments was often inefficient, although the practical significance of this inefficiency has still not been fully cleared up. But it so happened that other uses for these curves, other links with statistical theory, appeared over the horizon and that these were intimately connected with the development of statistical theory.

I am reminded of a remark which J. B. S. Haldene made about K.P. in a centenary address of 1957, in a rather different connection. Speaking of some of his work on heredity, Haldane said:

'I believe that this theory was incorrect in some fundamental
respects. So was Columbus' theory of geography. He set out
for China and discovered America! But he is not regarded as a
failure for this reason.'

In the case of the frequency curves, it was not that they were incorrect
but that the original purpose seemed to become less relevant to current
statistical practice.

The first step in a new direction must undoubtedly be associated
with the visit of W. S. Gosset ('Student') to K.P.'s Biometric Laboratory
during two terms of the 1906-7 university session. At some date in the
1890's Arthur Guinness Son & Co. Ltd. initiated the policy of appointing
to their staff in Dublin a number of university trained scientists. These
young men found before them an almost unexplored field lying open for in-
vestigation. Because the idea was novel to industry, the firm did not want
their rivals to be aware of what they had done, and for this reason all
published work had to appear under a pseudonym. Hence the name 'Student'.
Two later Guinness statisticians dubbed themselves 'Mathetes' and 'Sophister'.

Gosset was primarily a chemist, but having taken a year of Mathematics
at Oxford, was put onto the interpretation of the Brewery's extensive records,
both from the experimental brewery and from country-wide breeding experiments
with barley. He had only at first available text-books on the theory of
errors - Airy's and Merriman's, for example. In one direction he was search-
ing for a measure of the relationship between two or more variables, having
not come across the biometric work on correlation. Indeed, given a little
more time, it seems likely that he would have re-discovered the correlation
coefficient using an approach entirely different from Galton's. In another
direction he was not happy about referring the ratio, $(\bar{x} - \mu)\sqrt{n}/s$, to the
normal probability scale when a sample contained only five or 10 observa-
tions, as had to be the case in much of the Brewery experimentation.

With K.P.'s guidance, which he acknowledged fully, he therefore carried out a programme of research involving the following steps.

(a) Determining the 3rd and 4th moments of the sample estimate of variance, s^2 , (the 1st and 2nd moments were already known).

(b) Finding that the resulting moment ratios $\beta_1(s^2)$, $\beta_2(s^2)$ fell, for all sample sizes n , on the Type III, gamma or χ^2 line of the Pearson system.

(c) Hence inferring that the distribution of s^2 was <u>likely</u> to be represented by a Type III distribution.

(d) Proving that $\bar{x}$ and s^2 were uncorrelated in normal samples and hence, using (c), deriving the distribution of $z = (\bar{x} - \mu)/s \; (= t/\sqrt{n})$. This followed a Pearson Type VII curve.

(e) Carrying out a random sampling experiment, with n = 4 , and so satisfying himself that the distributions which he had derived were likely to be correct. He took as his population a bivariate distribution of physical measurements on 3000 men and wrote these on small cards. Then he drew cards randomly in succession from a box, wrote the entries down in order and divided them consecutively into 750 samples of 4 .

His famous paper 'On the probable error of a mean' appeared in <u>Biometrika</u> in 1908. In the same year he published another paper in which he used a similar intuitional process to infer that the distribution of the correlation coefficient, r , in samples of size n from uncorrelated normal material would be of the form

$$f(r) = \text{const.} \; (1 - r^2)^{\frac{n-4}{2}} \; ,$$

i.e., a Pearson Type II distribution. Again, he compared his theory with sampling results for n = 4 and 8 .

Now if we are concerned simply with priorities, we may note:

(a)   that in Germany, in the field of error theory, Abbe (1863) and Helmert (1876) had already derived the distribution of s^2 (although this work was quite unknown in England);

(b)   that Edgeworth in 1883, using prior probabilities, had derived a posterior distribution for the population mean, μ , given $\bar{x}$ and s , which was essentially Student's distribution.

(c)   that Edgeworth, in 1885, had also taken some samples from a rectangular distribution to show how quickly the mean tended to be normally distributed;

(d)   that Weldon had tossed dice in the 1890's to explore the nature and extent of sampling variation.

But this earlier work, apart perhaps from that of Weldon, was not closely related to the solution of practical problems and fell outside the main line of development of statistical theory. For 5-10 years little notice, either, was taken of Gosset's work, outside the Dublin Brewery. This I think was due to a number of reasons, which it is of interest to record briefly.

(a)   The early biometric work of Galton, Weldon and Pearson had not been concerned with laboratory experiments, but with collecting as large samples as possible 'in the field'. If you look through these early papers, as I have done recently, you will see that the analysis of data would rarely have gained from the use of any 'small sample' technique.

(b)   I suspect, also, that the early biometricians had a marked distrust of using small samples because of an instinctive feeling that much data were not homogeneous, either in space or time; that there would be small local variations which must be balanced out by collecting as large a sample as possible.

(c)   Also there was a genuine fear that by advocating small-sample
        techniques, the biologist or medical man would think that he
        could draw conclusions, in fact unwarranted, from scanty data.

Of course, with hindsight we can criticize this attitude and say that small-
sample theory was required to analyse such lack of homogeneity. But this
concept was not yet born in the early 1900's.

However, Gosset had planted several seeds in a place where they were
bound to grow, sooner or later. With regard to the history of frequency
curves, he had shown how the Pearson system might be used to provide approx-
imations (if not the true forms) to the sampling distributions of statistics
whose moments only were known. This may be described as the second great
use for these curves. He had also introduced the idea of making use of sys-
tematic random sampling to test, confirm or disprove mathematical theory.

Five or six years later, Student's work was followed up by R. A. Fisher
who in 1912, a few months after graduating in Cambridge, derived the distri-
butions of s^2 , and therefore of t , by an appeal to n-dimensioned space - a
brilliant piece of imagination. Shortly afterwards he derived the sampling
distribution of the correlation coefficient, r , for normal material and
confirmed Student's guess about the distribution of r , when $\rho = 0$. All
these results were published in <u>Biometrika</u> in 1915. By 1924 Fisher had
shown that if χ_1^2 and χ_2^2 were two independent "χ^2 's", then χ_1^2/χ_2^2 followed
a Pearson Type VI and $\chi_1^2/(\chi_1^2 + \chi_2^2)$ a Type I distribution. There was no
difficulty, of course, in deriving these last two distributions, but no
one had seen the need for them before Fisher had the simple but fundamental
idea of comparing two independent estimates of variance, through their ratio.
Student had in effect done this, but only in a particular case.

Thus entirely unexpectedly six of the seven main curves of K.P.'s
system had been placed right at the centre of the newer statistical theory.

Pearson's system of curves had been first developed because so many parent distributions had been found not to be normal. What about the constant claim that the new tests alone were 'valid'? To some extent there were mathematical tools to probe this question of 'robustness'. The moments of $\bar{x}$ and s^2 were known in terms of population moments but not those of r, t, the variance ratio and so on.

Early in the 1920's the idea occurred of using random sampling experiments (Monte Carlo methods) to test these points, taking as the parent population not the normal distribution but some non-normal distribution of K.P.'s system. Here we had a third use for the curves. This work was encouraged by Gosset who, with his sound practical sense, was undoubtedly a little sceptical about claims of 'validity' without accompanying evidence. The problem of drawing random samples at first presented some difficulties. Gosset's 1906 use of slips of cardboard was laborious and it was difficult to ensure proper mixing. Church, working in the Biometric Laboratory at University College in 1924-25 at first used coloured glass beads, a different colour to represent each group in the population distribution. These could be easily mixed, but commercial coloured beads were not all of a size, and bias was introduced.

Then Tippett, who worked in the Laboratory in 1923-25, had the idea of using random numbers of which he made a standard table by selecting laboriously from census returns. He used these random numbers (1925) to check his approximations to the sampling moments of the extremes and the range in samples from a normal population. Apart from this particular application to which I shall refer again in a moment, the existence of this set of 10,000 four-figure random numbers, published in 1927, increased enormously the facility for examining the sensitivity to departure from normality of various 'normal theory' tests. Just as the introduction of

the Brunsviga calculator in 1894 had made an enormous simplification in statistical computation, so Tippett's Numbers provided the answer in the case of what we now term Monte Carlo investigations.

Besides the work of Church (1926) and Storey ('Sophister')(1928) I had a programme which involved taking random samples of 5, 10 and 20 from five or six non-normal populations, represented by Pearson curves. We were thus beginning to get some idea of the limits of the 'validity' claimed for Fisher's different tests; the 'robustness' varied considerably. This laborious work of the late 1920's would, of course, be very greatly speeded up now using a computer to generate pseudo-random numbers.

Turn back now for a moment to the distribution of the range, ω . Gosset suggested it be used to provide quick estimates and tests of variability in place of the standard deviation. Using some rather more accurate estimates of the moments, which I had obtained in 1926, he assumed that we might represent the unknown true distribution of ω by Pearson curves with the correct first four moments; so, in 1927, he published a table of approximate upper 10, 5 and 2 per cent points of ω for $n = 2(1)10$. Later (1932) I extended this work, providing four upper and four lower percent points for $n = 2(1)30(5)100$. When accurate values were obtained in 1942 up to $n = 20$, by direct computation, the corrections needed were very small. This was one of the most successful examples of how, what I have termed the second function of the frequency curves, could be turned to account long before the arrival of the computer age.

Other examples of this type of application were made at this time and because they were closely linked with some important theoretical developments, it is worth mentioning them here. In 1931 Jerzy Neyman and I published our paper termed 'On the problem of k samples' in a Polish journal.

Here we used the likelihood ratio principle to derive a test of homogeneity

of variance. This was essentially what has since been termed 'Bartlett's

test'. We found its sampling moments and approximated its distribution with

a Type I (beta) curve. That same year I gave a summer session course in

the University of Iowa where Sam Wilks was completing a thesis under H. L.

Rietz. Part of this thesis, deriving some fundamental tests in multivariate

analysis, for which I think he will have got some ideas from Harold Hotelling,

was published in Biometrika in 1932. Wilks showed that his most important

test criteria were likelihood ratio criteria, and gave their moments. In

a joint paper of 1933, Sam and I took the case of two variables and applied

the tests to some examples, using Type I curves to find the significance

levels. Thus the curves provided a tool in developing tests derived from

the likelihood ratio principle.

But there was another lead from the curves into statistical theory.

We were accumulating alternative tests regarding:

(1)   the population mean, i.e., those based on the sample mean, median
       and mid-range;

(2)   the population standard deviation, i.e., those based on the root-
       mean-square, the mean deviation and the range.

Also we had the possibility of using Tippett's Random Numbers to find their

sampling distributions when the parent population was not normal. This

almost inevitably suggested to us in the years 1929-31 the idea of studying

the comparative power of alternative tests under various conditions. The

theory of this method of approach was discussed by Neyman and myself in

our paper of 1933. These examples could easily be extended, but I only

want to indicate how these different lines of attack were interlinked.

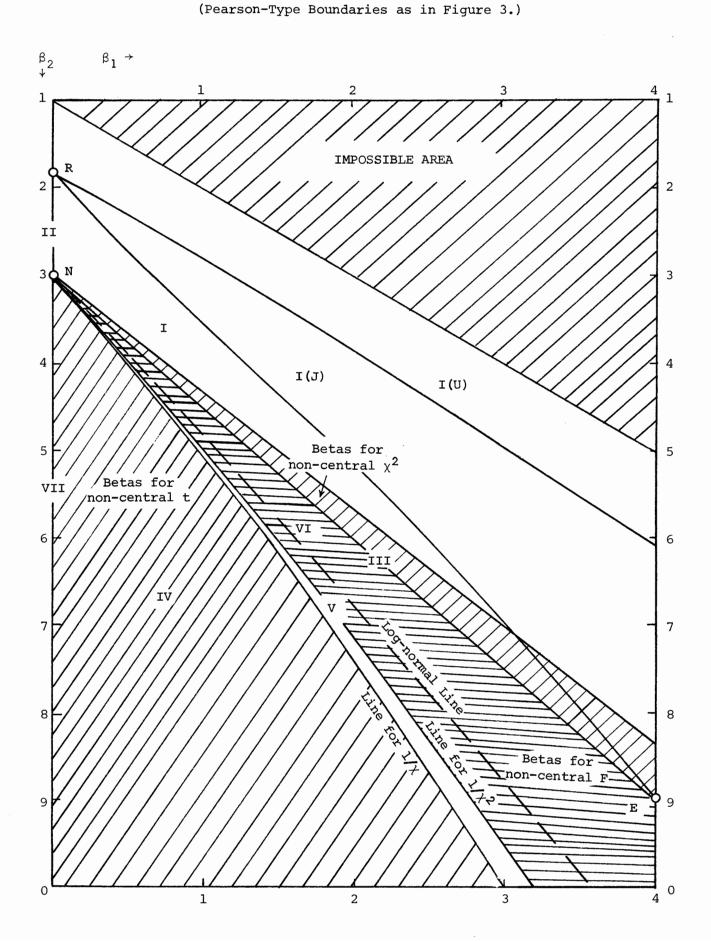Now let me come to a few last points. In his remarkable paper of

1928 Fisher had derived the distributions (in sampling normal material) of:

(a) Multiple R (when $\rho \neq 0$) .

(b) Non-central $\chi^2 = \sum_{i=1}^{\nu} (X_i + a_i)^2$.

(c) Non-central F = non-central $\chi^2(\nu_1)$/central $\chi^2(\nu_2)$.

(d) A little later Fisher (1931) had made use of the probability integral of non-central $t = (X + \Delta)/\chi/\sqrt{\nu}$, a function which Neyman was to make use of independently not long afterwards in Poland.

These multiparameter distributions could clearly assume a great variety of shapes, but did not (as their central counterparts) follow exactly distributions of the Pearson system. (b), (c) and (d) have been turned to use and tabled in connection with power functions. Tables of percentage points are also available for (b) and (d). But while it may be only of academic interest now, I have always wanted to relate the Pearson system to these non-central distributions if only as approximations. In the last few years it has become clear that we can, in fact, get surprisingly good approximations to (b), (c) and (d) by using a Pearson curve with the correct first four moments. The position is as shown in Figure 4. The beta points for non-central χ^2 lie in a narrow wedge-shaped region above the Type III or central χ^2 line; those for non-central F lie in the Type VI or inverted beta area, with a lower limit corresponding to the Type V or reciprocal of χ^2 line; while those for non-central t fall in the Type IV area, with an upper boundary on the line corresponding to the distributions of the reciprocal of χ .

Finally with the cooperation of several others it was possible to issue (Johnson et al, 1963) a form of table which it had long seemed to me

FIGURE 4. β_1, β_2 REGIONS ASSOCIATED WITH NON-CENTRAL DISTRIBUTIONS.

(Pearson-Type Boundaries as in Figure 3.)

$\beta_2\downarrow$ $\beta_1\rightarrow$

IMPOSSIBLE AREA

R

II

N

I

I(J)   I(U)

Betas for
non-central χ^2

Betas for
non-central t

VII

VI

III

IV

V

Log-normal Line

Line for $1/\chi$

Line for $1/\chi^2$

Betas for
non-central F

E

would be valuable, namely a table of some 15 percentage points of Pearson

curves, expressed in standard measure,

$$\text{i.e. by } X = (x - \mu)/\sigma \quad,$$

and entered with $\sqrt{\beta_1}$ and β_2 .

Apart from the fact that the table has already been used by several

persons to derive approximate percent points of distributions for which the

moments only are readily available, it acts as a kind of standard of com-

parison. We have been able to compare a number of distributions with those

of the Pearson system, using the same first four moments, e.g., the log-

normal (see Fig. 4), Johnson's S_U and S_B curves (which transform into the

normal and whose beta points lie, respectively below and above the log-

normal line), the Weibull distribution (betas in Type I area), the non-central

distributions I have just referred to and so on. There is generally pretty

good agreement, even as far out as the 0.5% points, particularly at the

long drawn out tail. Of course the distributions of these different systems

will not have the same moments beyond the fourth, but such differences have

their influence mainly in the extreme tails. One of the most striking things

is agreement at the 5% points. As I have said, I am not sure of the practi-

cal importance of some of these results and relationships now, today, when

we have the electronic computer. Nevertheless, the study has always fas-

cinated me and, as an editor, I found on a number of occasions that my con-

tributors would have made things clearer, indeed often have understood more

the meaning of their results, had they had a greater familiarity with some

of the long established properties of frequency curves.

All this has led me a long way from Galton's 'law of frequency of

error' which the Greeks, had they known of it, would have deified! How

far statisticians have moved in the last 80 years is indeed well brought

out by my final quotation from Galton's <u>Natural Inheritance</u>:

> 'It is difficult to understand why statisticians commonly
> limit their inquiries to Averages, and do not revel in
> more comprehensive views. Their souls seem as dull to the
> charm of variety as that of the native of one of our flat
> English counties, whose retrospect of Switzerland was that,
> if its mountains could be thrown into its lakes, two
> nuisances would be got rid of at once.'

<div align="right">(p. 63)</div>

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DOCUMENT CONTROL DATA - R & D

(Security classification of title, body of abstract and indexing annotation must be entered when the overall report is classified)

1. ORIGINATING ACTIVITY (Corporate author)	2a. REPORT SECURITY CLASSIFICATION
	UNCLASSIFIED
SOUTHERN METHODIST UNIVERSITY	2b. GROUP
	UNCLASSIFIED

3. REPORT TITLE

Some Historical Reflections Traced Through the Development of the Use of Frequency Curves.

4. DESCRIPTIVE NOTES *(Type of report and inclusive dates)*
Technical Report

5. AUTHOR(S) *(First name, middle initial, last name)*

Egon S. Pearson

6. REPORT DATE	7a. TOTAL NO. OF PAGES	7b. NO. OF REFS
July 3, 1969	23	26

8a. CONTRACT OR GRANT NO.	9a. ORIGINATOR'S REPORT NUMBER(S)
N00014-68-A-0515	
b. PROJECT NO.	38
NR 042-260	
c.	9b. OTHER REPORT NO(S) *(Any other numbers that may be assigned this report)*
d.	

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11. SUPPLEMENTARY NOTES	12. SPONSORING MILITARY ACTIVITY
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13 ABSTRACT

This paper is mainly concerned with the uses made of Karl Pearson's system of frequency curves, in particular with developments originating in the Department of Statistics at University College London. After a brief account of the history of the Normal curve from the time of DeMoivre to Francis Galton, Pearson's derivation of his system of curves from a single differential equation is described. The system was originally intended for the graduation of observed frequency distributions which the Normal curve would not fit. But since Student's pioneer work on the distributions of s^2, t and r (when $\rho = 0$) the curves have been used again and again to approximate the sampling distributions of statistics whose moments only are known. They have also been used in Monte Carlo experiments to examine the distribution of statistics based on samples taken from representative non-normal populations. The report illustrates some of this work and discusses conclusions to be drawn from it.

DD FORM 1473
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