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Oscar Sheynin

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I

Ch. Babbage

Of observations. Extract from writings

Annual Rept, Smithsonian Instn for 1873 (1874), pp. 187 – 197

There are several reflections connected with the art of making observations and experiments which may be conveniently arranged in this chapter.

Of minute precision

No person will deny that the highest degree of attainable accuracy is an object to be desired, and it is generally found that the last advances toward precision require a greater devotion of time, labour and expense than those which precede them. The first steps in the path of discovery and the first approximate measures are those which add most to the existing knowledge of mankind.

The extreme accuracy required in some of our modern inquiries has, in some respects, had an unfortunate influence by favouring the opinion that no experiments are valuable unless the measures are most minute and the accordance among them most perfect. It may perhaps be of some use to show that even with large instruments and most practiced observers this is but rarely the case. The following extract is taken from a representation made by the present astronomer-royal to the council of the Royal Society on the advantages to be derived from the employment of two mural circles:

That by observing with two instruments the same objects at the same time and in the same manner we should be able to estimate how much of that <u>occasional discordance from the mean which attends even the most careful observations</u>, ought to be attributed to irregularity of refraction, and how much to <u>the imperfections of instruments</u>.

In confirmation of this may be adduced the opinion of the late M. Delambre (Méchain, Delambre 1806 – 1810, discours préliminaire, p. 158) which is the more important from the statement it contains relative to the necessity of publishing *all* the observations which have been made:

Mais quelque soit le parti que l'on préfere, il me semble qu'on doit tout publier. Ces irregularités mêmes sont des faits qu'il importe de connoitre. <u>Les soins les plus attentifs n'en sauroient préserver les observations les plus exercés</u>, et celui qui ne produiroit que des angles toujours parfaitement d'accord auroit été singulièrement bien servi par les circonstances on ne seroit pas bien sincère.

The desire for extreme accuracy has called away the attention of experimenters from points of greater importance and it seems to have been too much overlooked in the present day that genius marks its track not by observation of quantities inappreciable to any but the acutest senses, but by placing Nature in such circumstances that she is forced to record her minutest variations on so magnified a scale that an observer possessing ordinary faculties shall find them legibly written. He who can see portions of matter beyond the ken of the rest of his species confers an obligation on them by recording what he sees. But their knowledge depends both on his testimony and on his judgment. He who contrives a method of rendering such atoms visible to ordinary observers communicates to mankind an instrument of discovery, and stamps his own observations with a character alike independent of testimony or of judgment.

On the art of observing

The remarks in this section are not proposed for the assistance of those who are already observers, but are intended to show to persons not familiar with the subject that, in observations demanding no unrivalled accuracy, the principles of common sense may be safely trusted, and that any gentleman of liberal education may, by perseverance and attention, ascertain the limits within which he may trust both his instrument and himself.

If the instrument is a divided one, the first thing is to learn to read the verniers. If the divisions are so fine that the coincidence is frequently doubtful, the best plan will be for the learner to get some acquaintance who is skilled in the use of instruments and, having set the instrument at hazard, to write down the readings of the verniers, and then request his friend to do the same. Whenever there is any difference, he should carefully examine the doubtful one, and ask his friend to point out the minute peculiarities on which he founds his decision. This should be repeated frequently, and, after some practice, he should note how many times in a hundred his readings differs from his friends', and also on how many divisions they usually differ.

The next point is, to ascertain the precision with which the learner can bisect the object with the wires of the telescope. This can be done without assistance. It is not necessary even to adjust the instrument but merely to point it at a distant object. When it bisects any remarkable point, read off the verniers and write down the result. Then displace the telescope a little and adjust it again. A series of such observations will show the confidence which is due to the observer's eye on bisecting an object, and also in reading the verniers. And as a first direction gave him some measure of the latter, he may, in a great measure, appreciate his skill. He should also, when he finds a deviation in the reading, return to the telescope and satisfy himself if he has made the bisection as complete as he can. In general, the student should practice each adjustment separately, and write down the results wherever he can measure its deviations.

Having thus practiced the adjustments, the next step is to make an observation. But in order to try both himself and the instrument, let him take the altitude of some fixed object, a terrestrial one, and having registered the result, let him derange the adjustment, and repeat the process 50 or a 100 times[1]. This will not only merely afford him excellent practice, but enable him to judge of his own skill.

The first step in the use of every instrument is to find the limits in which its employer can measure *the same object under the same circumstances*, and, after that, of *different objects under different circumstances*.

The principles are applicable to almost all instruments. If a person is desirous of ascertaining heights by a mountain barometer[2], let him begin by adjusting the instrument in his own study, and, having made the upper contact, let him write down the reading of the vernier, and then let him derange the *upper* adjustment *only*, readjust and repeat the reading. When he is satisfied about the limits within which he can make that adjustment, let him do the same repeatedly with the lower, but let him not, until he knows his own errors in reading and adjusting, pronounce upon those of the instrument. In the case of a barometer, he must also be assured that the temperature of the mercury does not change during the interval.

A friend once brought me a beautifully constructed piece of mechanism for marking minute portions of time. The three hundredth part of a second was indicated by it. It was a kind of watch, with a pin for stopping on of the hands. I proposed that we should each endeavour to stop it 20 times in succession at the same point. We were both equally unpractised, and our first endeavours showed that we could not be confident of the twentieth part of a second. In fact, both the time occupied in causing the extremities of the fingers to obey the volition, as well as the time employed in compressing the flesh before the fingers acted on the stop, appeared to influence the accuracy of our observations. From some few experiments I made I thought I perceived that the rapidity of the transmission of the effects of the will depended on the state of fatigue or health of the body. If anyone were to make experiments on this subject, it might be interesting to compare the rapidity of the transmission of volition in different persons with the time occupied in obliterating an impression made on one of the senses of the same persons. For example, by having a mechanism to make a piece of ignited charcoal revolve with different degrees of velocity, some persons will perceive a continuous circle of light before others, whose retina does not retain so long impressions that are made upon it.

On the frauds of observers

Scientific inquiries are more exposed than most others to the inroads of pretenders. And I feel that I shall deserve the thanks of all who really value truth, by stating some of the methods of deceiving practiced by unworthy claimants for its honours, while the mere circumstance of their arts being known may deter future offenders.

There are several species of impositions that have been practiced in science, which are but little known except to the initiated, and which it may perhaps be possible to render quite intelligible to ordinary understandings. These may be classed under the heads of hoaxing, forging, trimming and cooking.

Of hoaxing. This, perhaps will be better explained by an example. In the year 1788, M. Gioeni, a knight of Malta, published at Naples an account of a new family of *Testacea*, of which he described with great minuteness one species, the specific name of which has been taken from his *habitat*, and the generic he took from his own family, calling it *Gioenia sicula*. It consisted of two round triangular valves, united by the body of the animal to a smaller valve in front. He gave figures of the animal and of its parts; described its structure, its mode of advancing along the sand, the figure of the track it left, and estimated the velocity of its course at about two-thirds of an inch per minute. He then described the structure of the shell, which he treated with nitric acid and found it approached nearer to the nature of bone than any other shell.

The editors of the *Encyclopédie méthodique* have copied this description and have given figures of the *Gioenia sicula*. The fact, however, is, that no such animal exists, but that the knight of Malta, finding on the Sicilian shores the three internal bones of one of the species of *Bulla* of which some are found on the southwestern coast of England[3], described and figured these bones most accurately, and drew the whole of the rest of the description from the stores of his own imagination.

Such frauds are far from justifiable. The only excuse which has been made for them is, when they have been practiced on scientific academies which had reached the period of dotage.

It should however be remembered that the productions of nature are so various that mere strangeness[4] is very far from sufficient to render doubtful the existence of any creature for which there is evidence; and that, unless the memoir itself involves principles so contradictory[5] as to outweigh the evidence of a single witness, it can only be regarded as a deception without accompaniment of wit.

Forging differs from hoaxing, inasmuch as in the latter the deceit is intended to last for a time, and then discovered to the ridicule of those who have credited it; whereas the forger is one who, wishing to acquire a reputation for science, records observations which he has never made. This is sometime accomplished in astronomical observations by calculating the time and circumstances of the phenomenon from tables. The observations of the second comet of 1784, which was only seen by the Chevalier d'Angos, were long suspected to be a forgery, and were at length proved to be so by the calculations and reasoning of Encke. The pretended observations did not accord among each other in giving any possible orbit. But Encke detected an orbit, belonging to some of the observations, from which he found that all the rest might be almost precisely deduced, provided a mistake of a unit in the index of the logarithm of the radius vector were supposed to have been made in all the rest of the calculations[6] [2]. Fortunately instances of the occurrence of forging are rare.

Trimming consists in clipping off little bits here and there from those observations which differ most in excess from the mean and in sticking them on those which are too small; a species of *equitable adjustment*, as a radical would term it, which cannot be admitted in science.

This fraud is not, perhaps, so injurious (except to the character of the trimmer) as cooking which the next paragraph will teach. The reason of this is that the *average* given by the observations of the trimmer is the same, whether they are trimmed or untrimmed. His object is to gain a reputation for extreme accuracy in making observations. But from respect for truth or from a prudent foresight he does not distort the position of the fact he gets from nature, and it is usually difficult to detect him. He has more sense or less adventure than the cook.

Of cooking. This is an art of various forms, whose object is to give to ordinary observations the appearance and character of those of the highest degree of accuracy.

One of its numerous processes is to make multitudes of observations, and out of these to select only which agree or very nearly agree. If a hundred observations are made, the cook must be very unlucky if he cannot pick out fifteen or twenty which will do for serving up.

Another approved receipt, when the observations to be used will not come within the limit of accuracy which it has been resolved they shall possess, is to calculate them by two different formulas. The difference in the constants employed in those formulas has sometimes a most happy effect in promoting unanimity among discordant measures. If still greater accuracy is required, three or more formulas can be used.

It must be admitted that this receipt is in some instances most hazardous. But in the cases where the positions of stars, as given in different catalogues, occur, or different tables of specific gravities, specific heats etc., it may safely be employed. As no catalogue contains all stars, the computer must have recourse to several. And if he is obliged to use his judgement in the selection, it would be cruel to deny him any little advantage which might result from it. It may however be necessary to guard against one mistake into which persons might fall.

If an observer calculates particular stars from a catalogue which makes them accord precisely with the rest of his results whereas had they been computed from other catalogues the difference would have been considerable, and it is very unfair to accuse him of *cooking*. Those catalogues may have been notoriously inaccurate, or they may have been superseded by others more recent, or made with better instruments, or the observer may have been totally ignorant of their existence.

It sometimes happens that constant quantities in formulas given by the highest authorities, although they differ among themselves, yet they will not suit the materials. This is precisely the point in which the skill of the artist is shown. And an accomplished cook will carry himself triumphantly through it, provided, happily, some mean value of such constants fits his observations. He will discuss the relative merits of formulas he has just knowledge enough to use. And, with admirable candour, assigning their proper share of applause to Bessel, to Gauss and to Laplace, he will take *that* mean value of the constant used by three such philosophers which will make his own observations accord to a miracle.

There are some few reflections I would venture to suggest to those who cook, they deserve, from not coming from the pen of an adept.

In the first place, it must require much time to try different formulas. In the next place, it may happen that, in the progress of human knowledge, more correct formulas may be discovered, and constants may be determined with far greater precision. Or it may be found that some physical circumstance influences the result (although unsuspected at the time) the measure of which circumstance may perhaps be recovered from other contemporary registers of facts[7]. Or, if the selection of observations has been made with the view of its agreeing precisely with the latest determination, there is some little danger that the average of the whole may differ from that of the chosen ones, owing to some law of nature dependent on the interval between the two sets, which law some future philosopher may discover; and thus the very best observations may have been thrown aside.

In all these, and in numerous other cases, it would most probably happen that the cook would procure a temporary reputation for unrivalled accuracy at the expense of his permanent fame. It might also have the effect of rendering even all his crude observations of no value. For that part of the scientific world whose opinion is of most weight is generally so unreasonable as to neglect altogether the observations of those in whom they have, on any occasion, discovered traces of the artist. In fact, the character of an observer, as of a woman, if doubted, is destroyed.

The manner in which facts apparently lost are restored to light, even after considerable intervals of time, is sometimes very unexpected, and a few examples may not be without their use. The thermometers employed by the philosophers who composed the Accademia del Cimento[8] have been lost. And as they did not use the two fixed points of freezing and boiling water, the results of a great mass of observations have remained useless from our ignorance of the value of a degree on their instruments. M. Libri of Florence proposed to regain this knowledge by comparing their registers of the temperature of the human body and of that of some warm springs in Tuscany which have preserved their heat uniform during a century, as well as of other things similarly circumstanced.

Another illustration was pointed out to me by M. Gazzeri, the professor of chemistry at Florence. A few years ago an important suit in one of the legal courts of Tuscany depended on ascertaining whether a certain word had been erased by some chemical process from a deed then before the court. The party who insisted that an erasure had been made availed themselves of the knowledge of M. Gazzeri, who, concluding that those who committed the fraud would be satisfied by the disappearance of the colouring matter of the ink, suspected (either from some colourless matter remaining in the letters, or perhaps from the agency of the solvent having weakened the fabric of the paper itself beneath the supposed letters) that the effect of the slow application of heat would be to render some difference of texture or of applied substance evident by some variety in the shade of colour which heat in such circumstances might be expected to produce. Permission having been given to try the experiment on the application of heat the important word reappeared to the great satisfaction of the court.

[One of the most noted deceptions of this kind was that called the moon hoax, published in New York about 30 years ago, which purported to be a series of discoveries made in the moon by Sir John Herschel during his residence at the Cape of Good Hope. These discoveries were said to be the result of a great improvement in the telescope. It is well known that, with a given-sized object glass, the power of this instrument is limited by the degree to which the image in the focus of the glass can be magnified. The light remaining the same the more the size of the image is increased the darker it becomes. The alleged improvement consisted in the illumination of this image by artificial light. By the application of this idea, the telescope employed by the astronomer at the Cape of Good Hope admitted of an eye-glass of such magnifying power that moving objects on the surface of the moon were observable, and men and animals of remarkable forms were actually discovered.

It is astonishing the effect which the annunciation of these discoveries produce. Instead of detecting at once the scientific absurdity of illuminating a shadow that it might be more highly magnified, many persons, even professors in colleges, gave the announcement credence, and thus added to the popularity of the hoax. This fraud owed its success, in a great measure, to a want, at the time, of precise scientific knowledge in this country, and after the absurdity was pointed out the invention was cried up as a most extraordinary production, since those who had been hoaxed by it attributed their credulity to the ingenuity of the deception rather than to their own want of knowledge.

The success of this hoax has had an exceedingly bad influence on the character of our country for veracity. It was followed immediately after, and has been even down to the present time, by a series of contemptible imitations. And, indeed, to such an extent was this imitation carried on a few years ago, that scarcely any announcement of phenomena of unusual occurrence could be accepted as truth. Among these imitations within a few years, the most successful, and one which evinced considerable reading as well as ingenuity, was that of the pretended discovery of a series of Runic inscriptions on the face of a rock in the Potomac river near Washington. This was the invention of a young student of law in this city, and excited quite a sensation among the archaeologists of this and other countries. It was copied in various ethnological journals as a truth, and was hailed by the Scandinavians as a new evidence of the early explorations of the Northmen in the United States.

Such inventions must be classed with those practical jokes which have been happily termed *gymnastic wit*, of which a notable example was given in England, where a *society* was founded for *insulting women and frightening children*. The chronicler naively remarks that the members were never discovered, and, what is just as remarkable, the wit was equally a mystery. *Truth*, says Dr. Johnson, *is a matter of too much importance to be tampered with, even in trifles*. J. H.
(This J. H. is probably John Herschel, the author of the entire attachment.)]

On the permanent impression of our words and actions
on the globe we inhabit

The principle of the equality of action and reaction, when traced through all the consequences, opens views which will appear to many persons most unexpected. The pulsations of the air, once set in motion by the human voice, cease not to exist with the sounds to which they gave rise. Strong and audible as they may be in the immediate neighbourhood of the speaker, and at the immediate moment of utterance, their quickly attenuated force soon becomes inaudible to the human ears. The motions they have impressed on the particles of one portion of our atmosphere are communicated to constantly-increasing numbers, but the total quantity of motion measured in the same direction receives no addition. Each atom loses as much as it gives, and regains again from other atoms a portion of those motions which they in turn give up.

The waves of air thus raised perambulate the earth and the ocean's surface, and in less than 20 hours every atom of its atmosphere takes up the altered movement due to that infinitesimal portion of the primitive motion which has been conveyed to it through countless channels, and which must continue to influence its path throughout its entire existence[9]

But these aerial pulses, unseen by the keenest eye, unheard by the acutest ear, unperceived by human senses, are yet demonstrated to exist by human reason. And in some few and limited instances, by calling to our aid the most refined and comprehensive instrument of human thought, their courses are traced and their intensities are measured. If man enjoyed a larger command over mathematical analysis, his knowledge of these motions would be more extensive, but a being possessed of unbounded knowledge of that science could trace even the minutest consequence of that primary impulse. Such a being, however far exalted above our race, would still be immeasurably below even our conception of infinite intelligence.

But supposing the original conditions of each atom of the earth's atmosphere as well as all the extraneous causes acting on it, to be given. Supposing also the interference of no new causes, such a being would be able clearly to trace its future, but inevitable path, and he would distinctly foresee and might absolutely predict for any, even the remotest period of time, the circumstances and future history of every particle of that atmosphere.

Let us imagine a being, invested with such knowledge, to examine at a distant epoch the coincidence of the facts with those which his profound analysis had enabled him to predict. If any the slightest deviation existed, he would immediately read in its existence the action of a new cause. And through the aid of the same analysis, tracing this discordance back to its source, he would become aware of the time of its commencement and the point of space at which it originated.

Thus considered, what a strange chaos is this wide atmosphere we breathe! Every atom, impressed with good and with ill, retains at once the motions which philosophers and sages have imparted to it, mixed and combined in ten thousand ways with all that is worthless and base. The air itself is one vast library, on whose pages are forever written all that man has ever said or woman whispered. There, in their mutable but unerring characters, mixed with the earliest as well as with the latest sighs of mortality, stand forever recorded, vows unredeemed, promises unfulfilled, perpetuating in the united movements of each particle the testimony of man's changeful will.

But if the air we breathe is the never-failing historian of the sentiments we have uttered, earth, air and ocean are the eternal witnesses of the acts we have done. The same principle of the equality of action and reaction applies to them. Whatever movement is communicated to any of their particles is transmitted to all around it, the share of each being diminished by their number, and depending jointly on the number and position of those acted upon by the original sources of disturbance. The waves of air, although in many instances perceptible to the organs of hearing are only rendered visible to the eye by peculiar contrivances, but those of water offer to the sense of sight the most beautiful illustration of transmitted motion. Everyone who has thrown a pebble into the still waters of a sheltered pool has seen the circles it has raised, gradually expanding in size, and as uniformly diminishing in distinctness. He may have noticed also the perfect distinctness with which two, three, or more series of waves each pursues its own unimpeded course, when diverging from two, three, or more centres of disturbance. He may have seen, in such cases, the particles of water where the waves intersect each other partake of the movements due to each series.

No motion impressed by natural causes or by human agency is ever obliterated. The ripple of the ocean's surface caused by a gentle breeze, or the still water which marks the more immediate track of a ponderous vessel gliding with scarcely expanded sails over its bosom are equally indelible. The momentary waves raised by the passing breeze, apparently born but to die on the spot which saw their birth, have behind them an endless progeny, which, reviving with diminished energy in other seas, resisting a thousand shores, reflected from each, and perhaps again partially concentrated, will pursue their ceaseless course till ocean be itself annihilated.

The track of every canoe, or every vessel which has disturbed the surface of the ocean, whether impelled by manual force or elemental power, remains forever registered in the future movement of all succeeding particles which may occupy its place. The furrow which it left is, indeed, instantly filed up by the closing water, but they draw after them other and larger portions of the surrounding element, and these again once moved communicate motion to others in endless successsion.

The solid substance of the globe itself, whether we regard the minutest movement of the soft clay which receives its impression from the foot of animals or the concussion arising from the fall of mountains rent by earthquakes, equally communicates and retains, through all its countless atoms, their apportioned shares of the motions so impressed.

While the atmosphere we breathe is the ever-living witness of the sentiments we have uttered, the waters, and the more solid materials of the globe, bear equally enduring testimony of the acts we have committed.

If the Almighty stamped on the brow of the earliest murderer the inedible and visible mark of his guilt, he has also established laws by which every succeeding criminal is not less irrevocably chained to the testimony of his crime; for every atom of his mortal frame, through whatever changes its several particles may migrate, will still retain, adhering to it through every combination, some movement derived from that very muscular effort by which the crime itself was perpetrated.

The soul of the Negro whose fettered body, surviving the living charnel-house of his infected prison, was thrown into the sea to lighten the ship, that the Christian captor might escape the limited justice at length assigned by civilized man to crimes whose profit had long gilded their atrocity, will need, at the last great day of human account, no living witness of his earthly agony. When man and all his race shall have disappeared from the face of our planet, ask every particle of air still floating over the unpeopled earth, and it will record the cruel mandate of the tyrant.

Notes

1. The eye gets tired with time and furthermore 50 (let alone 100) readings are tiresome and barely endurable. O. S.
2. A mountain barometer has a scale for reading low air pressure. O. S.
3. *Bulla lignaria*. Ch. B.
4. The number of vertebrae in the neck of the Plesiosaurus is a strange but ascertained fact. Ch. B.
5. The kind of contradiction which is here alluded to is that which arises from well-ascertained final causes. For instance, the ruminating stomach of the hooted animals is in no case combined with the claw-shaped form of the extremities, frequent in many of the carnivorous animals and necessary to some of them for the purpose of seizing their pray. Ch. B.
6. Difficult to understand. O. S.
7. Imagine, by way of example, the state of the barometer or thermometer. Ch. B.
8. The Accademia del Cimento in Florence was founded in 1657. It studies experimental physics. O. S.
9. *The trajectory of a simple molecule of air or vapour is regulated in a manner as certain as that of the planetary orbits. The only difference between these is that which is contributed by our ignorance*. Laplace (1814/1995, p. 3). Ch. B. quoted the original text whereas I replaced it by its English translation. O. S.

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Laplace P. S. (1814, French), *Philosophical essay on probabilities*. New York, 1995. Translated by A. I. Dale.
Méchain, Delambre (1806, 1807, 1810), *Base du système métrique*. Paris.
Zach. F. X.von (date not given), *Corr. Astron*., Bd. 4, see p. 456.

The large part of this work is barely useful since Babbage had not applied the ideas of Gauss or Bessel and, in general, because his description is so pedestrian that he does not even mention systematic errors. On the contrary, his description of frauds is instructive. The statement about the preservation of the utterances of human speech is much too detailed. Incidentally, I came across similar recent statements. The author’s style is not good enough. Some sentences are too long and in many instances he inserts barely needed words or phrases. I was unable to document his piece and can only refer to his collected works (1989).

II

N. S. Dodge

Charles Babbage

Annual report Smithsonian Instn for 1873, 1874, pp. 162 – 187

[1] Charles Babbage, upon being urged to write his own biography, replied that he had no desire to do it while he had strength and means to do better work. Some men, he said, write their lives to save themselves from *ennui*, careless of the amount they inflict on their readers. Others, lest some kind surviving friend in showing off his own talent in writing personal history might show up theirs; and others still from fear that the vampires of literature might make them a prey. He belonged to no one of these classes. What a man had done for others, not what he might say about himself, formed his best life. And so to many who asked him to prepare an autobiography he sent a list of his works, which, he naively adds, *no one cared to insert*. Still, few persons who have made a name while living are insensible to posthumous fame, and Babbage was among the number. While professing to treat these applications lightly, he nevertheless set about placing on record an account of himself, and though he rejects the name of autobiography, he has left behind him, in a work which he entitles *Passages from the life of a philosopher*, a memoir which in variety of detail, clearness of description, liveliness of style and sententious remark, is almost without its parallel. Without being confined to this witty and erratic narrative, and putting the estimate of the thinking men of the age rather than his own upon what he was and what he did, this notice will aim to do justice to certainly not the least remarkable man of this 19th century.

[2] Of the mere personal history of this eminent philosopher and scientific mechanist little need be recorded. He was born of gentle blood and moderate competence on Dec. 6, 1792. From earliest years he showed great desire to inquire into the causes of things that astonish childish minds. He eviscerated toys to ascertain their manner of working; he sought to prove the reality of the devil by drawing with his blood a circle on the floor and repeating the Lord's prayer backward; he dissipated toothaches by reading Don Quixote; he bargained with another boy that whoever died first should appear to the survivor and spent a night of sleeplessness when the first event of the compact occurred, awaiting in vain his comrade's appearance.

In college he was perpetually puzzling his tutors by abstruse questions. When the circulation of the Bible with or without comment became a fierce controversy at Cambridge, he formed, with Herschel, Maule, D'Arblay and others, an analytical society for the translation of Lacroix's *Differential and Integral Calculus*, maintaining that the work needed no comment; that the *d's* of Leibniz were perfect, and consigning to perdition all who supported the heresy[1] of Newton's *dots*. It being hinted that the society was infidel, the young student replied

No! We advocate the principles of pure D'ism in opposition to the Dot'age of the university.

He studied the game of chess and beat every expert that was brought against him; formed a ghost club to collect all reliable evidence of the supernatural; joined high players at whist to show them that, staking only shillings, he could win at guinea-points; embarked in boating not more from the manual labour than from the intellectual art of sailing; and by making a collection of examples of mathematical problems, in which the notation of Leibniz was employed, he made it for the interest of tutors of the colleges to abandon the symbols of Newton.

[3] During Babbage's college life the course of his studies led him into a critical examination of the logarithmic tables then in use. The value of these tables had long been recognised in every part of the civilized world. Large sums of money were expended in their preparation, and the greatest care produced only proximate accuracy in the calculations. The young mathematician set himself to consider whether in the construction of these tables, in place of the perturbable processes of the intellect, it were not possible to substitute the unerring movements of mechanism. The thought was perpetually recurring during the latter portion of his college course. He gave up his leisure time to experiments having this end in view, discussed the subject with Herschel, Ryan, Maule and others of his class who were interested in philosophical mechanism, and no sooner graduated than he visited the various centres of machine labour in England and on the continent, that he might become familiar with the combinations in use and study their functions. Returning home, he began to sketch arrangements for a machine by which all mathematical tables might be computed by one uniform process.

The idea of a calculating machine did not originate with young Babbage. Pascal, nearly two hundred years before, had constructed, when in his nineteenth year, an ingenious machine for making arithmetical calculations, which excited admiration. In his *Pensées*, alluding to this engine, he remarks:

La machine arithmétique fait des effets qui approchent plus de la pensée que tout ce que font les animaux; mais elle ne fait rien qui puisse faire dire qu'elle a de la volonté comme les animaux.

Subsequently Leibniz invented a machine by which arithmetical computations could be made. Polenus, a learned and ingenious Italian, put together wheels by which multiplication was performed .And in the various industrial exhibitions since 1851, contrivances for performing certain arithmetical processes have been exhibited.

[4] The principle upon which Babbage's engines has been constructed, however, is entirely new, and intended to do work of a much more important character. On the 1st of April, 1823, a letter was received from the treasury by the president of the Royal Society, requesting him to ask the council to take into consideration a plan which had been submitted to government by Mr. Babbage for applying machinery to the purposes of calculating and printing mathematical tables, and desiring to be favoured with its opinion on the merits and utility of the invention.

This is the earliest allusion to the calculating machine on the records of the Royal Society. The invention, however, had been brought before the members in the previous year by a letter from Babbage to Sir Humphry Davy. In that, he had given an account of a small model of his engine for calculating differences, which produced figures at the rate of 44 a minute, and performed with rapidity and precision all those calculations for which it was designed. He had concluded this letter by saying

That though he had arrived at a point where success was no longer doubtful, it could be attained only at a very considerable expense, which would not probably be replaced by the works it might produce for a long period of time; and which is an undertaking I should feel unwilling to commence, as altogether foreign to my habits and pursuits.

The council of the Royal Society appointed a committee to take Babbage's plan into consideration. It was composed of the following gentlemen: Sir H. Davy, Brande, Combe, Baily, Brunel, Colby, Davies Gilbert, Sir John Hershel, Captain Kater, Pond, Wollaston, and Dr. Young[2]. On the 1st of May, 1823, this committee reported:

That it appears Mr. Babbage has displayed great talents and ingenuity in the construction of his machine for computation, which the committee think fully adequate to the attainment of the objects proposed by the inventor, and that they consider Mr. Babbage as highly deserving of public encouragement in the prosecution of his arduous undertaking.

This report was transmitted to the lords of the treasury, by whom it was printed and laid before Parliament. Two months after this a letter was sent from the treasury to the Royal Society, informing them that the issue of pounds 1,500 had been directed to Mr. Babbage

To enable him to bring his invention to perfection in the manner recommended.

[5] It is not within the purpose of this memoir to describe the misunderstanding which arose between Babbage and the British government, during the following 20 years in consequence of this letter, received by the Royal Society from the lords of the treasury. He regarded the machine he now undertook to build as the property of the government. They understood it to be his. He received the first advance of money as an earnest that all necessary funds would be furnished to complete this difference engine No. 1. They seemed to have regarded it in the light of a temporary assistance, given to a man of genius for the purpose of enabling him to complete an invention which would be of great public benefit.

He commenced the work, giving his own labours gratuitously, according to what he considered to be an order. Government looked on, furnished further moneys, consulted the Royal Society once and again as to the progress of the work, but declined committing itself further. Babbage advanced considerable sums, but was not reimbursed; made great improvements upon his original plans, but was not encouraged. Carried with him the convictions of the scientific men of his country and continental Europe, but was left behind by the treasury. And finally, when, in the opinion of such philosophical mechanists as Sir John Herschel, Sir Mark Brunel, Pond, the astronomer royal, and others, he was on the eve of results far surpassing in importance all that had been contemplated, he was informed that

Ultimate success appeared so problematic and the expense so large and so utterly incapable of being calculated, that the government would not be justified in taking upon itself any further liability.

[6] Thus terminated in 1842 the engagement which had existed more than a score of years between Charles Babbage and the British government. During this period of time he had made heavy sacrifices, both pecuniary and personal, had refused highly honourable and profitable situations; had employed in his own house, at his own expense, the most intelligent and skilled workmen to assist him in making experiments necessary for attaining a knowledge of every art which could possibly tend to the perfection of his engine; had repeatedly, at his own expense, visited the manufactories of England and the continent, had invented incidentally, and constructed mechanical tools and labour saving machines of great public value, not one of which he protected by letters-patents[3], and had gratuitously given the results of his energetic mind to the perfect construction of the machines which he regarded as the great purpose of his life. Whether success would have equated expectation had his government rendered him the required aid, can never be known. He has left behind him no thinker or philosophical mechanic capable of completing his work.

It was to calculate and print tables of figures connected with various sciences; with almost every department of the useful arts; with commerce, astronomy, navigation, surveying, engineering, and everything which depends on mathematical measurements.

[7] To show the immense importance of any method by which these numerical tables, absolutely accurate in every individual copy could be produced with facility and cheapness, let the reader revert to what European governments have attempted to do in the last hundred years. Dodson's Calculator[4], published in London in 1747, contained a table of multiplication extending to 10 times 1,000. In 1775 this table was extended to 10 times 10,000. The English board of longitude employed Dr. Hutton in 1781, to calculate numerical tables up to 100 times 1,000; and to add to these, tables of the squares of numbers as far as 25,400; and also tables of cubes of the first ten powers of numbers reaching to 100. In 1814, Professor Barlow, of Woolwich, published in an octavo volume the squares, cubes, square-roots, cube-roots, and reciprocals of all numbers from 1 to 1,000 – a table of the first ten powers from 1 to 100, and a table of the fourth and fifth powers of all numbers from 100 to 1,000.

To a still greater extent were similar tables prepared on the continent. In France, in the year 1785, was published an octavo volume of the tables of squares, cubes, square roots and cube roots of all numbers from 1 to 10,000; and in 1824 from 1,000 times to 100. A larger table of squares than at that time existing was published in Hanover in 1810; a larger still in Leipzig in 1812; a more perfect one at Berlin in 1825, and a similar table at Ghent in 1827.

This class of tables involves only the arithmetical dependence of abstract numbers upon each other. To express peculiar modes of quantity, such as angular, linear, superficial and solid magnitudes, a larger umber of computations are required. Volumes without numbers of these tables also have been computed and published at infinite labour and expense. Then come tables of a special nature, of importance not inferior, of labour more exacting, tables of interest, discount, and exchange; tables of annuities and life insurance, and tables of rates in general commerce. And then, above all others, tables of astronomy, the multiplicity and complexity of which it is impossible to describe, and the importance of which, in the kindred art of navigation, it would be difficult to over-estimate. The safety of the tens of thousands of ships upon the ocean, the accuracy of coast surveys, the exact position of light-houses, the track of every shore from headland to headland, the latitude and longitude of mid-sea islands, the course and motion of currents, direction and speed of winds, bearing and distance of mountains, and, in short, everything which constitutes the chief element of international commerce in modern times, depends upon the fullness and accuracy of logarithmic tables.

Inadequate as is the notion of the *importance* of these tables that has been conveyed, still more inadequate must be any notice of their errors. The expedients resorted to for even a limited degree of accuracy have been almost innumerable. The first French Republic, aspiring to lead the nations in science, undertook, through its mathematicians, by a division of labour so admirable that it seemed impossible errors should be committed, or, if committed, remain undetected, to produce a system of logarithmic and trigonometric tables so accurate that it should form a monument of the kind more imposing than had ever even been conceived. The attempt failed, for one singular reason among others, that the computers who committed the fewest errors were those who understood nothing beyond the process of addition. Dr. Lardner discovered in 40 tables, taken at random, no less than 3,700 *errata*. In the Nautical Almanac Baily detected more than 500 errors of calculation. The tables

Requisite to be used with the Nautical Ephemeris for finding latitude and longitude at sea

computed, revised and re-revised with the utmost care, under direction of the British board of longitude, and published by the government, was found to contain above a thousand errors. The tables of the distances of the moon from certain fixed stars, published by the same board, are followed by 1,100 *errata*, and these themselves contained so many errors as to make *errata* upon *errata* necessary.

For the special use of the national survey of Ireland, the logarithmic tables, most carefully prepared, were found to contain six errors, and these, by comparison, were found to exist not only in tables published during more than two hundred years in Paris and Gouda, Avignon and Berlin, Florence and London, but also in a set printed in China, in Chinese characters, and purporting to be original calculations. In fact, ab-solute correctness in logarithmic tables has never been attained. Year after year, through eight generations of mathematicians, one set has followed another to correct its predecessor. Even the last claims but approximate accuracy. Precautions, comparisons, revisions, and alterations from computer to computers, make advances *only toward* an end that is never absolutely reached. And no wonder. We need but consider the nature of a numerical table, where a thousand pages are covered with figures alone, where neither note nor comment, letters of the alphabet, nor rules of syntax, are permitted to intrude, to understand that the law of chance is on the side of error, and that for one mistake that may happen to be detected a score may escape unnoticed.

Besides the errors incidental to computation, there are those of *transcribing* for the press, and of *composition* into print. Nor does the liability to error stop even here, errors being often produced in the process of printing. A remarkable instance of this occurs in one of the six errors of the Irish Survey Tables, just mentioned. The last five figures of two successive numbers of a logarithmic table were 35875 and 10436. Both were erroneous. The 8 in the first figure should be 4, and the 4 in the second figure should be 8. It is evident that the types, as first composed, were correct, that both became loose, adhered to the inking ball, and were drawn out, and that the pressman in replacing transposed them. And this inadvertent error in Blacq's tables of 1628, travelled over three continents, and, with more or less of mischief, remained undetected for two hundred years.

[8] Numerical correctness in logarithmic tables, is then, and has ever been, the great *desideratum.* This Babbage proposed to attain by machinery; to calculate the tables unerringly, as if by a law of nature, and by the same law to reduce them as unerringly to type. Thus was the single purpose of the difference engine No. 1.

This engine was only partially completed. Confided to the care of King's College, it remained for 20 years in the museum at Somerset House. In 1862 it was exhibited at the Great Industrial Exhibition, since which time it has been stored at the South Kensington Museum. The finished portion of the engine showed itself capable of computing any table whose third difference is constant and less than 1,000 (?). At the same time it showed the position in the table of each tabular number. In Babbage's own words:

1st. The portion of the machine exhibited can calculate any table whose third difference is constant and less than 10.

2nd. It can show how much more rapidly astronomical tables can be calculated in any engine in which there is no constant difference.

3rd. It can be employed to illustrate those singular laws which might continue to be produced through ages, and yet after an enormous interval of time change into other different laws; each again to exist for ages, and then to be superseded by new laws.

It will be borne in mind that all work upon difference engine No. 1 was stopped in the early part of the year 1833. At the general meeting of the Royal Academy at Brussels in May, 1835, a letter received from Babbage was read announcing that he had been engaged for six months in making drawings of a new calculating machine of far greater power. He wrote:

I am myself astonished at the power I have been enabled to give to this machine: a year ago I should not have believed this result possible. The machine is intended to contain a hundred variables, each consisting of 25 figures; it will reduce to tables almost all equations of finite difference; it will calculate a thousand values (of e. g. a b c d by the formula $p = \sqrt{a+b}/ca)$ [how about *d?*], *print them, and reduce them to zero, and will then ring a bell to give notice that a new set of constants must be inserted. When there exists a relation between any* *number of successive coefficients of a series, provided it can be expressed, the machine will calculate them and make their terms known in succession; and it may afterward be disposed to find the value of the series for all the values of the variable*.

[9] This was the first announcement to the scientific world of a machine, capable of executing not merely arithmetical calculations, but even those of analysis when the laws are known. It was, in fact, the analytical engine, never destined to be completed by its inventor in actual fact, but so perfect in its drawings, so clear in its descriptions, so certain in its sequences, and so logical in all its principles, that, to the minds of men capable of comprehending the details, it became as certainly the realization of a gigantic idea as if it had been doing its work in their presence. If it be asked, how much such a machine could of itself, without recourse to thought, assume the successive dispositions necessary, Babbage answers that Jacquard solved the problem when he invented his loom.

In the manufacture of brocade there are two species of threads, the one longitudinal, which is the *warp*, the other transverse, which is the *woof*.

Of course the analytical engine could not originate. It would have always been the servant, never the master. It could have done whatever its inventor *knew how to order it to do*. No more. It assisted, marvelously indeed, but it only assisted in making the *known* available. It could have *followed* analysis, never *anticipated* it. But had it been constructed, it would have achieved three *desiderata* of science, *economy of time, economy of intelligence, rigid accuracy*. It would have made observations fertile that are now barren for lack of computer powers; it would have saved time for contemplation that is now wasted in arid calculations by men of genius, and it would have made *certain* arithmetical numbers, without the aid of which the veil that envelopes the mysteries of nature can never be raised.

As illustrative of the estimate put upon the operations of the analytical machine, it may not be inappropriate to quote here Babbage's own remarks:

An excellent friend of mine, the late Professor MacCallagh of Dublin, was discussing with me the various powers of the analytical engine. After a long conversation he inquired what the machine could do, if, in the midst of algebraic operations, it was required to perform logarithmic or trigonometric operations. My answer was, that whenever the analytical engine should exist, all the developments of formula would be directed by the condition, that the machine should be able to compute their numerical value in the shortest possible time. I then added that if this answer was not satisfactory, I had provided means by which, with equal accuracy, it might compute by logarithmic or other tables.

I explained that the tables to be used must, of course, be computed and punched on cards by the machine, in which case they would undoubtedly be correct. I then added, that when the machine wanted a tabular number it would ring a bell and then stop itself. On this the attendant would look at a certain part of the machine and find that it wanted the logarithm of a given number, say of 2303; the attendant would go to the drawer, take the required logarithmic card, and place *it on the machine. Upon this the engine would first ascertain whether the assistant had or had not given it the correct logarithm of the number. If so, it would use it and continue its work. But if the engine found the attendant had given it a wrong logarithm, it would then ring a louder bell and stop itself. On the attendant again examining the engine, he would observe the words, WRONG TABULAR NUMBER, and then discover that he really had given the wrong logarithm, and of course would have to replace it by the right one*.

As between the two engines, the difference and the analytical, their powers and principles of construction, the capabilities of the latter would have been immeasurably the more extensive. They hold to each other, in fact, the same relationship that analysis holds to arithmetic. The difference engine was intended to effect but one particular series of operations. It was not the *general* expression even of *one particular* function, much less of any and all possible functions of all degrees of generality. Indeed, it could do nothing but add. It certainly performed the processes of subtraction, multiplication, and division, but then only so far as these could be reduced to a series of additions. The analytical machine, on the contrary, would have been able to add or subtract, multiply or divide, it could have done either and all with equal facility, and it would have performed these operations directly in each case without the aid of any of the other three. This fact implies everything. The one engine merely tabulated but never developed; the other both tabulated and developed.

[10] Babbage's third invention, which he named *difference engine, No. 2,* need not be dwelt upon here. It was never built. Its drawings even were never quite completed. As an entity it had no existence out of his own mind. In labouring to perfect the analytical machine he discovered the means of simplifying and expediting the mechanical processes of difference engine No. 1. The Earl of Rosse, who was greatly interested in the application of mechanism to purposes of calculation, and who was well acquainted with the drawings and notations of the second difference engine so far as made, proposed that Babbage should perfect and give them to the government, upon condition that they would undertake to construct it. To this, with some reluctance, he consented. It was then proposed to the Earl of Derby, he being prime minister, that the government should apply to the president of the Institution of Civil Engineers to ascertain

1st. Whether it was possible from Babbage's drawings and notations, to make an estimate of the cost of constructing the machine.

2nd. In case this question was answered in the affirmative, then could a mechanical engineer be found who would undertake to construct it and at what expense.

It was explained to Lord Derby that the cessation of work upon the first difference engine was owing to no fault of Babbage; that, being new in design and construction, it had necessarily been costly; that the necessity of constructing and, in many instances, inventing tools and machinery of great complexity for forming with requisite precision parts of the apparatus dissimilar to any used in ordinary mechanical works, had produced unavoidable delays, and that the foremost men of practical science all over Europe who were acquainted with the facts, so far from being surprised at the time and expense that had been required to bring the engine to its then present state, felt much more disposed to wonder that it had been possible to accomplish so much.

And Babbage wrote to the minister:

If this work upon which I have bestowed so much time and thought were a mere triumph over mechanical difficulties, or simply curious, or if the execution of such engines were of doubtful practicability or utility, some justification might be found for the course which has been taken; but I venture to assert that no mathematician who has a reputation to lose will ever publicly express an opinion that such a machine would be useless if made, and that no man distinguished as a civil engineer will venture to declare the construction of such machinery impracticable.

It seemed now (1852) as if there were a probability that government would order a resumption of the work. The Earl of Derby was a man of large gifts and extended views, and his chancellor of the exchequer, himself the son of a philosopher, was known as widely almost by his philosophic sentiments as by his great power of debate. The country was at peace. The first exhibition of the whole world's industry had by its marvellous success the previous year given a new impulse to the arts. Politics, indeed, ran high, but in every other aspect there was encouragement. The Royal Society; the Society of Civil Engineers; the Royal Academy of Sciences at Brussels; the principal philosophical mechanics of the three kingdoms[5], led by the Earl of Rosse and Sir Benjamin Hawes; the astronomical observers following in the bold path opened by Sir John Herschel[6]; and Prince Albert, the most accomplished, as he was the most judicious, of thinking men; together with Plana. Menabria, MacCullagh, Mosotti, Plantamour, Dr. Lardner, and Lady Lovelace – the last an example, almost equal to that of Mrs. Somerville, of the power sometimes possessed by the female mind in dealing with abstract truths – all gave the weight of their opinion in favour of the difference engine, when completed, as fully adequate to the attainment of the objects proposed by the inventor.

[11] *No enterprise*, said the president of the Royal Society, when reciting the history of the engine at their anniversary in 1854, –

No enterprise could have had its beginning under more auspicious circumstances. The government had taken the initiative; they had called for advice, and the adviser was the highest scientific authority in this country – your council guided by such men as Derby, Wollaston, and Herschel. By your council the undertaking was inaugurated; by your council it was watched over in its progress. That the first great effort to employ the powers of calculating mechanism, in aid of the human intellect should have been suffered in this great country to expire fruitless because there was no tangible evidence of immediate profit, as a British subject I deeply regret, and as a fellow my regret is accompanied with feelings of bitter disappointment. Where a question has once been disposed of, succeeding governments rarely reopen it. Still, I thought I should not be doing my duty if I did not take some opportunity of bringing the facts once more before government.

This was accordingly done. It was shown that mechanical engineering tools, trained workmen, the founder's art, and screw-cutting machines, had made such progress during the years the difference engine had been laid aside that it was probable persons could be found willing to complete it for a specific sum. Never had a ministry a nobler opportunity to illustrate its history by the encouragement of science. It was, however, all in vain. Art was weighed against gold, and the former touched the beam. The chancellor of the exchequer, to whom Lord Derby referred the question, pronounced the project as

1. Indefinitely expensive
2. The ultimate success problematic.
3. The expenditure utterly incapable of being calculated.

Babbage characteristically remarked that

This Herostratus of science, if he escape oblivion, will be linked with the destroyer of the Ephesian Temple.

It would be unjust to the memory of the great philosophical mechanist were no reference made to the incidental invention of a mechanical notation which Babbage explained in a paper read before the Royal Society in 1826. Dr. Lardner entitled it a discovery of *the utmost practical value*, and it has long been adopted as a topic of lectures in institutions all over Europe for the instruction of civil engineers. It came up in this wise: Memory has its limit. There cannot be borne in mind a great variety of motions propagated simultaneously through complicated trains of mechanism. Incompatible motions will encounter each other. The memory can neither guard against, nor correct them. Some expedient which at a glance could exhibit what every moving piece in the machinery was doing at each instant was needed. Necessity, the mother of invention, suggested to Babbage a system of signs by which the mechanist, simply moving his finger along a certain line could follow out the motion of every piece from effect to cause until he arrived at the prime mover. The same sign which indicated the *source* of motion indicated also its *species*. It also divided time into parts, showing what was being done by a machine at any moment.

[12] By this means the contriver understood the situation *instanter*, saw as if by intuition the fault, and discovered the *niche* in which to place the movement required. It also enabled the inventor to dismiss from is mind the arrangement of the mechanism. Like algebraic signs, it reduced wheels and valves, rods and levers, to an equation. In fact, what algebra is to arithmetic Babbage's notation was to mechanism.

During the construction of some parts of the calculating machinery a question arose as to the best method of producing and arranging certain series of motions necessary to calculate and print a number. Babbage, with his assistant, an eminent practical engineer, had so arranged these motions that they might be performed by twelve revolutions of the principal axis. It was desirable there should be less. To this end each put himself to work, the engineer to a study of the complicated working machinery, the inventor to a consideration of his notation symbols. After a short time, by some transposition of these, the latter succeeded in producing the series by *eight* turns of the axis. Pushing his inquiries, he proceeded to ascertain whether his scheme of symbols did not admit of a still more compact arrangement, and whether eight revolutions were not needless waste of power. The question was exceedingly abstruse.

Finding every effort to keep in mind the order and arrangement of wheels and pulleys, levers and shafts, claws and bolts, to suggest any improved arrangement, the engineer completely broke down. Babbage, however, with scarcely any mental exertion, and merely by sliding a bit of ruled pasteboard up and down his plan in search of vacant places, contrived at length to reduce the eight motions to six, to five, and to three. This application of an almost metaphysical system of abstract signs, by which the motion of the hand alternately performs the office of the mind and practical mechanics, to the construction of a complicated engine, is regarded by many eminent engineers as the most wonderful and useful discovery the great inventor ever made.

Although no one of the principal inventions of the philosophical mechanist has ever been completed, and though his marvellously comprehensive thoughts of what machinery, working on the borderland of intellect, might be made to accomplish would seem to have passed from the world without good yet his work was not in vain. Hundreds of mechanical appliances in the factories and workshops of Europe and America, scores of ingenious expedients in mining and architecture, the construction of bridges and boring of tunnels, and a world of tools by which labour is benefited and the arts improved – all the overflowing of a mind so rich that its very waste became valuable to utilize – came from Charles Babbage. He more, perhaps, than any man who ever lived, narrowed the chasm that from earliest ages has separated science and practical mechanics.

[13] This memoir has thus far treated its subject as a mathematician and philosophical mechanist. He was both, in a degree that made his name famous. But he was more than this. As a scientific man, keeping himself abreast with the progress of modern discovery; as a man of intellect, accepting, analysing and suggesting thought that is emancipating mind from old traditions; and as a man of his time, the associate for more than half a century of statesmen and poets, chemists and geographers, engineers and philologists, he is worthy of notice. Upon whatever he spoke or wrote he was always perspicuous. Language was to him pre-eminently the embodiment of ideas. Logical sequence was the one essential element of his train of thinking. His estimate of men was formed less from what they were than from what they did. He was neither tuft-hunter nor cynic. Faults his character possessed, grievous and ridiculous, perchance, when viewed in certain lights, but they were never inconsistent with his independent manliness, nor derogatory to his elevated philosophy. He knew his own worth; asserted his rightful claims; kept an un-quailing aspect in his long single-handed fight in behalf of his inventions with purblind rulers; victorious never, but never vanquished; heroic in most that he said and all that he did; above ordinary stature; and, saving perhaps the acceptance of certain rules of obedience to law, without which no one can wisely govern himself, played a part in the drama of life that will not be soon forgotten.

[14] It is proposed now to speak of Charles Babbage in the two characters: of an *observer of his time* and as a *contributor to knowledge*. In each, as the most certain way to reach the end in view, we shall quote without restriction or further acknowledgement from his

own writings. After a friendly breakfast he said to some scientific friends:

My engine will count the natural numbers as far as the millionth term. It will then commence a new series, following a different law. This it suddenly abandons and calculates another series by another law. This again is followed by another, and still another. It may go on throughout all time. An observer, seeing a new law coming at certain periods, and going out at others, might find in the mechanism a parallel to the laws of life. That all men die is the result of a vast induction of instances. That one or more men at given times shall be restored to life, may be as much a consequence of the law of existence appointed for man at his creation, as the appearance and re-appearance of the isolated cases of apparent exception in the arithmetical machine. Miracles, therefore may not be the breach of established laws, but the very circumstances that indicate the existence of higher laws, which at appointed times produce the pre-intended results.

For example, the analytical engine might be so set that at definite periods known only to its maker, a certain lever might become movable during the calculations. The consequence of moving it might be to cause the then existing law to be violated for one or more times, after which the original law would resume its reign. Of course, the maker of the calculating machine might confide this fact to the person using it, who would thus be gifted with the power of prophesy if he foretold the event, or of working a miracle at the proper time. Such is the analogy between the construction of machinery to calculate, and the occurrences of miracles. A further illustration may be taken from geometry: curves are represented by equations. In certain curves there are portions such as ovals disconnected from the rest of the curve. By properly assigning the values of the constants, these ovals may be reduced to single points. These singular points may exist upon a branch of a curve, or may be entirely isolated from it; yet these points fulfil by their position the law of the curve as perfectly as any of those which, by their juxtaposition and continuity, form any of its branches.

Miracles are not therefore the breach of established laws, but the very circumstances that indicate the existence of far higher laws which, at the appointed times, produce their results.

Now, whatever may be thought of the conclusiveness of this reasoning its originality is obvious, and its ingenuity undeniable. That it was satisfactory to a mind whose reach was as wide and whose logic as consecutive as that of Charles Babbage is sufficient to demand for it fair consideration. He evidently believed it; urged it upon other minds upon the same level with his own, and received no answers that detected in it a fallacy or showed it to be a sophism.

[15] There is surpassing interest in watching the working of a great mind in *honest* search after truth. There are no volumes of the fathers; no sermons of Laurin or Bossuet; no essays of Fénelon or Pascal; no personal narrative of Arnaud, Francoise de Sales, de Raneé, or of the saints of Port Royal; no memoirs of the pietists of France, or martyrs of England; no lives of foreign missionaries, Protestant or Catholic, who gave their all, even to death, to propagate what to them was Divine that in our apprehension can confine the attention or challenge

the judgement of a sincere, intelligent inquirer after truth, like the 30th chapter in the *Passages from the life of a philosopher*. One sees in it no favourite opinion to be defended; no peculiar error to be denounced; no class, no creed, no caste to be built up; no prejudice to be favoured nor tradition exempted from trial; nothing in fact but the record of the thoughts of a great mind in honest pursuit of truth. It would be marred by quotations[7], and its life deadened by condensation; though it does not traverse he ground of more modern scepticism, and deals only with the old positions of the encyclopaedists and Hume, it assumes a position in regard to Divine revelation which, if not impregnable, has never yet been overturned.

We cannot really resist the temptation to quote a few of his clear and vigorous remarks from the chapter in question. Speaking of an examination of the Creator's works as one of the sources of our knowledge of His existence, Babbage says:

Unlike transmitted testimony, which is weakened at every stage, its evidence derives confirmation from the progress of the individual as well as from the advancement of the knowledge of the race.

Almost all thinking men who have studied the laws which govern the animate and inanimate world around us agree that the belief in the existence of one Supreme Creator, possessed of infinite wisdom and power, is open to far less difficulties than the supposition of the absence of any cause, or the existence of a plurality of causes.

In the works of the Creator, ever open to our examination, we possess a firm basis on which to raise the superstructure of an enlightened creed. The more man inquires into the laws which regulate the material universe, the more he is convinced that all its varied forms arise from the action of a few simple principles. These principles themselves converge, with accelerated force, toward one still more comprehensive law to which all matter seems to be submitted. Simple as that law may possibly be, it must be remembered that it is only one among an infinite number of simple laws; that each of these laws has consequences at least as extensive ass the existing one, and therefore that the Creator who selected the present law must have foreseen the consequences of all other laws.

The works of the Creator, ever present to our senses, give a living and perpetual testimony of his wisdom and goodness far surpassing any evidence transmitted through human testimony. The testimony of men becomes fainter at every stage of transmission, while each new inquiry into the works of the Almighty gives to us more exalted views of his wisdom, his goodness and his power.

The true value of the Christian religion in Babbage's estimation rested not upon speculative views of the Creator, which must necessarily be different in each individual according to the extent of the finite being who employs his own feeble powers in contemplating the infinite, but rather upon those doctrines of kindness and benevolence which that religion claims and enforces, not merely in favour of man himself but of every creature susceptible of pain or of happiness.

There is something exceedingly refreshing in the original views Babbage takes of every subject that comes within the scope of his vision. His autobiography, for such in spite of his disclaimer it really is, has the interest of a romance. He is never dull, never tiresome, never cloudy. His style is clear as limpid water and natural as a running brook. He possesses a rich fund of humour which flecks and dapples even his mathematical descriptions like sunshine falling through foliage.

In the chapter we do not willingly leave he says;

A curious reflection presents itself, when we meditate upon a state of rewards and punishments in a future life. We must possess the memory of what we did during our existence upon this earth to give them those characteristics. In fact, memory seems to be the only faculty which must of necessity be preserved to render a future state possible.

If memory be absolutely destroyed, our personal identity is lost.

Further reflection suggests that in a future state we may, as it were, awake to the recollection that, previously to this our present life, we existed in some former state, possibly in many former ones, and that the then state of existence may have been the consequences of our conduct in those former states.

It would be a very interesting research if naturalists could devise any means for showing that the dragon fly, in its three stages of a grub beneath the soil, an animal living in the water, and that of a flying insect, had in the last stage any memory of its existence in its first.

Another question connected with this subject offers still greater difficulty. Man possesses five sources of knowledge through his senses. He proudly thinks himself the highest work of the Almighty Architect, but it is quite possible that he may be the very lowest. If other animals possess senses of a different nature from ours, it can scarcely be possible that we could ever be aware of the fact. Yet those animals, having other sources of information and of pleasure might, though despised by us, yet enjoy a corporal as well as intellectual existence far higher than our own.

[16] Babbage's autobiography, relating isolated facts, which, with a sort of indifference to the estimate history might put upon his character, strongly in contrast with even the best class of journals and diaries, say, Sir Walter Scott's or Dr. Chalmer's, or Edward Payson's, or Missionary Johnson's, as if while it was necessary that *they* should take care of their *post mortem* fame *his* possessed the vitality to care for itself, are arranged without order of time or similarity of subject, after all divides itself very naturally into the two branches of personal recollections and personal experiences. He remembers Wollaston, Rogers and Sir Humphrey Davy, and gives pen-outlines of their characters as vivid and living as the portraits of Duow. He has discussed mathematics with Laplace, compared analyses with Fourier, exhibited and explained his inventions to Biot, and lived on terms of intimacy with Humboldt. He was the frequent companion of the Duke of Wellington; was the associate of various branches of the Bonaparte family; was the friend of Mosotti, Menabria, and Prince Albert, and throughout life, from collegiate competitions to the mutual respect of mature years, held firmly as his friend the younger Herschel. Of all these his notes are pictures, unequalled even by the descriptions which Boswell gives of the associates of the great lexicographer.

It is the same with his experiences. He risks drowning by water and baking by fire, loss of life by railway speed and loss of reputation by picking locks, character in exploring the secrets of theatrical displays, and purse in traversing the haunts of St. Giles. His thirst for knowledge knew no bounds. Into an electioneering contest he entered with the same indomitable energy that he has pursued a mathematical calculus (!). The same keen activity that detected a logarithmic error was applied to suppressing a street nuisance. He vitalized whatever he touched. If life gives beauty it might be more truly said of Babbage than of most men of mark, *Nihil tetigit qoud non ornavit* [He touched nothing without embellishing it]. In fact there was no secret of nature he hesitated to explore, no enigma of the sphynx which he was afraid to question. Impulsiveness, want of patience and hatred of shams have indeed left many of is investigations partial and fragmentary, but about every one of them there is rich compensation in striking aphorisms, profound observations, wisdom applicable to human need, and wit available for its enjoyment. He says of himself:

I have always carefully watched the exercise of my own faculties, and I have always endeavoured to collect from the light reflected by other minds some explanation of the question.

I think one of my most important guiding principles has been this:

That every moment of my waking hours has always been occupied by some train of inquiry. In far the largest number of instances the subject might be trivial, but still work of inquiry was always going on.

The difficulty consisted in adapting the work to the state of the body. The necessary training was difficult. Whenever at night I found myself sleepless and wished to sleep, I took a subject for examination that required little mental effort, and which also had little dependence on wordly affairs by its success or failure.

On the other hand, when I wanted to concentrate my whole mind upon an important subject, I studied during the day all the minor accessories and after 2 o'clock in the morning I found that the repose which the nuisances of the London streets only allow from that hour until 6 in the morning.

At first I had many a sleepless night before I could thus train myself.

I believe my early perception of the immense power of signs in aiding the reasoning faculty contributed much to whatever success I may have had. Probably a still more important element was the intimate conviction I possessed that the highest object a reasonable being could pursue was to endeavour to discover those laws of mind by which man's intellect passes from the known to the discovery of the unknown.

In perusing the writings of Babbage, one is constantly struck with the philosophical nature of his mind. His style is not only pregnant with thought, but, like Montaigne's, is perpetually shaping itself into apothegms. He writes, when managing an election contest,:

Men will always give themselves tenfold more trouble to crush a man obnoxious to their hatred, than they will take to serve their most favoured ally.

Again, speaking of Dr. Lardner, who had candidly admitted that some of those doctrines he had once supported further information had shown him were erroneous, our author says:

Nothing is more injurious to the progress of truth than to reproach any man who honestly admits he has been in error.

[17] To put down street organ-grinders, with whom he had lifelong quarrels, he proposes to himself to act upon this principle:

To make it more unprofitable to the offender to do the wrong than the right.

It requires considerable training to become an accurate witness of facts. No two persons, however well trained, ever express in the same form of words the series of facts they have both observed.

Once, at a large dinner party, Mr. Rogers, author of Italy and other poems, was speaking of an inconvenience arising from the custom, then commencing, of having windows formed of one large sheet of glass. He said that a short time ago he sat at dinner with his back to one of these single panes of plate-glass; it appeared to him that the window was wide open and such was the force of the imagination that he actually caught cold.

It so happened that I was sitting just opposite to the poet. Hearing this remark, I immediately said, Dear me, how odd it is, Mr. Rogers, that you and I should make such a very different use of the faculty of imagination. When I go to the house of a friend in the country and unexpectedly remain for the night, having no night-cap I should naturally catch cold. But by tying a piece of pack-thread tightly round my head, I go to sleep imagining that I have a night cap on; consequently I catch no cold at all.

I was once asked by an astute and sarcastic magistrate, whether I seriously believed that a man's brain would be injured by listening to an organ. My reply was, Certainly not, for the obvious reason that no man having brains ever listened to street musicians.

These fragmentary quotations, however, scarcely do Babbage justice. Let us allow him to tell one of the many experiences of his life in his own way. Under the head of *Hints for travellers* in his *Passages from …* Babbage says:

A man may, without being a proficient in any science, often make himself useful to those who are most instructed. However limited the path he may himself pursue, he will insensibly acquire other information in return for that which he can communicate. I will illustrate this by one of my own pursuits. I possess the smallest possible acquaintness with the vast fields of animal life, but at an early period I was struck by the numerical regularity of the pulsations and the breathings. It appeared to me that there must exist some relation between these two functions. Accordingly I took every opportunity of counting the numbers of pulsations and the breathings of various animals. The pig fair at Pavia and the book fair at Leipzig equally placed before me menageries in which I could collect such facts. Every zoological collection of animals which I visited thus became to me a source of facts relating to that subject. This led me at another period to generalise the subject of inquiry, and to print a skeleton form for the constants of the class mammalia. It was reprinted by the British Association at Cambridge in 1833, and also at Brussels in the Travaux du Congrès Général de Statistique in 1853[8].

One of the most useful accomplishments for a philosophical traveller I learned from a workman who taught me how to punch a hole in a *plate of glass. The process is simple. Two centre-punches, a hammer, an ordinary bench vise, and an old file, are all the tools required. Having decided upon the part of the glass, scratch a cross (x) upon the spot with the point of an old file, turn the glass over and scratch the same on the other side corresponding. Fix one of the small centre-punches with its point upward in the vise. Let an assistant hold the glass with its scratched point (x) resting upon the point of the punch. Take the other punch, place its point in the centre of the upper scratch, hit it very slightly twice or thrice, turn the glass two or three times, repeating the slight blows, and the hole is formed.*

The principles of this are, that glass is a material breaking in every direction with a conchoidal fracture, and that the vibrations which would have caused cracking are checked by the support of the fixed centre-punch.

In the year 1825, during a visit to Devonport, I had apartments in the house of a glazier, of whom I inquired one day if he knew this secret. He answered that he did not, and expressed great curiosity to see it done. Finding that at a short distance there was a blacksmith, we went to his shop, and selecting from his rough tools the centre-punches and the hammer, I executed the whole process.

On the eve of my departure I asked for my landlord's account, which was sent up correct except the omission of charge for apartments. I added the eight guineas for my lodgings; and the next morning, having placed the total amount upon the bill, I sent for my host to pay him, remarking that he had omitted the principal article of his account, which I had inserted.

He replied that he had intentionally omitted the lodgings, as he could not think of taking payment for them from a gentleman who had done him so great service. Quite unconscious of having rendered him any service, I asked him to explain. He replied that he had the contract for the supply and repair of the lamps of Devonport, and that the art in which I had instructed him would save him more than 20 pounds a year. I found some difficulty in prevailing on my grateful landlord to accept what was justly his due.

[18] Scarcely at the risk of being tedious, which no passages in the life of this extraordinary man can ever be, but at the greater risk of space which must be devoted to his contributions to knowledge, we cannot forbear a single quotation further, which, like a dash from the brush of Rubens, depicts the multifariousness of his character:

While I was preparing materials for the *Economy* … (1832) I had occasion frequently to travel through our mining and manufacturing districts. On these occasions I found the travellers' inn or travellers' room was usually the best adapted to my purpose, both in regard to economy and to information. As my inquiries had a wide range, I found ample assistance in carrying them on. Nobody doubted that I was one of the craft; but opinions were widely different as to the department in which I practiced my vocation.

In one of my tours I passed a very agreeable week at the Commercial Hotel in Sheffield. One evening we sat up after supper much later than usual, discussing a variety of commercial subjects.

When I came down rather late for breakfast I found only one of my acquaintances of the previous evening remaining. He remarked that we had had a very agreeable party last night, to which I assented. He referred to the intelligent remarks of some of our party, and then added that when I left them they began to talk about me. I merely added that I felt quite safe in their hands, but should be glad to profit by their remarks. It appeared, when I retired for the night, that they debated about what trade I travelled for. My informant said that

The tall gentleman in the corner maintained that you were in the hardware line, while the fat gentleman, who sat next you at supper, was quite sure that you were in the spirit trade. Another of the party declared that they were both mistaken; he said he had met you before, and that you were travelling f or a great iron-master.

I said, well, you, I presume, knew my vocation better than your friends? *Yes, said my informant. I knew perfectly well that you were in the Nottingham lace trade*.

In the year 1828 Babbage was nominated to the Lucasian professorship of mathematics in his old university, occupying in that capacity a chair which had been held by no less a man than Sir Isaac Newton. This chair he held for eleven years It was while holding this professsorship, at the general election of November, 1832, which followed on the passage of the first reform bill[9], that he was put forward as a candidate for the representation of Finsbury in Parliament. He stood in the advanced liberal interest as a supporter not only of parliamenttary, financial, and fiscal reform, but of the ballot, triennial parliaments, and the abolition of all sinecure posts and offices. But the electors did not care to choose a philosopher; so he was unsuccessful, and never again wooed the suffrages of any constituency.

Babbage was the author of published works to the extent of some 80 papers. A full list of these, however, would not interest or edify the reader. Perhaps the best known of them all is what he styled the *Ninth Bridgewater Treatiuse*[10] (which it was not), a work designed at once to refute the doctrine, supposed to be implied in the first volume of that learned series, that an ardent devotion to mathematical studies is unfavourable to a real religious faith; and also to adduce specimens of the defensive aid which the science of numbers may give to the evidences of Christianity, if that science be studied in the proper spirit. As compared with the eight treatises written by Chalmers, Whewell, Sir Charles Bell, Dr. Buckland, and others, so far from discrediting its supposititious name, it has probably been more generally read than any work of the series.

Babbage's contributions to political economy were both incidental and direct. The tendency of his mind, upon whatever it was engaged, was toward the practical. There is scarcely one of his works, nay, there is hardly one of the various employments in which he engaged himself with his whole soul during his long life that in its ultimate reach does not lay hold of the industrial condition of mankind. Keen in investigation, acute in analysis, subtle in detection of error, and preeminently logical in conclusions, no matter how purely intellectual may be the laboratory of his workings, the experiments he makes and the outlooks in which he indulges have for their end invariably the material benefit of the working classes.

[19] Whether it be the solution of *problems relating to the calculus of functions* or to the *knight's move in chess*, whether the *determina-*

tion of the general term of a new class of infinite series or the *application of machinery to the computation of mathematical tables*, the *measurement of heights* or *the improvement of diving bells, proportion of letters occurring in various languages* or *observations on the Temple of Serapis, thoughts on the principles of taxation* or *statistics of lighthouses*, his purpose in every essay is practical good. He enlivens the dry subject of political economy by the most interesting and pertinent anecdotes; draws the attention of engine-drivers and stokers to his abstruse discussions of curves and gauges on railways by maxims and rules that are of constant use; discusses the subject of Greenwich time-signals[11] with a variety of illustrations that makes it attractive to every shipmaster, mingles his philosophical theories on occulting lights with narratives of observations and experiences that amuse and instruct the most ordinary minds: and treats the vexed questions of glaciers with a liveliness and perspicuity which interest if they do not convince.

The reader will judge whether we have overestimated or misunderstood the real characteristics of Babbage's mind from the examples we now propose to give from some of his contributions to knowledge.

Babbage was one of the oldest members of the Royal Society at the time of his death in October of last year. He was also more than half a century ago one of the founders of the Astronomical Society, and he and Sir John Herschel were the last survivors of those founders. He was also an active and zealous member of many of the leading learned societies of London and Edinburgh, and, in former years at least, an extensive contributor to their published transactions. His last important publication was the amusing and only too characteristic autobiographical work from which we have freely quoted, *Passages from the Life of a Philosopher*.

There were methods of action, qualities they might perhaps be more properly be called, in the mind of Charles Babbage that recall to the philosophical peruser of his works in exact sciences traits not dissimilar in kind, however distinct in degree to those possessed by that most original of all thinkers, Sir Isaac Newton. He possessed in common with Newton extraordinary powers of intellectual introversion. What he desired to accomplish he *thought out*. His mind, like a photographic plate, was cleansed by a continued force of will to think rightly, and when cleansed received the impressions from the light of truth. Not only his contributions to knowledge and his complex and intricate calculating machines, but the scores of lesser inventions which he produced from time to time, are illustrative of this. Like Newton, he first pondered his facts, illuminated them by persistent thought, and then proceeded to the principles on which these facts depend.

[20] Pestalozzi, the Italian philanthropist, after a long life spent in works of benevolence, came at last to the conclusion that no man could be much helped or hindered by anyone than himself. The remark is applicable to Babbage more than to most persons. He both made and marred his own fortune. There was not a place which he ever sought (the Lucasian chair he did not seek) that he gained. He aspired to the professorship of mathematics at the East India College at Harleyburgh; to Playfair's chair at Edinburgh, to a seat at the Board

of Longitude; to the mastership of the mint; and to the office of registrar-general of births and deaths – and failed in all. On the other hand, there was not an invention connected with his name – and in mathematical mechanics he ranks among the foremost the world ever produced – which, in the opinion of the best-disciplined minds of his day, he could not have perfected had sufficient pecuniary means been at his command. Unfortunately, he measured everything by his own unaided impressions, and judged himself by others instead of judging others by himself.

To rest all claim to greatness on self-assertion rather than self-denial, though it may have made the heroes of the classic ages, cannot but be a grave fault in the conduct of any modern life[12]. Still, he bore his disappointments bravely, possessed his intellect undimmed up to the verge of his fourth score year, made his old age a lesson – not unwisely at any time enforced – of the philosophy with which the rest of death may be awaited, and was to the last ready to contemplate calmly in his own case what arose to the thought of Antony –

I have been sitting longer at life's feast
Than does me good. I will rise and go.

Notes

1. This *heresy* exists now also, although in a restricted sense.
2. Both father and son Brunel were Fellows of the Royal Society (the son, since 1830). The father is also mentioned in § 5. It was the Swedish engineer Brinel who introduced the scale of hardness.
3. The letter-patent entitled its holder to complete a definite work or enjoy some privilege.
4. Various tables are described in Lebedev & Fedorova (1956) and Fletcher et al (1962).
5. The three kingdoms: England, Scotland, Ireland.
6. The astronomer John Herschel (1792 – 1871) is primarily known by discovery of more than three thousand double stars and compilation of a catalogue of nebulas and star clusters.
7. But the author does quote this chapter.
8. Babbage's preliminary publication appeared in 1826. Some questions concerned man (e. g., morbidity of workmen) as well as distribution of animals and plants.
9. This bill was officially called *Representation of the People Act*.
10. F. H. Bridgewater who died in 1829 left moneys for publication of books proving Divine Providence by studies of the animated nature. The executor of his will, the then President of the Royal Society, had selected eight authors for the planned series, and eight books have duly appeared.
11. These signals became radio signals.
12. This contradicts the author's remark in § 13.

Information about some figures

Albert, Prince (1819 – 1861), public figure. One of the organizers of the industrial exhibition of 1851.

Baily Fr. (1774 – 1844), astronomer, mathematician

Barlow P. (1776 – 1862), mathematician, physicist

Boswell J. (1740 – 1795), lawyer, writer

Brunel I. K. (1806 – 1859), engineer, naval architect.

Brunel M. I., father of I. K. (1769 – 1859), architect, engineer

Colby T. F. (1784 – 1852), geographer

D'Arbley A.-J.-B.-P. (1754 – 1818), mathematician

Davies G. (1767 – 1834), engineer, President of Royal Society
Davy H. (1778 – 1829), chemist
Dodson J. (1705 – 1757), mathematician, actuary
Earl of Derby, E. S. Stanley (1775 – 1851), statesman
Earl of Rosse, W. Parsons (1800 – 1867), astronomer
Herostratos, ca. 356BC destroyed the Temple of Artemis
Howes B. (1797 – 1862), statesman. A namesake: a scientist.
Hutton Ch. (1737 – 1823), mathematician
Lardner D. (1793 – 1859), astronomer, popularizer
Lovelace Ada (1815 – 1852). Mathematician. Daughter of Byron
Lucas H. In 1663 a chair of mathematics was established at Cambridge and named after him since he gave the necessary moneys
MacCullagh J. (1809 – 1847), mathematician
Maule W. H. (1788 – 1858), mathematician
Montagne M. (1533 – 1592), philosopher
Mosotti, physicist
Pestalozzi J. H. (1746 – 1827), pedagogue, philanthropist
Plantamour E. (1815 – 1882), astronomer
Playfair J. (1748 – 1819), mathematician, geologist
Rogers S. (1763 – 1855), poet
Sommerville M. F. (1780 – 1872), mathematician, physicist. One of first scientists among women
Whewell W. (1794 – 1866), naturalist
Wollaston W. H. (1766 – 1828), physician, physicist, chemist
Young T. (1773 – 1829), mathematician, naturalist

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1824, Observations on the measurement of heights by the barometer. *Works*, vol. 4, pp. 11 – 13.

1826, On a method of expressing by signs the action of machinery. *Works*, vol. 3, pp. 209 – 223.

1826, Diving bell. *Works*, vol. 4, pp. 74 – 103.

1831, Sur l'emploi plus ou moins fréquent des mêmes lettres dans les différente langues. *Works*, vol. 4, pp. 124 – 125. English transla-tion pp. 126 – 127.

1847, Observations on the temple of Serapi etc. *Works*, vol. 4, pp. 165 – 217.

1852, Thoughts on the principle of taxation etc. *Works*, vol. 5, pp. 31 – 56.

1853, On the statistics of lighthouses. *Works*, vol. 5, pp. 57 – 65.

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Authorship of Dodge is ascertained by his paper (2000).

He inserted a great number of very long but not very needful quotations from Babbage whereas their summaries would have been more useful. His (and at least partially Babbage's) thoughts about religion are much more properly discussed at the end of my paper on Arbuthnot in this collection. I will only add that apparently even in the 17th century scientists decided that, since nature was the direct (without any intermediate human efforts) creation of God, its study was more important than Biblical studies. And a particular comment. In § 18 Dodge very weakly called mathematics a science of numbers.

III

A. Quetelet

Charles Babbage

Excerpts from unnamed paper in *Annuaire de l'Observatoire royal de Bruxelles*, pour 1873, 1873, pp. 183 – 187

Babbage says, in his passage from the *Life of a philosopher*,

From my earliest years I had a great desire to inquire into the causes of all things and events which astonish the childish mind. At a later period I commenced the still more important inquiry into those laws of thought and those aids which assist the human mind in passing from received knowledge to that other knowledge then unknown to our race.

These few lines express sufficiently well the character of the distinguished *savant* whose career we shall endeavour rapidly to sketch. .Notwithstanding his own ardent desire to inquire into everything which could interest himself, our author never seems to have dreamed of informing others as to his exact age. According to his friends, he was born in 1792, and was consequently about 80 at the time of his death.

He did not begin seriously the study of mathematics until after the age of 22, when he was with his friend Herschel at Trinity College, Cambridge. They soon after published a joint work on mathematics, which did much toward introducing the continental methods and notation of this science into England. Fourteen years after this, while Babbage was in Rome, he accidently read in an English newspaper the following paragraph:

Yesterday the bells of St. Mary rang out a peal to celebrate the election of Charles Babbage as Lucasian professor of mathematics at Cambridge,

or, in other words, his appointment to the chair formerly occupied by Newton.

It was in Paris, in 1826, at a dinner given by Bouvard, the astronomer, that I had an opportunity to become acquainted with Babbage. There were at the time present Poisson and several other of the scientists who then made Paris illustrious, which all of whom he was a centre of interest. He, with truly fraternal kindness, offered me his assistance in procuring from the English mechanicians, among whom was the celebrated Troughton, the instruments for the Belgian observatory. He also proposed my cooperation in a work which he had projected which was to contain a register of everything capable of being measured, such as the specific gravity of bodies; the linear expansion of metals; their weight; the size of animals; the quantity of air they breathe; the nourishment they need etc. I said,

The extent of this work is too vast to be carried out unless by the cooperation of many minds. The outline of what may be necessary for man alone is so great that with the help of many friends I could not hope to complete more than a skeleton of the whole.

The reply was that time is an element of solution which overcomes the greatest difficulties of investigation; and if our efforts are properly directed our descendants will finish what we have properly begun.

Notwithstanding his immense labour connected with the calculating machine, Babbage, in April 1835 turned his attention to assist his friend Hershel, then at the Cape of Good Hope, in carrying out over the whole world, on certain days, a system of meteorological observations. These days, which were called *term-days*, were the 21st of December, of March, June and September. At these times continued observations were to be made at every hour, commencing at noon on these days and terminating the next day at the same hour. These observations, in whose introduction Babbage took an active part, were continued in Europe, America, India, and Africa, and led finally to the establishment of the various systems of simultaneous weather-reports of the present day.

While I was in London, in 1851, at the great exhibition of industrial products, Babbage made me acquainted with Lord Lovelace, a gentleman of great ability and high reputation, who had married the cherished daughter of Lord Byron. This charming lady, remarkable for her beauty and personal accomplishments, and noted for her intellectual powers, had published a translation of an Italian account of the calculating machine. She received me very graciously, and urged Babbage and myself to visit her frequently for conversation on literary and scientific subjects, with which she was familiar. She was especially interested in the calculus of probabilities, and so far did we carry out discussions on this point that it was agreed that we should compose and publish a joint work on this subject. Unfortunately, the plan was prevented from being carried out by the premature death of this interesting lady.

I owe it to the friendship which long united me with Babbage to having seen in London, on several occasions and in the greatest detail, all the parts of the calculating machine, and to having been able to form for myself a just conception of a labour of which I had often heard but of which very few people knew the particulars. The machine is certainly very complicated, and extreme attention is needed to follow the action of its different parts. Hence, I shall not attempt to give its description which would unquestionably fill quite a considerable volume if we paid respect to the ideas of the inventor, to the extreme perfection of the mechanical workmanship, and to all the mathematical calculations which the machine can perform.

Researches into statistics also claimed the attention of Babbage, and he was personally instrumental in adding to the committees of the British Association [for the Advancement of Science] one on this subject. The attention of the commission on statistics was first turned to the need of exact documents in regard to population, a want much felt in England, especially as to everything relative to births, deaths, etc. Meetings were afterward held in London of persons interested in the subject of statistics, in which Babbage took an active part, and to which I was admitted. They examined, among other questions, that of the labour imposed upon children in manufactures. The following questions were propounded to me in regard to Belgium, which I transmitted to the minister of interior, who promised to have collected the necessary data for a satisfactory reply. The honourable savants asked

The number of births produced by each marriage during the entire length.

The proportional number of children who reach the period of marriage.

The number of children living by each marriage.

The salaries paid in manufactures and agriculture in different provinces, especially the price of an average day's labour in agriculture.

The quantity of wheat which such a day's pay can procure in ordinary times.

The mean price of different kinds of grain.

The habitual food of the day labourer.

The proportional number of sterile marriages.

The proportional number of marriages having five or six children living.

As an instance of our friend's singular disposition to enter upon investigations of the most out of the way character, I may mention that for a time he lost sight of the profound speculations of political economy, and busied himself with the question as to how many times any letter in different languages doubles itself in 10,000 words. The following table gives the result which he obtained in English, French, Italian, German and Latin. [I only mention some figures]:

The most doubled letter;
E: letter E, 18.9 times; F: letter L, 53.5 times; I: letter L, 70.6 times; G: letter S, 53.5 times; L: letter R, 41.7 times
Total number of all double letters, correspondingly,
141.8; 215.5; 230.8; 166.5; 147.7

[In regard to the question of what use is this, we would remark that this question is never asked by the student of nature; since every item of knowledge is connected in some way with all other knowledge. Nothing can be said to be useless which tends to exhibit new relations, and indeed it is impossible to say *a priori* that a given fact may not find an application even in practice, however remote it may seem from anything of this kind. The results given in the foregoing investigation may be of importance in determining the casting of double types. The number of occurrences of a given letter in 10,000 words of any language determines the number of types of that letter in a font. J. H. (Apparently John Herschel. O. S.)]

Our physicist always took care in travelling, to carry with him those instruments which would enable him to carry on some investigations. He was essentially a man of experiment. He held that the eye and the ear were great aids to the judgement, and a demonstration never seemed to him complete until he knew how to render it evident to the sense and the reason. Toward the end of his life his vivacity was considerably moderated, and the mortification which he felt on account of not being able to complete his calculating machine, and the loss of friends, cast a shadow over his latter days.

[I had myself the pleasure to make the acquaintance of Babbage in 1837, while he was in the zenith of his mental power, and to witness the operation of his first calculating machine. I again visited him in 1870, after an interval of just one third of a century. I found him in the same house, still interested in the calculating machine, with apparently but little diminution of mental activity. He informed me that he felt himself gradually declining; that he endeavoured to note the change in himself; that he found it difficult to enter upon new subjects of thought, but that he could reason and mentally act on materials already in his mind in the way of new computations and new deductions. He regretted the loss of memory, since with it was the loss of personal identity. J. H.]

IV

Oscar Sheynin

Gumbel, Einstein and Russia

1. Introduction

Emil Julius Gumbel (1891 – 1966) was an outstanding German and later American statistician best known for his work on the extreme-value theory. I describe his political activities (not leaving aside its statistical element) and his unpublished correspondence with Einstein, and attempt to show why he, and many more celebrated Western intellectuals had been supporting the Soviet Union in the 1920s – 1930s in spite of the horrors there perpetrated. I also dwell on Gumbel's unknown connections with other mathematicians and natural scientists including Mises and Bortkiewicz.

In the 1920s, Gumbel tirelessly battled against the rightist movement in his native Germany, and among his likeminded colleagues was Einstein with whom Gumbel became closely associated. This activity coupled with his Jewish origin made him a prominent target of various attacks, in particular by students infected with Nazism. His academic career had been blocked for many years and his very life was endangered[1].

In 1933 he emigrated to France, and in 1940 barely escaped to the United States where he lived and worked until his death. Gumbel never ceased his social and political activities. In France, he tried his best to help his fellow-refugees and denounced Nazism, and in the US he published several political papers and letters in newspapers[2] and became a member of two bodies for the liberation of Germany (Jansen 1991, p. 390).

In 1926, Gumbel worked for several months in Moscow and visited the Soviet Union in 1932. Because of the situation in Germany, he wished for some time to remain there permanently, but happily failed (end of § 4).

Johnson & Kotz (1997) briefly described Gumbel's life and work and cited previous pertinent writings[3] whereas Jansen (1991) and Vogt (1991) examined his political activities. Both Jansen and Gumbel [28] include reprints of quite a few of his political contributions and the former, drawing on archival sources, also appended a valuable list of 583 Gumbel's writings and related materials[4]. However, it is composed pell-mell: scientific works, tiny reviews, popular pieces (about 30 in all), some independently entered translations of Gumbel's works, anniversary articles, abstracts, political writings, and literature about him, – all these items follow one after another chronologically. A few of Gumbel's papers in the Russian periodical *Matematichesky Sbornik* are recorded there twice, the second time as though having also appeared in *Recueil Math. Soc. Math. Moscou* which is the additional French title of the same journal.

Jansen's description of Gumbel's life and work is based on many archival and newspaper sources, but he had not provided a bibliography of the pertinent comments, nor did he furnish a list of his numerous abbreviations. Again, he had not offered a proper bibliographic description of Gumbel's contributions included in his book: in a few cases he mentioned the appropriate English articles, – but who translated them, and/or changed their original titles?

I consider Gumbel's writings and statements on Russia (§ 2) and his unpublished correspondence with Einstein (§ 3)[5]. In § 2 I also indicate some previously unknown points concerning Gumbel the statistician. In a special section (§ 4) I examine the implications of § 2 and provide Gumbel's conclusions in a historical perspective by describing the relevant views of other intellectuals. I consider the Einstein – Gumbel correspondence in several subsections one of which is devoted to Einstein's political thinking. There, drawing on previous authors, I begin by sketching his attitude towards the Soviet Union.

Gumbel allegedly desired to describe Russia comprehensively and readers might have indeed expected that he, having been a statistician and an economist[6], had painted a truthful picture, but he did not.

I draw on the *Bolshaia Sovetskaia Enziklopedia* [Great Soviet Encyclopaedia], three editions: 66 vols, 1926 – 1947, 51 vol., 1950 – 1958, 30 vols, 1969 – 1978, respectively. The third edition is available in an English translation (separate translation of each volume). I abbreviate this source as BSE or GSE respectively and in the latter case I indicate the appropriate years of both versions.

I conclude here by a letter from Gustav Radbruch[7] of 24 Nov. 1930 to Einstein (46519, see Note 5) and a description of the related developments. Here is the letter itself.

Gestatten Sie mir, streng vertraulich und ohne Wissen des Hauptbeteiligten mich mit einer Bitte an Sie zu wenden, die ich nur durch das Bewusstsein der Gesinnungsgemeinschaft zu rechtfertigen vermag. Sie haben früher bereits an der Angelegenheit des hiesigen Privatdozenten und jetzigen Professors[8] *Dr. Gumbel Anteil genommen.*

Sie wissen auch, dass in den letzten Wochen von nationalsozialistischer Seite aus Anlass der Ernennung Gumbels zum Titularprofessor nicht nur unter Berufung auf die sechs Jahre alte unglückliche Äußerung Gumbels vom "Feld der Unehre" [9]*, sondern auch auf seine gesamte Enthüllungspolitik gegen Geheimrüstungen, politische Morde und Fememorde der Kampf gegen Gumbel erneuert worden ist. Wie die Dinge auf deutschen Universitäten einmal liegen, fürchte ich, dass, – weniger infolge einer entschiedenen politischen Rechtseinstellung als, was schlimmer ist, infolge von Konfliktsangst, – kaum eine Fakultät mehr den Mut finden wird, Gumbel zu berufen.*

Für Heidelberg aber ist der Fall Gumbel eine unerschöpfliche Quelle immer neuer Beunruhigungen, die gerade auch wegen der Angreifbarkeit des ursprünglichen Ausgangspunktes der ganzen Hetze die Stellung der links stehenden Heidelberger Professoren sehr erschweren. Ich glaube dass Gumbel trotz unleugbarer Taktfehler in seiner Vergangenheit durch seinen ebenso unleugbar großen politischen Mut es verdienst, dass man sich seiner Zukunft annimmt. Über Gumbels mathematische und statistische Fähigkeiten und Leistungen *steht mir zwar kein Urteil zu, aber sie werden, soweit mir bekannt ist, von Fachleuten hoch eingeschätzt.*

Und so möchte ich die Frage und Bitte an Sie, hochverehrter Herr Professor richten, ob Sie nicht in der Lage sind, Ihren großen Einfluss für eine Berufung Gumbels in eine seinen Fähigkeiten und Leistungen entsprechende andere Position, etwa bei der Kaiser Wilhelm Gesellschaft[10]*, einzusetzen. Ich darf nochmals betonen, dass dieser Brief ohne Wissen Gumbels ergeht, er ergeht aber im Einverständnis meines nationalökonomischen Kollegen Lederer, der die akademischen Aussichten Gumbels unter den gegebenen Verhältnissen ebenso ungünstig beurteilt wie ich und auch seinerseits nur von Ihnen noch Hilfe erwartet.*

On 29 Nov. 1930 Radbruch (46520) thanked Einstein for his answer, and on 27 Nov. Lederer (46522) wrote to Einstein as well. He largely repeated Radbruch's letter; described Gumbel's strained circumstances; and stated that the "nationalsozialistischen Studenten" will likely resort to ruthless attacks against Gumbel. And he also explained how Gumbel was invited to Heidelberg:

Er wurde uns seinerzeit, als wir einen Statistiker gewinnen mussten, von Prof. Von Bortkiewicz – Berlin aufswärmste empfohlen, und die Wertschätzung der Fachkreise geht ja auch aus der guten Resonanz seiner Publikationen in der Literatur hervor.

Bortkiewicz rarely recommended anyone (Woytinsky 1961, pp. 452 – 453)! Extracts from Einstein's answers to Radbruch (§ 3.1.1 and § 3.3) were published as a single whole in the Editorial (1931, p. 109). I partly reproduce Einstein's answer to Lederer in § 3.3.

Acknowledgement. The Albert Einstein Archives, The Jewish national and University Library, Hebrew University of Jerusalem, that keeps the Einstein correspondence, allowed me to quote/publish the relevant letters. I am also thankful to Dr. Barbara Wolff, Assistant Curator of the Archives, for copies of the relevant letters and to Dr. A. L. Dmitriev (Petersburg) for some Russian materials.

2. Russia

2.1. The Year 1922. For a leftist intellectual whom Gumbel became, it was natural to turn his attention to Russia; in 1922, he published his first pertinent publication [2]. There, he stated that Soviet Russia served as a catalyst of social struggle the world over (p. 194) and that communism was "our wish" (p. 195).

Gumbel added, however, that the Bolshevist way to it led "durch Blut und Hunger"[11]. He thought that the transition to communism by parliamentary methods was impossible (p. 195)[12], that the Soviets failed to ensure the participation of masses in governing Russia (p. 199) with all power having gone to the Bolshevist party (p. 200). However, the downfall of the Soviets will not necessarily be repeated elsewhere (Ibidem)[13]. The proper way to communism, Gumbel also stated, lay through a "geistiger Wechsel" with which the Bolsheviks do not agree because of their materialistic philosophy (p. 198). Other necessary conditions for the transition of a country to communism are its healthy economy and a majority approval of the changes (p. 202). Gumbel (p. 199) recognized that Russia must have a "gebundenes Wirtschaftssystem" with yet unknown features but he did not elaborate[14].

2.2. The Year 1926

2.2.1. Marx's Mathematical Manuscripts. On 21 June 1925 Gumbel (43811) asked Einstein to recommend him, in particular, to the eminent biologist Julius Schaxel hoping that the latter will help him find a position in Moscow[15]. His plan proved only partly suc-cessful. Indeed, on 30 April 1926 Gumbel (43814) informed Einstein:

Ich war jetzt sechs Monate in Moskau und habe im Marx – Engels[16] *Institut die sogen. Mathematischen Manuskripte von Marx druckreif gemacht. Es handelt sich dabei um Notizen zur Differenzialrechnung, die ein gewisses philosophisches Interesse besitzen und zeigen, dass Marx die Anfangsgründe des Differenzierens wohl beherrscht hat. Meine Arbeitsbedingungen waren außerordentlich günstig. Allerdings lebt die Mehrzahl der dortigen Gelehrten in großer Notlage.*

An article by Kolman (1968)[17] preceded the publication of Marx's manuscripts (MSS) (1968). He (p. 104) ridiculously alleged that Marx's statements on mean values in economics were of exceptional methodological value for mathematical statistics. Then Kolman (p. 106) reported that the Marx – Engels Institute had entrusted Gumbel with "working on the manuscripts", but that he "was unable to appreciate in full measure either the importance of their publication or their philosophical and historical – mathematical significance"[18].

This is doubtful (see Gumbel's letter above) and in any case Gumbel published a preliminary report [9] where he classified the MSS; then, his final report had never appeared (Vogt 1991, pp. 20 – 22) whereas Yanovskaia, the future eminent specialist in mathematical logic who eventually prepared the MSS for publication, had spent incomparably more time on this than Gumbel had[19].

Quite a few mathematical articles were devoted to these MSS, e.g., Kennedy (1977), who referred to earlier commentators. A preliminary version of the MSS is Marx (1933). It was accompanied by Yanovskaia's commentary (1933) and preceded by an introductory note by the Marx – Engels – Lenin Institute (where Gumbel was not mentioned). It is also necessary to cite Glivenko (1934). To conclude, Marx's contributions do not reflect his studies of mathematics[20] and that his MSS contain no items on statistics or probability[21].

2.2.2. Statistics and Class Struggle. During his work in Moscow, Gumbel apparently met Schmidt who then held some position at the Communist Academy there[22]. Indeed, on 14 Dec. 1926 he wrote a letter to Schmidt [28, pp. 179 – 180] describing the contents of five of his prepared "works" on probability and asked whether they will interest the "verehrter Genosse Otto Julewitsch". At least three of these had really been put out in Russian periodicals. In all, Gumbel published five political writings (1923 – 1937) and ten scientific contributions in the Soviet Union[23], not all of them in Russian. Some of them appeared earlier, and some later in the West.

Soon after leaving Moscow, Gumbel published a paper on statistics and class struggle [6] and his observations on life in the Soviet Union, see below and § 2.2.3. Regarding the class nature of statistics in capitalist countries, Gumbel [6] stated that

Due to moral and economic reasons, statistics is unable to discover the causes of social phenomena (p. 132).

Statistical data (on harvests, p. 142; unemployment and industrial accidents, p. 147), are distorted or hushed up (prostitution, abortions, p. 139 so that the ensuing calculations, e.g., of subsistence levels, p. 147) are wrong.

Although statisticians may well consider themselves objective, the application of statistics "belongs" to the ideological class struggle (p. 133).

Many statisticians attempt to prove Malthusianism (p. 134). However, taken by itself the notion of overpopulation is meaningless (p. 135). And the aging of the population is of no consequence as compared with the other evils of the society[24].

Criminal statistics shows the devastating nature of capitalism (p. 140). It reflects the intensity of class struggle; not by chance did Czarist Russia possess ideal pertinent data (pp. 141 – 142).

It is difficult to understand his last statement especially since elsewhere Gumbel [8, p. 106] maintained that statistics in pre-revolutionary Russia was "ganz unentwickelt"[25] (but where is zemstvo sta.-tistics?). Gumbel tacitly assumed that capitalism was unable to change and naively thought that the socialist system was much superior. Thus, the shackles restricting statistics will only disappear in a classless society[26]. Two additional points. First, Gumbel noted that statistics was connected with national economy which was the reason for its low scientific level (p. 134); that an empirical check of the so-called laws of the latter was still impossible (p. 142); and that (p. 148) only mathematical statistics will be able to solve the problems of economics. These statements may be regarded as heuristic arguments in favour of creating the then not yet existing econometrics[27].

Second, I quote Gumbel's extraordinary declaration (p. 141):

Bei politischen Morden selbst ist zu unterscheiden, ob sie revolutionär oder konterrevolutionär sind.

Only one step thus separated him from exonerating the death sentences meted out by phoney courts in Russia[28]. To some extent, Gumbel repeated his deliberations elsewhere [11, Bd. 5] and the notorious statement just above is also there (p. 19).

2.2.3. Gumbel's Travel Notes. Gumbel [8, p. 83] saw the overall social problem confronting the world as tracing the route to socialism; and the main question (p. 164) was, how long capitalism will still survive[29]. The "usual formal democracy" of the Western type will not do, what is needed is dictatorship of the proletariat (p. 113)[30].

Accordingly, the restoration of Russia's economy achieved in the absence of private ownership is the Russian communists' "immortal merit" (p. 112).

The terrorism, that the communists unleashed during the previous years against profiteers and even petty violators of the draconian commercial regulations, was economically justified (p. 99). Horrible political terrorism also took place (pp. 100 and 125) but it was only a side-effect of the civil war (p. 125) and partly occasioned by sabotage (p. 95). At present, capital punishment is "often" pronounced (p. 126), and the secret police, the GPU, enjoys the right to exile citizens from the main cities; again, the GPU "often" imprisons people for months on end before even beginning the investigation[31]. The New Economic Policy (NEP) which was introduced in 1921 brought about some economic freedom, and Gumbel noted the presence of street vendors (p. 120), privately working physicians (p. 125) and private publishers (p. 144). Overall, the existing economic system is state capitalism with a socio-political bias ("Einschlag") (p. 110); or, state capitalism coupled with a detestable bureaucracy (p. 164)[32], also see below.

In spite of the official materialistic philosophy (p. 132), practical idealism is widespread (p. 133) and this constitutes "perhaps" the greatest ethical merit of the Russian communists; top people remain poor (Ibidem; but see § 2.3), and, more generally, party members are not allowed to earn more than an established amount of money (p. 114)[33].

"Usual" prisoners may leave jail once in a month (p. 126), soldiers are free to spend nights outside the barracks (p. 149) and foreign newspapers are sold in town (p. 135). Naïve comments on the relation between the state and the Russian Orthodox Church follow (p. 140)[34]. All power belongs to the party within which there exists democracy (p. 113) and quite exceptional opinions are tolerated (p. 136). The author apparently sees no contradiction between these statements and his other observations: "from time to time" purges are taking place in the party (p. 114) and deviationists are punished and even expelled from the party (p. 142). He (p. 159) also notes "political struggles" going on in the party, names Zinoviev and Trotsky[35] and correctly remarks that communism is a religion of sorts (pp. 140 – 141).

Civil rights do not exist (p. 116); even foxtrot is banned (p. 119). The complicated voting system ensures "necessary" results (pp. 103 and 115), the national republics cannot actually leave the Union (p. 116) and Zionism is forbidden (p. 139). Only 60% of the children attend school (p. 129), the professorial staff is underpaid (p. 130) but researchers fare good enough (p. 131). The evolution theory is the most important discipline of natural sciences whereas the theory of relativity was for a long time regarded as hostile (p. 133) and all scientific problems are considered together with their "final philosophical consequences" (p. 134)[36].

The housing conditions in Moscow are horrible which is a corollary of its having become the capital and of the influx of rural population rather than the communists' fault (pp. 121 – 123)[37]. Bureaucracy is omnipotent (pp. 116 – 117 and 155). So as to prevent the build-up of a new bourgeoisie, draconian measures are being taken since 1924 against successful NEP-men (p. 157). Gumbel lists these measures (both political and economic) and adds that economic steps should be applied instead; he apparently thought about subtle "European" methods. The black-market value of the rouble is lower than its official value and often experiences slumps (p. 109).

The agrarian problem is the most acute issue (p. 105). A half of the peasants is poor (p. 107) and depends on the rich ones, the kulaks (p. 102). The situation is dangerous and agricultural cooperation is necessary (pp. 107 and 162)[38]. Either the state, or the kulaks and the NEP-men will accumulate capital more rapidly and the stability will persist or not, respectively (p. 163). Gumbel is thus apparently prepared to abandon his advice regarding subtle economic measures (above).

Colonies are the soft spots of imperialism; Russia supports their nations (p. 152) and the Red Army might possibly help a revolution elsewhere (p. 148)[39]. The independence of Finland, the Baltic states and Poland was recognized on the strength of the right of nations to self-determination (p. 147)[40].

2.3. The Year 1932. In 1932 Gumbel spent three weeks in Moscow and published his new travel notes [17]. As compared with 1926, Moscow became better-looking (not many beggars; no waifs or strays; less hawkers; more state-owned cars and trucks), but the housing situation worsened [still more] (p. 298). Inflation did exist and is dangerous because industrial plans, when formulated financially, become fictitious; however, without any capitalist class present, nobody benefits from its action (p. 302). He should have said: nobody benefits except the state (for example, due to almost forced participation of the working people in yearly long-term state loans) whose interests did not at all coincide with the desires of the population, see Note 14. Food was rationed and its shortages led to hoarding (p. 300); the black-market cost of a Deutschmark was ten times higher than its official value (p. 301)[41].

The top people were poor ("persönlich arm")[42] but frightfully powerful (p. 299) whereas scholars were compelled to toe the political line (p. 301). In principle, Russian problems are solved (p. 305); contrary to the situation in the West, people are living better than before; "from their sweat, blood and tears new factories belonging to them [?] are being built" (p. 306).

2.4. The Eye-Opening years. From 1934 onward, Gumbel began to express second thoughts (Jansen 1991, p. 67). In his letters of 1936 and 1938 he wrote about his deep disillusionment. "Insbesondere", as Jansen claims, he was affected by the Moscow "Schauprozesse" of these years.

No less indicative was the decision of Heinrich Mann, Gumbel and "andere" who founded, in 1937, a *Bund freiheitlicher Sozialisten*, to separate themselves "programmatisch scharf gegen den Marxismus" (Jansen 1991, p. 42)[43]. It seems nevertheless that (Ibidem, p. 67)

Bei aller Skepsis [much too weak] *über den sowjetischen Weg zum Sozialismus hatte er* [Gumbel, in 1934 – 1936] *doch am historisch materialistischen Fortschrittsdenken festgehalten.*

Also in 1937, Gumbel undoubtedly had to note the absence of any Soviet mathematician (e.g., of Kolmogorov and Khinchin) at a conference on probability theory (Compliments 1937) attended by such figures as Cramér, de Finetti, Feller, Hostinský and Polya and by him himself.

In 1939 Gumbel signed a manifesto prepared by the German members of the *Union Franco-Allemande* which claimed that the Hitler – Stalin pact was a betrayal of peace by Russia (Jansen 1991, p. 44).

In 1954, Gumbel [23, p. 329] scornfully described the situation in East Berlin, and, on p. 330, he mockingly called the late Stalin the greatest philosopher "of our time".

In 1957, reporting on his travel impressions, Gumbel (Jansen 1991, p. 70) said that

Die Stalinisten der Sowjetzone [of Germany] *sind Papageien, die Worte eines Herrn nachplappern, der längst töt ist.*

In 1960, Gumbel [25, p. 338] did not restrict his criticism of East Germany to food shortages (Note 14). His verdict was, that the emigration from there

Verdankt sich nicht nur materiellen Gründen. Grundlegend ist der intellektuelle Druck und der Mangel an Sicherheit.

In 1961, Gumbel [26, pp. 264 – 268] described Russia's participation in Germany's secret rearmament (1922 – 1933)[44] and remarked (pp. 265 – 266) that "All diese Tatsachen … waren bereits in der Weimarer Republik bekannt" – and to him as well?

Then, he [26, pp. 268 – 269] denounced the "russischen Prozesse":

Von 1937 an reinigte Stalin die Partei von den alten Bolschewiken.… tausende wurden nach geheimen Verfahren hingerichtet … [In 1956] *hat Chruschtschow Stalin als großen wahnsinnigen Tyrannen angeprangert …*

Finally, in 1964 Gumbel reviewed an English translation of one of Solzhenitsin's officially published novels. He [27] remarked that the real situation in the Soviet Union became known even earlier[45] and that the author had properly chosen to show the fate of an ordinary man who was thrown into a labour camp just in case, and, practically speaking, for life. Although Gumbel believed that there were "perhaps" 10 *mln* such victims [see § 4], he did not say anything about his earlier illusions.

3. Einstein

3.1. He Tries To Help Gumbel. From 1923 to 1932 Einstein wrote at least six letters recommending Gumbel to five universities, all of them beyond Germany, and in a few other cases he expressed his willingness to help him secure an academic position and/or his high opinion of Gumbel.

3.1.1. Einstein's Opinion. His letter of 28 Nov. 1930 (46526) to Radbruch.

Herr Gumbel ist zweifellos als Fachman[n] *hinreichend tüchtig, um als Vertreter seines Faches an einer Hochschule zu wirken. Als Persönlichkeit schätze ich ihm noch viel höher. Sein politisches Wirken und seine Publikationen sind von einem hohen Ethos getragen …*

Das Richtigste für Herrn Gumbel dürfte es wohl sein, an einer ausländischen Universität eine Stelle zu suchen. Ich habe mich in *diesen Sinne schon öfter für ihn bemüht und bin gerne bereit, mich jederzeit für ihn einzusetzen …*

His letter of 25 July 1932 (50120) to Gumbel.

Es ist mir klar, dass Sie von hier fort sollen. … Wenn Sie mir eine Stelle oder eine Persönlichkeit angeben, will ich gerne dorthin schreiben.

His letter of 2 Jan. 1932 (50110), probably to E. Montel[46].

Ich schätze ihn [Gumbel] *sehr hoch … unter den gegenwärtigen Verhältnissen nicht nur seine Position, sondern auch sein Leben bedroht ist.*

His letter of 16 May 1933 (38615) to Gumbel.

Charakterleistungen sind ebenso viel Wert wie wissenschaftliche; deshalb brauchen Sie nicht in den Schatten zu stellen.

This was Einstein's partial response to Gumbel's letter of 10 May 1933 (38614). There, Gumbel described the difficult conditions of life for German academics who had fled to France, mentioned an appropriate "Vorschlag" made by Perrin and concluded by stating (more generally) that

Ein großer Teil der Abgesetzten, wie etwa Franck, Born etc. [et al] *steht so hoch, dass ein Vorschlag meinerseits gar nicht notwendig erscheint*[47].

His letter of 12 Oct. 1943 (55236) to Gumbel. "… bin ich bereit Sie dort [wo Statistiker gebraucht werden] zu empfehlen".

3.1.2. He Recommends Gumbel. His letter of 15 April 1923 (43810) to C. F. [?] Nicolai in Cordova [evidently, South America][48].

Herr Dr. Gumbel ist mir seit einer Reihe von Jahren als ein scharfer wissenschaftlicher Geist und als vortrefflicher Mensch aufs beste bekannt. Von Studium Physiker hat er sich als Spezialgebiet die Statistik im weitesten Sinn gewählt, deren Berührungspunkte mit der Nationalökonomie ja zutage liegen. In seiner schriftstellerischen Tätigkeit hat er allgemein politische und nationalökonomische Fragen behandelt, soweit sie die Gegenwart betreffen. … Ich bin überzeugt, dass er vermöge seiner großen Belesenheit und der Beweglichkeit seines Geistes sehr wohl geeignet wäre als Lehrer der Nationalökonomie zu wirken.

His letter of 25 Jan. 1928 (46508) to Karl Pearson.

Ich schätze Herrn Dr. Gumbel sowohl persönlich wie als außerordentlich intelligenten wissenschaftlichen Arbeiter sehr hoch, wenn *ich auch in dem hauptsächlich von ihm bearbeiteten Spezialgebiet der Statistik mir kein Urteil erlauben darf.*

Ich möchte erwähnen, dass Herr Gumbel durch zahlreiche mutige politische Schriften sich große Verdienste im öffentlichen Leben Deutschlands um die Gerechtigkeit erworben hat[49].

A few years before that Gumbel published two notes in *Biometrika*, and quite a few letters were exchanged in 1928 in connection with his attempts to secure a (provisional) position at the Galton Laboratory, University College. To achieve this goal, Gumbel applied for a fellowship to the European Office of the then existing International Educational Board[50].

Pearson agreed to take Gumbel on; see the Board's letter of 18 Jan. 1928 to him (46504), and Gumbel's letter of 26 Jan. 1928 to Einstein (46509). Einstein (his letter to Gumbel of 25 Jan. 1928, 46506), however, mentioned "mehrfache schlechte Erfahrungen, die ich [er] mit dem Education Board schon gemacht habe …" Gumbel, as he remarked there, had already overstepped "die obere Altergrenze" for a fellowship.

On 12 May 1928 Gumbel informed Pearson (Pearson Papers 709) that Mises as *proposer* and Bortkiewicz as *seconder* will formally apply for the fellowship, and he also adduced a letter of recommendation from Einstein (apparently lost).

Neither Mises nor Bortliewicz is known to have been engaged in political life of Germany, and a few years later, in 1931, the latter died[51] and the former fled Germany by the end of 1933 or very early in 1934. It is therefore all the more interesting to put on record their attempt to help Gumbel. Furthermore, on 22 April 1931 (46545) a *Geh. Regierungsrat*, Prof. Holde, in a letter to Einstein, listed quite a few intellectuals who were prepared to sign an "Erklärung" supporting Gumbel's efforts to hold his academic position against political attacks. Among these personalities were Radbruch, Rademacher and Mises. Einstein (his previous letter to Holde of 21 April 1931, 46544) was "selbstverständlich bereit Ihren Erklärung zu unterzeichnen".

His letter of 13 April 1931 (46538) to Prof. Berwald (Prague).

Ich habe gehört, dass an der deutschen Universität eine Lehrstelle für theoretische und praktische Wahrscheinlichkeitslehre[52] *zu besetzen ist. Ich empfehle Ihnen für diese Stelle den fähigen und fleißigen Herrn Dr. Gumbel, der an der Universität Heidelberg Privat-Dozent* [see however Note 1] *ist, und von dem ich überzeugt bin, dass er als Lehrer und Forscher die auf ihn gesetzten Erwartungen getreulich erfüllen wird. Er hat sich auch durch Publikationen rechtlich-politischen Charakters große Verdienste erworben, die ihm gegenwärtig gehässige Verfolgungen eintragen, die aber wohl später ihre gerechte Würdigung finden werden.*

His letter of the same date (46540) to *Lieber Herr* Professor Philipp Franck at the same university.

Herr Gumbel ist ein klüger Kopf und hat sich durch seine mutigen Bücher über die Entgleisungen der Militärgewalt in Deutschland ein *wirklich großes Verdienst erworben. Er wird deshalb von der reaktionären akademischen Kamarilla wütend verfolgt. Lassen Sie sich nichts weismachen, sondern stehen Sie bitte mannhaft für ihn ein, wie er es verdient.*

His letter of 2 Jan. 1932 (50110) partly quoted in §3.1.1, likely to Montel.

Herr Gumbel ist zweifellos ein Mann, der mit einem seltenen Mute und seltener Hingabe für Gerechtigkeit und Verbesserung der zwischen-staatlichen Verhältnisse gekämpft hat[53]*. … Gumbel ist auch als wissenschaftlicher Statistiker* (*angewandte Wahrscheinlichkeitstheorie*) *als tüchtiger Fachmann bekannt, wenn auch seine fachlichen Leistungen nicht als* <u>*außergewöhnlich*</u> *bezeichnet werden können.*

And so, Einstein understood statistics as applied probability; above, when mentioning the "practical theory of probability", he also apparently meant statistics. I (1998b; 1999) have discussed the relations between probability and statistics and (1998b, p. 104) noted that Mises, evidently in the 1940s or a bit later, and Neyman, in 1950, had thought that some classes of probability problems belonged to statistics. However, Kolmogorov, in 1938, had kept to the opposite opinion: statistics gradually ceases to be applied probability and probability ought to be considered as a "structural part" of statistics.

Montel answered Einstein on 4 Febr. 1932 (50111): Gumbel was luckily invited to deliver lectures at the *Institut Henri Poincaré*; and Langevin *lui-même* will certainly confirm this.

His letter of 3 Dec. 1932 (50124) to Prof. MacClelland at University of Pennsylvania.

Ich habe erfahren, dass an Ihrer Universität eventuell eine Lehrstelle für mathematische Statistik gegründet wird. Mit Rücksicht auf diese Eventualität erlaube ich mir hiermit, Sie auf Herrn Professor Dr. Gumbel aufmerksam zu machen … Herr Gumbel ist bezüglich seiner Fähigkeiten und seiner menschlichen Qualitäten ein in hohem Masse würdiger Kandidat für eine derartige Lehrstelle. Er wäre wohl schon Inhaber einer ordentlichen Professur an einer deutschen Universität, wenn er nicht durch wertvolle Publikationen allgemeinen allgemein-politischen Inhalts den Zorn der gegenwärtig leider in so hohen Masse irrgeführten studentischen Jugend dieses Landes erweckt hatte.

3.2. His Participation Desired. Gumbel's letter to him of 26 Dec. 1934 (50133).

Das Institut de Science Financière et d'Assurances der Universität Lyon, an dem ich als Assistent tätig bin, beabsichtigt demnächst eine kleine Zeitschrift herauszugeben, welche sich mit Wahrscheinlichkeitstheorie und verwandten Gebieten beschäftigen soll. Bisher haben I. Hadamard, M. Fréchet, G. Darmois und Francis Perrin ihre Mitarbeit zugesagt.

Ich gestatte mir die Anfrage, ob Sie prinzipiell bereit wären, ebenfalls als Mitarbeiter zu figurieren. Darüber hinaus wäre ich Ihnen sehr verbunden, falls Sie bereit wären, uns ein kurzes Leitwort zu senden das wir zu Beginn der ersten Nummer publizieren dürften.

Einstein's response is unknown, but the periodical hardly ever appeared.

Gumbel's letter to him of 18 Nov. 1935 (50135).

Ich erlaube mir, Ihnen in der Anlage [lost] *den Plan zu einem Buch zu übersenden. Obwohl ich mit den Vorbereitungen erst heute anfange, möchte ich Sie bereits in diesem Stadium sei es um Ihre Mitarbeit, sei es um ein Vorwort bitten. Am liebsten wäre es mir, wenn Sie sich mit beidem, zunächst prinzipiell, einverstanden erklären würden. Jede Zeile von Ihnen wäre mir wertvoll.*

Einstein answered on 3 Dec. 1935 (50137).

Ich kann mich mit Ihrer Idee nicht befreunden. Ein Buch mit kurzen Referaten über Facharbeiten aus allen Gebieten kulturellen Schaffens dürfte kaum Absatz finden. Der Umstand, dass die Arbeiten von Vertriebenen herstammen, dürfte kaum für die Käufer einen hinreichenden Anreiz bieten. Was mich betrifft, so wüsste ich überhaupt nicht, wie ich über meine Publikationen in einem solchen Rahmen referieren sollte. Ein Geleitwort könnte ich vielleicht geben, wenn die Sache wirklich gelingen sollte, der ich einstweilen skeptisch gegenüber stehe.

Apparently Einstein had not indeed published any popular account of his work.

Gumbel's letter to him of 1936 (50130).

Gumbel appends a list of participants in his project and the seven titles of their future contributions and again asks Einstein to submit a foreword. The titles include: *Die Gleisschaltung der deutschen Wissenschaft; Finanzpolitik des Nationalsozialismus; Obituary of Emmy Nöther*.

Gumbel's letter to him of 24 Jan. 1936 (50138).

Gumbel lists the seven authors adding that he hopes that about a dozen more will agree. All the authors are refugees from Germany, and among them is Schaxel (Moscow), see beginning of § 2.2.1.

Einstein's letter of 9 July 1936 (50139) to Gumbel; apparently his answer to a missing letter.

Ich kann mich nicht dazu entschließen, das gewünschte Vorwort zu schreiben, zumal ich die geplante Publikation für verfehlt halte. Eine derartige Publikation, welche so bunt gemischte Beiträge enthält, kann weder wirksam, noch finanziell erfolgreich sein.

Gumbel's letter to him of 25 April 1938 (53267).

Einstein's negative answer led Gumbel to change the plan of the proposed book. It will be a collection of contributions written by authors

Die von den Nazionalsozialisten auf ihrem Wissensgebiet erhobenen Forderungen zurückweisen. Insofern ist das Buch gleichzeitig bunt gemischt und doch einheitlich.

Once more, the extant correspondence is apparently incomplete; no answer from Einstein is available. Anyway, the book [28] appeared without Einstein's participation. Gumbel himself contributed an Introduction and wrote several pieces. There is also a section providing information about the authors, Gumbel included (his biography and bibliography, on pp. 231 – 233).

One of Gumbel's notes entitled "Arische Mathematik" [28, pp. 218 – 221] is a non-mathematical review of the first two issues of *Deutsche Mathematik.* Here is what he (p. 221) had to say about Einstein as pictured there:

Einstein spielt die Rolle des bösen Geistes. Sein Werk wird von einem Studenten [!] *als "eine Kampfansage mit dem Ziel der Vernichtung der nordisch-germanischen Naturgefühl" bezeichnet.*

At the same time, as Gumbel remarks, Jewish contributions are cited and generalized in the periodical and the original representation of the “Relativitätsprinzip” is [correctly] attributed to Einstein.

3.3. His Political Views. Over the years, Einstein made many attempts to help the victims of political oppression. In 1947 he (Sayen 1985, p. 207) wrote a letter to Stalin on behalf of Raoul Wallenberg and in 1950 he (Courtois et al 1997, p. 442) protested against the death sentence meted out to a Czech, Milada Horakova, on trumped-up political charges. For Einstein, his endeavours concerning Gumbel, although exceptionally numerous, were not therefore unusual.

During the 1920s – 1930s, Einstein (1960, pp. 194 – 199), together with likeminded intellectuals, had been striving to prevent war in Europe but he avoided anything that would support the Soviet regime; he apparently knew the real situation in the Soviet Union. Even in 1928 he (Courtois et al 1997, p. 819) protested against an earlier trial there of the so-called Industrial Party. Then, in 1932, he (1960, p. 196) remarked that his close friend, Henri Barbusse, had he been a Soviet citizen, would have likely found himself in prison or in exile if left alive at all[54].

Nevertheless, Einstein (Ibidem, p. 334) believed that the Soviet Union laboured to promote international security; actually, did its damnedest to stir up world revolution. And, back in 1926, he praised Gumbel’s essay [8], then not yet published, calling it objective (Jansen 1991, p. 84, without sufficient documentation).

Just the same, by the end of the 1940s he (letter of 1948, Sayen 1985, p. 112) explained away the Russian expansion into Eastern Europe and saw some “great merits” in the doings of the Soviet government. In 1946, because of the threat of a new world war, Einstein (1960, p. 381) proposed to establish a single world government, but the Soviet authorities and obedient Soviet scholars rejected his (not really original) idea (Ibidem, pp. 444 – 450).

In a letter of 1953 Einstein (Sayen 1985, Chapter 17, Note 2) again condemned the Soviet and Czech political trials. Next year, however, in another letter, he (Ibidem, p. 210) stated that criticisms “cannot help” because “the Russians” will not hear them. He was patently wrong. In spite of permanent jamming, many Russians had by that time acquired the habit of listening to programmes broadcast from abroad by several stations.

I continue with describing Einstein’s archival materials concerned with Gumbel.

His letter of 28 Nov. 1930 (46526) to Radbruch partly quoted in § 3.1.1.

Das Verhalten der akademischen Jugend gegen ihm [Gumbel] *ist eines der traurigen Zeichen der Zeit, welche das Ideal der Gerechtigkeit, Toleranz und Wahrheit so wenig hochhält. Was soll aus einem Volke werden, dass solche Zeitgenossen brutal verfolgt und dessen Führer* [Hindenburg] *dem gemeinen Haufen keinen Widerstand entgegensetzen?*

His letter of 3 Dec. 1930 (46524) to E. Lederer.

Es scheint, dass man in Deutschland dem Studententerror gegenübersteht wie einem Naturereignis. Der Balkan hat seine Grenzen westwärts verschoben[55] *… Zum großen Teil beruht die Verblendung der Jugend auf einer in diesem Lande früher kultivierten, jetzt wenigstens geduldeten Glorifizierung des Militarismus und "Heldentums". Auch die Demokraten und Sozialisten machen in diesem gefährlichen Punkt Kompromisse und sehen nicht, dass sie an diesem Strick leicht aufgehängt werden können.*

The last phrase was prophetic!

His letter of 25 March 1931 (46529) to Radbruch; see its beginning in § 2.2.2.

Gumbel's Buch [13] *habe ich neulich zum Teil gelesen und aufs Neue den Mann, seine Intelligenz, seine noble Gesinnung und seine Energie bewundert. Es ist furchtbar, wie man die unerfahrene Jugend hier aus eigennützigen Beweg Gründen irreführt. Wenn es so weitergeht, werden wir über ein fasc*[h]*istisches Gewaltregime zum roten Terror kommen.*

Einstein had not explained his last statement, but at least he correctly noted the similarity between Nazism and practical communism, as I would say.

His letter of the same date (46527) to Gumbel.

Ich habe neulich in Ihrem Buche [13] *mit voller Bewunderung gelesen. Wie schrecklich wird doch die Jugend in diesem Lande irregeführt, aus wie niederen Motiven!*

His letter of 9 July 1936 (50139) to Gumbel.

Ich finde, dass es sich in Amerika gut lebt und arbeitet. Ich habe seit Jahren nicht die Möglichkeit gehabt, so still und zurückgezogen zu leben. Frankreich ist einstweilen der einzige Lichtblick, aber wie Lange? Wird Blum[56] *wirklich genug sein, um mit seinen mächtigen und raffinierten Gegnern fertig zu werden?*

His letter of 28 June 1952 (59894) to Gumbel.

Der Gedanke, einen solchen korporativen Brief einzusenden, hat etwelche Berechtigung. Der Haken liegt aber in Folgendem. Wenn der Brief ausschließlich oder hauptsächlich von Refugees unterzeichnet wird, also von Juden, dann werden die Gegner sagen, er komme von nicht objektiven Leuten. Wenn aber koschere Gojim mitmachen sollen, kann man sich schwer auf einen Text einigen.

Der vorgeschlagene Text ist meiner Absicht nach nicht gut. Das Hauptargument ist doch, dass die Remilitarisierung fast zwangsläufig zum Weltkriege führen muss. Aus diesem Grunde ist nach meiner Ansicht der Plan hier ursprünglich in Szene gesetzt worden. Heute aber, wo die Pleite in Korea etwas moderierend gewirkt haben dürfte, ist es schwer, einen honorigen Rückzug zu bewerkstelligen, nach der langen systematischen Hetze.

Wenn so ein Brief überhaupt inszeniert wird, muss James Warburg[57] *genannt werden, der den Kampf sozusagen allein geführt und durch sehr gute Argumentation gestützt hat.*

The response above was apparently occasioned by a draft (June 1952, 59895) of what likely became a letter co-authored by Gumbel, but not Einstein, and soon published in several American newspapers [22] which I have not seen. Here are a few extracts from the draft.

The rearming of Germany in any form will soon harm the interests of the United States. … The German masses are against remilitarization. … The militarists and the rightist elements would rather make an accord with the Soviets … The treaty[58] *will strengthen Russian domination of Eastern Europe and Russian influence in the West.*

So much for Gumbel's toying with communism!

Einstein's letter of 25 Nov. 1948 to Solovine (also see Note 56) apparently throws light on this issue.

There are attempts to uphold "our" policy of bringing the Nazism back to power in Germany in order to use them against the wicked Russians. It is hard to believe that men learn so little from their toughest experiences. Following his suggestion, I sent Hadamard a telegram to support opposition to the policy.

4. The Soviet Union: Facts and Impressions

During ca. 70 years, the Soviet regime either exterminated or indirectly brought to death 20 *mln* of its citizens (Courtous et al 1997, p. 14)[59]. No wonder that Upton Sinclair (1962, p. 305) in 1957 compared Stalin ("the Lenin of today" with "Tamerlane [Timur] or Genghis Khan, or any other of the wholesale slaughterers of history"[60]. Just one illustration (Solzhenitsin 1974, vol. 1, pt. 1, Chapter 11, p. 424): In 1932, six kolkhozniks (collective farmers) were executed for mowing the grass left round the tussocks after the harvesting of their kolkhoz' meadow. For this crime alone, the author concluded, Stalin should have been quartered.

Here are devastating descriptions of another kind. In very general terms Russell (1920a) condemned the communist regime; on p. 114 he remarked that the adoption of the Bolshevik methods by the "Western nations" would result in a "relapse into the barbarism of the Dark Ages". He (1958, p. 110) "hated" Russia and he (1920b, p. 180) stated that the "better" Bolsheviks were endeavouring to "create a Plato's Republic", – a slave-owning society ruled by an elite[61]!

Gide (1936 – 1937) formulated many negative conclusions about what he saw in Russia; and in particular about the lack of political freedom (pp. 69 and 132 – 133). He (pp. 116 – 117) referred to Soviet newspapers listing astonishing setbacks in industry, mentioned the "new law" prohibiting abortions, terrible housing conditions and (pp. 194 – 195) scarcity and low quality of condoms and cited a local physician to the effect that "masturbation is practiced most generally"…

So why did many foreigners paint rosy pictures of the Soviet Union?

The difficult economic situation in the 1930s the world over; the dangers posed by Nazi Germany and its allies; and, later, Russia's part in winning World War II against them; and (§ 3.3) the threat of World War III, – all this contributed to distort the harsh reality.

Political blindness and/or premeditated deceit. In 1937, a French newspaper (Courtois et al 1997, p. 324) mentioned Stalin's "monstrous deeds" and accused several men including Romain Rolland[62] and Paul Langevin (a friend of Einstein, cf. § 3.1.2) of being "delighted" by the Soviet regime. In 1930 – 1951 Theodore Dreiser published about 35 papers and short notes in the Soviet Union (some of them translated from Western leftist periodicals) and constituting a volume of his works (1955). And Louis Aragon (1972), who was Stalin's henchman, pure and simple, contrived to omit any mention of communist atrocities.

Among those politically blind I cite Feuchtwanger (Note 54)[63] and Bernard Shaw. In 1921, the latter sent Lenin a complimentary copy of his book *Back to Methuselah* (published in 1921) with an inscription (translated back from the BSE, 2nd ed., vol. 48, 1957, p. 159):

To Lenin, who, alone from among the statesmen of Europe, possesses the talent, the character, and the knowledge required of a man holding such a responsible position.

Superficiality. It was incumbent of any author to analyse beforehand the inferences formulated by his predecessors, the more so since some visitors to Russia doctored their accounts (Russell 1920a, p. 20), and, in addition, since they disagreed one with another (Zweig 1945, p. 308). Nevertheless, each author apparently only relied on his own impressions[64].

Then, visitors hardly realized that a positive conclusion should have been thoroughly checked rather than taken at face value. A similar statement was (and is) well known to statisticians, and I note that Einstein (1979, p. 19) made an analogous utterance with respect to experiments, but Gumbel obviously forgot this requirement. A special point here is that many Soviet citizens, especially before 1928, felt themselves like participants of a great mission (Zweig 1945, p. 305).

Earlier Russell (1920a, p. 60) had denied this, but I myself heard similar statements from older men.

Propaganda. Year in and year out, the poverty-ridden and hungry nation spent a lot of money to keep communist parties abroad. At home, two events marked the beginning of the Great Terror: the appearance of a patriotic song that swept the country[65] and the adoption of a sham constitution.

The life of Maxim Gorky is highly relevant. From 1917 onward he managed to save the lives of many intellectuals, and he tried to defend national science and culture against the Bolsheviks (Vaksberg 1999). He also began to adapt himself to the Establishment but continued to be a meddler and in 1921 he was forced to emigrate (Ibidem, p. 48).

In 1928 Gorky visited the Soviet Union and next year returned for good; in Europe, he only was a one-time writer whereas in Russia he remained a classic. During his last years, Gorky became the most authoritative propagandist of the Stalinist regime (below), but he was unable (to bring himself?) to write Stalin's biography (Ibidem, p. 263). Furthermore, *The Great Leader and Teacher* felt himself crowded by Gorky (Ibidem, p. 360) and in 1936 he was poisoned

(Ibidem, p. 374)[66]. I would add that with the Great Terror already under way, Gorky remained potentially dangerous.

In 1929 Gorky visited a labour camp and approved of the methods of *re-educating* the inmates, and a youngster, who dared tell him the truth, was immediately executed (Solzhenitsin 1974, vol. 2, pt. 3, Chapter 2). Then, without waiting for the (stipulated beforehand) verdict, Gorky (1930a, p. 3ff) condemned the defendants at a phoney trial in Moscow as guilty of high treason. He (Ibidem, p. 15) also blamed the kulaks for "organizing famine", cf. Note 38. On the same page he maintained that, "With the blessing of the head of the Christian Church"[?], European politicians "are preparing a marauding attack on the Union of Soviets".

Soon Gorky (1932, p. 23) declared that the dictatorship of the proletariat [?] was temporary, necessary for "re-educating" tens of millions of people[67]. Actually, Gorky for a long time was experiencing hostile feelings with respect to his own people. Russians are "apathetic" (1922, p. 9) and "very fond of beating, no matter whom" (p. 20); "special cruelty" is in their nature (p. 17)[68]. And, just as the Jews who fled Egypt did not live to see the Promised Land, Gorky (p. 43) finally declared, so also the

Semi-barbarian, stupid, difficult people in the Russian villages will die out … and a new generation will replace them.

Was not this idea formulated during his talks with leading Party figures?

I return now to Gumbel (§ 2). Recall that his last travel notes described the year 1932 so that he should have known enough. Nevertheless, he had not noticed the brutish nature of the Stalinist system; he either had not realized the essence, or had believed in the fairness of the trial, in 1928, of the Industrial Party, cf. Einstein's proper attitude (§ 3.3). Earlier he [2, p. 202] mentioned the "wilful sabotage" allegedly committed by intellectuals. But still, Gumbel surely heard truthful stories from his friends in Moscow. Even Zweig (1945), who only spent a fortnight in the Soviet Union (p. 302), discovered an anonymous note in his pocket explaining that Soviet citizens did not dare tell him their real opinions (p. 308).

Concerning his professional level, I do not believe that Gumbel managed, in 1932, to miss Kolman's notorious paper (1931), "Sabotage in science", appropriately published in the Party's leading organ, or that he knew nothing about the decimation of Soviet statisticians[69]. Again, did not he feel that a rigidly planned economy (§ 2.1) coupled with dictatorial rule had imposed great difficulties on the population (and led to falsification of statistical returns)?

Although he had made many interesting observations, Gumbel compiled a false account of the Soviet Union. As a finale, consider two of his statements taken together [19, p. 94; 8, p. 159], both of them describing the year 1926:

Ich fand Moskau zwar sehr interessant, aber ich wollte dort nicht mein Leben verbringen. Ich wusste nicht, was aus Russland unter Stalin werden würde …

A hundred million peasants are freed from the knout and millions of workers may look with proud hope on the first attempt at realizing socialism [with a brutish face].

Serfdom was abolished in Russia in 1861 and about 1927 workers lost any such hopes.

Gumbel was lucky in that his later (in 1932) attempt to find a position in Moscow failed (Vogt 1991, p. 29), otherwise he would have likely perished, cf. Note 15, or at least been *re-educated* in the Gulag.

Notes

1. Gumbel began his academic career in Heidelberg in 1924 and only became *außerordentlicher Professor* in 1930 (Jansen 1991, pp. 385 and 387). Here is a newspaper account (Anonymous 1931) of one of the pertinent episodes:

Prof. Gumbel sei von jungen Studenten in der übelsten unakademischen Weise in seiner Lehrtätigkeit behindert worden … Prof. Albert Einstein mahnte, die inkriminierten [political] *Bücher Gumbel zu lesen, er habe aus ihnen gelernt. Prof. Gumbel nannte den Kampf gegen ihn eines Kampf des Faschismus gegen die Republik.*

A long Editorial (1931) which I also mention below was mostly devoted to defending Gumbel from the rightists. This proves that he was indeed one of their main opponents.

2. See § 3.3, but especially [26].

3 The appearance of Gumbel's biography in their book certainly honoured his memory.

4. Pinl (1972) listed several of Gumbel's writings lacking in Jansen's bibliography.

5. I cite the letters by date and the provided five-digit numbers. In two cases, I mention the Pearson Papers kept at University College London.

6. Gumbel studied economics (Jansen 1991, p. 10). In 1923, Einstein (§ 3.1.2) recommended him as an economist to a foreign university and in 1926 Gumbel read *Gastvorlesungen über Mathematik für Nationalökonomen* in Hamburg (Pinl 1972, p. 158).

7. A Professor *der Rechts*, and, at the time, the *Reichsjustizminister*. In Note 43 to § 2.4.2 I refer to one of his letters published in Bd. 18 (!) of his *Gesamtausgabe*. Below, I also mention Emil Lederer, a prominent economist (Jansen 1991, p. 18) and several mathematicians and physicists who are certainly remembered at least by the appropriate specialists.

8. See Note 1.

9. In 1924 Gumbel presided at a meeting commemorating the beginning of the world war and "in einem improvisierten Schlusswort" recalled those perished: "Ich will nicht sagen – auf dem Felde der Unehre gefallen aber doch auf grässliche Weise ums Leben kamen" (Jansen 1991, p. 19). He used "diese Formel" once more in 1924 (Ibidem, p. 364; Note 107). In 1932, in another public speech, Gumbel (Ibidem, p. 35) proposed "als Denkmal des Krieges … eine große Kohlrübe" because in 1917/1918 swede had become the staple food for the Germans.

I also note that in 1927 Gumbel [8, p. 117) suggested that the "wahre Symbol" of Soviet Russia was not the Hammer and Sickle, but the bureaucrat's abacus. A bit later a Soviet citizen found guilty of suchlike blasphemy, even if whispered privately, would have landed in a labour camp.

10. The predecessor of the present *Max-Planck-Gesellschaft zur Förderung der Wissenschaften.*

11. He continued: "und er [der Weg] muss, wenn integral angewandt, dazu führen"!

12. Gumbel listed three reasons: the dissociation of those elected from the working population; the ideological influence of the capitalists; and the resistance of other institutions to the parliament. He failed to notice that under socialism the top people might be no less separated from the man in the street (Note 42) whose interests were hardly taken into account (Note 14).

In 1918, Gumbel [11, p. 194] thought that the transition to socialism should be achieved peacefully: "Schritt um Schritt baue man den Kapitalismus ab".

13. Suchlike declarations are heard even now. The do-gooders still preach communism just like the believers in perpetual motion persisted in dreaming about the paradise they will be offering to mankind. Cf. Gorky's warning (1930b, p. 3) addressed abroad: "You will also have to deal with traitors of the same brand".

14. Anyway, the Soviet Union moved towards a planned economy suppressing its own New Economic Policy (§ 2.2.3). And experience showed that, apart from the impossibility of predicting the requirements for each commodity (including, for example, nails of every type and size) and the respective capacities, the plans were always geared to the needs of the state (as understood by the Party) rather than to the vital requirements of the population.

Horrible housing conditions in Moscow (Note 37) is an appropriate example. Late in life Gumbel [25, pp. 337 – 338] described the situation in the German Democratic Republic:

An einem Tag gibt es kaum Kartoffeln, aber Milch im Überfluss. An anderem Tag gibt es genug Kartoffeln, aber keine Milch.

15. Schaxel himself was invited by the Soviet Academy of Sciences and moved to Moscow. There, he came out against the notorious high-ranking humbug Lysenko, was imprisoned and then died, in 1943, "under obscure circumstances" (Dictionary 1983, p. 1026).

16. Later this institution was called the Marx – Engels – Lenin Institute, then Stalin's name was added to it, – a fact shyly passed over in silence in the GSE

17. *Moskau* (1932).Jansen (pp. 297 – 306).

18. A petty mathematician and a diehard top communist (1892 – 1979) who eventually lost faith in the Soviet system and fled the country. Demidov & Tokareva (1995) published a letter of an eminent historian of mathematics, G. F. Rybkin, who edited Kolman's manuscript of a booklet on Lobachevsky. He listed many glaring mistakes contained there and added that Kolman never blushed.

19. He repeated this statement twice: in the published text of the MSS (Marx 1983, p. 226) and in his last contribution (Kolman 1982, p. 172). In the later instance he, as noticed by Vogt (1991, p. 22), had shamelessly called Gumbel a "mediocre mathematician". Vogt put on record some more information about Kolman; also see Vogt (1983). Thus, in 1931, at the International Congress of Mathematicians in Zürich, he reported on the preparation of the Marx MSS for publication without mentioning Gumbel.

20. The BSE (1st ed., vol. 19, 1930, p. 799) carried Gumbel's biography. It described his scientific work and political activities in Germany and stated that "for some time" he had lived in Moscow "preparing Marx' mathematical heritage for publication". At the time, the Chief Editor of the BSE was Schmidt which likely explains why Gumbel was entered there, cf. beginning of § 2.2.2 and Note 22.

21. In a letter of 1901 to his father, an eminent statistician of the old, non-mathematical school, Chuprov (Sheynin 1990/2011, p. 34) expressed his dissatisfaction with the "arithmetical manner of exposition" of vol. 2 of *Das Kapital*.

22. In 1881, Pearson thought about translating *Das Kapital* but it seems that Marx rejected his trial attempt (Porter 2004, p. 69ff). Pearson was critical of Bolshevism. He (1978, p. 243) remarked that [in 1924] Petersburg [actually, Petrograd] "has now for some inscrutable reason been given the name of the man who has practically ruined it".

23. From 1927 (until?) he was member of the Presidium, and (from?) to 1930, head of its section on natural sciences; during 1939 – 1942, Vice-President of the Soviet Academy of Sciences.

24. His book [24] was translated into Russian in 1965. In the Foreword, B. V. Gnedenko properly stated that Gumbel had written the only monograph on its subject, which, moreover, will be easily understood by a broader circle of specialists, but that he had restricted his attention to studying independent trials. One of Gumbel's scientific papers [7] was translated in 1928 by Youshkevich who later became the most eminent Soviet historian of mathematics.

25. The present situation proves that Gumbel was wrong.

26. In a letter of 1915 to Markov, Chuprov (Sheynin 1990/2011, p. 130) remarked that "the figures now published by the Central Statistical Committee exaggerate the population [of Russia] by five if not ten million".

27. See p. 10 of the original Russian edition to which I refer when the pertinent statement is missing, or omitted in the German version (abridged by Jansen). The page numbers of the two versions greatly differ and it is not difficult to distinguish between them. Even when Gumbel foresaw that sex criminality will persist under socialism (p. 19), the Editor(s) of the Russian edition disagreed!

28. Gumbel [10] said a few words about the study of conjuncture made at Harvard University. Then, he published a short review [14] on *Konjunkturforschung* without however mentioning Kondratiev, see Note 29. Elsewhere, he [15, p. 110] stated that *Konjunkturkunde* was a new statistical discipline.

29. On 25 March 1931 Einstein wrote two letters, one to Gumbel (46527), the other one, which I also quote in § 3.3, to Radbruch (46529). In each of them, he stated that he was glad to have read the latter's article and in the second one he added:

Ich freue mich, dass in diesem Lande noch aufrechte und rechtliche Männer gibt, wie Sie einer sind. Ihr Artikel war mir eine wirkliche Freude.

The paper in question was likely Radbruch (1929 – 1930) where the author condemned political murders substantiated by *la raison d'Etat*. Einstein hardly knew about Gumbel's pertinent pronouncement to the contrary.

30. In 1922 Chuprov (Sheynin 1990/2011, p. 33) stated that

The intrinsic contradictions of capitalism are great and deep, but at present the ability to manage them is still greater.

In 1923, Kondratiev predicted the crisis of the capitalist system (although not its starting point). His fate was tragic (Ibidem, pp. 29 – 30). In 1952 Gumbel [20, p. 161] formulated another "fundamentale Frage":

Ob die neue Gesellschaft einen humanitären Sozialismus oder eine totalitäre und vielleicht sogar theokratische Struktur bringen wird. Die russische Regierung ähnelt heute der Ecclesia [general assembly] *Militans* … (*Das älteste Beispiel für die Übereinstimmung beider Ziele war die kommunistische Regierung der Jesuiten in Paraguay.*)

This passage is extremely interesting. First, it anticipated the idealistic phrase *Socialism with a humane face*. Second, in the 1980s, the eminent Soviet mathematician (and notorious anti-Semite) Shafarevich declared that socialism was defined by an appropriate ideology rather than by social ownership of the means of production. Accordingly, he argued that the Inca state (a slave-holding despotic state) was a socialist country.

31. Which does not really exist, as he himself stated on the same page! And how about the necessary conditions for the transition to socialism (§ 2.1) which were never fulfilled in Russia?

32, But was the civil war necessary? Also see § 2.3. In 1927 the GPU (more correctly, the OGPU) acquired the right to arrest and even to execute citizens without trial (Stetsovsky 1997, vol. 1, p. 244).

33, On p. 91 Gumbel mentioned the "present communist government" and added a curious remark: "so far as [it] … really has communist tendencies". After Khroushchev, Soviet leaders hardly believed in a communist future. They kept pretending to their faith to continue in absolute power and instantly abandoned this attitude after the downfall of the Soviet Union.

34 This restriction was later abandoned.

35. Gumbel hardly realized that in 1921 – 1922 several thousand clergymen, monks and nuns of the Orthodox Church were executed on false charges, – alleged refusal to give up the Church valuables necessary for saving the starving population (Courtois et al 1997, p. 140ff), cf. Note 59. The BSE (1st ed., vol. 46, 1940, p. 665) even accused the Church of "espionage, treason and betrayal", although its later editions dropped this charge. The second antireligious wave occurred in 1929 – 1930; Flügge (1930) made public additional horrible facts concerning Mennonites and Baptists.

36. Zinoviev was expelled from the Party in 1927, 1932 and 1934 (he was twice re-admitted) and executed in 1936. Trotsky was exiled from the country in 1929 and assassinated by a Stalinist agent in Mexico in 1940. About 1934, Gumbel (Jansen 1991, p. 67) denounced Trotsky's exile.

37. Read: All issues are subordinated to Marxist philosophy. The attitude towards relativity theory was not at all established. For example, Kolman (1939) believed that velocities can exceed 300,000 *km/sec*. The contrary statement, he declared, went against dialectical materialism. Then, a certain Vislobokov (1952), writing in a leading ideological journal, denied the theory. Even in the 1970s a (state) publishing house in Moscow rejected a manuscript describing Einstein's life and work, because, as the reviewer claimed, he was a Zionist. I heard about this from the author herself.

38. Was it so difficult to foresee the impending breakdown of the housing? The powers that were had hardly done anything at all not to mention that, in 1933 – 1934, because of their possible anti-Soviet inclinations, *undesirable elements* were forced to leave Moscow (60 thousand during two months of 1934) as well as several other cities (Courtois et al 1997, Chapter 9 of pt 1). Gumbel published photographs depicting the ugly conditions of housing in Moscow but did not dare disclose his authorship or even to let them appear in Germany (Jansen 1991, p. 16).

39. Gumbel believed, naively or otherwise, that young workers were being sent to rural areas "to examine the feelings" of the peasants rather than to organize a ruthless struggle against the kulaks. A few years later two million of these poor wretches were exiled and six million of peasants died of starvation (Courtois et al 1997, p. 164).

40, This would have been tantamount to intervention. Again, Gumbel's text hardly tallies with his belief [12, p. 174] in the sincerity of contemporary Russian proposals for disarmament.

41. Actually, the Soviet military force was not sufficient for preventing these nations from securing independence.

42. When comparing this statement with Gumbel's own previous report (§ 2.2.3) on the value of the rouble, it occurs that the Russian currency experienced a downfall which apparently meant that a large portion of the population was impoverished.

43. Their salaries were low as compared with their Western counterparts. However, fringe benefits had been (and still are) so diverse and considerable that the "poor top people" constitute an altogether separate population. Some time ago it became generally known that for several decades they had been buying foodstuffs (and other goods?) at prices existing in 1926. And some of them were even being serviced by clandestine state-maintained brothels.

44. Radbruch provided a related testimony. In a letter of 1949 to a certain Hugo Marx he (1995, p. 316) wrote:

Schrieb mir Gumbel über seine jetzige Ansicht vom Marxismus, sehr abgewogen Zustimmung und Kritik und ganz in dem mir richtig erscheinenden Sinne. Sogar er scheint weiser geworden zu sein.

45. He [21, p. 284] mentioned this fact already in 1952, although in passing. In 1925 he [5] did not say anything about it.

46, Gumbel mentioned Leonhard (1956). On p. 723 she cited Einstein's statement "kein Ziel ist so hoch dass es unwürdige Methoden rechtfertigen könnte" choosing it as an epigraph to one of her chapters. Following a nasty tradition, she had not indicated the exact source. Bearing in mind Russian communists, she could have well written "… unwürdige [much less cannibalistic] Methoden …"

47. The handwritten draft of this letter has No. 46547 and Einstein wrote it beneath Montel's letter to him dated 5 Dec. 1931 (46546). Montel mentioned Gumbel

and stated that "ce [?] serait pour lui naturellement la meilleure de recommandation". Montel's answer to letter 50110 (see §3.1.2, No. 5) had the letterhead *Ecole Municipale de Physique et de Chimie Industrielles* whose director was then Langevin, and Montel indeed mentioned him. He apparently substituted for Langevin.

48. Gumbel again informed Einstein about the German refugees in France on 10 Jan. and 18 Nov. 1935 (50134 and 50135).

49. Jansen (1991, p. 12) reported that in 1915/1916

Neben mathematischen und naturwissenschaftlichen Vorlesungen und Übungen, darunter auch die Einsteins [cf. the text of this letter 43810], *hörte er* [Gumbel] *den bekannten und angefeindeten Pazifisten Georg Friedrich Nicolai.*

Jansen added that Nicolai had written a foreword to one of Gumbel's political notes.

50. A copy of this letter is also kept among the Pearson Papers (709), but the words "um die Gerechtigkeit" inserted by hand are absent there.

51. *The National Union Catalog, Pre-1956 Imprints*, vols. 1 – 754, 1968 – 1981 (vol. 269, pp. 595 – 596) lists *Annual Reports* of this American-based Board for 1924/25 and 1925/26.

52. Gumbel [20] published an obituary notice of Bortkiewicz. I can now add that Mises left a manuscript on mathematics in Nazi Germany (Sheynin 2003).

53. See below.

54. In 1924 Gumbel addressed a French – German peace meeting (Note 9) and published an appropriate paper [3]. Also see [4].

55. Einstein kept Barbusse's portrait in his study "next to the portrait of my [of his] late mother" (Einstein 1922). Later Barbusse (1935, p. 312) stated that Stalin was "the Lenin of today". Yes, of course; and the next ones in line were Mao Zedong and Pol Pot!

After Barbusse's death Stalin sent his condolences to *L'Humanité* (BSE, 2nd ed., vol. 4, 1950, p. 235). Feuchtwanger (1937, p. 109) echoed Barbusse's maxim: If Lenin had been the Caesar of the Soviet Union, then Stalin is their Augustus. Cf. Gide (1936 – 1937, p. 69): Stalin is the *raison* of everything.

56. In 1929, after a coup d'état, a militaristic-monarchic dictatorship was established in Yugoslavia.

57. Léon Blum, the then Prime Minister of France. And here is Einstein's later statement (letter to Maurice Solovine, the translator of some of his contributions into French, of 23 Dec. 1938; Einstein 1993, p. 93):

France's betrayal of Spain and Czechoslovakia is frightful. The worst part is that the consequences will be deplorable.

58. During the 1930s – 1940s, James Paul Warburg published quite a few books on foreign relations.

59. Which one? NATO was established in 1949; the Bundesrepublik joined it in 1955.

60. Thus, in 1921 – 1922 more than five million died of starvation whereas grain had been sold abroad (Stetsovsky 1997, vol. 1, p. 28), – apparently, in part, to finance revolutionary movements worldwide.

61. Russell (1920a, p. 119) reasonably feared the "revival of Jenghis Khan and Timur".

62. Russell (1920a, p. 7) also believed that "Socialism is necessary for the world" and Gumbel (Russell 1917, p. 102n) thought that he might be called an "antibolshevistic communist".

63. The main text of Rolland (1935 – 1938) could have been meant.

64. Feuchtwanger essentially drew on his talks with Soviet leaders, Stalin included! He possibly felt an instinctive thirst for replacing reality by desire. On the other hand, I ought to add that his collected works were published soon afterwards. Feuchwanger's book (1937) on Russia also appeared in a Russian translation although it contained some criticism of the Soviet regime. Strange as it may seem, I have it on good authority that those who discussed it in public were being imprisoned and the translated book withdrawn from libraries.

65. I have not seen a single reference to Dostoevsky's *Besy* (1873; several English translations from 1931 onward entitled either *The Devils* or *The Possessed*; French and German translations made at the end of the 19th century). This is a prophetic and destructive criticism of revolutionists. Neither did I see any mention of Russell (beginning of § 4).

66. I quote its two lines: *There is no other nation/ Where a man is breathing as freely as here.*

67. Vaksberg has only partly documented his account. In this case hard evidence is lacking. On p. 376 the author maintained in passing that Wallenberg was poisoned as well.

68. On p. 11 he called Charles Chaplin "sentimental and dismal"! Chaplin's films with a happy end for the man in the street in a capitalist society, – this was, as I suspect, the real cause of Gorky's remark.

69. How can a cruel people *re-educate* tens of millions of their compatriots? Another statement seems, however, partly true: Not the "atrocities" of the leaders of the revolution, but the cruelty of the people was solely responsible for the post-revolutionary events (p. 41; Gorky's own inverted commas).

70. Here is a literal translation of a troglodyte's contented statement (Smit 1931, p. 4): "The crowds of arrested saboteurs are full of statisticians". In a few years she became Corresponding Member of the Soviet Academy of Sciences …

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Abbreviation: Jansen = Jansen (1991); PZM = *Pod Znamenem Marksisma*; R = in Russian

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This story shows how the diehard Bolshevik Schmidt unblushingly duped Gumbel, and how the latter occurred to be inscrutably gullible. Of special interest is § 2.2.3 where Gumbel describes some aspects of the poorly known life of the Soviet population during the first years after 1917.

V

H. L. Rietz

Review of A. A. Tschuprow, Grundbegriffe und Grundprobleme der Korrelationstheorie. Leipzig – Berlin, 1925

Bull. Amer. Math. Soc., vol. 32, 1926, pp. 561 – 562

This book presents in extended form a series of lectures given in the insurance seminar of the University of Christiania [Oslo]. The main purpose of the book is to give a unified treatment of correlation theory with special reference to the fundamental conceptions and logical foundations of the theory. It seems to be very properly held that the treatment of the logical foundations of the method of correlation has not kept pace with the wide range of application. The exposition does not proceed from the standpoint of the analysis of the numerical data, but from the standpoint of prior probability. The theory of correlation is regarded as an organic part of the theory of probability. The treatment seems fairly well described as an idealisation of the somewhat empirical concepts of the English school of statisticians by a sharper formulation of definitions and underlying concepts.

Much is made of an expressive phraseology involving the concepts of chance variable and stochastic connection. A chance variable of order k is defined as a variable which takes any one of k values with assigned probabilities. For example, the number that will be thrown with a die in a single throw is a chance variable. When x is assigned, and y is a corresponding chance variable which takes values with definite probabilities, there is said to be a stochastic connection between x and y. For example, if in throwing two dice, the first gives a value $x = 3$, then the corresponding total y for the two dice is $y = 4, 5, 6, 7, 8$ or 9, and there is a stochastic connection between the chance variables x and y. Much is made of the conception of stochastic dependence as distinguished from the more familiar conception of the functional dependence of two variables. In fact, the recognition of a clear distinction between the conceptions of stochastic connection and functional dependence constitutes a first step in following the exposition in this book.

The explanation of the stochastic connection of y with x follows the regression method, and calls for a complete characterisation of the theoretical array (das bedingte Verteilungsgesetz) of y's for any assigned x. While this conception is quite an advance over the early Pearson concept that y is correlated with x when the mean values of the theoretical arrays of y's are not constant but are functions of x, the author has hardly given adequate credit to Pearson for a much more general view of correlation given in Draper's Company Research Memoirs, Biometrric Series II, 1905, p. 9. In this more general view we may say that whenever any characteristic of the theoretical arrays of y's changes from one assigned value of x to another, there is a stochastic dependence.

For the characterisation of arrays, it is assumed that moments and product moments will serve to determine the necessary parameters for a complete characterisation of arrays.

One of the most important parts of the book is concerned with estimating the prior values of correlation coefficients, correlation ratios, and other statistical constants from the corresponding empirical values. In this part of the book, there is given a careful treatment of the sampling problems involved.

Taken as a whole, the reviewer considers that the book is an important contribution to more critical and rigorous thinking on the methods and theory of correlation.

An English translation of Chuprov's book appeared in 1939, and the same periodical (vol. 46, 1940, p. 389) carried its review by A. R. Crawthorne. Now, I can only repeat his estimate: *a real tonic; …, will fill a real need.*

VI-I

E. Fels

Oskar Anderson, 1887 – 1960

Econometrica, vol. 29, No. 1, 1961, pp. 74 – 79

Oskar Anderson[1], Fellow and charter member of the Econometric Society, Professor Emeritus of statistics[2] in the University of Munich, holder of several honorary doctorates, died on Febr. 12, 1960. A direct descendent of the Lexis – Chuprov – Bortkiewicz *continental* school of statisticians and a versatile economist, Anderson was in theory and action a vigorous pioneer of econometric method and in post-war Germany to his death the undisputed leader of a small group of enthusiasts for the advancement of economics in its relation to mathematics and statistics.

Anderson was the author of over 150 publications[3] in German, English, Russian and Bulgarian, ranging from the arid tract to the lusty polemic, and mostly written with a characteristic polyglot picturesqueness – and an aroma of Russian syntax – that are the pedantic editor's despair and the sympathetic reader's delight.

In a climate that measures a scholar's worth almost wholly by his publication record, it must be stressed that Anderson's printed legacy, remarkable as it is, gives an incomplete idea of the man's scholarly range and depth. Tragedy struck hard and repeatedly. A daughter perished when the Andersons were refugees; a son a little later. Another son fell in WWII. There was for Anderson vexingly little of the thinker's yearned-for quietude. Also, to churn out another research memoir instead of conscientiously spending time on students or instead of studying technical literature not quite the fashion of the day, this would have offended his sense of duty. He was not by the standards of our education departments a good teacher. His lectures were poorly organized – in very much the sense in which this has been said of Dostoevski's novels. New students dreaded his imposing grandfather figure: he looked strikingly like a more handsome Marshall Bulganin. But even the statistically hopeless ones usually came to love him by the time of graduation. Those who have heard him in his seminar are fated to find comparable gatherings rather colourless and languid.

Anderson was born on Aug. 2, 1887, in Minsk, of ethnically German parents. His father later became professor of Finnish-Ugric languages in the University of Kazan, where Oskar finished gymnasium in 1906, awarded with a gold medal. After a few semesters of mathematics in Kazan, Anderson entered the economic department of the Polytechnic Institute in St. Petersburg. He became an outstanding pupil of Chuprov (1874 – 1926), a tutelage which Anderson respectfully acknowledged throughout his life in great and small matters[4]. There s no doubt that, more than to find himself recognized[5], Anderson would have greatly enjoyed to see his old master rehabilitated in his native country[6]. From 1912 to 1917 Anderson taught in a commercial gymnasium in St. Petersburg. In-between, during 1915, he took a trip to Turkestan for field work. Here we have one of the first try-outs of sampling methods. In 1917 he served as research economist for a big cooperative society in southern Russia. More academic training in theoretical and applied statistics at the Commercial Institute in Kiev in 1918 and simultaneously an executive position in the Demographic Institute of the Kiev Academy of Sciences.

Then the Andersons leave Russia, in a hurry. The reasons for this are not entirely obvious. As a student, Anderson had shown rather leftist inclinations; also, an unauthenticated story has it that emissaries of Lenin had offered Anderson a very high position in the economic administration of the country. We may safely conjecture that not Anderson's personal condition, but that of former superiors and associates had become politically untenable; that, with his strong personal loyalty, was probably decisive[7].

After a hitch in Constantinople, Anderson in 1921 becomes principal of a high school in Budapest. In 1924, the Commercial Institute in Varna (Bulgaria) offers him a teaching position. He accepts and lectures there till 1934, on statistical theory and a large variety of other topics. In 1933 a Rockefeller stipend takes him to Germany and England. The result is his first book[8], From the mid-1930s on, he also serves as an expert for the League of Nations. Meanwhile he has gone deeply into business cycle research[9], done work that J. A. Schumpeter (1954, p. 1104) later calls *conspicuous for excellence of workmanship*[10]. Since 1935 he has been director of the Institute of Statistics and Economic Research at the University of Sofia, and the ensuing years are notably prolific[11]. In 1940 the Bulgarian government sends Anderson to Germany, then already at war, to study rationing.

In 1942 he accepts a position at the University of Kiel, also doing research on East-European economics. This inevitably entails political unpleasantness. He manages to stay free of any Party affiliation. In 1947 he accepts the chair in statistics at the University of Munich. One hears him call himself *an extinct volcano*, and he is not a well man, yet he carries stupendous teaching loads, still writes original papers, co-edits the *Mitteilungsblätter f. math. Statistics* (later *Metrika*) and finishes his second German textbook (in fact, rather a personal encyclopaedic survey than a text)[12], which during three years goes through as many editions with considerable alterations.

When he dies, he has, almost literally single-handed, seen to it that the teaching of statistics, as the term is internationally understood, in Germany has not ground to a halt in the morass of apathy and the anti-mathematical ill will of those who for various reasons have not overcome the moribund *Methodenstreit*. He leaves capable students, who are proud to have been *his* students, and some of whom have attained recognition and responsible teaching positions, notably Hans Kellerer (Munich), Heinrich Strecker (Tübingen),and Anderson's son Oskar (Mannheim).

With all his grandfatherly kindness and warm loyalty to friends, Anderson was a *character*, at times pretty pugnatious. When attacked he could hit back so that it hurt[13]. And he exposed himself to all sides, took all comers. Nothing half-hearted, nothing vague. On the one hand, he was forced to preach in the wilderness what is so obvious to econometricians: that mathematics is a tool without which statistical techniques could not get far; without which theories of any kind could not even be sharply profiled. On the other, he could not accept the hyper-elegant conception of statistics as, say, measure theory plus additional conventions. *Applied* to him meant *actually feasible, and now*. He was overtly sceptical, even scornful, of some tendencies in econometrics toward scrupulousness with sampling errors but less or none with specification, observation, and propagated errors. To the phrase *Let us assume that …* his instinct was to retort, *Let's not …*

From the original contributions, what is likely to remain? What has a secure position in the history of our field? This is for his peers to decide[14]. Pupils have to plead myopia, but one may venture this much:

Apart from applied sampling in Turkestan, his systematic, large scale use of sampling methods in Bulgaria in the 1920s is enough to secure him a lasting place in the history of our sience[15].

In the West, Anderson became perhaps first, and permanently known for the Variate Difference method that he developed independently of W. S. Gosset[16]. As elsewhere, sense of proportion prevented him from making too much of it. Students of his could pass their examinations with him without a clear notion of what it was all about. Now, as though our time-series-analysis arsenal were so ideally stocked for *available economic data*, the method is too often lightly dismissed[17], rather than seen in the light of Anderson's own scepticism[18].

Especially after witnessing a renewed interest in the quantity theory of money[19], it is somewhat painful to see Anderson's early study on this theory[20] completely neglected. For here we have good Fisherian theorizing completely blended with a judicious evaluation of Bulgarian data. The statistical techniques may not be those one would use today, but in how many truly econometric studies prior to 1930 is there a workmanlike concern for confidence limits etc.?

Anderson's name will also remain insolubly connected with his critique of the ill starred Harvard Barometer[21] in the late 1920s, still a source of glee for his fellow countrymen[22].

Anderson's concept of *social-statistical probability*[23], well thought of by some, is probably not of enduring solidity. He did not, however, consider himself as a specialist in these matters. His tolerance and interest ranged all the way from Fisher, Keynes, Kolmogorov and Jeffreys (whom he knew particularly well) to Reichenbach and Carnap.

Anderson's *bête noire* was sloppy or no error analysis[24]. His attitude to problems of statistical index-number theory and practice[25] cannot be understood without an appreciation of his concern for errors of all kinds, matters usually relegated, but not by him, to tracts on numerical analysis. His enormously versatile experience with the genesis of actual data and actual bureaucratic computer techniques stood him here in particularly good stead. In oral discussions even more than in published works he proved himself a formidable connoisseur and shrewd debunker of all index-theoretic matters.

In his *extinct volcano* years, he became more and more concerned with nonparametric test procedures and made some noteworthy contributions[26]. Further endeavours of his could no doubt have gone in this direction, but old age, sickness, and other pressing duties finally got the better of him.

Death came as a friend and saviour after a true Odyssey: Anderson himself would have thought it tasteless and pompous to call it untimely. But one quails at the thought of what would have happened to German academic statistics had he died, say, 15 years earlier.

Notes

1. The second initial *N*, found in earlier titles stands for the Russian style patronym Nikolaevich and was hardly ever used by Anderson in his later years.

2. According to Anglo-American usage, Anderson would have been Professor of *Economics*. There are, to the writer's knowledge, no departments of statistics in German universities.

3. For a list see the bibliography in the third edition of Anderson's (1954a/1957) and the forthcoming Wold's obituary [in this collection].

4. For instance, that Anderson should have maintained the (arguable) distinction between *nomographic* and *ideographic* sciences is probably in deference to Chuprov (1909/1959, p. 37). [Not arguable but antiscientific. Chuprov thoughtlessly repeated the statement of some German so-called philosophers. There are no, and there cannot be any science which only describes facts. History, for example, will then degenerate into chronology. O. S.]

5. Such recognition can now be found in … [I may now refer to Sheynin (1990/2011, p. 160). O. S.]

6. [After Chuprov's death sittings in Moscow and Leningrad were devoted to his memory, and the latter's minutes were published. But only one obituary appeared in a periodical (in a little-known newspaper). Some Soviet statisticians had nevertheless published obituaries abroad. Chuprov, along with other statisticians even including Süssmilch, was called a defender of capitalism. He was exonerated only about 1958 (Sheynin 1990/2011, p. 160). O. S.]

7. The following occurrence, communicated by Oskar Anderson, Jr, is typical of Anderson's character. He had written, in Bulgaria, an article in which he criticized, severely and in great detail, N. D. Kondratiev's *long wave* work. When the article was about to go to press, Anderson learned Kondratiev had fallen into political disfavour. Not to compound Kondratiev's misfortune (which certainly had nothing to do with long waves), Anderson withdrew his article. Its manuscript is lost somewhere in Bulgaria. [Disfavour! Kondratiev published a paper which was called *wrecking*. In 1931 he was arrested and shot in 1938. Even in 1923 he predicted the crisis of the capitalist system (although not its starting point) as well as its non-deadly character. See Sheynin (1990/2011, pp. 39 – 40). The Harvard Barometer proved dangerous: extrapolation of economics without study of economic processes is impossible, which Anderson indicated (only in 1929). O. S.]

8. Anderson (1935). This was incidentally written in the unusual attempt to build up post-Fisherian statistical theory with only gymnasium-level building blocks. He more or less failed. Anderson had had too rosy ideas of prevalent German high school training in mathematics. Later, when *real* statistics students studied high-powered mathematics first, his book again did not fit. It was, however, used as a prescribed text in Sweden (at least) as late as 1952, as the writer remembers from a Swedish student's university catalogue (Stockholm?). We find this book praised again in Karpenko (1957, p. 317) as the exposition of Chuprov's statistical teachings.

9. Anderson (1931).

10. Not needed.

11. In his capacity as director of the Institute, he edited about 50 monographs, some of book length.

12. Anderson (1954b).

13. For impressive examples cf. Anderson (1949a; 1950).

14. In this context, the reader is referred to Wold [in this collection] and Tintner (1961). It is with respect to these appraisals that the present writer, a former student of Anderson, has here concentrated on his personal side.
15. Anderson (1949b).
16. Anderson (1914; 1932; 1926 – 1927).
17. Valvanis (1959, p. 180).
18. Anderson (1954a, pp. 178 – 180).
19. Friedman (1956).
20. Anderson (1931).
21. Anderson (1929).
22. Karpenko (1957, p. 315),
23. Anderson (1947; 1949c).
24. Anderson (1954a; 1954b).
25. Anderson (1949d; 1952).
26. Anderson (1955; 1956).

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VI-II

H. Wold

Oskar Anderson, 1887 – 1960

Annals math. stat., vol. 32, 1961, pp. 651 – 660

Born 2 Aug. 1887 in Minsk, Russia; deceased 12 Febr. 1960 in Munich, Germany [in Federal Republic Germany]. These dates span a web of drama and colour both in personal life and scientific career. The course of outer events in Oskar Anderson's life reflect the turbulence and agonies of a Europe torn by wars and revolutions. His scientific work, always marked by personal involvement, is of sufficient stature to be of lasting interest, in part along with the epoch making developments in statistics during the first decades of this century, in part independently of these developments. Some of Anderson's endeavours were ahead of his time, along lines that have not yet received adequate attention. Thus his emphasis on causal analysis of nonexperimental data is a reminder that this important sector of applied statistics is far less developed than descriptive statistics and [or] experimental analysis. In an appraisal of Anderson's work this aspect is highly significant.

Anderson's ethnic origin was Baltic-German. We follow him from his school years in Kazan, where his father was university professor of Finno-Ugric languages. He graduated from secondary school in 1906 with a gold medal, studied mathematics for a year at Kazan university, entered in 1907 the Economic Faculty of the renowned Polytechnic Institute of St. Petersburg, and studied economics for five years. His interests were in the broad area that connects economics and statistics, and in these formative years he developed two main specialities: time series analysis and sample surveys. As a pupil of Chuprov he submitted in 1911 a diploma thesis on correlation analysis of time series data. In the summer of 1915 he did field work as sampling surveyor, participating in a scientific expedition to Turkestan for an economic-technical study of the irrigation system of the Ferghana oasis.

During the years 1912 – 1917 he was teacher in a commercial secondary school in Petersburg. During and after the Russian revolution he moved about, first inside Russia and then, leaving his country as a refugee, working as a teacher and scientific specialist. As statistician in a big cooperative centre in the Ukraine he edited a number of monographs on the economic conditions in South Russia. In 1918 he qualified for the habilitation degree in mathematical-statistical methods at the Institute of Commerce at Kiev. At the same time he worked at the Demographic Institute of the Ukrainian Academy of Sciences. Via Constantinople he came in 1921 to Budapest where he founded and led a secondary school. From 1923 onwards he was a member of the Supreme Statistical Council n Bulgaria, the country where in 1924 he found stable ground under his feet. During 1924 – 1934, at the Institute of Commerce at Varna, he taught statistics and several economic subjects, from 1929 as professor of economics and statistics. Then follows a period of intense activity. He goes deeply into economic research and in 1932 he goes to Germany and England on a Rockefeller stipend. In 1935 comes his statistical textbook published in German. From 1935 he is director of the Statistical Institute for Economic Research at the State University of Sofia. Under his directorship, the Institute publishes some 50 monographs and books on the economic conditions of Bulgaria. In several capacities (one being statistical expert to the League of Nations) he writes many articles and memoranda on statistical methods. In 1940 the Bulgarian government sent him to Germany to study the system of rationing. In 1942 the University of Kiel called upon him to become professor of statistics. Moreover, he headed the department for Eastern Studies at the Kiel Institute of World Economy. From 1947 he was professor of statistics at the University of Munich[1].

Dangers and hardships were Anderson's lot in WWI and II. When leaving Russia he lost a daughter and a son died not long afterwards. A second son died in WWII as a paratrooper. Anderson was shattered but not crushed by the hard blows of fate. It is characteristic of his moral integrity that he did not allow politics to interfere with his scientific work, and his loyalty in personal contacts was beyond praise. Typical instances are on record, from the refugee years around 1920 as well as from the Nazi period in Germany.

Dominant features in Anderson's scientific profile are his intense engagement in his work and his strong belief in the mission of statistical method in the socio-economic area. In particular, there is first the large volume of Anderson's published work: in all, some 150 items if minor articles and book reviews are included. The appended bibliography is a selection, in the main compiled from lists edited by Anderson himself[2]. There is further the high level of aspiration: in theoretical research he made significant contributions towards developing new approaches, and his applied work is marked by a keen desire to make full use of the best possible techniques. Typical in this respect is his systematic use of random sampling in the surveys in Turkestan in 1915 and later in Bulgaria (1929d). Best known among his theoretical contributions is the variate difference method, which was introduced independently by Anderson and *Student* – Gosset in 1914[3]. Briefly stated, when studying the intercorrelations, interregressions etc. of a set of time series the device is to analyse not the series themselves but their consecutive differences with regard to the time variable. Typical assumptions are that a given time series x_t may be written (read epsilon instead of squares)

$$x_t = P(t) + \square_i = 0, \pm 1, \pm 2, \dots \qquad (1)$$

Here, P is a polynomial in t of finite order, and the residual component is a sequence of random variables that are independent and all have the same distribution. Third, there is the polemical pitch in many of his articles. The use and abuse of index numbers is a favourite topic (1937; 1950c; 1952). A consequential contribution of the 1920's is his criticism of the Harvard business barometer (1929b), his main argument being that the underlying time series decomposition was a shallow and too mechanical approach. Fourth, and finally, I refer to Anderson's educational work. His statistical *credo* is voiced in his two textbooks (1935; 1954): the great responsibility of the statistician is to obtain accurate data, and to use sound methods to analyse the data. At Munich, in the last period of his life, educational problems were in the centre of his interest (1949d; 1956a). It is largely thanks to Anderson's initiative and efforts that Germany [the Federal Republic Germany] after WWII has been making headway in restoring and developing statistical teaching in the socioeconomic sciences.

The main strength of Anderson's scientific *oeuvre* lies, I think, in the systematic coordination of theory and application. Only to a relatively small extent does his importance derive from specific contributions, such as the variate difference method, or his work in the 1950s on nonparametric methods (1953a; 1955b; 1956b). His most fruitful period was the early and middle 1930s. The peak is perhaps marked by his paper on the quantity theory of money (1931a). The paper is pioneering in subjecting the theory to statistical tests on the basis of time series data, and is of considerable historical importance also because his articulate discussion of residuals and their properties sheds light on the gradual evolution of regression methods. Anderson writes the basic relation in two ways (read Greek epsilon instead of the squares)

$$M_i = KP_i + \square_i; \; P_i = (1/K)M_i - (\square_i /K) \qquad (2a\text{-}b)$$

Here M_i is the money in circulation in the ith time period, K a constant, P_i the price index and $M_i = KP_i + \square_i; P_i = (1/K)M_i - (\square_i /K)$ an error term that he refers to as a *disturbance* and interprets as a random variable. Relation (2b) is sta-tistically estimated by the regression of P on M, and in a key passage (pp. 538 – 541) Anderson postulates that the has expectation zero, and says that (2b) *follows immediately* from (2a). This last conclusion shows that Anderson deals with the residuals as measurement errors, as *errors in variables*, not as *errors in equations* that would allow the twofold interpretation of being due to neglected causal factors, and of having zero expectation since they constitute the deviation from the conditional expectation of the left-hand variable. More precisely, the residuals cannot be interpreted as *errors in equations* both [either – or] in (2a) and (2b), for conditional expectations and theoretical regres-sions are not reversible in the sense of (2a- b), as has been well known since the beginnings of correlation theory [8]. Thus we see from (2) that model construction had begun to take deviations between theory and observation into explicit account as random variables, but the statistical implications were only partly understood. It is tantalizing that Anderson came very near to an explicit formulation of the ques-tion whether it is M that influences P or vice versa. Two hypotheses about causal directions that can be formulated as in (2a-b) and equally tantalizing that only a few years later Holbrook Working [9] found a statistical device that can be used for discriminating between such ca-usal hypotheses, a device that was left unnoticed for some 25 years[4].

It is no easy task to coordinate theory and observation in applied work. Anderson was well aware of the difficulties. In this vein is his constant warning that ever so refined statistical technique are of no use unless they are applied to reliable observations. In the same vein is his critical attitude towards the modern tendencies of developing statistical theory for theory's own sake. His sneers in this direction had a special sting when referring to some of the lofty developments of econometrics. To comment upon this last point, Anderson's scepticism, valid or not, was partly intuitive. Econometrics in the 1920s and early 1930s was a melting pot for new developments, but the time was not yet ripe for an adequate treatment of some of the ensuing problems. The situation is amply illustrated by Anderson's work on the variate difference method. The residual assumptions in (1) are often too narrow, possibilities for a rigorous treatment of more realistic assumptions (such as autocorrelation in the residuals) did not arrive until 1933 when Kolmogorov [10] strengthened the mathematical basis of probability theory and thereby laid the foundations for the theory of stochastic processes. Another case in point, more important with regard to the general developments in applied statistics, is Anderson's emphasis on correlation and regression methods for purposes of causal analysis. In accordance with the general trend of econometrics he makes a gradual shift from correlation to regression, as is clearly seen from his textbooks of 1935 and 1954.

Similarly, his early works (1929c; 1931b) involve half-truths in line with the famous dictum *Correlation is not the same as causation.* Later he realizes that regression analysis is an important tool for the empirical assessment of causal relations. His treatment of the basic questions is somewhat vague and intuitive, and to some extent it had to be at the time. As illustrated by (2), modern builders had begun to take residual errors into explicit account. The transition from exact to disturbed relations was a radical generalization of the model, and so was the ensuing reinterpretation of exact forecasts as stochastic forecasts in terms of conditional expectations. The generalization had implications at a basic level that could be understood and developed only gradually. There is here a direct connection between the situation in (2a-b) and the basic problems about simultaneous equations that later have been much discussed in econometrics[5]. For example, if we consider a theoretical autoregression, a theoretical autoregression, say

$$y_t = \alpha\eta_{t-1} + \varepsilon_t,\ \mathrm{E}(\eta_t|\eta_{t-1}) = \alpha\eta_{t-1},\ t = 0, \pm 1, \pm 2, \ldots, \qquad (3)$$

then, under very general conditions that α can be consistently estimated by the least squares regression of y_t on y_{t-1} and that

$$y_t = \alpha^2\eta_{t-2} + \varepsilon^*_t \text{ if } \mathrm{E}(\eta_t|\eta_{t-2}) = \alpha^2\eta_{t-1},\ \varepsilon^*_t = \varepsilon_t + \alpha\varepsilon_{t-1}. \qquad (4)$$

A rigorous deduction of the substitutive relation (4) requires some general theorems on conditional expectations and stochastic processes first established by Kolmogorov [10].

Oskar Anderson in his most active years was one of the leaders of econometrics, and thereby a pioneer in a broad sector of applied statistics: causal analysis on the basis of nonexperimental data. The same period, say from 1915 to 1940, was one of epoch making developments in other sectors of statistics, with R. A. Fisher and J. Neyman for leading names, developments that in common parlance constitute *modern statistics* and are too well known to be elaborated here. A point I wish to stress is that the powerful methods of *modern statistics* are primarely designed for three broad sectors of applied statistics: (i) – (ii) descriptive and causal analysis on the basis of experimental data, and (iii) description (by sampling techniques) on the basis of nonexperimental data. Sector (iv), causal analysis of nonexperimental data, an area where the model builder is confronted with more difficult problems in specifying the stochastic structure of the models as well as in their statistical treatment, has long been neglected by the cadre of professional statisticians[6]. This is clearly seen if Anderson's textbooks with their emphasis on sector (iv) are compared with the textbooks of *modern statistics*, with their emphasis on the three other sectors. In the last ten years or so sector (iv) has gradually come forward, but it is still relatively underdeveloped.

We have described Anderson as a pioneer in a difficult and important area of statistics, or perhaps as a forerunner rather than a pioneer, for the area was not yet ripe for systematic development. The handicap only makes his work so much the more significant, and so do other handicaps of a more local nature. One is the anti-theoretical attitude of statistical science in Germany in the beginning of the 20th century with names like Lexis in social statistics and statistics in general, Becker, Knapp and Zeuner in demography, Paasche and Laspeyres in economics, Weber and Ebbinghaus in psychology. It is something of a mystery how the development could stagnate so rapidly.

And not only this. The socioeconomic sciences in Germany were the arena of an unfruitful struggle between two lines of thought. A typical example is sociology, where the *historical* school had Max Weber as leading name, and the *systematic* school was headed by Georg Simmel. What I am thinking of here is that model building was almost completely non-existent in the camps that were lined up in the *Methodenstreit,* while, on the contemporary international scene, model building had already become the vehicle for steady progress in economics and econometrics. It would seem that Anderson's contributions in the direction of model building were hampered by the *Methodenstreit*. Yet the germs are there, and even if the seedlings got mixed with some weeds, in a general statistical setting that allows us to view the principles and methods at issue as applicable not only in econometrics but over the entire area of nonexperimental model building. These germs emerge as Anderson's most valuable and important contribution. I wish to pay personal tribute to the inspiring influence of this aspect of Anderson's work.

Oskar Anderson's scientific status was marked by several distinctions, among those

Honorary Doctor at the University of Vienna and the Institute of Economics, Mannheim;

Honorary Member of the London Royal Statistical Society and of the German Statistical Society;

Founder and Fellow of the Econometric Society;

Member of the International Statistical Institute;

Fellow of the American Statistical Association and of the Institute of Mathematical Statistics.

Anderson was a man of grandeur, both in his work and his personal appearance. His tall, handsome and somewhat stout figure was seen at several scientific meetings after WWII. Particularly dear to me are the memories from the Scandinavian week at Munich University, July 1958, when I had the privilege of visiting him on his own milieu: the institute that he had founded, his graduate seminar, and his large group of students.

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VI-III

O. Sheynin

Anderson, Oskar Johann Victor

Dict. Scient. Biogr., vol. 1, 1970, pp. 154 – 155

Born, Minsk, Russia, 2 Aug. 1887, died Munich, Federal Rep. Germany, 12 Febr. 1960.

After studying for one term at the mathematical faculty of Kazan University, Anderson in 1907 entered the economic faculty of the Petersburg Polytechnic Institute. He graduated in 1912 as candidate in economics. His dissertation, in which he developed the variance-difference method for analysing time series, was published in *Biometrika* almost simultaneously with similar work by Student (W. S. Gosset).

Anderson was a pupil and an assistant of Chuprov and always considered himself a representative of the Continental direction of mathematical statistics exemplified by Lexis, Bortkiewicz and Chuprov. From 1912 until he left Russia in 1920, Anderson taught in commercial colleges at Petersburg and Kiev and engaged in research. In 1915 he participated in a study of the agriculture of Turkestan by sampling methods (he was one of the pioneers in this field) and in 1918 worked at the Demographic Institute of the Kiev Academy of Sciences.

After he left Russia Anderson spent four years in Hungary, continuing his pedagogic and scientific activities. From 1924 to 1942 he lived in Bulgaria, where he was extraordinary professor of statistics and economic geography at the Varna Commercial College until 1929 and full professor from then on; member of the Supreme Scientific Council of the Central Board of Statistics, and from 1935 director of the Statistical Institute of Economic Researches at Sofia University. Anderson was mainly engaged in the application of statistics to economics and in 1938 published a review of the general status of Bulgarian economics. Later economical-statistical investigations in Bulgaria were always conducted in the spirit of his traditions, and in this sense he founded a school in that country,

Anderson also became internationally known: he published a primer (1935), in 1936 delivered lectures at the London School of Economics and was an adviser to the League of Nations and a charter member of the Econometric Society, also honorary member of the Royal London and West German Statistical Societies, member of the International Statistical Institute and Fellow of the American Statistical Association.

In 1942 Anderson accepted a professorship at Kiel University and from 1947 until death held the chair of statistics at the economics faculty of the University of Munich, was the recognised leader of West German statisticians. His pedagogic activities resulted in higher standards of statistical education for student economists in West Germany.

Besides developing the variance-difference method Anderson researched the quantity theory of money and the index-number theory. Seeing no significant advantage in the application of classical mathematics to economics, he advocated the application of mathematical statistics. Anderson believed that the application of statistics distinguished modern economics from economics based on Robinson Crusoe theories and the *homo oeconomicus*. He especially believed that statistics, based on the law of large numbers and the sorting out the random deviations is the only substitute for the impossible experimentation. Sensibly estimating the difficulties inherent in economics as a science, Anderson was opposed to the use of *refined* statistical methods and to the acceptance of preconditions about the laws of distribution. This led him to nonparametric methods and to the necessity of causal analysis in economics.

Bibliography

1. Original works. Anderson published some 80 books, papers, reports to national and international bodies, reviews and obituaries, mainly in German and Bulgarian. Three papers are in Russian. His books are

Einführung in die mathematische Statistik. Wien, 1935.

Struktur und Konjunktur der bulgarischen Volkswirtschaft. Jena, 1938.

Probleme der statistischen Methodenlehre in den Sozialwissenschaft. Würzburg, 1962, fourth edition. A list of the author's publications is at least in the book's third edition of 1957.

These books provide a sufficient overall notion of Anderson's work. Intended for a broad circle of readers with a pre-university mathematical background, they are less known beyond the German-speaking countries than they deserve to be.

Aside from the books, Anderson's main writings are in his selected works

Ausgewählte Schriften, Bde 1 – 2. Tübingen, 1963.

They contain 46 works with translations from Bulgarian into German. Bd. 2 contains a list of 32 other works and at least two works are missing there. Bd. 1 contains about fifteen obituaries of Anderson and his biography.

2. Secondary literature. General information about Anderson is in

Capelli, Editor, *Bibliografie con brevi cenni biografici. Biblioteca di statistica,* II, pt. 1, 1959.

Kürschners deutscher Gelehrten-Kalender. Berlin, 1961.

The most recent biography is E. M. Fels in this collection.

The late Heinrich Strecker, one of the three most eminent students of Anderson, once told me that Anderson felt himself as Chuprov's son, and that he, Strecker, feels himself as Anderson's son, i. e., as Chuprov's grandson.

VI-IV

S. Sagoroff

Nachruf für Oskar Anderson

Metrika, vol. 3, 1960, pp. 89 – 94

Am 12. Februar 1960 starb in München Oskar Anderson. Mit ihm verlor *Metrika* einen ihrer Begründer und Herausgeber. Eine stille Trauer senkt sich auf unsere wissenschaftliche Gemeinde und verpflichtet uns, des lieben Freundes, des großen Lehrers und des verehrten Kollegen zu gedenken. Der Tod, der ihn von seinen langen körperlichen Leiden erlöste, erlaubt uns, sein Werk zu würdigen, ohne jeden Verdacht, schmeicheln oder gefallen zu wollen.

Von deutschen Eltern in Minsk, Weißrussland, im Jahre 1887 geboren, wuchs Anderson in der Tradition des gründlichen deutschen Gelertentums und vor dem weiten Horizont der großen russischen Erde auf. Sein Vater war Universitätsprofessor für finnisch-ugrische Sprachen, sein Bruder gleichfalls Universitätsprofessor der Philologie. Er verbrachte seine Jugend in Kasan, wo er das Gymnasium als Primus seiner Klasse im Jahre 1906 beendete und an der Universität Physik und Mathematik studierte, und später in St. Petersburg wo er die Nationalökonomisch Fakultät des Polytechnischen Institutes [Schule] als Schüler des berühmten Vertreters der Kontinentalen Schule der mathematischen Statistik, A. A. Tschuprov, im Jahre 1912 absolvierte. Er promovierte als Dipl. Volkswirt mit einer Dissertation über die Anwendung der Korrelationsrechnung auf Zeitreihen. Im Jahre 1914 legte er auch das juristische Staatsexamen an der Universität St. Petersburg ab.

Mit 25 Jahren begann Anderson seine wissenschaftliche Tätigkeit und seine berufliche Laufbahn als Lehrer. Es wurde im Jahre 1912 Assistent am Statistischen Institut [Studierzimmer!] des von ihm absolvierten Polytechnischen Institutes und gleichzeitig Lehrer für Nationalökonomie, Wirtschaftsgeographie und Gesetzeskunde an einer Handelsoberschule in St. Petersburg.

Die Erste Weltkrieg brachte den jungen Gelehrten in das wirtschaftliche Leben hinein und stellte ihn vor seine ersten Forschungsaufgaben, Er wurde in leitenden Stellung zunächst – im Jahre 1915 – in der staatlichen Bewirtschaftungsorganisation der Brennstoffe und dann – im Jahre 1917 – in einer südrussischen Genossenschaftszentrale eingesetzt. Im Jahre 1915 erlebte Anderson etwas, was seine statistische Praxis besonders bereicherte und einem Geist die Freuden der Forschung enthüllte: die Teilnahme an der staatlichen wissenschaftlichen Expedition in Turkestan. Als Mitglied dieser Expedition leitete er eine der ersten repräsentativen Erhebungen in der Geschichte der Statistik – die statistische Beobachtung der landwirtschaftlichen Betriebe in den künstlich bewässerten Oasen am mittleren und oberen Land des Flusses Syr-Daria.

Im Herbst 1918 habilitierte sich Anderson für Statistik an der Handelshochschule Kiew und begann dort als Privatdozent über mathematische Statistik zu lesen. Der staatspolitische Umbruch in Russland zwang ihn aber, im Jahre 1920 mit seiner Familie auszuwandern. Konstantinopel in der Türkei und Budapest in Ungarn waren Stationen auf einer schwierigen Strecke seines Lebensweges, auf der er – noch während der Flucht in Russland – seine einzige Tochter verlor. Eine glückliche Wendung nahm sein Leben, als er im Jahre 1924 einen Ruf als Außerordentlicher Professor für Statistik und Nationalökonomie an die Handelshochschule Warna in Bulgaria erhielt. Fünf Jahre später wurde er zum Ordinarius ernannt und nach weiteren fünf Jahren an die Universität Sofia berufen. In der Hauptstadt Bulgariens leitete er von 1935 bis 1942 das Statistische Institut für Wirtschaftsforschung an der Universität Sofia.

Im Jahre 1942 erhielt Anderson einen Ruf als Ordinarius für Statistik an die Universität Kiel. Hier leitete er gleichzeitig die Abteilung für Ostforschung an dem Weltwirtschaftlichen Institut der Universität Kiel. Die Verwüstungen des Luftkrieges in Kiel und der Tod einer seiner Söhne auf dem Schlachtfeld in Tunis erschütterten ihn seelisch und schwächsten ihn körperlich ungemein. Daher verließ er im Jahre 1947 Kiel mit dem Gefühl der Erlösung, um einem Ruf an die Universität München zu folgen.

Die achtzehn Jahre, die Anderson in Bulgarien verbrachte, bilden seine an theoretischen Arbeiten reichste Periode. Nach zwei Richtungen hin hat sich sein schöpferischer Geist betätigt: manchmal baute er auf, manchmal ging er kritisch vor.

Aufbauender Natur waren zahlreiche Arbeiten über Begriffe und Methoden der mathematischen Statistik, die mit der Schaffung des sogenannten Differenzen-Verfahrens begannen (siehe insbesondere den Aufsatz Über die Anwendung der Differenzenmethode – Variate Difference Method – bei Reihenausgleichungen, Stabilitätsuntersuchungen und Korrelationsmessungen, *Biometrika*, vol. 18, 1926 und vol. 19, 1927) und in seinem Hauptwerk, in dem Buch *Einführung in die mathematische Statistik*, 1935, gipfelten. Das *Differenzenverfahren* von Anderson und Student (W. S. Gosset), unabhängig von einander ausgearbeitet, hat in der Wissenschaft verdiente Anerkennung gefunden, vermöchte aber nicht, sich zu einer allgemein verwendbaren analytischen Methode, wie z. B. die Korrelations-rechnung, zu entwickeln. Anderson's *Einführung* stellt eines der in der statistischen Literatur so seltenen Werke dar, die sowohl originell als auch systematisch und einheitlich aufgebaut sind. Sie könnte formell etwa mit der Laplaceschen *Théorie analytique* oder mit der Mises-schen *Wahrscheinlichkeitsrechnung* verglichen werden.

Wenn dieses Werk, trotz seiner Vorzüge, die ihm gebührende Rolle in der Entwicklung der statistischen Theorie nicht gespielt hat, so ist dies, meines Erachtens, auf zwei Umstände zurückzuführen; erstens auf den ungünstigen Zeitpunkt seiner Erscheinung – die Götterdämmerungsstimmung, die in der Wissenschaft am Vorabend und während des Zweiten Weltkrieges in Europa herrschte; und zweitens auf seine Grundkonzeption – auf den Versuch, die allgemeine statistische Theorie auf einem Spezialfall – auf dem Begriff der *sozial-statistischen Wahrscheinlichkeit* – aufzubauen. Darf man Anderson deshalb einen Vorwurf machen? Ihn kritisieren? Ich würde mit Bestimmtheit sagen: Nein! Die Statistik ist die Brücke zwischen der Sinneswelt der Substanzwissenschaften und der gedachten Welt der Mathematik. Anderson wollte in ihr der sozialen und ökonomischen Wirklichkeit, die endlich und unstetig ist, näher sein, als der Mathematik, die auf Unendlichkeit und Kontinuum baut. Seine unbestechliche Liebe zur Wahrheit, wie er sie sah und sein Drang zum selbstständigen Urteil schrieben ihm den Weg vor, der nicht so breit und weit ist wie der Weg, den die moderne mathematische Statistik eingeschlagen hat, dennoch aber nicht unrichtig ist.

Die aufbauenden Arbeiten Andersons, die alle ausschließlich zum Gebiete der Statistik gehören, hätten ausgereicht, um ihn international berühmt zu machen. Von grundlegender Bedeutung für die Wissenschaft im allgemeinen und für die Ökonometrie im besonderen waren aber seine kritischen Arbeiten. Anderson hatte das Glück, in der Zeit zu wirken, in der die Konjunkturforschung aufkam, und dabei zu den wenigen Gelehrten zu gehören, die in sich alle für einen Ökonometriker wichtigen Eigenschaften vereinigen: Beherrschung der Wirtschaftsmorphologie, der Wirtschaftstheorie, der Mathematik, sowie der reinen und der angewandten Statistik. Die Konjunkturforschung, die in den 1920-er Jahren in den Vereinigten Staaten entstand und in den 1930-er Jahren in Europa zur Blüte gelangte, strebte die Mathematisierung der Konjunkturtheorie an. Man glaubte anfänglich, das Ziel auf rein empirisch-induktivem Wege erreichen zu können. Der Zusammenbruch des Harvardschen Konjunkturbarometers brachte die Ernüchterung. Es war Anderson derjenige, der die großen Mängel der empirisch-statistischen Forschungsmethode mathematisch nachwies. In einer Reihe von kurz aufeinanderfolgenden Werken

On the logic of the decomposition of statistical series into separate components. *J. Roy. Stat. Soc.*, vol. 90, 1927, pp. 548 – 569.

Zur Problematik der empirisch-statistischen Konjunkturforschung, kritische Betrachtungen der Harvard-Methode, Veröff. Frankfurter Ges. Konjunkturforschung, No. 1, 1929.

Die Korrelationsrechnung in der Konjunkturforschung. Ibidem, No. 4, pp. 166 – 301.

zeigte er die Willkür der damals vorherrschenden Reihenzerlegungsmethoden. Damit wurde der *mechanistischen* Auffassung vom Wesen der Konjunktur der Boden entzogen und der Weg zu einer umwälzenden Erkenntnis frei gemacht, nämlich zur Erkenntnis, dass die wissenschaftliche Erfassung der wahrnehmbaren Wirklichkeit ohne theoretische Hypothesen nicht möglich ist. Aus dieser Erkenntnis heraus entstand die moderne Theorie des wirtschaftlichen Verlaufs: der ökonometrische Modellbau. Heutzutage ist für uns selbstverständlich, zwischen den theoretischen und den empirischen Werten der in dem Modell erscheinenden Größen zu unterscheiden. Diese Unterscheidung, die übrigens für alle Substanzwissenschaften gilt, kam in der Ökonometrie während der 1940-er Jahre als eine Entdeckung auf, die mit den Namen von Haavelmo und Koopmans verbunden ist. Anderson beschäftigte sich mit dem Problem der empirischen Formulierung der theoretischen Beziehungen etwa zehn Jahre früher (siehe seinen Aufsatz Ist die Quantitätstheorie des Geldes statistisch nachweisbar, *Z. f. Nationalökonomie*, Bd. 2, 1931, pp. 523 – 578).

Während die *bulgarischen Jahre* die Stellung Andersons als Theoretiker begründeten, waren seine *deutschen Jahre* für seine Bedeutung für die Entwicklung der Statistik in Deutschland maßgebend. Nach dem Tode von Ladislaus v. Bortkiewicz war die mathematische Statistik für ein Jahrzehnt als Lehre an den deutschen Universitäten ausgestorben. Es war Anderson derjenige, der in dieser Zeit das Banner der kontinentalen Schule der mathematischen Statistik trug. Seine Berufung an die größte deutsche Universität nach dem Kriege gab ihm viel Möglichkeit, für die Verbreitung der mathematischen Statistik zu kämpfen. Von München aus beeinflusste Anderson mit Vorträgen und Schriften den Unterricht und das theoretische Denken an den deutschen Universitäten, sowie die deutsche amtliche Statistik. Wenn heute in Deutschland die mathematische Statistik als Lehre vorherrscht, so ist dies in hohem Maße ein Verdienst Andersons.

Will man seine Stellung in der statistischen Wissenschaft bestimmen, so kann man sagen, dass Anderson der letzte Vertreter der von Lexis, Bortkiewicz und Tschuprov ins Leben gerufenen kontinentalen Schule der mathematischen Statistik war. Faßt man diese Schule mit der alten – von Karl Pearson, Bowley und Yule begründeten – englischen Schule als klassische Schule der mathematischen Statistik zusammen, so ist Anderson als einer der letzten Klassiker zu bezeichnen. Anderson war für den Fortschritt der Statistik sehr aufgeschlossen. Er nahm die Beiträge von Fisher, den er besonders verehrte, zur Gänze in seinen Unterricht auf. Der von J. Neyman und E. Pearson geführten Richtung stand er nicht so nahe. Die einzige Einschränkung, die er gegenüber der modernen mathematischen Statistik machte, bestand darin, dass die für die Naturwissenschaften geltenden statistischen Methoden nicht ohne weiteres auf die Sozialwissenschaften übertragen werden können.

In jedem Lande, in dem er wirkte, pflegte Anderson mit der amtlichen Statistik enge Beziehungen. Dies entsprach seiner Auffassung vom Wesen der Statistik, dass sie zum Teil – in ihrer reinen Theorie – eine formale Wissenschaft und zum Teil – in der Theorie der Datenproduktion (als *statistische Betriebslehre*) – eine Substanzwissenschaft ist. Der Umstand, dass sich die stochastisch-repräsentative Methode in der amtlichen Statistik in Russland sehr früh eingebürgert hatte, kam ihm besonders zu gute.

Es ist nämlich zu wenig bekannt, dass eine Volkszählung nach dem Stichprobenverfahren zuerst in Russland, und zwar in den Jahren 1916 – 1917 aufgenommen wurde (das Beobachtungsmaterial wurde nicht bearbeitet – es ging in den Wirren der Revolution verloren). Anderson brachte die russische Tradition der repräsentativen Methode nach Bulgarien. Als Mitglied des Obersten Statistischen Rates (1926 – 1942) und als Konsulent der Bulgarischen Generaldirektion für Statistik gelang es ihm, das Stichprobenverfahren in die allgemeine Volks- und Betriebszählung vom Jahre 1926 einmal erschöpfend und einmal repräsentativ aufbereitet, um die Verlässlichkeit der mathematischen Methoden zu prüfen (das Ergebnis des Vergleiches war verblüffend gut) und später, im Jahre 1931 – 1932, eine landwirtschaftliche Betriebs- und Produktions-Enquete nach dem Stichprobenverfahren durchgeführt. Nach seiner Berufung nach München arbeitete Anderson mit dem Bayerischen Statistischen Landesamt zusammen. Ihm und seinem Nachfolger, Professor Kellerer, ist im hohen Maße zu verdanken, dass sich die mathematischen Methoden der statistischen Beobachtung und Aufbereitung auch in Bayern eingebürgert haben.

Die langjährige und erfolgreiche Tätigkeit Andersons in Theorie und Praxis brachte ihm viele Ehrungen und Auszeichnungen. [Sagorov lists them. In addition to those listed in other obituaries, he mentioned Fellow of Amer. Assoc. for the Advancement of Science.]

Bibliography

O. Anderson (see also his works mentioned in the text)

1926 – 1927, Über die Anwendung der Differenzmethode […] bei Reihen-ausgleichungen, Stabilitätsuntersuchungen und Korrelationsmessungen. *Biometrika*, vol. 18, pp. 293 – 320; vol. 19, pp. 53 – 86.

1935, *Einführung in die mathematische Statistik*. Wien.

Other authors

Sheynin O. (1990, Russian), *Alexandr A. Chuprov. Life, work, correspondence*. Göttingen, 2011.

You Poh Seng (1951), On the development o sampling theories and practice. *J. Roy. Stat. Soc*. vol. A114, pp. 214 – 231.

This obituary sheds light on quite a few facts not mentioned elsewhere. Some critical remarks are however necessary. First, Sagorov belittled Anderson's scientific work during his German period of life. However, the second volume of Anderson's *Ausgewählte Schriften*. Tübingen, Bde. 1 – 2, 1963 includes about 25 of his pertinent works. And concerning the history of sampling see You Poh Seng (1951).

Second. As compared with the Continental direction of statistics, the Biometric school was not old, and Bowley and Yule were not its co-founders. Third. On the history of censuses of population in Russia see Sheynin (1990/2011, pp. 129 – 131).

VII

Norman T. J. Bailey

The scope of medical statistics

Applied stat., vol. 1, 1952, pp. 149 – 162

Introduction

Medical statistics may, I think, fairly be said to have started in the second half of the 17th century with the work of John Graunt and William Petty, who based many of their observations and conclusions on the famous London Bills of Mortality. Although vital statistics, with suitable modern refinements, is still an important part of the whole corpus of medical statistics, the latter has, in the last 200 years, gradually increased its scope. With the assimilation of the advances of modern mathematical statistics, especially those occurring in the last 30 years, medical statistics may now reasonably claim applications to the whole realm of medical science.

It is of course impossible in the space available to review more than a small proportion of the whole field, but I shall try to give as wide a selection of topics falling within the province of medical statistics as possible. My object is twofold: first, to illustrate the variety and scope of the subject; and, second, to indicate a number of problems which seem worthy of further study and understanding. I make no special apology for drawing on much material with which I have been personally concerned, partly because it is easier to do justice to subjects to which one has given particular attention, and partly that my remarks should reflect the statistical interests of the Cambridge Medical School.

1. Medical records and socio-medical surveys

A place of fundamental importance in medical statistics is held by that part of the subject which is based on medical records. Much valuable information can be gleaned from a consideration of mortality and morbidity rates for various diseases, especially in relation to environment and social class. An enormous amount of work of this kind is regularly carried out at various centres and it would be impossible to make an adequate summary. There are, however, a number of useful introductions to the potentialities of this approach[1, 2, 3]. Reliable data are of course nearly always essential in any kind of investigation, but when, in contradistinction to comparatively simple laboratory readings and measurements, one is concerned with the analysis of vast amounts of information collected at considerable expense over long periods of time about thousands of individuals, then some special attention to methods of recording and analysing the data is called forth.

One of the most obvious examples is the collection of hospital statistics, which are particularly useful, not only for the routine collection of standard morbidity and mortality data, but also in the study of caseloads for administrative purposes to see, for example, whether the supply of medical facilities is adequate for meeting the demand. Again, a hospital should have an efficient records system so that the notes relating to a given patient can be easily found, or so that all the cases of a given disease can be extracted for detailed study. On the other hand, the use of an elaborate punched-card system, with the object of doing detailed retrospective research into the effects of various medical and surgical treatments, is in my opinion rarely justifiable. In the first place, the valid comparison of different methods and treatments usually requires carefully controlled experimental conditions, which are hardly ever found to have occurred naturally in a retrospective survey. Second, it is extremely difficult, if not impossible, to design a record form which will cover adequately all the data which may be needed from every kind of patient for some future research. It seems to me much more satisfactory to use a relatively straightforward and inexpensive system for producing routine statistics. Then, if a particular piece of research is to be undertaken, special record forms can be designed and all the paraphernalia of punched-card equipment brought into action.

The kind of investigation for which punched-card methods are invaluable is the socio-medical type of work where a great deal of clinical, social and economic data is collected about each of a large number of patients suffering from a particular disease, together with a number of controls, living in a broadly specified environment. Extensive record forms can be specially designed to serve as both original clinical records and transcription forms for punched-card work. It should be said that with the most modest range of equipment, say, a hand-punch and a counter-sorter only, quite extensive surveys of this type can be undertaken and conveniently analysed if one makes a special effort to compress all the data for a single individual onto one punched card. When several cards are used for each individual the correlation of data on different cards usually entails special machinery which greatly adds to the cost of the simple set-up referred to above.

2. The provision of treatment

Under this general heading come all the problems connected with efficiency of supplying patients or prospective patients with known methods of remedial or preventive treatment. We want to know whether the general practitioners, specialists, nurses, social welfare workers, etc. are available in sufficient numbers for the needs of the particular community they work in. We also want to know whether the services in which these individuals play a vital role are efficiently organized. This is a particularly pressing problem with present-day shortages of trained staff and housing accommodation. Very little building of new hospitals, clinics, and health centres can be done in the near future, and the main opportunities for the better provision of medical treatment apart from fundamental advancement in actual medical knowledge, will probably be found in the more efficient utilisation of existing resources.

The question of hospital function and design, together with various allied problems, is the object of a special study which is being made by a research team under the auspices of the Nuffield Provincial Hospitals trust[4].

One particularly important topic is the rationalisation of traffic flow within a hospital. This is concerned with reducing to a minimum the movement from one part of the hospital to another of patients, nurses, doctors, visitors and equipment, and is desirable on grounds of both convenience and hygiene. One can conceive a hospital as consisting of a number of units. There are the beds, duty rooms, utility rooms, lavatories, etc., for each ward-unit in several specialities. In addition there are a variety of special services and facilities: operating theatres, kitchens, X-ray department, pathology laboratory, etc. On medical grounds certain types of traffic from one place to another will be essential: we can form a general pattern of the necessary traffic in terms of the number of journeys that have to be made from one point to another in a certain period of time. At its simplest the problem is:

How to site all the various units to reduce to a minimum the total volume of traffic – measured in units involving distance as well as numbers of persons or amount of material. There are several complications, of course, including the difficulty of coping with peak-hour requirements. A general solution of the simplest problem, which would involve a combination of statistics and the calculus of variations, is not yet available. An *ad hoc* procedure is to draw up a number of alternative designs which are acceptable both architecturally and medically, and then to apply to each the standard pattern of traffic with the object of discovering which design involves the smallest total flow. A fair degree of success has already been obtained with this approach, both with regard to the relative siting of some of the major units within a hospital and also with regard to the arrangement within a ward-unit to cut down the total walking distances of nurses on duty.

Another problem is concerned with deciding how many beds in a ward-unit should be separately accommodated in single rooms. Separate accommodation is desirable on medical grounds for dying patients, those who require special treatment, patients who are highly infectious or peculiarly susceptible to infection, etc. Dr. Goodall[5] has discussed this problem and has made a further distinction between separately accommodated patients who must be immediately accessible and those who need not be. The number of single rooms available is of course a fixed quantity, but the demand shows considerable day-to-say fluctuations. It is evident that a simple estimate of the average requirement will not give a proper indication of the number of single rooms that should be provided and I have undertaken a statistical treatment[6]. For any given number of single rooms provided we can calculate the extent to which the demand for single rooms can be met (the *efficiency of provision*) and the extent to which the single rooms actually provided would be occupied for the proper purpose (the *efficiency of utilisation*). The former can be made very high only at the expense of the latter, and vice versa. For if enough single rooms are provided to cope with nearly all requirements even on *peak* days, many will not be used for their proper purpose on other days. If a satisfactory balance is to be struck between these conflicting factors, namely the purely medical needs and the necessity for economising space and finance, then hospital planners must try to choose an optimum number of single rooms, giving as high an efficiency of provision as possible without allowing the efficiency of utilisation to drop too low. I found, for example, that with an average 16-bed surgical unit two single rooms should be provided for patients who must be easily accessible: the two rooms would satisfy 84% of the demand and be used 73% of the time. Three rooms would satisfy 92% of the demand but would be used for their proper purpose only 59% of the time, and so on.

Most people will immediately regard the design of appointment systems for out-patient clinics as being of some urgency. The number of people waiting for medical examinations and the rate of examination both bear very strongly on the design of the department. It is important to have a good appointment system, not only to reduce waiting times to a minimum, but also to use the available accommodation in the best possible way. It is not uncommon for the average consulting time to be of the order of ten minutes while the average waiting time is upward of an hour.

In the queueing problems so ably discussed by D. G. Kendall[7] the main attention was concentrated on the situation where customers arrived at random. In out-patient clinics, on the other hand, the patients can, within limits, be made to arrive at predetermined times, while the consultation time is variable, having some characteristic frequency distribution.

The basic idea is that, given the distribution of consultation time, we ask what is the optimum appointment system which will save the patients the greatest amount of waiting time without the consultant having too long an idle period. Theoretical work of a fairly general nature by Lindley[8] has made available the limiting distribution of waiting times for an infinitely long queueing process when the average consultation time is less than the appointment interval. Using the alternative approach of random number studies, I[9] have investigated the case of relatively short queueing processes involving, say, 25 patients. Assuming a suitably chosen Pearson Type III curve for the frequency distribution of consultation times I showed that an optimum appointment system is as follows. Suppose that the average consultation time is 5 minutes. Then patients are given appointments at regular 5 minutes intervals, the consultant commencing work as the second patient arrives. For a clinic of 25 patients the patients' average waiting time would be 9.1 minutes, while the consultant would waste, on average, 5.7 minutes per clinic. A further discussion of the practical applications has been made by Brigadier Welch and me [10].

3. The efficacy of treatment

As the primary object of medical science is either to treat or prevent disease and disablement, so one of the main applications of medical statistics is in the testing of such measures. Actually, as I see it, the real difficulty is not statistical at all, but consists in the wide-spread need for arranging that adequate tests be done. Although, of course, many drugs in current use have been subjected to properly controlled tests, it is equally true that large numbers are, at least statistically speaking, of doubtful or unknown value. It is not easy to organise genuine experiments on human subjects, and in any case the responsibility for initiating such work does not lie with the statistician. However, the statistician can do much to improve the situation by encouraging those whose main concern is with the discovery and application of new methods both to examine more critically existing medical procedures and not to accept new ones without first carrying out stringent tests.

Given the opportunity to undertake such tests it is usually not difficult to apply standard statistical methods to the design and analysis of the appropriate medical data, whether it is estimating the recovery or relapse rate for a new treatment or comparing the degrees of protection against certain diseases given by various vaccines, etc. If one has sufficient patients (who may have to be volunteers) then it is often possible to run the standard *factorial experiments* involving several treatments on different types of patient under a variety of environmental conditions. However, it is a salient characteristic of medical work that it is often not easy to get a sufficient number of individuals to obtain conclusive results. The disease in question may be uncommon, and volunteers may be few and far between. This has led to the increasing use of *matched samples,* whereby several different treatment are tried on each individual [1]. With quantitative data this can be taken into account by regarding the individual as a block of plots, as in agricultural experiments. But with qualitative data special methods are required. With appropriate safeguards the method of matching appears to lead to an increase in the precision of the results obtained. For example, J. E. Denton and H. K. Beecher[11] in their tests of a new analgesic drug gave the drug and a control injection at different times to every patient. The mathematical implications of matched samples do not yet seem to have been fully explored, although a valuable discussion of significance tests in the multiple-sample case has been given by Professor Cochran[12], who has extended the two-sample test originally given by Q. McNemar[13].

Another statistical technique which does not yet seem to have been made full use of in medical research is *sequential analysis*[14]. As is well known, this method involves sampling according to a prearranged plan, whereby one stops the sampling process as soon as a definite decision is obtained. The process continues to be applied only so long as the criteria adopted still indicate that no decision is yet possible. In general, sequential methods require smaller samples to achieve a given result, so that the need for such methods in medical work is immediately obvious. Another advantage, which so far as I am aware has not been completely realised, is that the use of a sequential method would afford some relief from the perennial question:

When should a promising drug, which is not yet fully tested, be given to every patient?

This is a very real ethical complication which adds to the other difficulties of carrying out satisfactory tests of medical treatment and I do not suggest that the use of a sequential test would avoid all the moral pitfalls. But it would enable the criteria for decisions to be discussed and agreed upon before the experiment started – that is the important point. It would avoid adding to the doctor's burden of responsibility by making him try to decide during the course of an ordinary non-sequential test just whether he ought to continue or break off the experiment at any stage. We might, for example, be comparing the recovery rate in batches of hospital patients, half being given the old and half the new drug, and could decide before the experiment precisely what action would be taken if any particular series of recovery rates were observed for the two drugs. A sequential procedure could then be drawn up which satisfied both medical ethics and the statistical requirements. This would ensure that whatever the outcome of the experiment at any particular stage, the decision taken would be the best possible.

4. Epidemiology

The existence of widespread infections or contagious diseases has always been regarded as one of the major scourges of mankind and, judging from their prevalence in both animal populations and primitive human communities, such infections and diseases have presumably been with us since our earliest origins. Many factors contribute to modify the behaviour of epidemic outbreaks in the course of time. Old diseases may become less virulent or show changes in age incidence and new diseases of obscure origin may arise. The operation of natural selection in eliminating the unduly susceptible; improvements in general health, nutrition, and hygiene; advances in methods of treatment; and so on. All these make their contribution to the fight against epidemic disease. In spite of great improvements the problem is still serious. Although the general picture is for many diseases reasonably clear, detailed quantitative knowledge about such factors as the actual mechanism of infection, transit times, incubation times, variations in infectivity, numbers of susceptibles, and carriers in the population are often very obscure.

Epidemiologists have always made great use of the so-called *epidemic curves*, that is, curves giving the numbers of new cases of disease occurring each day or week in a given area, in the hope that they will reveal facts about the character of the epidemic that would otherwise not have been obvious. There does seem to be a reasonable hope of advance in this direction, but so far many of the conclusions drawn are still only speculative. The basic principle is that we construct various theoretical epidemic curves based on a variety of different assumptions and then see which fits actual data most satisfactorily.

Most of the theoretical work done in the past has been of a *deterministic* nature, that is, it has been assumed that, for given numbers of susceptible and infectious individuals and given infection, death and recovery rates, a certain definite number of fresh cases would arise in a given time. The early work of Ross[15 – 17] on what he called *prior pathometry* and of Brownlee[18] was of this type. So also were the much more elaborate studies later undertaken by Kermack & McKendrick[19] and Soper[20]. The former showed that for a given set of infection and recovery rates there was a certain threshold density of population. If the actual population density were initially below this value the introduction of an infected person would produce no epidemic. If the density were above the threshold an epidemic would occur in such a way ass to reduce the density of susceptibles as far below the threshold as originally it was above. With diseases transmitted through a host there is a similar threshold theorem involving the product of the densities of the populations of man and host.

Soper's work was mainly on the periodicity observed in the recurrence of epidemics in diseases like measles. Again, some insight was gained into the underlying mechanism and it was evident that the continual entry into schools of young susceptibles was one of the important factors.

Little success has in the past been obtained by trying to predict the course of an epidemic by fitting curves to the initial data. In any case curve-fitting can hardly be expected to afford any clue to the physical and biological processes taking place unless it is based on some particular sort of mathematical model.

One of the difficulties inherent in the notified returns of a disease is that they usually relate to such large areas. It is well known that an overall epidemic can often be broken down into smaller epidemics occurring in separate regional subdivisions. These regional epidemics are not necessarily in phase and may interact with each other. Taking the process of subdivision a stage farther, we can consider a single borough or district, or finally, the *effective group* in which a single individual moves, since in many cases he will be in close contact with a small number of people, only say about 10 – 50. The observed epidemic for the whole region can then be thought of as built up from a number of small epidemics taking place simultaneously in several relatively small groups of associates and acquaintances. Although in practice the groups may overlap, we can employ the concept of an *effective* number of independent groups or isolates. A typical simplified model involves a community of k independent groups each of size n. We imagine an epidemic started by the simultaneous appearance or introduction of k primary cases, one for each group. Now, for comparatively small values of n statistical fluctuations will be appreciable; the older deterministic treatments will be unsuitable and we shall have to appeal to the so-called *stochastic* methods. Professor Bartlett[21] has emphasized this need and has devoted some discussion to various partial attacks already made on the problem. He[22] has also referred to the simplest stochastic epidemic in which only infection but not recovery is considered, and I have discussed this problem in greater detail[23]. It should be mentioned that these results for epidemics with no recovery are in fact also applicable to epidemics for which the time taken to recover is fairly long compared with the period occupied by the humped part of the epidemic curve. With the assumptions made previously about epidemics taking place simultaneously in separate groups, it is evident that with a fairly large number of groups the epidemic curve for the whole region will resemble in shape the curve corresponding to the stochastic mean [2] for a single small group. In epidemic processses, it should be remarked, stochastic means are not the same as the corresponding deterministic values.

The mathematical treatment of epidemic processes presents many analytic difficulties not the least of which is to find expressions for the stochastic mean, for example, which are suitable for computation and practical application. It may be that the greater chance of progress lies in the alternative approach of using random umber studies.

A word should be said in passing about the possibility of experimental work on epidemics using a suitable animal such as the mouse. Work of this sort has been very skilfully undertaken by Greenwood, Topley & Wilson [somewhat strange reference below] and has included studies of the waxing and waning of epidemics in infected herds, the effect of introducing new unprotected immigrants and the effect of withdrawing animals from the herd[24].

Mouse communities exhibit many of the phenomena observed in human communities with regard to the occurrence of epidemic disease. The advantage of the animal work is that the time scale is short and conditions can be closely controlled. There would seem to be great possibilities in further work of this kind for increasing our understanding of both human and animal epidemics. Incidentally, properly designed experiments coupled with appropriate statistical analysis might shed some fresh light on the role of vaccination in the prevention of disease, e. g., smallpox vaccination in man and fowl-pest vaccination in fowls.

Another way in which statistical analysis can be of use to the epidemiologist is in the study of the distribution of multiple cases of disease in households. It is well known that measles tends to show an approximately biennial periodicity. In the discussions of this by Hamer and later by Soper it was shown how much a periodicity could arise from a number of assumptions, one of which was that a given case was highly infectious for a short time only. Can some test be made of this latter hypothesis?

Broadly speaking we want in the first instance to differentiate between the situation in which we expect a straightforward binomial distribution of secondary cases, e. g., groups of people drinking typhoid-infected water, and the situation in which the disease is passed from person to person. Discussing a measles epidemic occurring in St. Pancras [in London] in 1926, Greenwood[25] showed that the first hypothesis of a binomial distribution of cases was quite inadequate [3]. Now consider the second hypothesis. The argument used by Greenwood was that the distribution of secondary cases resulting immediately from the first case would be binomial. But then susceptibles escaping this risk might be infected later by the secondary cases themselves, giving rise to tertiary cases etc. This is the principle of the so-called *chain* binomial argument. It should be valid when a disease is highly infectious for short periods. Greenwood estimated the chance of infection by fitting mean values. How efficient a procedure this is seems never to have been investigated, though it was evident that quite good agreement was obtained. A more sophisticated test of significance for seeing whether the multiple cases in a household are distributed at random should be derivable from a discussion of the highly interesting mathematical problems involved in the random division of an interval[26, 27].

Further problems of the same type arise from asking whether the geographical distribution of disease is random; compare, for example, the discussion by Dr. Cruickshank[28, 29] of the incidence of tuberculosis and cancer in the UK, and the statistical treatment of so-called *statistical maps* by Professor Moran[30].

I believe that much important work remains to be done in connection with both the household and geographical distribution of cases. These problems certainly present several theoretical difficulties, especially in regard to finding convenient tests of significance and efficient methods of estimation, though these difficulties are no doubt not insuperable.

5. Increasing the efficacy of medical techniques

Basically, one considers whether the technique employed achieves the greatest accuracy to be expected when the inevitable statistical fluctuations are taken into account. If this optimum has not been reached it may be possible to suggest improvements that will enable better results to be obtained. The use of a modified technique with greater intrinsic accuracy means either that one can obtain more accurate estimates for the same effort, or that one can achieve the same overall accuracy as before with less effort.

Let us consider as a typical example the routine examination of differential white cell counts. With good technique we should expect a multinomial distribution of the proportions of the different kinds of leucocyte. In practice the observed accuracy of the percentages of different kinds of cells, polymorphonuclear cells, lymphocytes, monocytes, etc., may be less than that expected, owing to various disturbing factors. These may be the tendency of the cells to clump or the liability of certain types of cells to be displaced to particular portions of the slide. Indeed, the latter phenomenon will lead to systematic errors as well unless special steps are taken to counteract it, such as the employment of the so-called *battlement* count. Moreover, it may be found that with certain techniques the expected accuracies are achieved only if the total number of cells counted exceeds a certain level. For example, in discussing a new mechanical aid for making blood films Marks, Bailey & Gunz[31] showed that for the new method the estimate of the proportion of polymorphonuclear cells was effectively binomial in distribution for counts as low as 100 (actually unpublished data show this may be extended down to as low as 50). With the old method of spreading the films, on the other hand, the binomial distribution held only if the total count was 300 or more.

Again, the problem may be not so much to improve the technique as to find a more efficient method of analysing the data. A much-discussed example of this occurs in the field of biometrical assay where one is assaying the potency of a drug by considering the series of mortality obtained in batches of test animals for different strengths of the drug concerned. In large samples in any case with a normal distribution of tolerances, the greatest efficiency in assessing the data is obtained by using the methods of probit analysis[32]. For a single series of experiments designed to answer certain specific questions it is undoubtedly worth the effort of undertaking the full-dress statistical analysis, but very often it is desirable to have approximate estimates which can be found quickly in the course of preliminary laboratory experiments. Several such alternative methods have been suggested and widely used in the present context. A good deal of work has been done on the relative merits of these alternative procedures[33, 34].

6. Medical genetics

Although genetics stands on its own as an autonomous field of study, certain results, particularly those concerned with human heredity, are of considerable importance to medicine. For the adequate assessment and interpretation of a great deal of genetic data a fair amount of mathematics is often essential, and thus it comes about quite naturally that medical statistics finds itself involved in genetic problems .

First, there are those *population* studies carried out mainly in the late 1920s and early 1930s by men like Sir Ronald Fisher, Professor Haldane and Professor Wright. Evolutionary progress was to be explained by the action of natural selection on the genetically diverse material provided by random mutation. Specific gene models were considered and for given mutation rates and selection pressures the evolutionary consequences could be ascertained by mathematical analysis. This approach can be used in considering the long-term evolutionary changes in the general level of health or disease in the population as a whole. One application was a discussion by Professor Haldane[35] of the selection against heterozygotes owing to the occurrence of haemolytic disease in connection with the Rhesus blood group. Another application is in the estimation of the harmful effects which may appear in future generations if sources of irradiation like X-rays and atomic energy are used irresponsibly[36, 37].

Second, there are what we may call *individual* studies. These deal with the inheritance of common well-defined characters like blood-groups as well as with inherited diseases and abnormalities. Doctors are frequently consulted by patients about the possibility of their developing or handing on to their children some disease or defect which is known to be in the family. Popular ideas in this matter are often quite unnecessarily alarmist. For example, suppose a man who wishes to marry his first cousin had a brother who died from juvenile amaurotic idiocy. The man and his cousin are both healthy and want to know the risks with respect to this disease if they marry and have children. Juvenile amaurotic idiocy is inherited as a simple autosomal recessive, and it can be shown by examining the pedigree in this particular case that there is 1 chance in 6 that both the man and his cousin are carrying a *bad* gene. Thus the chance of the first child being affectted is 1 in 24. The couple may well be prepared to take this chance, but it is desirable for them to know what it is. More complicated situations may require more careful consideration of the probabilities involved. This sort of eugenic prognosis could be made very much more precise if common characters were known to be genetically linked to the abnormality in question. A good example is available involving the not infrequent character of colour-blindness. It is usually not possible to say more than that the childless daughters of a haemophilic man have a 1 in 2 chance of carrying the haemophilia gene and passing it on to half their [future?] sons. Now the loci for haemophilia and colour-blindness are linked. In a certain Dutch family, where both these abnormalities were liable to appear either separately or together in certain individuals, it was possible to estimate that the colour-blind daughter had 9 chances in 10 of carrying a haemophilia gene while her normal sisters had only 1 in 10. It is clear that the discovery of such linkages is of great importance, but, apart from sex-linkages, few if any have yet been definitely established in man.

Not only are human families relatively small but it is also difficult to find marriages between individuals of the appropriate genetic constitution: even then we may know the mating-type only with a certain degree of probability. It has, however, proved possible to develop powerful statistical methods for examining material of the right kind for the existence of genetic linkage. These methods vary according to whether the data consist of a number of isolated pedigrees each containing several individuals related by various degrees of kinship, some of whom may be affected with the abnormality in question, or whether we have a series of specially selected compact family groups, each normally consisting of just parents and children. For the former type of material the methods used by Professor Haldane[38] are appropriate: a particularly useful discussion is the one by Haldane & Smith[39] on estimating the linkage of haemophilia and colour-blindness. For the second type of material Professor Penrose's sib-pair method[40, 41] has been widely used, though it is in general less efficient than the method of *u*-statistics.

The latter method was first developed by Sir Ronald Fisher[42 – 44] and has the advantage of being efficient and enabling data from families of various size to be combined directly. Dr. Finney[45, 46] subsequently extended the method to cover a wide variety of mating types and to deal with such complications as multiple allelomorphs and incomplete parental records. He provided conveniently tabulated lists of the appropriate scores and information functions. I have made a further extension of the method to the cases of rare abnormalities where the gene or genes, though present, may fail to be expressed in a certain proportion of individuals[47, 48] as with Huntington's chorea for example. Another important problem is the estimation of population gene frequencies. When the individuals in a sample are related, as in data collected from family records, special methods of analysis are required[49 – 52].

A great deal of the work in medical genetics is concerned with the inheritance of common characters like the nine main blood-group systems and the ability to taste phenylthiocarbamide. Knowledge of the modes of inheritance of blood-groups and also the corresponding gene frequencies are also valuable not only for questions like estimating the probability of the risk of haemolytic disease in babies of Rhesus negative women with Rhesus positive husbands, but also in medico-legal work. The main applications of the latter are to problems of relationship, especially those involved in disputed maternity or paternity.

Notes

1. Such experiments were carried out by Fisher.
2. So what is the stochastic mean?
3. Have Markov chains been applied in epidemiology?

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In § 1 a highly appropriate reference to Florence Nightingale is missing. Also there the essential problem of data belonging or not to a single statistical population is ignored. The problem discussed in § 2 is treated by logistics. In § 3 much is made of sequential analysis which has its drawbacks. Indeed, when decisions are based on a series of trials, external checks do not exist and systematic errors remain undetected. In § 4 the ideas and results of Ferdinand Tönnies on statistics of small groups of people are not applied.

VIII-I

G. J. Chaitin

Undecidability and randomness in pure mathematics

Lecture of 1989 at Europalia 89 Conf., Brussels

[This lecture was hardly ever published in the usual way.]

Abstract

I have shown that God not only plays dice in physics, but even in pure mathematics, in elementary number theory and in arithmetic. My work is a fundamental extension of the work of Gödel and Turing on unndecidability in pure mathematics. I show that not only does undecidability occur, but in fact sometimes there is complete randomness and mathematical truth becomes a perfect coin loss.

1. Randomness in physics

What I'd like to talk today is taking an important and fundamental idea from physics and applying it to mathematics. That idea is the notion of randomness, which I think it is fair to say obsesses physicists. The question of to what extent is the future predictable, to what extent is our inability to predict the future our limitation, or whether it is impossible to predict the future.

This idea has of course a long history in physics. In Newtonian physics there was Laplacian determinism. Then came quantum mechanics. One of the controversial features of quantum mechanics was that probability and randomness were introduced at a fundamental level to physics. This greatly upset Einstein. And then surprisingly enough with the modern study of nonlinear dynamics we realize that classical physics after all really did have randomness and unpredictability at its very core. So the notion of randomness and unpredictability began to look like a unifying principle, and I would like to suggest that this even extends to mathematics,

I would like to suggest that the situation in mathematics is related to the one in physics. If we cannot prove something, if we do not see a pattern or a law, or we cannot prove a theorem, the question is, is this our fault, is it just a human limitation because we are not bright enough or we have not worked long enough on the question to be able to settle it? Or is it possible that sometimes there simply is no mathematical structure to be discovered, no mathematical law, no mathematical theorem, and in fact no answer to a mathematical question? This is the question about randomness and unpredictability in physics, transferred to the domain of mathematics.

2. The Hilbert problems

One way to orient our thinking on this question is to recall the famous lecture given by Hilbert 90 years ago in 1900 in which he proposed 23 problems as a challenge to the new century, a century which is now almost over. One of the questions, his sixth, had to do with axiomatizing physics. And one of the points in this question was probability theory. Because to Hilbert probability was a notion that came from physics having to do with the real world[1].

Another question that he talked about was the tenth problem having to do with solving so-called Diophantine equations, that is to say, algebraic equations where you are dealing only with integers. He asked: *Is there a way to decide whether an algebraic equation has a solution in whole numbers or not*? Little did Hilbert imagine that these two questions have a close connection!

There was something so basic to Hilbert's thinking that he did not formulate it as a question in his 1900 talk. That was the idea that every mathematical problem has a solution, that if you ask a clear question you will get a clear answer. Maybe we are not smart enough to do it or have not worked long enough on the problem, but Hilbert believed that in principle it was possible to settle every mathematical question, that it is a black or white situation. Later he formulated this as a problem to be studied, but in 1900 it was a principle that he emphasized and did not question.

What I would like to explain is that randomness does occur in pure mathematics, it occurs in number theory, it occurs in arithmetic. And the way occurs ties together these three issues that Hilbert considered because you can find randomness in connection with the problem of solving algebraic equations in whole numbers. That is Hilbert's tenth problem about Diophantine equations.

Then looking at Hilbert's sixth question where he refers to probability theory, one sees that probability is not just a field of applied mathematics. It certainly is a field of applied mathematics, but that is not the only context in which probability occurs. It is perhaps more surprising that one finds probability and randomness even in pure mathematics, in number theory, the theory of whole numbers, which is one of the oldest branches of pure mathematics going back to the ancient Greeks. That is the point I am going ro be making,

This touches also on the basic assumption of Hilbert's talk because it turns out that it is not always the case that clear simple mathematical questions have clear answers. I will talk about some conceptually simple questions that arise in elementary arithmetic, in elementary number theory involving Diophantine equations where the answer is completely random and looks gray rather than black or white. The answer is random not just because I cannot solve it today or tomorrow or in a thousand years but because it does not matter what methods of reasoning you use, the answer will always look random.

So a fancy way to summarize what I will be talking about, going back to Einstein's displeasure with quantum mechanics is to say, *Not only does God play dice in quantum mechanics and in nonlinear dynamics, which is to say in quantum and in classical physics, but even in arithmetic, even in pure mathematics*.

3. Formal axiom systems

What is the evolution of ideas leading to this surprising conclusion? A first point which is surprising but is very easy to understand has to do with the notion of axiomatic reasoning, of formal mathematical reasoning based on axioms, which was studied by many people including Hilbert. In particular he demanded that when one sets up a formal axiom system there would be a mechanical procedure for deciding if a proof is correct or not. That is a requirement of clarity really, and of objectivity.

Here is a surprising fact. If one sets up a system of axioms and it is consistent, which means that you cannot prove a result and its contrary, and also it is complete which means that for any assertion you can either prove that it is true or false. Then it follows immediately that the so-called decision problem is solvable. In other words, the whole subject becomes trivial because there is a mechanical procedure that in principle would enable you to settle any question that can be formulated in the theory There is a colourful way to explain this, the so-called *British Museum algorithm*. What one does (it cannot be done in practice, it would take forever) but in principle one could run through all possible proofs in the formal language, in the formal axiom system, in order of their size, in lexicographic order. That is, you simply look through all possible proofs. And check which ones are correct, which ones follow the rules and are accepted as valid. That way in principle one can find all theorems. One will find everything that can be proven from this set of axioms. And if it is consistent and complete, then any question that one wants to settle, eventually one will either find a proof or a proof of the contrary and know that it is false.

This gives a mechanical procedure for deciding whether any assertion is correct or not, can be proven or not. Which means that in a sense one no longer needs ingenuity or inspiration and in principle the subject becomes mechanical.

I am sure you all know that in fact mathematics is not trivial. We know due to the absolutely fundamental work of Gödel and Turing that this cannot be done. One cannot get a consistent and complete axiomatic theory of mathematics, and one cannot get a mechanical procedure for deciding if an arbitrary mathematical assertion is true or false, is provable or not.

4. The halting problem

Gödel originally came up with a very ingenious proof of this, but I think that Turing's approach in some ways is more fundamental and easier to understand. I am talking about the halting problem, Turing's fundamental theorem on the unsolvability of the halting problem, which says that there is no mechanical procedure for deciding if a computer programme will ever halt.

Here, it is important that the programme has all its data inside, that it be self contained. One sets the programme running on a mathematical idealization of a digital computer, There is no time limit, so this is a very ideal mathematical question. One simply asks: Will the programme go on forever, or at some point will it say: *I am finished and halt* ?

What Turing showed is that there is no mechanical procedure for doing this, There is no algorithm or computer programme that will decide this. Gödel's incompleteness theorem follows immediately, because if there is no mechanical procedure for deciding if a programme will ever halt, then there also cannot be a set of axioms to deduce whether a programme will halt or not. If one had it, then that would give one a mechanical procedure by running through all possible proofs. In principle that would work although it would all be incredibly slow.

I do not want to give too many details, but let me outline one way to prove that the halting problem is unsolvable by means of a *reductio ad absurdum*. Let us assume that there is a mechanical procedure for deciding if a programme will ever halt. If there is, one can construct a programme which contains the number N and that programme will look at all programmes up to N bits in size and check for each one whether it halts. It then simulates running the ones that halt, all programmes up to N bits in size, and looks at the output. Let us assume the output is natural numbers. Then what you do is you maximize over all of this, that is, you take the biggest output produced by any programme that halts that has up to N bits in size and let us double the result. I am talking about a programme that does this given N.

However, the programme that I have just described really is only about log N bits long because to know N you need only $\log_2 N$ bits in binary, right? This programme is $\log_2 N$ bits long, but it is producing a result which is much smaller than N. So this programme is producing an output which is least twice as big as its own output which is impossible. Therefore the halting problem is unsolvable. This is an information-theoretic way of proving the unsolvability of the halting problem.

5. The halting probability

So I start withTuring's fundamental result on the unsolvabilty of the halting problem, and to get my result on randomness in mathematics I just change the wording. It is a sort of a mathematical pun. From the unsolvability of the halting problem I go to the randomness of the halting probability.

What is the halting probability? How do I transform Turing's halting problem to get my stronger result, that not only you have undecidability in pure mathematics but you even have complete randomness?

The halting problem is just this idea: instead of asking for a specific programme whether or not it halts in principle given an arbitrary amount of time, one looks at the ensemble of all possible computer programmes. One does this thought experiment using a general-purpose computer, which is in mathematical terms a universal Turing machine. And you have to have a probability associated with each computer programme to talk about what is the probability that a computer programme will halt.

One chooses each bit of the computer programme by tossing a fair coin, an independent toss for each bit, so that an N-bit programme will have probability 2^{-n}. Once you have chosen the probability measure this way and you choose your general-purpose computer (which is a universal Turing machine) this will define a specific halting probability.

This puts in one big bag the question of whether every programme halts. It is all combined into this one number, the halting probability. So it takes all of Turing's problems and combines it into one real number. I call this number Ω. This halting probability Ω is determined once you specify the general-purpose computer, but the choice of computer does not really matter very much.

My Ω is a probability, and therefore it is a real number between 0 and 1, And one could write it in a binary or any other base, but it is particularly convenient to take it in binary. And when one defines this halting probability and writes in in binary, then the question arises, What is the Nth bit of the halting probability?

My claim is that to Turing's assertion that the halting problem is undecidable corresponds my result that the halting probability is random or irreducible mathematical information. In other words, each bit in base-two of this real number Ω is an independent mathematical fact. To know whether that bit is 0 or 1 is an irreducible mathematical fact which cannot be compressed or reduced any further.

The technical way of saying this is to say that the halting probability is algorithmically random, which means that to get N bits of the real number in binary out of a computer programme one needs at least a programme N bits long. But a simpler way to say it is this: The assertion that the Nth bit of Ω is a 0 or a 1 for a particular N, to know which way each of the bits goes, is an irreducible independent mathematical fact, a random mathematical fact, that looks like tossing a coin.

6. Arithmetization

Now you will of course immediately say: *This is not the kind of mathematical assertion that I normally encounter in pure mathematics*. What one would like of course is to translate it into number theory, the bedrock of mathematics. And Gödel had the same problem. When he originally constructed his unprovable true assertion it was bizarre. It said *I am unprovable*. That is not the kind of mathematical assertion that one normally considers as a working mathematician. Gödel devoted a lot of ingenuity, some very clever, brilliant and dense mathematics, to making *I am unprovable* into an assertion about whole numbers. This includes the trick of Gödel numbering and a lot of number theory.

There has been a lot of work deriving from that original work of Gödel's. It was ultimately used to show that Hilbert's tenth problem is unsolvable. A number of people worked on that. I can take advantage of all that work that had been done over the past 60 years. There is a particularly dramatic development, the work of Jones and Matijasevic which was published about five years ago.

They discovered that the whole subject is really easy which is surprising because it had been very intricate and messy. They discovered that there was a theorem proved by Edouard Lucas a hundred years ago, a very simple theorem that does the whole job if one knows how to use it properly, as Jones and Matijasevic showed how to do. So one needs very little number theory to convert the assertion about Ω that I talked about into an assertion about whole numbers, an arithmetic assertion. Let me just state this result of Lucas because it is delightful and surprisingly powerful. That was of course the achievement of Jones and Matijasevic to realize this.

The hundred-year old theorem of Lucas has to do with when a binomial coefficient is even and when it is odd. If one asks what is the coefficient of X^K in the expansion of $(1 + X)^N$ or in other words what is the Kth binomial coefficient of order N, the answer is that it is odd if and only if K implies N on a bit by bit basis considered as bit strings.

In other words, to know if a binomial coefficient C_N^K (N choose K) is odd, you look at each bit in K that's on and check if the corresponding bit in N is also on. If that is always the case on a bit by bit basis, then, and only then, the binomial coefficient will be odd. Otherwise it will be even. This is a remarkable fact, and it turns out to be all the number theory one really needs to know, amazingly enough.

7. Randomness in arithmetic

So what is the result of using this technique of Jones and Matijasevic based on this remarkable theorem of Lucas? The result of this is a Diophantine equation. I thought it would be fun to write it down since my assertion that there is randomness in pure mathematics would have more force if I can exhibit it as concretely as possible. So I spent some time and effort on a large computer and wrote down a two-hundred page equation with 17 thousand variables.

This is what is called an exponential Diophantine equation. It involves only whole numbers, 0, 1, 2, … the natural numbers [how about the 0?]. All the variables and constants are non-negative integers. It is called exponential Diophantine, exponential because in addition to addition and multiplication one allows also exponentiation, an integer raised to an integer power. That is also allowed in normal polynomial Diophantine equations but the power has to be a constant. Here the power can be a variable. So in addition to seeing X^3 one can also see X^Y in this equation.

So it is a single equation with 17,000 variables and everything is considered to be non-negative integers, unsigned whole numbers. And this equation of mine has a single parameter, the variable N. For any particular value of this parameter I ask: *Does this equation have a finite number of whole-number solutions or does this equation have an infinite number of solutions*?

The answer to this question is my random arithmetic fact. It turns out to correspond to tossing a coin. It *encodes* arithmetically whether the Nth bit of Ω is a 0 or a 1. If it is 0, then this equation for that particular value of N has finitely many solutions. If the Nth bit of the halting probability Ω is a 1, then this equation for that value of the parameter N has an infinite number of solutions.

The change from Hilbert is twofold. Hilbert looked at polynomial Diophantine equations. One was never allowed to raise X to the Y power, only X to the 5th power. Second, Hilbert asked *Is there a solution*? This is undecidable, but not completely random, it only gives a certain amount of randomness. To get complete randomness, like an independent fair coin toss, one needs to ask: I*s there an infinite or a finite number of solutions*? Let me point out by the way that if there are no solutions, that is a finite number of solutions. So it is one way or the other. It either has to be an infinite or a finite number of solutions. The problem is to know which. And my assertion is that we can never know!

In other words, if we want to be able to settle K cases of this question, whether the number of solutions is finite or not for K particular values of the parameter N, that would require that K bits of information be put into the axioms that we use in our formal axiom system. That is a very strong sense of saying that these are irreducible mathematical facts. I think it is fair to say whether the number of solutions is finite or infinite can therefore be considered a random mathematical or arithmetical fact. To recapitulate, Hilbert's tenth problem asks *Is there a solution*? and does not allow exponentiation. I ask; *Is the number of solutions finite*? And I allow exponentiation.

In the sixth problem it is proposed to axiomatize probability theory as part of physics, as part of Hilbert's programme to axiomatize physics but I have found an extreme form of randomness, of irreducibility in pure mathematics, in a part of elementary number theory associated with the name of Diophantos which goes back two thousand years to classical Greek mathematics.

Moreover, my work is an extension of the work of Gödel and Türing which refuted Hilbert's basic assumption that every mathematical truth is black or white, that something is either true or false. It now looks like it is grey, even when you are just thinking about the unsigned whole numbers, the bedrock of mathematics.

8. The philosophy of mathematics

This has been a century of much excitement in the philosophy and in the foundations of mathematics. Part of it was the effort to understand how the calculus (the notion of real number, of limit) could be made rigorous) that goes back even more than a hundred years.

Modern mathematical self-examination really starts, I believe it is fair to say, with Cantor's theory of the infinite and the paradoxes and surprises that it engendered, and with the efforts of people like Peano and Russell and Whitehead to give a firm foundation for mathematics.

Much hope was placed on set theory which seemed very wonderful and promising but it was a pyrrhic victory: set theory does not help! Originally the effort was made to define the whole numbers 1, 2, 3, … in terms of sets to make the whole numbers clearer and more definite.

Note to § 2

In 1850, because of some administrative difficulties, Poisson crea-ted a chair of probability theory and mathematical physics. Indeed, here is the title-page of Poincaré's second edition (1912) of his well-known treatise of 1896:

Cours de la faculté des sciences de Paris // Cours de physique mathématique // Calcul des probabilités

This explains why Hilbert, in his sixth problem, connected physics and probability.

B.Bru (1981), Poisson et l'instruction publique. In *S. D. Poisson. Les mathématique au service de la science*. Palaiseau, 2013,
pp. 63 – 75, see p. 64. Editor Yvette Kosmann-Schwarzbach.

VIII-II

G. J. Chaitin

Randomness and mathematical proof

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Introduction

Although randomness can be precisely defined and even measured, a given number cannot be proved to be random. This enigma establishes a limit to what is possible in mathematics

Almost everyone has an intuitive notion of what a random number is. For example, consider these two series of binary digits
01010101010101010101
01101100110111100010

The first is obviously constructed according to a simple rule. It consists of the number 01 repeated many times. One could predict with considerable confidence that the next two digits would be 0 and 1. The second series yields no such comprehensive pattern. There is no obvious rule governing the formation of the number and there is no rational way to guess the succeeding digits. The sequence appears to be a random assortment of 0's and 1's.

The second series was generated by flipping a coin 20 times and writing a 1 if the outcome was heads, and a 0 if it was tails. Tossing a coin is a classical procedure for producing a random number, and one might think that the provenance of the series alone certifies that it is random. This is wrong. Tossing a coin 20 times can produce any one of 2^{20} binary series each of them with the same probability. It will be no more surprising to obtain a series with an obvious pattern than one that seems random. Each represents an event with probability 2^{-20}. If origin in a probabilistic event is the sole criterion of randomness, both series will be considered random and so would all others since the same mechanism can generate all the possible series. This conclusion is singularly unhelpful and a more sensible definition of randomness is required that does not contradict the intuitive concept of a *patternless* number. Such a definition was devised only in the past 10 years. It depends entirely on the characteristics of the sequence of digits, enables us to describe the properties of a random number more precisely and establishes a hierarchy of degrees of randomness. Even of greater interest are its limitations. It cannot help to determine except in very special cases whether or not a given series of digits is random or only seems random. This limitation is not a flaw in the definition but a consequence of a subtle but fundamental anomaly in the foundation of mathematics. It is closely related to Gödel's incompleteness theorem of 1931. This theorem and recent discoveries help to define the boundaries that constrain certain mathematical methods.

1. Algorithmic definition

The new definition of randomness has its heritage in information theory. Suppose you have a friend who is visiting a planet in another galaxy[1]. He forgot his tables of trigonometric functions and asked you to supply hem. Even the most modest tables of the six (??) functions have a few thousand digits and the cost would be high. Much cheaper is to transmit instructions such as the Euler equation $e^{ix} = \cos x + i\sin x$.

But if your friend likes to know the scores of some baseball games for a few thousand years of his absence you can only transmit the entire list of scores.

So here is the germ of a new definition of randomness. The information embodied in a random series of numbers cannot be compressed. But it is preferable to consider communications with a computer. The friend might infer about numbers or construct a series from partial information or vague instructions but the computer has no such capacities. It needs complete and explicit instructions, a programme, an algorithm, for proceeding step by step without comprehending the result. The algorithm can demand any finite number of mechanical manipulations but not judgements about their meaning.

The definition also requires that we are able to measure the information content of a message in some more precise way than by the cost of sending it. The fundamental unit of information is the *bit*, the smallest item of information capable of indicating a choice between two equally likely things. In binary, one bit is equivalent to one digit, either a 0 or a 1.

We are now able to describe more precisely the differences between the two series of digits presented in the Introduction. The first can be specified to a computer by a very simple algorithm, such as *Print 01 so many times*. If this series is extended according to the same rule, the algorithm will be only slightly larger, for example *Print 01 a million times*[2]. The number of bits in such an algorithm is a small fraction of the number of bits in the series it specifies and as the series grows larger the size of the programme increases at a much slower rate.

For the second series there is no corresponding shortcut. The most economical way to express it is to write it out in full. If the series were much larger (but still apparently patternless) the algorithm would have to be expanded to the corresponding size. This incompressibility is a property of all random numbers. Indeed, we can define randomness in terms of incompressibility. A series of numbers is random if the smallest algorithm capable of specifying it to a computer has about the same number of bits of information as the series itself.

This definition was independently proposed about 1965 by Kolmogorov and by me, an undergraduate at the City College of the City University of New York. We were then unaware of related proposals of 1960 by Ray J. Solomonoff of the Zator Company in an endeavour to measure the simplicity of scientific theories. During the past decade we and others continued to explore the meaning of randomness, The original formulations were improved and the feasibility of the approach amply confirmed.

2. Model of inductive method

The algorithmic definition of randomness provides a new foundation to the theory of probability. By no means it supersedes classical probability theory which is based on an ensemble of possibilities each of which is assigned a probability. The algorithmic approach rather complements the ensemble method by giving precise meaning to concepts that had been intuitively appealing but could not be formally adopted.

The ensemble theory of probability originated in the 17th century and remains of great practical importance. It is the foundation of statistics and is applied to a wide range of problems in science and engineering. The algorithmic theory also has important implications, but they are primarily theoretical. The area of broadest interest is its amplifications of Gödel's incompleteness theorem. Another application (which preceded the formulation of the theory itself) is in Solomonoff's model of scientific induction.

Solomonoff represented a scientist's observations as a series of binary digits. The scientist seeks to explain these observations through a theory which can be regarded as an algorithm capable of generating the series and extending it, that is, predicting future observations. For any given series of observations there are always several competing theories, and the scientist must choose among them. The model demands that the smallest algorithm, consisting of the fewest bits, be selected. This is the familiar formulation of Occam's razor[3]. Given differing theories of apparently equal merit, the simplest is to be preferred.

Thus in the Solomonoff model a theory that enables one to understand a series of observations is seen as a small computer programme that reproduces the observations and makes predictions about possible future observations. The smallest the programme, the more comprehensive the theory and the greater the degree of understanding. Observations that are random cannot be reproduced by a small programme and therefore cannot be explained by a theory[4]. In addition the future behaviour of a random system cannot be predicted. For random data the most compact way for the scientist to communicate his observations is to publish them in their entirety.

Defining randomness or the simplicity of theories through the capabilities of the computer seems to introduce a spurious element into these essentially abstract notions: the peculiarities of the particular computer machine employed. Different machines communicate through different computer languages and a set of instructions expressed in one of these languages might require more or fewer bits when the instructions are translated into another language, However, the choice of computer matters very little. The problem can be entirely avoided by insisting that the randomness of all numbers be tested on the same machine. Even when different machines are employed, the idiosyncrasies of various languages can readily be compensated for. Suppose someone has a programme written in English and wishes to utilize it with a computer that reads only French. Instead of translating the algorithm itself he could preface the programme with a complete English course written in French. Another mathematician with a French programme and an English machine would follow the opposite procedure[5]. In this way only a fixed number of bits need be added to the programme and that number grows less significant[ly?] as the size of the series specified by the programmes increases. In practice a device called a compiler often ensures ignorance of the differences between languages when one is addressing a computer.

Since the choice of a particular machine is largely irrelevant, we can choose for our calculations an ideal computer with an unlimited storage capacity and unlimited time to complete its calculations. Input to, and output from the machine, are both in the form of binary digits. The machine begins to operate as soon as the programme is given in, and continues until it has finished printing the binary series that is the result. The machine then halts. Unless an error is made in the programme, the computer produces exactly one output for any given programme.

3. Minimal programmes and complexity

Any specified series of numbers can be generated by an infinite number of algorithms. Consider for example the three-digit decimal series 123. It can be produced by an algorithm such as *Subtract 1 from 124 and print the result*, or *subtract 2 from 125 and print …* or an infinity of other similar programmes[6]. The programmes of greatest interest however are the smallest that will yield a given numerical series. The smallest programmes are called minimal, and for a given series there may be only one minimal programme or many.

Any minimal programme is necessarily random, whether or not the series it generates is random. This conclusion is a direct result of the way we defined randomness. Consider programme P, minimal for a series of digits S, Suppose P is not random, then by definition there exists programme T substantially smaller than P that will generate it. Then we can thus produce S: *From T calculate P, then from P calculate S*. This programme is only a few bits longer than T and therefore is substantially shorter than P. Therefore P is not a minimal programme.

The minimal programme is closely related to another fundamental concept in the algorithmic theory of randomness, the concept of complexity. The complexity of a series of digits is the number of bits that must be put into a computing machine to obtain the original series as the output. The complexity is therefore equal to the size in bits of the minimal programmes of the series. And now we can restate our definition of randomness in more rigorous terms. A random series of digits is one whose complexity is approximately equal to its size in bits.

The notion of complexity serves not only to define randomness but also to measure it[7]. Given several series of numbers, n digits each, it is theoretically possible to identify all those of complexity $n-1$, $n-10$, $n-100$ and so forth and thereby to rank the series in decreasing order of randomness. The exact value of complexity below which a series is no longer considered random remains somewhat arbitrary. The value ought to be set low enough for numbers with obviously random properties not to be excluded and high enough for numbers with a conspicuous pattern to be disqualified, but to set a value is to judge what degree of randomness constitutes actual randomness. It is this uncertainty that is reflected in the qualified statement that the complexity of a random series is *approximately* equal to the size of the series.

4. Properties of random numbers

The methods of the algorithmic theory of probability can illuminate many properties of both random and non-random numbers. The frequency distribution of digits in a series, for example, can be shown to have an important influence on the randomness of the series. Simple inspection suggests that a series consisting entirely of either 0's or 1's is far from random, and the algorithmic approach confirms that conclusion. If such a series is *n* digits long, its complexity is approximately equal to the logarithm to the base 2 of *n*. (The exact value depends on the machine language employed.) The series can be produced by a simple algorithm such as *Print 0 n times*, in which virtually all the information needed is contained in the binary numeral for *n*. The size of this number is about $\log_2 n$ bits. Since for even a moderately long series the logarithm of *n* is much smaller than *n* itself, such numbers are of low complexity. Their intuitively perceived pattern is mathematically confirmed.

Another binary series that can be profitably analysed in this way is one where 0's and 1's are present with relative frequencies of 3/4 and 1/4. If the series is of size *n*, it can be demonstrated that its complexity is not greater than 4/5 of *n*. A programme that will produce the series can be written in $4n/5$ bits. This maximum applies regardless of the sequence of the digits, so that no series with such a frequency distribution can be considered very random. It can be proved that in any long random binary series the relative frequencies of 0's and 1's must be very close to 1/2. (In a random decimal series the relative frequency of each digit is of course 1/10.)

Numbers having a non-random frequency distribution are exceptional. Of all the possible *n*-digit binary numbers there is only one, for example, that consists entirely of 0's and only one that is all 1's. All the rest are less orderly and the great majority must, by any reasonable standard, be called random. To choose an arbitrary limit, we can calculate the fraction of all *n*-digit binary numbers that have complexity less than $n - 10$.

There are 2^1 programmes one digit long that might generate an *n*-digit series, 2^2 programmes two digit long that could yield such a series, 2^3 programmes three digits long and so forth, up to the longest programmes permitted within the allowed complexity. Of these there are 2^{n-11}. The sum of this series ($2^1 + 2^2 + \ldots + 2^{n-11}$) is equal to $2^{n-10} - 2$. Hence there are fewer than 2^{n-10} programmes of size less than $n - 10$ and since each of these programmes can specify no more than one series of digits, fewer than 2^{n-10} of the 2^n numbers have a complexity less than $n - 10$. Since $2^{n-10}/2^n = 1/1024$ it follows that of all the *n*-digit binary numbers only about one in a thousand have [has] a complexity less than $n - 10$. Only about one series in a thousand can be compressed into a programme more than 10 digits smaller than itself.

A necessary corollary of this calculation is that more than 999 of every 1000*n*-digit binary numbers have a complexity equal to or greater than $n - 10$. If that degree of complexity can be taken as an appropriate test of randomness, then almost all *n*-digit numbers are in fact random. If a fair coin is tossed *n* times, the probability is greater than 0.999 that the result will be random to this extent[8]. It would therefore seem easy to exhibit a specimen of a long series of random digits, actually, it is impossible.

5. Formal systems

It can readily be shown that a specific series of digits is not random, it is sufficient to find a programme that will generate the series and that is substantially smaller than the series itself. The programme need not be minimal for the series, it need only to be small. To demonstrate that a particular series of digits is random, on the other hand, one must prove that no small programme for calculating it exists.

It is in the realm of mathematical proof that Gödel's incompleteness theorem is such a conspicuous landmark; my version of that theorem predicts that the required proof of randomness cannot be found. Consequences are just as interesting for what they reveal about Gödel's theorem as they are for what they indicate about the nature of random numbers.

Gödel's theorem represents the resolution of a controversy that preoccupied mathematicians during the early years of the 20^{th} century. The question was: What constitutes a valid proof in mathematics and how to recognize such a proof? Hilbert had attempted to resolve the controversy by designing an artificial language in which valid proofs could be found mechanically, without any need for human insight or judgement. Gödel showed that there is no such perfect language.

Hilbert established a finite alphabet of symbols, an unambiguous grammar specifying how a meaningful statement could be formed, a finite list of axioms or initial assumptions and a finite list of rules of inference for deducing theorems from the axioms or from other theorems. Such a language with its rules is called a formal system.

A formal system is defined so precisely that a proof can be evaluated by a recursive procedure involving only simple logical and arithmetical manipulations. In other words, in the formal system there is an algorithm for testing the validity of proofs. Today, although not in Hilbert's time, the algorithm could be executed on a computer and the machine could be asked to *judge* the merits of the proof.

Because of Hilbert's requirement that a formal system have [has] a proof-checking algorithm, it is possible in theory to list one by one all the theorems that can be proved in a particular system. First, one lists in alphabetical order all sequences of symbols one character long and applies the proof-testing algorithm to each of them, thereby finding all theorems (if any) whose proofs consist of a single character.

Then, one tests all the two-character sequences of symbols and so on. In this way all potential proofs can be checked and eventually all theorems can be discovered in the order of the size of their proofs. (This method is of course only theoretical; the procedure is too lengthy to be practical,)

5.1. Unprovable statements. Gödel showed that Hilbert's plan for a completely systematic mathematics cannot be fulfilled. He constructed an assertion about the positive integers in the language of the formal system that is true but that cannot be proved in the system. The formal system, no matter how large or how carefully constructed, cannot encompass all true theorems and is therefore incomplete. Gödel's technique can be applied to virtually any formal system and it therefore demands the surprising and for many discomforting conclusion that there can be no definitive answer to the question *What is a valid proof*?

Gödel's proof of the incompleteness theorem is based on the paradox of Epimenides the Cretan who is said to have averred *All Cretans are liars.* [See *Paradox* by W. V. Quine, *Scient. American,* April 1962. Editor.] The paradox can be rephrased in more general terms as *This statement is false,* an assertion that is true if and only if it is false and that is therefore neither true nor false. Gödel replaced the concept of truth with that of provability and therefore constructed the sentence *This statement is unprovable,* an assertion that in a specific formal system is provable if and only if it is false. Thus either a falsehood is provable, which is forbidden, or a true statement is unprovable, hence the formal system is incomplete.

Gödel then applied a technique that uniquely numbers all statements and proofs in the formal system and thereby converts the sentence *This statement is unprovable* into an assertion about the properties of the positive integers. Because this transformation is possible, the incompleteness theorem applies with equal cogency to all formal systems in which it is possible to deal with the positive integers. [See Gödel's proof by E. Nagel & J. R. Newman, *Scient. American*, June 1956. Editor.]

The intimate association between Gödel's proof and the theory of random numbers can be made plain through another paradox, similar in form to the paradox of Epimenides, It is a variant of the Berry paradox first published in 1908 by Bertrand Russell. It reads: *Find the smallest positive integer which to be specified requires more characters than there is in this sentence.* The sentence has 114 characters (counting spaces between words and the period but not the quotation marks), yet it supposedly specifies an integer that, by definition, requires more than 114 characters to be specified.

As before, to apply the paradox of the incompleteness theorem it is necessary to remove it from the realm of truth to the realm of provability. The phrase *which requires* must be replaced by *which can be proved to require,* it being understood that all statements will be expressed in a particular formal system. In addition, the vague notion of *the number of characters required to specify* an integer can be replaced by the precisely defined concept of complexity which is measured in bits rather than in characters.

The result of these transformations is the following computer programme:

Find a series of binary digits that can be proved to be of a complexity greater than the number of bits in this programme.

The programme tests all possible proofs in the formal system in order of their size until it encounters the first one proving that a specific binary sequence is of a complexity greater than the number of bits in the programme. Then it prints the series it has found and halts. Of course, the paradox in the statement from which the programme was derived has not been eliminated. The programme supposedly calculates a number that no programme its size should be able to calculate.

In fact, the programme finds the first number that it can be proved incapable of finding.

The absurdity of this conclusion merely demonstrates that the programme will never find the number it is designed to look for. In a formal system one cannot prove that a particular series of digits is of a complexity greater than the number of bits in the programme employed to specify the series.

A further generalization can be made about this paradox. It is not the number of bits in the programme itself that is the limiting factor but the number of bits in the formal system as a whole. Hidden in the programme are the axioms and rules of inferences that determine the behaviour of the system and provide the algorithm for testing proofs. The information content of these axioms and rules can be measured and can be designated the complexity of the formal system. The size of the entire programme therefore exceeds the complexity of the formal system by a fixed number of bits c, (The actual value of c depends on the machine language employed.) The theorem proved by the paradox can therefore be stated as follows: In a formal system of complexity n it is impossible to prove that a particular series of binary digits is of complexity greater than $n + c$ where c is a constant that is independent of the particular system employed.

5,2. Limits of formal systems. Since complexity was defined as a measure of randomness, this theorem implies that in a formal system no number can be proved to be random unless the complexity of the number is less than that of the system itself. Because all minimal programmes are random the theorem also implies that a system of greater complexity is required to prove that a programme is minimal for a particular series of digits.

The complexity of the formal system has such an important bearing on the proof of randomness because it is a measure of the amount of information the system contains, hence of the amount of information that can be derived from it. The formal system rests on axioms: fundamental statements that are irreducible in the same sense that a minimal programme is. (If an axiom could be expressed more compactly then the briefer statement will become a new axiom and the old one will become a derived theorem.) The information embodied in the axioms is thus in itself random and can be employed to test the randomness of other data. The randomness of some numbers can therefore be proved, but only if they are smaller than the formal system. Moreover, any formal system is of necessity finite, whereas any series of digits can be made arbitrarily large. Hence there will always be numbers whose randomness cannot be proved.

The endeavour to define and measure randomness has greatly clarified the significance and the implications of Gödel's incompleteness theorem. That theorem can now be seen not as an isolated paradox, but as a natural consequence of the constrains imposed by information theory. In 1946 Hermann Weyl said that the doubt induced by such discoveries as Gödel's theorem had been

A constant drain on the enthusiasm and determination with which I pursued my research work.

From the viewpoint of information theory, however, Gödel's theorem does not appear to give cause for pessimism. Instead, it seems simply to suggest that to progress, mathematicians, like investigators in other sciences, must search for new axioms.

Boxes included in the text of § 5.1
Box No. 1

Russell paradox. *Consider the set of all sets that are not members of themselves. Is this set a member of itself*?

Epimenides paradox. Consider this statement: *This statement is false.* Is this statement true?

Berry paradox. Quoted in the text of § 5.1 itself.

Three paradoxes delimit what can be proved. The first indicated that informal reasoning in mathematics can yield contradictions, and it led to the creation of formal systems. The second attributed to Epimenides was adapted by Gödel to show that even within formal systems there are unprovable true statements. The third leads to the demonstration that a specific number cannot be proved random

Box No 2

The complexity of 01101100110111100010 is greater than 15 bits

The series of digits 1110110011011100010 is random

10100 is a minimal programme for the series 1111111111111111111

Unprovable statements can be shown to be false. Then they cannot be shown to be true. A proof that *This statement is unprovable*

1. Reveals a self-contradiction in a formal system. The assignment of a numerical value to the complexity of a particular number.

2, Requires a proof that no smaller algorithm for generating the number exists. The proof could be supplied only if the formal system itself were more complex than the number. Statements *c* and *d* (?) are subject to the same limitation since the identification of a random number or a minimal programme requires the determination of complexity.

Notes

1. An artificial and unneeded example.
2. Why is the algorithm larger?
3. Occam (ca. 1285 – 1349). Newton and even Maimonidas, in the 12th century, expressed the same opinion (Sheynin 1998, p. 196).
4. The unsolvable problem of dealing with outlying observations is forgotten.
5. I doubt that human translations are unnecessary.
6. For readers inadequately acquainted with computer programmes this is mystery. Why not print 123 directly? Cf. Note 2
7. Some mathematicians question the ranking of randomness. On this notion see Sheynin (2014). Regrettably, Chaitin ignored the Mises frequentist approach. And even Lambert (Sheynin 1971, p. 238) reasoned about randomness. He distinguished local and coherent regularities (and therefore irregularities).
8. How to connect this with the coin-tossing experiment described in the Introduction?

Sheynin O. (1971), Newton and the classical theory of probability. *Arch. hist. ex. sci*., vol. 7, pp. 217 – 243.

--- (1998), Statistical thinking in the Bible and the Talmud. *Annals sci*., vol. 55, pp. 185 – 198.

--- (2014), Randomness and determinism. *Silesian stat. rev.*, No. 12 (18), pp. 57 – 74.

IX

Chaim Sokolin

New Rezume. Date of publication difficult to ascertain
Commentary to a description of the orgy of Muslim violence in Cologne in the first days of 2021

The main demographic trend of this century is the change of the ethnos. We are able to make a justified and sinister inference about it. It means a purposeful replacement of the indigenous ethnos of economically developed civilised countries by the nations of economically passive and culturally backward nations having more vital force, aggressiveness and religious fanaticism.

It cannot be doubted that this trend will not be restricted by demography. It will lead to a complete change of the political, economic, cultural and moral situation in the countries which become the victims of that concentrated expansion. All this happens if the point of no return had been passed, and it certainly was passed.

That trend will surpass in a certain sense the scale and consequences of such previous and current historical events as the downfall of the Greek civilisation, the breakdown of the Roman Empire, the Crusades, the Mongolian invasion of Europe, the Hundred Years War, the Thirty Years War, the Seven Years War, WWI and WWII. The world will change beyond recognition. It is possible to imagine the remote consequences of those changes by studying the current events in the Arab world. But it is still impossible to forecast how remote they will be. Nevertheless, the situation will clear up very soon. Many observers think that it is already quite clear and definite.

My own comment

About six or seven years ago the Deputy Oberbürgermeister of Berlin, a Muslim, declared that Allah had created Germany for everyone (read: for Muslims) but that some Germans did not yet understand it.