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A letter across two centuries

Reverend

Thomas Bayes

Little Mount Sion

Tunbridge Wells, England

Prague, April 16th, 1991

Dear Reverend Bayes:

First of all, I feel myself obliged to beg your kind pardon for the boldness with which an unknown and insignificant statician of our twentieth century, who only applies the great ideas of yours to concrete situations, is addressing himself to you - the founder of the scientific methods of analyzing experimental data. However, it may be of interest to you to know what your last paper had caused in the development of the methods of statistical analysis and how it can, even nowadays, help to a statistician in practical situations.

Two years and a half after your retreat to the Kingdom of the Almighty, exactly in November 1763, a friend of yours, Mr. Richard Price, sent your paper "An Essay towards Solving a Problem in the Doctrine of Chances" – unpublished before then - to the Royal Society, together with a beautiful letter of recommendation in which he had briefly stated

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the purpose of your treatise, viz. " to find a way by which it would be possible to draw some conclusions regarding the probability with which, under certain given conditions, an event would occur about what nothing else is known that the number of times it had occurred in a given number of independent experiments performed under the conditions given". Mr. Price, in his letter to the Royal Society, writes:"Any judicious person will certainly be aware that this problem is not a mere puzzle in the field of probability theory, but that it constitutes a problem which must be solved in order to give a solid basis for any appraisal of past experience and of the prediction what can follow them." And in another place: "It is sure that we cannot determine the point up to which repeated experiments confirm a certain conclusion, without having studied profoundly the problem mentioned above, that anybody who would want to express clearly and exactly the force or credibility of analogue or inferential judgements must cope with."

I believe that your "Essay ..." together with the words of Mr. Price just quoted, constitute the beginning of a branch of statistical art (or should we say handicraft?) that is called statistical inference nowadays. Only a few of statisticians and mathematicians working in probability theory today have read the original version of your treatise. Actually we interpret its contents as follows:

If nothing is known about some event, before performing any experiments or observations, any value of its probability is "equally well possible" for us. This fact is expressed by the assumption that the probability of the event in question is a realization of a random variable distributed uniformly over the interval $(0,1)$. If n independent experiments are performed in each of which the event has the probability p, then the number of occurrences of this event has the binomial distribution

$$P(X=x \mid p) = \binom{n}{x} p^x (1-p)^{n-x}, \quad x = 0,1,\ldots,n.$$

Regarding p as a number randomly chosen in the interval $(0,1)$ according to the uniform distribution, the total probability of the event $X = x$ equals

$$P(X=x) = \int_0^1 \binom{n}{x} p^x (1-p)^{n-x} dp = \binom{n}{x} \frac{x!(n-x)!}{(n+1)!}.$$

According to a theorem - that you have never enounced explicitly in the form in which it is known today, but related to your problem and later on, in honour of you, received your name - the conditional distribution of p given $X = x$ is

$$h(p \mid X = x)dp = \frac{P(X = x \mid p).h(p)dp}{P(X = x)} = \frac{1}{B(x+1, n-x+1)} p^x (1-p)^{n-x} dp.$$

The unknown probability p of the event under study is then estimated, on the basis of the given result $X = x$, as the mean value of the conditional distribution $h(p \mid X = x)$, i.e. as

$$\tilde{p} = \frac{(x+1)}{(n+2)}, \quad x = 0, 1, 2, \ldots, n.$$

It is worthwhile to note that this result has a desirable property which our contemporaneous procedures do not posess: in the cases that $X = 0$ or $X = n$,respectively, the estimate does not yield the values 0 or 1, respectively, under which the distribution would "degenerate" and which are, up to a certain point, in contradiction with "common sense": if, e.g., in a series of $n = 10$ experiments $X = 0$ occurrences of an event were observed, we scarcely would conclude that the probability of this event is $p = 0$.

The procedure just described, derived from your brilliant treatise, has predetermined the development of the art (or the office) of "statistical inference" for a period of more than one entire century. It was applied to many problems and only you yourself could be able to judge whether all those applications had been always in accordance with your line of thinking and whether they would obtain your approval. The general scheme of all these applications was the following:

An experiment is performed whose result has a distribution $f(x;\theta)$ dependent on an unknown number θ (called "parameter" today). Here $f(x;\theta)$ denotes the probability of the outcome $X = x$ or, eventually, the probability density function of X in the point x. To obtain an estimate of θ on the basis of the result $X = x$, a suitable distribution $h(\theta)$ on the set of all possible values of θ is chosen; this distribution should express or describe the state of our knowledge (or our conjectures about the possible value of θ) prior to the performance of the experiment, just like your choice of the uniform distribution over the interval $(0,1)$ for the possible values of the probability p of an event. Then the distribution of conditioned by the given result $X = x$ say $h(\theta|x)$ is found and the parameter θ is estimated by the mean value of this latter distribution, say as

$$\tilde{\theta} = E[\theta|X = x] = \int \theta h(\theta|x) d\theta.$$

Of course, your idea had not only enthusiast adherents and users, but also ferocious adversaries. In the beginnings of the present century another great statistican R.A. Fisher wrote that he " did not know of other mathematical technique which such contradictory opinions would exist about, which would be accepted and recognized by some and categorically refused by others like the procedure derived from your "Essay ...." He (R.A.Fisher) himself suggested a way to avoid the necessity to introduce any "probability distribution for θ " and to base the estimate solely and exclusively on the result of the observation; his ideas resulted in considering $\hat{\theta}$ as an estimate for θ, given the outcome of the observation $X = x$, that value $\hat{\theta} = \hat{\theta}(x)$, under which the given result $X = x$ would have the largest probability (or, eventually, the largest density of probability). This is the method used perhaps most frequently now, with eventual slight modifications; it is called the method of maximum likelihood.

Another procedure frequently applied is the so called method of moments. It consists in choosing as the estimate of the parameter θ that value $\bar{\theta}$, under which the mathematical expectation of the random variable that is observed is equal to the average of n independent observations, i.e.

$$\int x f(x;\bar{\theta})dx = \bar{x} = \left(\frac{1}{n}\right)\sum_{i=1}^{n} x_i.$$

A large part of the theory of estimation (in fact initiated by you in your "Essay...") is based on the minimization of the so called mean square error, i.e. on the search for a function $\theta^* = \theta^*(x)$ of the observations such that the expectation of the squared deviation of θ^* from the true value θ is minimal, i.e.

$$E\left[(\theta^*(X) - \theta)^2\right] = min;$$

since a function $\theta^*(X)$ satisfying this requirement uniformly in θ usually does not exist, additional requirements are imposed; most frequently we require the so called unbiasedness, i.e.

$$E\left[(\theta^*(X)\right] = \theta$$

for all θ. I will not continue to survey all the approaches to the problem; there would be too many of them and we never would come to the concrete example that I mentioned at the beginning of this letter and that gave the original impulse to write it. Let me only state one important fact: your approach to the problem of determining (or, as we say now, of estimating) an unknown parameter was based on the distribution of the parameter conditioned by the given result of the observation, assuming that the parameter itself is a realization of a random variable with a certain distribution; the majority of statisticians up today are rather accustomed to judge the quality of estimates and to construct the estimates on the basis of their distribution under different values of the parameter. I think that it is no exaggeration to say that more than one half of statisticians engaged in applications do not use the methods of estimation derived form your approach; many of them even do not know them, others consider them more or less a historical curiosity (and, to be complete, the more theoretically oriented use them as a part of their equipment to prove other properties of estimators).

However, there are situations in which exactly your original approach may be useful to find an estimate acceptable to"common sense" and at the same time justified by a sound mathematical argument. One such problem - originated by practical activity, not artificially constructed - is this. Let $X_1, X_2, \ldots, X_n$ be mutually independent random variables with exponential distribution

$$f(x;\lambda) = \lambda.exp(-\lambda x), \quad x > 0,$$

where λ is an unknown positive parameter. Further let $t_1, t_2, \ldots, t_n$ be given positive numbers. The variable X_i $(i = 1, 2, \ldots, n)$ can be observed only if it assumes a value smaller than t_i ; otherwise only the fact $X_i \geq t_i$ is known. Thus, in fact, we observe independent random variables

$$Z_i = min(X_i, t_i), \quad i = 1, 2, \ldots, n.$$

The variables Z_i have a somewhat curious distribution; their distribution functions are

$$F(z_i;\lambda) = \begin{cases} 0, & \text{for } z_i \leq 0 \\ 1 - exp(-\lambda z_i), & \text{for } 0 < z_i < t_i \\ 1, & \text{for } z_i \geq t_i \end{cases}$$

That means that Z_i has in the interval $(-\infty, t_i)$ an absolutely continuous distribution with probability density

$$f(z_i;\lambda) = \begin{cases} 0, & \text{for } z_i \leq 0 \\ \lambda.exp(-\lambda z_i), & \text{for } 0 < z_i < t_i \end{cases}$$

and takes the value t_i with probability

$$P_\lambda(Z_i = t_i) = exp(-\lambda t_i).$$

If we try to estimate the parameter by the method of maximum likelihood, we have to maximize the function

$$\begin{aligned} L(\hat{\lambda}) &= \hat{\lambda}^r.exp(-\hat{\lambda}\sum_{i=1}^{n} z_i) \\ &= \hat{\lambda}^r.exp[-\hat{\lambda}(\sum_{i\in A} X_i + \sum_{i\in \bar{A}} t_i)] \end{aligned}$$

where $A = \{i : X_i < t_i\}$, $\bar{A} = \{i : X_i \geq t_i\}$ and r equals to the number of observations X_i smaller than t_i. The resulting estimate is

$$\hat{\lambda} = \frac{r}{\sum_{i=1}^{n} z_i} = \frac{r}{\sum_{i\in A} X_i + \sum_{i\in \bar{A}} t_i},$$

whenever $r \geq 1$. But if $r = 0$, i.e. $X_i \geq t_i$ for all $i = 1, 2, \ldots, n$ the function $L(\hat{\lambda})$ is decreasing and the estimate would be

$$\hat{\lambda} = 0 .$$

Such a result naturally disappointed the experimenters; the experiment was a medical one, X was the time of survival after a certain diagnosis and λ the so called rate of mortality. The value $\lambda = 0$ would imply $F(x; \lambda) = 0$ for any x, i.e. the survival time arbitrarily long with probability 1. Besides, the estimate $\hat{\lambda} = 0$ does not take in account the values t_i, the lengths of the time periods during which the patients could be followed up. With low values of the t_i's it would be absurd to infer, from the fact that n patients survived their respective t_i's, a "practically infinite" survival time. The authors of the study referred to solved this problem by putting, for $r = 0$,

$$\hat{\lambda} = \frac{1}{2\sum_{i=1}^{n} t_i} .$$

The choice of the coefficient 2 was justified by a considerably entangled (with less courtesy, completely senseless) argument, so that it may be considered arbitrary, justifiable only by the fact that the resulting value of the estimate lies midway between the value corresponding to $r = 0$ and $r = 1$ with some of the X_i's "just below" the corresponding t_i's.

The method of moments is not of much use either. The expected value of Z_i is

$$E(Z_i) = \frac{1}{\lambda}(1 - e^{-\lambda t_i})$$

so that the expectation of the average $\bar{Z} = \frac{1}{n}\sum Z_i$ is

$$E(\frac{1}{n}\sum_{i=1}^{n} Z_i) = \frac{1}{n\lambda}\sum_{i=1}^{n}(1 - e^{-\lambda t_i}) .$$

The average $\bar{Z}$ of the Z_i values takes the values from the interval $(0, \bar{t}_i = \frac{1}{n}\sum t_i)$.

The left-hand side of the equation

$$E(\frac{1}{n}\sum Z_i) = \bar{Z}$$

is a decreasing function of λ converging to 0 with $\lambda \to \infty$ and to $\bar{t}$ with $\lambda \to 0$.

Thus there exists a unique solution of the equation, say, $\bar{\lambda}$ but this solution is in the region of (reasonably) acceptable values of λ (viz., $\lambda > 0$) only if at least one of the X_i's is less than the corresponding t_i; otherwise $\bar{\lambda}$ is equal to 0.

One solution of this embarassing situation, i.e. an estimate mathematically well justifiable and at the same time acceptable to "common sense" and practical experience, is offered by your method. Parting from the assumption of "absolute a priori ignorance" of the parameter λ we suppose that it is a realization of a random variable distributed uniformly over the interval $(0, a)$, i.e. with the density

$$h(\lambda) = \begin{cases} 0, & \text{for } \lambda \leq 0 \\ \frac{1}{a}, & \text{for } 0 < \lambda < a \\ 0, & \text{for } \lambda \geq a. \end{cases}$$

The distribution of λ conditioned by a given set $z = (z_1, z_2, \dots, z_n)$ of observations then is

$$h\Big(\lambda|z\Big) = \frac{\frac{1}{a}\lambda^r exp[-\lambda(S_A + S_{\bar{A}})]}{\int_0^a \frac{1}{a}\lambda^r exp[-\lambda(S_A + S_{\bar{A}})]d\lambda},$$

where we have, for simplicity of symbolism, put

$$S_A = \sum_{i\in A} X_i , \qquad S_{\bar{A}} = \sum_{i\in\bar{A}} t_i .$$

Since we have no idea about the upper limit a of the interval of the possible values of λ, we pass to the limit for $a \to \infty$.

Thus we obtain

$$h\Big(\lambda|z\Big) = \frac{1}{\Gamma(r+1)}\lambda^r\big[S_A + S_{\bar{A}}\big]^{r+1} exp\big[-\lambda(S_A + S_{\bar{A}})\big] ,$$

i.e., a gamma distribution with "shape parameter" equal to $r+1$. When $r = 0$, i.e., $X_i \geq t_i$ for all $i = 1, 2, \ldots, n$, this reduces to the usual exponential distribution. Its mean value is equal to

$$\tilde{\lambda} = \frac{(r+1)}{S_A + S_{\bar{A}}}.$$

This is an estimate that has many appealing features: it is always positive (even in the case $r = 0$), so that it avoids the doubts resulting in the case $r = 0$ mentioned above. Besides, it has a sound mathematical basis, precisely that of yours. It is possible that it overestimates somewhat the real value of λ systematically - but this would be a question of applying other criteria of estimation, referred to above. The comparison of the estimator $\tilde{\lambda}$ would certainly be interesting but, unfortunately, the expressions for the expectation, variance, etc. of are very awkward.

Thanking you for the kind attention devoted to these lines, I remain to be an up to now somewhat incredulous (since brought up in the spirit of later theories) but now nearly to your faith converted

statistician of the 20 th century.

Christmas inequality

Jiří Anděl

A year ago I visited Yugoslavia. I am not going to describe the official program of my stay at the University of Belgrade because the unofficial one is usually more interesting. During a supper with my colleagues we were talking about interesting problems and paradoxes in probability and mathematical statistics. Here I describe one of such problems which was new for me. Let $X_1,\ldots,X_n$ be i.i.d. random variables with a positive expectation μ and a finite variance σ^2 . Assume that the random variables are non-negative. Define

$$S_n = X_1 + \ldots + X_n.$$

An elementary well known property of S_n is that

$$var\ S_n = n\sigma^2.$$

It means that the variance of the sum of i.i.d. random variables depends linearly on the sample size n.

Problem. *How does the expression*

$$var\ S_n^{\frac{1}{2}}$$

depend on n*?*

One could expect that $var\ S_n^{\frac{1}{2}}$ is proportional to $n^{\frac{1}{2}}$, or perhaps to $n^{\frac{1}{4}}$. However, the correct answer is rather surprizing because $var\ S_n$ is bounded!!!

Theorem 1. *Under the assumptions given above we have*

$$var\ S_n^{\frac{1}{2}} \leq \frac{\sigma^2}{\mu}\,. \tag{1}$$

Proof is based on the foolowing Theorem 2.

Theorem 2. *Let* S *be a non-negative random variable such that* $ES > 0, var\ S < \infty$ *. Then we have*

$$var\ S^{\frac{1}{2}} \leq \frac{var\ S}{ES\,.}$$

Proof of Theorem 2. The assertion is a special case of results in the paper Banjevič D., Bratičevič D. (1983): Note on dispersion of X^α , Publ. Inst. Math., Nouvelle Série **33** (47), 23-28.

Although the proof of Theorem 2 is elementary, it is rather long. I guess that I should need about one hour to explain it to students. The reason is that the derivation of $varX^\alpha$ is not much simpler when we insert $\alpha = 0.5$.

Since the inequality (1) appeared in our Bulletin during Christmas, let us call it "Christmas inequality".

Proof of Theorem 1. If we define $S = S_n$, then we have

$$var\ S_n = n\sigma^2, \quad ES_n = n\mu$$

and it follows from Theorem 2 that

$$var\ S_n^{\frac{1}{2}} \leq \frac{\sigma^2}{\mu}.$$

However, Dr. J. Á. Víšek from ÚTIA ČSAV , Prague, proposed a very short proof of Thoerem 2. It is based on the two following assertions.

Lemma 3. *Let Y be a random variable with a finite second moment. Then*

$$E(Y - EY)^2 \leq E(Y - a)^2$$

for arbitrary a.

Proof. The inequality follows from

$$E(Y - a)^2 = E(Y - EY)^2 + (a - EY)^2.$$

Lemma 4. *Let $a \geq 0, b > 0$. Then*

$$(a - b)^2 \leq \frac{(a^2 - b^2)^2}{b^2}.$$

Proof. Because $\frac{(a+b)^2}{b^2} \geq 1$, we get

$$(a - b)^2 \leq (a - b)^2 \frac{(a + b)^2}{b^2} = \frac{(a^2 - b^2)^2}{b^2}.$$

Proof of Theorem 2. Combining Lemmas 2 and 3 we can write

$$var\ S^{\frac{1}{2}} = E(S^{\frac{1}{2}} - ES^{\frac{1}{2}})^2 \leq E[S^{\frac{1}{2}} - (ES)^{\frac{1}{2}}]^2 \leq$$

$$\leq E\left[\frac{(S - ES)^2}{ES}\right] = \frac{var\ S}{ES}.$$

A Few Words on Economic Statistics

Jaroslav Jílek

Due to the fact than the term "economic statistics" made itself at home in our part of the world in the mid-fiftiles, we have recently been faced with the question of the future of this discipline. It appears that some of the enquirers were not too clear about the role of this discipline even in the past and so it will perhaps be useful to begin with a little information on this theme.

We took over the concept of economic statistics from the Soviet Union in connection with the concept of national economic planning: there were periods when the main role of economic statistics was considered to be checking on the fulfilment of the state plan. In second place there was the analysis of the state and developmet of the national economy, which usually included social phenomena and processes closely connected with economic phenomena and processes. The analysis and the checking on the fulfilment of the plan naturally presumed a depiction of the reality, i.e. the measurement of the level or extent of changes in social-economic phenomena and processes. This measurement was often influenced by planning practices so that the resulting characteristics were distorted. Thus, for example, in the measurement of production priority went for a long time to indices which were suitable for the specification of volumes of production, so-called gross production. From the state-wide view-point, however, the overall volumes of gross production changed not only under the influence of changes in the extend of production activity in individual organisations, but also under the influence of changes in the intesity of production cooperation between these organizations. Analytical problems were limited to the clarification of the non-fulfilment of planned tasks. Judgement of the overall intentions of economic activity, i.e. first and foremost of the national economic plans themselves, was tabooo because they were under the patronage of supreme political authorities. Directivist management practically excluded the need for complex analysis at lower levels of management, so that enterprise statistics became an empty concept (the filling-in of statistical forms was mainly a matter of copying from the records).

The statistical mirroring of economic and life is among the key tasks of state administration in all the advanced and less advanced countries of the world. At the level of the state this task is usually over by a single office and therefore the term official statistics is used. The knowledge acquired from advanced European countries shows that contemporary official statistics are not merely counting of heads, uncoordinated classification and unsystematic comparisons as official statistics before the 2nd World War was sometimes characterised. The indicators of economic life are governed first and foremost by a system of national accounting in which are impemented internationally agreed standards of the content definition of indices, statistical units and international classifications. Within the framework of the activity of international organisations there occurs not only the amassing, arrangement and publication of internationally comparable indices, but also we find the results of various comparisons and analyses. These results are often acquired with the aid of specific procedures which are different from the traditional approaches of both descriptive and mathematical statistics. Quite irreplaceable now is the role of modern computer and communications techniques. The international importance of the official statistic of individual countries makes it essential that the employees of statistical offices master foreign languages.

Official statistics do not exhaust the tasks allotted to statistics in the sphere of economic and social phenomena and processes. Under the conditions of a democratic pluralist society and a market economy the individual legal subjects also become factually independent, which forces them to find their bearings and make decisions independently. Foreign experience shows that statistical activity is essential for the correct functioning of economic organisations, special-interest (especially enterprising) organisations (unions, trade unions) and other institutions. In certain cases the focal point of activity lies in the independent surveying of, for instance, measurements, elsewhere rather in analytical activity and the making of decisions, which again is unthinkable without good expert knowledge of a subjective nature.

From the above clarification of the former concentration of economic statistics in this country and from the comparison of this trend with the nature of official statistics, enterprise statistics and other statistics in a democratic society it emerges that in this country a number of changes are essential. Whereas formerly the system of economic-statistical indices was culminated by the balance of the national economy, now we must create a system in connection with the national accounts: it is not merely a matter of exchanging with various content definitions, but also the thorough utilisation of sets of statistical units (including their state-wide registers) and international classifications. These changes in methodology are accompanied by penetrating changes in sets of potential information units: whereas at the end of 1989 there were around 40,000 of them, at the end of 1991 there are over a million (registered). At the same time only a half of workers in statistics are capable of becoming promptly acquainted with the alternative standards and experience of their implementation in practice. We nevertheless believe that the statistical offices will master their tasks, partly also thanks to the promised aid from the Office of Statistics of the European Community - EUROSTAT.

The system of selected acquisition of information from the public, formed at present from quarterly-processed statistics on family accounts and microcensuses carried out roughly every four years, must be supplemented by sample surveying of manpower and also in the future by regular income investigation of households (at intervals of about two years) in connection with the methods of surveying in the EEC countries. Is is not only a matter of the fact that the former orientation of state statistics considered the sphere of social statistics marginal, but also in particular that from the former statistics of economic organisations it will not be possible to transfer all the necessary indices to the system of production statistics and especially impossible will be to cover through enterprise or establishment accounting the activity of small private enterpreneurs not entred in the Enterprise Register, or other forms of business at one's own expense. Given existence of hundreds of thousands of economic subjects is not possible to organise exhaustive surveying of numbers of employees. Sample surveying gives (according to foreign experience) more reliable results if respondents are citizens or families and at the same time more complete and more profoundly structured characteristics of unemployment are acquired according to accepted international definitions, than the results which emerge from the files of the labour bureaus. Only in this way is it possible to inquire the role of small private businessmen, especially in the sphere of the provision of services. The sample surveying of manpower should be realised quarterly beginning with the fourth quarter of 1992.

At present, our greatest problems are with the appropriate division of work between the Federal Statistical Office on the one hand and the Czech and Slovak Statistical Offices on the other hand in connection with the Constitutional Law on the Czechoslovak Federation No.556/1990 Sb. (on competencies). The main thing is that the essential methodological unity should not remain a mere declaration, but should be manifest in the results of official statistics (methodology is dealt with by the Federal Statistical Office, collection and processing of data is realised by the Offices of Republics). We also have certain problems with the correct timing in the abandoning of old problems and with the introducion of the new procedures, because they are also closely connected with the alteration of the practices of other bodies of state administration in connection with the consolidation of pluralistic democracy and the market economy. (The ideal parallel course of old and new statistical mirroring over a period of several years comes up against the barriers of budgetary economy.) The Federal Statistical Office has agreed to the provision of information for a charge: partially it is a matter of acquisition or processing of information according to the particular requirements of users. The means acquired by such sale, however, we must divert to the state budget.

So far there is left on one side the newly emerging sphere of enterprise statistics and the statistics of special-interest organisations. Here we see suitable fields for the activity of employees of the Chairs of Statistics of universities and research institutes who, after a thorough study of foreign practice, should develop wide further education activity. I feel that this is the way in which they would best strengthen their own positions in the coming competition for the favour of the wide specialist public and their own ordinary students. Due to the constantly decreasing possibilities of Czechoslovak publishing activity it will be both desirable and possible to reinvestigate the publishing possibilities of our own research institute and of the offices of statistics.

From the above explanation it is clear that economic statistics in the Czech and Slovak Federal Republic are on a certain watershed both as regards practice and theory, with I see as the generation of practice. I would not like to become involved in a discussion on whether economic statistics should be regarded as a science or a theory because I doubt the value of such discussions. Even the purely pragmatical practice of official and enterprise statistics abroad shows, however, that there exists an objective need both for the generalisation of practical experience and for the discussion of the method and results of these generalisations, as well as the discussion of new approaches. As an example, one might mention the Revue of Income and Wealth, the studies of the Munich. Kiel and Berlin Research Institutes, etc. Economic statistics are also part of the study plans of universities as can be seen from a number of European textbooks. I therefore feel that it would be useful for tuition and research and publishing activity in the field to continue in Czechoslovakia. It might perhaps be useful also to express the essential change in former trends formally and speak either of the courses of economic statistics or courses of national accounting and enterprise statistics. Whereas in the courses of national accounting it would not only be a matter of the actual systems of accounting but also the methods of the collection of data for their filling-in and the methods of their analytical utilisation (including the time and international comparison of macro-aggregates, separation of price influences, trends, etc.), in the courses of enterprise statistics it would be more a matter of the application of selected statistical methods in marketing, intake of goods, quality checking, utilisation of capacities, employees, etc. Question concerning social statistics should not be ignored. Most probably they could be clarified in connection with national accounting (the so-called question of satellite accounting or the Social Accounting Matrix). Intentionally I have not mentioned demographical statistics, wich I consider as a separate subject.

If I am to state briefly in conclusion in what I see the focal point of the change from the former economic to future economic statistics, then it lies in the mastering of the principles of modern official and enterprise statistics, on the one hand through translations and on the other hand through attentive adaptation to our conditions, through the development of former domestics and modern foreign procedure.

Statistics statistically evaluate

Stanislav Komenda

"When an experiment is carried out, treatments are imposed and subsequent events are followed, these procedures make causal inference much more direct, but dependence on outside knowledge is unlikely to be wholly absent. The point is quickly illustrated by the experiment in which subjects were given large drinks of whiskey and water, rum and water, or brandy and water and all showed signs of intoxication. It is "outside knowledge" that supports the conclusion that the effect was due to the alcohol, not the water".

1. What is biometrics

"Efficiency" is one of the key words of our time, similarly as in the case of efficiency in the economy or political reforms as well, also the efficiency of an application of some principles and procedures is meaningful to discuss, those of the statistics do not represent any exception.

Under the term of "biometrics" less is being considered than promised by the ethymology: "to measure live", but such a situation seems to be quite common in the denotations of particular branches of science. Biometrics is, briefly speaking, a set of mathematical, mostly statistical methods applied and able to apply to solve biological and biomedical problems, including modelling, experimental design and methodology of application. Object of this discipline - biometrics - is to take as open, without any aspiration to delimitate it in an exhaustive manner.

The extent of biometrical activities in the everyday life of biomedical research is uneasy to evaluate. The papers and articles where statistics contributed to the solution of the problems introduced are disseminated in any part of the literature, in any field of research interest, so that a systematic looking up reaches over the possibilities of not only an individual. No database of this kind seems to exist till now in our country.

2. History and presence

Statistical induction is to consider as one among the most important solutions of the Hume's scepticism, as the response to the challenge of misgiving verifiability of the laws derived from empirical observations. To take experimental situation and to transform it into the form of a statistical problem able to be solved by means of well-grounded rules, offers us something like "reasoning technology" where the risks of errors and possibilities to succeed are not only declared but also measured quantitatively. One hundred years after the "Fathers Promotors", Karl Pearson and R. A. Fisher, complete "industry" of technologies has been developed offering procedures to the experimenter as a kind of "statistical service".

Let us get aside particular procedures with their intellectual refinements and prefer to concentrate on the difficulties and complications accompanying application. They can be summarized under the title "problem of method adequancy".

In the recent years, the importance of an adequate application of statistics in the given experimental situation has been stressed by the fundamentally changed state in informatics and the development of computation systems. Statisticians do not more elaborate computation programs and algorithms in particular problem solving. Existing packages of statistical programs make available an application of statistical methods to anybody who knows how to enter data into the system. The problem arises that no way exists making it possible to elaborate algorithms for the choice of the optimum statistical procedure and of the rules of the adequate interpretation of the conclusions derived from such statistical procedure. Besides it something like a meta-problem exists in that the problem mentioned above should not be recognized by the experimenter, the application of a statistical method being considered by him only as a matter of some arithmetic, this meta-problem remains hidden. In our culture and our education the questions like "How ?" are being considered as more respectful when compared with those like "What ?". We commonly expect any "Somehow" to be supported by a "Something" and think about the self-evidence with which the 2nd should be derived from the 1st one. To consider over that the purpose of statistical reasoning is for any manifest event (data) to search for the reality most plausible (most likely), i.e. for the reality (theory, hypothesis) able to explain the data as more probable than the other realities do, seems for an experimenter to be very strange and distant from his/her way of thinking.

3. Painful modules of an application

30-year experience accumulated in the Laboratory of Biometrics at the Faculty of Medicine, Palacký University in Olomouc, allows some chronically painful knots to recognize in the interaction of a biometrician and an experimenter entering with his/her problem to solve. The experiences introduced below do not claim completeness and consistency as well, quite simply - they exist.

(a) Delimitation of the reference population. Otherwise speaking, the problem is to generalize what has been found in the sample, as valid for the population declared. This should be guarranted by the random mechanism in the sampling formation, such a guarrantee could never be expected perfect in the biomedical research.

(b) Adequacy of the model applied. The problem concerns the type of data and scale of measurement (nominal, ordinal, metric), choice of the measurement unit, adequate interpretation of the results, information loss due to the introduction of the reducing characteristics, outliers manipulation. It's the statistician's duty to assure himself/herself that the data indroduced are primary ones or to be informed on the transformation applied to the data.

(c) Object of endless discussions the statistician has with the experimenter is the chain of questions connected with the sample size determination, the compromise between the sharpness of a conclusion and its reliability, and the relation of the statistical significance of the conclusion and its objective importance. Everyday solutions proceed usually so that the sample size is determined by the outer (technical and economic) circumstances, and from the level of significance of the test or confidence interval given in advance sharpness of the conclusion is yielded, the relation between significance and importance of the conclusion being discussed as a rule in case of relatively large samples.

(d) When a statistical procedure is reduced to a technical routine (arithmetic) only, the chance increases for inadequate interpretations of the statistical procedure. To require level of significance to be given in advance might be considered as a manifestation of the "statistical bureaucracy", similarly as the preference of the two-sided test against the one-sided test. It is not easy to be convincing when stating that a hypothesis could be valid or invalid, but not valid more or less, and that the probability could be property exclusively of a random event (with the possible modification in case of Bayesian approach where the roles of the hypotheses and the events become relative).

(e) Everybody declares his/her experiment double-blind, often without more sophisticate considerations of the meaning in the particular situation. This problem is a part of a more general class of vulnerability, namely affecting of the requirement the data be independent, icluding all consequences in the interpretation. Having taken a single observation ($n = 1$) and like the Jesus Christ multiplied it repeatedly - surely I would be considered cynic. If, on the other hand, $n = 200$ observations composed of some subsets impossible to identify, yielded by repeated measurement with unknown multiplicity on 30 patients, have been presented for the statistical analysis - we have to do with an affair acceptable for the common moral of experimentation.

4. How to evaluate statistically statistical evaluation

Within the period 1988-89 the group of Czech biometricians led by the Statistical Research Staff of the Institute for Hygiene and Epidemiology, Prague, initiated and carried out an evaluation of the applications of the statistical methods in the papers published on the pages of the medical journal edited by the Publishing House AVICENUM. There were hundreds of papers included (categorized mostly as the so-called "original contributions" with the own experimental and clinical data), these papers having been selected by the specialists not engaged in this evaluation. The standardized questionnaire has been filled up for each paper where (a) the kind of the statistical procedure applied and (b) adequacy of the application itself with respect to the purpose of the paper, were evaluated. It was the first attempt of this kind carried out in the nation-wide scale. I am not informed on the evaluation of these questionnaires and the respective publication of the conclusions, if not made, it should be carried out as soon as possible.

In the recent days I met the monograph by Bailar and Mosteller (1986) which occurred for the same reason, an evaluation of application of the statistical procedures in the set of 760 papers published within two years (1978 and 1979, Vol. 298, 299, 300 and 301) of the New England Journal of Medicine. In the monograph cited, the authors - well known statisticians - deal with the type of the procedures applied and the frequency of their application as well, and particularly concentrate on the analysis of misapplication and the discussion of the adequate rules and principles which should be respected in this application as consistently as possible. (Quotation presented under the title of this paper originated from this book.)

Beginning from the basic findings, the instruction could be derived useful for the future "biometrical education". From this evaluation it follows that:

58% of the papers under study did not use any statistical procedures at all or the methods of description only (calculation of per cent, histograms, averages, S. D.'s),

67% of these papers had enough having applied the procedures introduced above, or moreover Student's t-test,

73% of these referenced papers had enough having applied the procedures introduced above, or moreover an analysis of the contingency tables including chi-square test.

Thus, three quarters among the papers published in the most ancient U. S. medical journal are expected - from the viewpoint of statistical knowledge - to be accessible for the reader familiar with the procedures of statistical description, t-test and contingency tables. Only the remaining quarter among the referenced papers require the reader to be familiar with some other statistical procedures: non-parametric methods, epidemiological statistics, correlation, linear regression, variance analysis, primary data transformation, survival analysis and some multivariate statistical method. To understand statistical part of the content in 90% of the papers referenced, the knowledge of 11 statistical procedures is sufficient (the fact that more various statistical procedures could be used in the same paper being respected in this evaluation).

5. Statistical ethics

Different statisticians exist. Few of them are pure mathematicians dealing with the theory of statistics. Their morale is codified by the principles of the theoretical research. Most of the statisticians are responsible not only to their own conscience and profession, but also to the experimenter whose data are to be evaluated. In this dimension the statistician's responsibility seems to be similar to that of a physician, judge and any other public man

(Finney 1991).

Some specific ethical aspects could be formed in the interaction of a statistician with the academic and research environment. These should be object of education for the students being prepared to become statisticians - not through the lectures, but through the constitution of a desirable ethical atmosphere.

The document "A Declaration on Professional Ethics" has been published by ISI (International Statistical Institute), in 1985 - presenting something like the rules of a statistical gentleman. The stress is given on the responsibility of a statistician (1) in relation to the society as far as the information objectivity, adequacy of the method applied, refuse of a priori limitations (censorship) and data confidence safety concerns, (2) in relation to the profession - nobody being entitled to proceed in such a way that his/her activity would lower the prestige of the profession and (3) in relation to the object of investigation for whom the right to privacy protection must be guarranted, as well as to his/her conscious agreement to participate in the survay.

There is no reason acceptable by which omitting or addition of the data would be entitled with the goal to "improve" the reality under study. Professional conscience has to be supreme judge for the statistician, ranked over his/her economic or personal interests and over the respective non-objective interests of the experimenter.

To use the data to evaluate as a part of his/her own publication is the statistician allowed only with an explicit experimenter's consense. Curiosity belongs among the positive properties of a qualified statistician, to accept adjusted and transformed data for an evaluation is not allowed without making an inquiry on their primary form and origin - not to do it is an example of statistical frivolity.

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The COMPSTAT Conference and the Artificial Intelligence

Tomáš Havránek

The artificial intelligence (AI) as one of computer science disciplines has in recent years strongly influenced the contents of COMPSTAT conferences. Let us outline this development and some of its principal features.

In 1978 (Leiden) J. A. Nelder presented a lecture "The future of statistical software", in which no mention about the artificial intelligence appears yet; he paid attention above all to more classical computer science techniques (data structures, operating systems, statistical languages). In 1980 (Edinburgh) there is an entire section devoted to interactive computing with papers of type "Interactive or batch?", but still nothing about the artificial intelligence (set aside the pattern recognition and cluster analysis domain, which is also sometimes attached to the artificial intelligence). In 1982 (Toulouse) an invited paper by P. Hájek and J. Ivánek "Artificial intelligence and data analysis" appears, covering two subjects - the use of AI techniques in controlling the data analysis as a cognitive activity (the ambitious, but unrealized GUHA 80 project) and the possibility of using expert system techniques to support decision making in setting run parameters of statistical software. The same year, the first lecture on parallel data processing, namely for generating pseudorandom selections (Sylwestrowicz), takes place. In return, in 1984 (Prague), a whole section is devoted to AI techniques (Intelligent software in statistical data analysis), with four lectures: W. Gale and D. Pregibon "Constructing expert system for data analysis by working example" (invited lecture), D. Pregibon and W. Gale "REX - an expert system for regression analysis", A. Kowalski and A. Shutt "Analysis of suppositions" and T. Pukkila, S. Puntanen and O. Stenman "On the possibilities to automate the study of statistical dependence between two variables". Especially the first two lectures are of great importance; the latter informs about a working experimental decision support system in regression analysis, the former opens the problem of acquiring the knowledge to be used by a statistical expert system and suggests also a certain solution. In 1986 (Rome) there is already an explicit expert systems section comprising nine lectures. Most of them deal with projects of expert systems for decision support in statistical data analysis, especially for assisting statistical software users; projects are more or (rather) less elaborated or implemented. In 1988 an entire section is again devoted to expert systems (15 papers). Topics of lectures begin to differentiate; the above mentioned projects belong to classical topics. They fall into two classes - the working, useful, but little intelligent ones (e.g. P. Hietala reviews the ESTES system for time series analysis), and the others, those being intelligent, but not working yet. The second topic deals with the problem of knowledge acquisition for expert systems (in general) on the basis of statistical data (Jiroušek and Kříž, Ivánek and Stejskal, Norbotten). The third topic is isolated - why do expert systems work as they do - and is covered with P. Hájek's paper "Towards a probabilistic analysis of MYCIN-like expert systems". A program named GLIMSE has been demonstrated, which in dialog with the user controls the work of GLIM - the common system for generalized linear models. For the sake of completeness let us point out that in Copenhagen, W. Gale was asked for another tutorial. It is interesting that a lecture on neural nets application also appeared. The invited lecture on parallel methods in linear algebra (G. W. Stewart) remained isolated. The Copenhagen lectures probably called up a reaction by B. Streitberg "On the non-existence of expert systems" (Statistical Software Newsletters, 19 (1988), 55-62), who concludes that we do not need artificial intelligence, but "less stupid systems" (the author was a member of the programme committee of the COMPSTAT 80 conference). As a rarity I mention that Streitberg quotes within the text (Nelder, 1977) as an essential paper while in the references he has Nelder J. A. (1978): Intelligent programs, the next stage in statistical computing, COMPSTAT 78, Physica-Verlag. In the COMPSTAT 78 proceedings appeared, on the other hand, quite another paper by Nelder, the one mentioned at the beginning of this article. In the discussion following in the SSN nobody noticed it, including Nelder himself. Finally, this year (Dubrovnik), the Expert Systems in Statistics section was on the programme with eight contributions; only about a half of them had something to do with expert systems. The respective lectures were: Wittkowski, K. M.: Statistical knowledge based systems - critical remarks and requirements for approval, Kuzmenkov, V. V., Terskin, O. I.: New approach to Guha-method from the reliability viewpoint, Hebrail, G., Suchard, M.: Classifying documents: a discriminant analysis and an expert system work together, Gale, W. A., Church, K. W.: Estimation procedures for language context: poor estimates are worse than none, van den Berg, G. M., Visser, R. A.: Knowledge modelling for statistical consultation systems; two empirical studies, Gebhardt, F.: An expert system strategy for selecting interesting results, Gottee, M. J.: Computer assisted interpretation of conditional independence graphs, Dorda, W., Froechl, K. A., Grossmann, W.: Wamastex - heuristic guidance for statistical analysis. We can trace a development back to more general understanding of problems of AI in statistics and a deflection from "mere" individual expert systems projects.

An overall diagram of development of numbers of articles devoted to AI at COMPSTAT conferences in recent years recalls me something. There might remain an interesting but demanding core for further research and applications?

Not to be afraid and not to steal - valid for software

Josef Tvrdík

This variation of a known Masaryk's sentence was used as the motto of a panel discussion. This discussion was held at the XIV-th national conference on Programming in June, 1989. Since then many things have changed, fortunately. However I feel that the motto is still topical. Let me add several remarks to this matter. In order to satisfy the friends of old formalism we express the sentence from the title in the form of a statement:

[We are afraid] and [We steal software].

The first person plural seems to me adequate. If there is anyone who does not live in the shadow of this sentence, I apologize to him in advance. Let us denote the statements in brackets p, q respectively. The evaluation of the conjunction of p & q is generally known. I can continue directly with comments on each of four evaluations of the conjunction. I will take them in chronological order.

10: An ancient time of computers in our country. The thieving of software was not our main problem. There were almost no software to be stolen and no computers for application. Our anxiety was not connected with software in those days.

01: The "splendid" time of the semipermeable Iron Curtain. Many western users could not believe it when seeing the contents of our PC hard disks. Programs of almost identical functions were doubled or tripled on each computer. Several versions of the same software product on one PC was considered the normal way. The lack of documentation was substituted by the adventures of individual research. The methods of "trial and error" and "genius'intuition" were the most popular. This research was cheap, because it was paid by the state administration, mostly poorly. Our principals were satisfied, because they were not troubled. We got our troubles enough. This work required no money. The wages were planned. They were in no correlations with the results of the staff. Our work was fruitful: viruses in computers, almost all computers were "oversoftwared". A reliable application could not be run.

11: The hot present times. Neither Iron Curtain separates us nor it protects us from the world. The world sees us. The companies dealing with the investigating of the illegal use of software can see us, too. We can read the news on the successes of FAST. We can read about the amount of dollars paid by illegal users caught by FAST. It is not a pleasant reading. We have not enough money to buy all the software and we are keen to have the software and to understand to it. It is hard suddenly to abandon this style of work. So two items must be added to our time schedule : preparing ourselves for punishment due to the illegal using of software and the exercising of the RESTORE command.

00: The advantage of this approach is commonly accepted since the time of our first president. We have two alternatives before us:
- to accept this approach to the software as a free act of our own will
- to be forced to this approach by our surroundings.

No other possibility can be seen. If it is a frustrating imagining for you (especially in the combination with the evaluation in the label of this paragraph), you should continue in reading. I am going to tell you a calming story. I have heard it from my colleague: He has got relatives in the U.S.A. His brother-in-law deals with some business. He uses the PC in the management of his work. They met on holiday several years ago. My friend explained to his brother-in-law our common practice in getting software. Then he proposed to him to safe money by using illegal software as it was used in Czechoslovakia in that time. Software is a special kind of goods. The source is not destroyed by illegal copying , the legal user is not affected by it - we can hear the devil's voice. The American businessman did not reject the proposal at once. He refused it after a rational analysis a day later. He was not an ethical hero from fairy tales. The reason of the rejection was economic. He would lose all the advantages of service: hot line consultations, upgrading, technical help etc.. Moreover, he would be stealing from himself: he would lose the time required on the substitution of missing services. We should try to understand his behavior. Modern society is accustomed to an advanced specialization in work. The professional level of goods is a common standard. The to buy goods is the only normal way, how to get them. And this way is the fastest and the cheapest one.

I wish all of us to live in a society, where not to be afraid and not to steal is not abnormal.

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