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STATISTICS IN SCIENCE

The Foundations of Statistical Methods in Biology, Physics and Economics

Edited by

ROGER COOKE

Department of Mathematics, Delft University of Technology, The Netherlands

and

DOMENICO COSTANTINI

Institute of Statistics, University of Genoa, Italy

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INTRODUCTION

An *inference* may be defined as a passage of thought according to some method. In the theory of knowledge it is customary to distinguish *deductive* and *non-deductive* inferences. Deductive inferences are *truth preserving*, that is, the truth of the premises is preserved in the conclusion. As a result, the conclusion of a deductive inference is already 'contained' in the premises, although we may not *know* this fact until the inference is performed. Standard examples of deductive inferences are taken from logic and mathematics. Non-deductive inferences need not preserve truth, that is, 'thought may pass' from true premises to false conclusions. Such inferences can be *expansive*, or, *ampliative* in the sense that the performances of such inferences actually *increases* our putative knowledge. *Standard* non-deductive inferences do not really exist, but one may think of elementary inductive inferences in which conclusions regarding the future are drawn from knowledge of the past.

Since the body of scientific knowledge is increasing, it is obvious that the method of science must allow non-deductive as well as deductive inferences. Indeed, the explosive growth of science in recent times points to a prominent role for the former. Philosophers of science have long tried to isolate and study the non-deductive inferences in science. The inevitability of such inferences one the one hand, juxtaposed with the poverty of all efforts to identify them, constitutes one of the major cognitive embarrassments of our time.

The reasons for compling a book on a subject as expansive as 'Statistics in Science' can be traced to the conviction that all non-deductive inferences in science are ultimately statistical inferences. In other words, a non-deductive inference in science ultimately reduces to drawing conclusions about the degree to which hypotheses are supported by data. Moreover, this support must be probabilistic in character and cannot be adequately formalized without making use of probability theory.

As is well known, the first modern use of probability was related to problems of gambling and insurance involved with predicting future events. Such predictions are made by stating a probability of occurrence for the events in question, on the basis of certain probabilistic hypotheses (e.g. independence in the case of gambling, known relative frequencies in the case of insurance).

T. Bayes enlarged the statistical methodology by considering the probability of a probability distribution. P. S. Laplace and C. F. Gauss introduced this method in science making use of prior probabilities and evidencs. J. C. Maxwell, L. Boltzmann, R. Galton and K. Pearson, being unable to specify any prior distribution for the problems they were treating, gave up the method envisaged by Bayes and succeeded in adapting the Galilei hypothetical-deductive methods to cases where the hypotheses are statistical in character. The kinetic theory of gas and the 'objectivist' theory of statistical testing are the splendid results of their efforts.

The two dominant streams in statistical methodology, Bayesian and objectivist, cover most of what can be called 'data analysis', i.e. trying to determine what conclusions to draw from data. However, the province of non-deductive inference has become much wider than data analysis. For example, the multivariate techniques originally introduced for data *reduction* are now used as an exploratory tool in genetics, economics and social science. Subjective probability was originally introduced to draw conclusions about statistical hypotheses from data, but is becoming very widely used in the form of *expert opinion* in quantitative risk analysis, policy analysis and mathematical decision support. Probabilistic symmetry originally introduced by B. de Finetti to explain the prominent role of relative frequencies in probabilistic reasoning, has become an important tool in understanding the behavior of elementary particles in quantum physics. In all these cases, 'probabilistic reasoning' has wandered off the reservation of mainstream statistics and has become an integral part of diverse scientific disciplines.

Perhaps the most dramatic example of this is the role of probability itself in quantum mechanics. In appropriating probabilistic notions, quantum theory wrought radical changes both in the formalism and in the interpretation of probability. The extent of these changes is a subject of a long and very rich discussion which is still in progress.

The International Conference on Statistics in Science was not organized in an attempt to bring probabilistic thinking back on the reservation of standard statistical reasoning. On the contrary it is believed that the proliferation of quantitative probabilistic methods in diverse scientific disciplines is salutory and inevitable. However, proliferation makes it difficult to keep track of developments in diverse disciplines, and probalistic thinking runs the risk of becoming fragmented. Those who have experienced the power of analogy in probabilistic reasoning know how high the costs of such fragementation might be. Researchers in scientific disciplines who employ probalistic methods in methodologically innovative must communicate with their brethern in other discipolines.

The conference in Luino was intended to be a first step in this direction. We cannot claim that the contributors from econometrics, game theory, risk analysis, population genetics, biology, and quantum physics embraced each other as long lost family. However, there was a clear sense of common purpose. Everyone *wanted* to know what the others were doing, and everyone came away knowing more than he/she knew before. Moreover, there were certain common themes which emerged repeatedly. One such theme was the notion of symmetry, or exchangeability, which seems to turn up in unlikely places. Another theme was the recurrent need to codify and justify procedures. Indeed, the strongest force driving the fragmentation of our discipline is the pressure to get results within a pre-defined time frame. One does not have the time to reflect on interesting methodological questions whose bearing on the application at hand is secondary. Hence, one takes recourse to ad hoc procedures. These adhockeries accumulate, get canonized by default, and subsequently pose formidable barriers to communication.

The organizers of the conference in Luino hope that these proceedings will give the reader a flavor of the atmosphere of the conference, and will help to establish a unified vision of probabilistic reasoning in science. The Luino Conference was organized by the Societá Italiana di Logica e Filosofia delle Scienze and the Istituto Ludovico Geymonat. It was supported by Acciaiere e Ferriere Vicentine Beltrame S.p.A, Banca Popolare di Luino e di Varese, Comunita Montana Valli del Luinese and Comune di Luino. Moreover the Comune di Luino gave hospitality to the Conference in the Palazzo Verbania thus ensuring a marvelous venue.

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ROGER COOKE

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METHOD, THEORY, AND STATISTICS: THE LESSON OF PHYSICS

INTRODUCTION

The subtitle of my paper — The Lesson of Physics — is an overstatement in several ways: *The* lesson is, of course, no more than the lesson *I* have extracted from the history of physics for myself, and that I would like others to believe as well. Hence, it is only one lesson among several possible lessons, i.e. a view suggested for discussion. Moreover the singular 'the lesson' requires a historical survey of about 200 years of physics and a systematic analysis of its present results. Needless to say, then, that I rely heavily on the work of others, among them several participants of this conference.

When we talk about knowledge, especially scientific knowledge, we imply that the things of which we claim knowledge are as we say they are; and if we say something well-defined, we appear to assume that they are in a certain well-determined way. If, then, something sometimes happens in this way and sometimes in another, as chance would have it, we appear to be deprived of knowledge about that thing. This traditional opposition of chance and knowledge was generally accepted when modern science emerged. There are even pictures confronting two allegoric figures *Fortuna* and *Sapientia*. A frontispiece of a 16th century edition of Petrarch, for example, shows Fortuna on the left, blindfold, holding her wheel and seated on a round, hence unstable seat. Facing her, Sapientia is firmly established on a square and stable throne; not only can she see, she can even become certain of herself, since she is equipped with a mirror that reflects her face.

It is one of the surprising achievements of modern science, if not its most important achievement, that it has brought even the accidental under its control. In the terms of the allegory, it has merged Fortuna and Sapientia into one figure: Scientia Statistica. How has that feat, undreamed of in Antiquity and the Middle Ages, been made possible? One necessary condition has been the discovery of the concept of probability in the second half of the seventeenth century.

Although the concept of probability and modern physics emerged in the same century, they were by no means natural allies. On the con-

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trary, physics soon became the most powerful instantiation of the Sapentia of our allegoric picture. Modern physics, if any science, acquired its fame as the science of the strict law-governed order of nature. Under the Newtonian program of classical mechanics the material world is viewed as matter in motion under the impact of forces that completely determine its course.

How then did physics turn from a bullwark of determinism into a bridgehead of statistical science? This drama, as fits a classical play, has five acts. For brevity let me label them as follows:

1. the statistics of observations,
2. the statistical description of complex systems,
3. the statistical theory of irreversibility,
4. the statistics of elementary processes,
5. quantum statistics.

(A more detailed account of 1. through 4. will be found in: Gigerenzer *et al.*, 1989, Chapter 5.)

The drama turned into a tragedy for the metaphysical religion of determinism. In the first act it still looks as if statistics were marginal and a mere methodological tool; but in the end statistical structures are found at the heart of matter. Of course, the drama is as well known as, say, *King Lear*; but it is not viewed as the defeat of determinism by all beholders. I shall, therefore, review some of its key scenes and try to expose those episodes during which, as it were, the statistical plot thickens.

1. THE STATISTICS OF OBSERVATION

Repeated measurements of what is supposed to be one and the same magnitude as a rule do not agree with each other. How are they to be combined? The question arose early on in the history of modern science, i.e. around 1600 (Eisenhart, 1971). But it took a while — roughly a century and a half — to recognize that it should be solved by investigating the form of the statistical distribution of measured values. The actual problem facing, say, the astronomer was still more complicated, because often the data had to be related to an entire series of interconnected magnitudes, e.g. to the successive positions of a planet along its path. Around 1800 Legendre and Gauss independently found the solution that has since been generally accepted: the method of least squares. It was Gauss (1809) who showed that this method can be justified on the assumptions that ever larger discrepancies between the true and the observed values of a magnitude become ever more improbable, and that the arithmetical mean of the observed values is the most probable estimate of the true value. This argument marks the beginning of scientific statistics in physics, i.e. a statistics guided by theoretical understanding.

Laplace, in 1810, clarified the accidental statistical character of the error curve further by showing that a sum of many independent errors will always be normally distributed (cp. Sheynin, 1977). His result was applied by Hagen (1837) and Bessel (1838), who explained the normal or Gaussian distribution by arguing that each single observable error is itself a superposition of many independent invisible elementary errors — an idea first suggested by Thomas Young in 1819.

Thus it happened that statistics took a firm hold in physics. But it did not seem to threaten the deterministic mechanical ideal, since it was strictly confined to the level of *method*. Provided that the observational errors were not systematic, as they are for instance in the so-called 'personal equation' for the individual observer, they were not taken seriously as natural phenomena. The purpose of statistics was to ascertain the one true and precise value of each magnitude, a value whose existence appeared to be guaranteed by the available theories. Physicists had good reasons to believe that their theories would always be strong enough to tell them what structure they were to expect in their objects. Hence in physics, as opposed to the social sciences, statistics remained auxiliary for quite a while. This changed only when error as the subject matter of the statistical distributions was replaced by something more physical and more substantial. That occurred in 1860 when Maxwell announced that the velocities of molecules in a gas were also distributed according to the law of error. His first paper on the kinetic theory of matter opens the second act of our drama.

2. THE STATISTICAL DESCRIPTION OF COMPLEX SYSTEMS

The basic plot of this second act is very simple. In a nutshell, it runs thus: The mechanical program in combination with the atomistic conception of matter results in a statistical theory. Simple as this plot may be, it is not obvious how one ought to assess its epistemological, or possibly even ontological, impact. It is build into the classical mechanical approach that it serves the deterministic ideal only to the extent that a full and fully determined description of all relevant details of a mechanical system can be attained for a given time. Now, it is obvious that this requirement cannot be fulfilled any more as soon as the atomistic constitution of matter becomes relevant for the explanation of observable phenomena, as it did, for instance, in the case of thermal and chemical phenomena in the second half of the 19th century.

How should we describe the epistemological status of the statistical theory of matter as developed mainly by Maxwell and Boltzmann? Not only are we *incapable* to follow the dynamical history of a single molecule, let alone of all individual molecules, we are also *uninterested* to do so. The atomistic and molecular constitution of matter is assumed in order to explain a range of qualitatively different macroscopic phenomena. Now, the very success of this explanatory program requires that we correlate an enormously large number of different mechanical microstates with each single distinguishable macrostate. In other words, it belongs to the nature of this explanatory theoretical attempt that large sets of alternative microstates must somehow be dealt with simultaneously. And here, these states are alternative precisely in the sense that they do *not* belong to a common dynamical history; on the contrary, only one of them can be actual. In this sense, then, statistical theory is entirely different from dynamical theory. The connection between these two theoretical approaches consists only in the fact that the statistics is a statistics of the dynamical magnitudes as defined in the framework of traditional mechanics.

The additional principles of the statistical theory, however, do not follow from mechanics; nor do they follow from observation. They are independent assumptions whose validity is tested by examining the experimental data that are derivable from them. As a matter of historical record, this is the correct description. At least, it is largely correct up to the present. It is true that since Maxwell's last paper on the kinetic theory of matter (1878) there was the idea and the program of deriving those statistical assumptions from the dynamical history of the single real system, i.e. there was the ergodic hypothesis and later also the impressive and rapidly expanding field of ergodic theory (see e.g. von Plato, 1987). But applications to realistic physical cases are at best in its early beginnings. It seems therefore, appropriate to view the ergodic program as one of a reconciliation between the traditional mechanical picture and the more recent statistical principles, principles however that had long proved their mettle on independent grounds.

What this means to the epistemologist is that we are entitled to consider the following question to be open: Does mechanics provide the adequate picture of macroscopic matter? Or does an adequate account perhaps require additional concepts which only make sense in the context of a more abstract theory, a theory that deliberately ignores a complete description of elementary processes in space and time? Temperature, for instance, is a good candidate for such a concept. And after we have been taught by quantum theory that the microworld resists visualization in classical terms anyhow, the suggestion of a theory violating some of the traditional pictorial requirements need no longer be looked upon as still somehow defective. In other words: it is not clear any more that the basic constitution of matter can be spelled out in terms of a small set of manifest properties, preferably all primary properties in the common philosophical sense of that term. Moreover, at the end of the 19th century, mechanics could not be taken any more to support philosophical determinism, as it clearly could around 1800.

Nevertheless, one might well say, classical mechanics had not (yet) been violated; it had just been complemented by additional assumptions. Indeed, the hero of the drama, mechanical determinism, was still well and alive; his rule remained unbroken in most parts of the scientific empire. The thrid act, however, will show how dangereously this dominion was undermined already then.

3. THE STATISTICAL EXPLANATION OF IRREVERSIBILITY

There are a number of attempts at explaining the pervasive irreversibility of our world: some cosmological, others related to the decay of elementary particles. But the overwhelming majority of irreversible processes of our daily experience, like mixing milk and coffee or preparing a well-tempered bath from hot and cold water, proceed according to the second law of thermodynamics. Although this law had been a major explanandum of the kinetic theory of matter from its beginnings around 1860, it remained its stumbling stone. Maxwell's statistical physics dealt only with equilibrium states; ergodic theory was and is similarly restricted. Ludwig Boltzmann struggled with the mechanical explanation of irreversibility during his whole life. In the end he scored at best a partial success; but — though this is still a matter of ongoing research and debate — it may be claimed that he pointed into the right direction.

In 1872 Boltzmann, on the basis of an ingenious treatment of molecular collisions, succeeded in defining a certain function of the molecular velocities, which he called H and which always decreases with time until it reaches a minimum. Its negative, therefore, could be identified with the macroscopically defined thermodynamic function of entropy. At first Boltzmann believed to have given 'a strict proof' of the entropy law on purely mechanical grounds (1872, p. 345). But well-known objections by his teacher Loschmidt and the mathematician Zermelo showed that such a proof must be faulty. For the laws of mechanics are strictly symmetric with respect to time, so that irreversible phenomena could not possibly be derived from them. In our context, the details of this story cannot and need not be told again. (An excellent account is found in Brush, 1976).

For my present argument it is enough to remember one pivotal feature of Boltzmann's defense (1877): He showed that states with low H-value, i.e. high entropy, can be realized by overwhelmingly many microstates, whereas states with high H-value, or low entropy, are realized by comparatively few microstates. He then interpreted the respective numbers of possible microstates as the probability of the corresponding macrostates. Finally, he argued that a system will always move from less probable to more probable states. This argument is ingenious and persuasive. The trouble is only that it bypasses all dynamical considerations. For, as long as no dynamical connection between successive states is taken into account, there is no less reason to argue that the system *has been* in a more probable state in the past than that it *will be* in a more probable state in the further. Hence, the source of irreversibility must lie elsewhere. Eventually, Boltzmann turned to cosmological speculations.

In view of this result of his life-long effort, we are led to ask how he could ever have obtained his irreversible equation for H, now called the 'Boltzmann equation'. The answer is simple: In his equation the ordinary mechanical magnitudes, positions and velocities, have been replaced by statistical distributions of such magnitudes; only the latter enter into dynamical relationships. In other words, in order to obtain irreversible processes, one needs a dynamical equation that is itself asymmetric with respect to time, and this type of equation required statistical magnitudes rather than individual mechanical magnitudes. Ilya Prigogine and his followers have taken this lesson seriously. Prigogine argues that time-irreversible phenomena demand a completely new conception of dynamics (1984, VII.1, X.1) based on distribution functions as basic magnitudes rather than on positions and momenta (1962, p. 6). It is not for the philosopher to say whether or not Prigogine's program will finally succeed. But this program is certainly a good reason to direct our philosophical curiosity towards the question of what the deeper connection is between the statistical characterization of moving matter on the one hand and the irreversibility of the motion on the other.

A question like this one may sound less strange when we see it against the background of the statistical constitution of matter as discovered in quantum physics, to which I now turn.

4. THE STATISTICS OF ELEMENTARY PROCESSES

Until about 1900, or so one might argue, the use of statistics in physics was motivated either by methodological convenience, or by certain goals of theoretical explanation, or finally by a (possibly temporary) incapability of reconciling mechanics with irreversibility without the use of statistical theory. Thus a purely epistemic interpretation of statistics became very common among physicists. It is indeed supported by the fact that all statistical phenomena discussed so far are mass phenomena, i.e. they appear only in systems consisting of many different parts. It is therefore possible to assume that each part taken in isolation has nothing to do with statistics.

This situation changed fundamentally shortly after 1900 when it became clear that radioactive decay is most naturally explained in terms of disintegration probabilities. Soon after the discovery of radioactivity Pierre and Eve Curie, Rutherford and others showed that the decay rate is not affected by any circumstance outside the instable atoms, especially that it does not depend on the mutual interaction of those atoms. The uniform statistical behaviour of the molecules of a gas had been assumed to result from the innumerable collisions that rapidly and effectively redistribute the mechanical properties of the individual particles. But this explanation was barred in the case of radio-atoms, so that, in 1903, Rutherford and Soddy announced the idea of a constant decay probability per unit of time characterizing *each individual atom.* Soon thereafter the consequences of this assumption were worked out: there must be chance fluctuations of actual decay rates and of the time intervals between two successive distintegrations. All this was very nicely confirmed by experiment.

Thus the combination of the following two circumstances created an entirely new rôle for statistical ideas in physics:

1. the immediate visibility of statistical fluctuations, and
2. the impossibility of explaining them as mass phenomena.

The disintegration probabilities inherent in individual atoms were a new kind of source of statistical phenomena.

Of course, radioactive decay by itself would not have been sufficient to secure, indeed even to recognize, this new source. This recognition emerged only gradually as quantum physics grew, and was only completed with the event of quantum mechanics around 1926. In 1928 also radioactive decay was explicitly integrated into the new theory (Gamow, 1928; Gurney and Condon, 1929). In this theory the discovery that had first been made with respect to radioactivity was generalized to all phenomena on the atomic level: Statistical appearances emerge from the constitution of individual systems or from the nature of individual processes. That means: Although statistics as a *phenomenon* requires by its very concept a sufficiently large number of observable cases, its *cause* is not longer sought in the overall structure of the large assembly of those cases, but in the nature of each and every individual part of the assembly alike. Quantum mechanics has taught us the idea that it may belong to the internal constitution of an elementary physical system to display a statistical pattern of actions or reactions instead of a uniquely determined behaviour under given circumstances.

Many philosophers, perhaps also some scientists, will protest against the realistic interpretation of statistical patterns implicit in my description. But I hope to have at least indicated, if not sufficiently explicated, why this interpretation can hardly be avoided, once two points have been accepted:

a) that quantum mechanics is correct, at least in the relevant respects; and
b) that the elementary systems it deals with, e.g. atoms or nuclei, are as real as stones and stars, though they certainly are different kinds of real things with some rather strange properties.

One of those strange properties belongs in my story, since it has to do with statistics. Its discovery is the fifth and last of act of our drama.

5. QUANTUM STATISTICS

So far we have only considered a completely general feature of quantum mechanics: it unexceptionally characterizes its objects by probabilities, hence by patterns of statistical behaviour. Yet, statistics enters quantum theory in a much more specific and surprising way whenever a system contains two or more particles of the same kind. In the pre-quantum statistics of Maxwell and Boltzmann two microstates are already counted as different when only two particles of the same kind have exchanged their mechanical properties, even though the two states are, of course, *empirically* indistinguishable. In other words, the Maxwell–Boltzmann statistics relies on the mere *conceptual* distinguishability, in a mere analogy to a corresponding macroscopic situation where the identity of the particles might be ascertained by following their continuous paths through space and time. In quantum theory, however, two states that differ only in their labels for like particles are considered to be not only *experimentally* undistinguishable but also *conceptually* the same state. For physical theory, this means that all expectation values of observable magnitudes must be invariant under a permutation of particles of the same kind. A simple (though not the only) way of satisfying this requirement is to restrict the admissible functions that describe the state of the particles to two types: symmetric functions that remain unchanged under the exchange of like particles, and antisymmetric functions that change their sign under such an exchange. Philosophically, quantum statistics means that like particles are *in principle* indistinguishable entities. (Lucid philosophical analyses are contained in van Fraassen, 1984. A unified conceptual framework for quantum statistics is developed in Costantini *et al.*, 1983 and related papers. A helpful recent overview is given by Stöckler, 1987).

The important point about this indistinguishability is, of course, that it has testable empirical consequences. One of the most important consequences in the case of antisymmetry, and historically the root of the discovery of indistinguishability, is Pauli's exclusion principle that — if the completeness of the quantum mechanical description is granted — forbids any two electrons to assume the same state and thereby secures the existence of the periodic system of chemical elements. It was again Pauli (1940) who discovered a fundamental theorem about the statistical features of matter and radiation: Under very general and highly plausible assumptions (e.g. the validity of the special theory of relativity for the microworld), like particles with integer spin cannot be in antisymmetric states, and particles with half-integer spin cannot be in symmetric states. If, in agreement with experience, it is moreover assumed that more complicated symmetry properties do not occur in nature, like particles with integer spin, or bosons, are always in symmetric states, those with half-integer spin, or fermions, are always in antisymmetric states. Intuitively, this means that arbitrarily many bosons can behave in the same way, whereas no two fermions can. In other words, particles with integer spin can cluster; they show one possible type of statistical behaviour: Bose-Einstein statistics. Particles with half-integer spin, however, obey another type of statistics: Fermi-Dirac statistics. Intuitively speaking, they push other particles of their kind out of the state they occupy, of which Pauli's original principle is a special case.

Now, what does Pauli's spin-statistics theorem — or the experimental facts that it explains — mean in our context? The spin of elementary particles is an invariant internal property of any such particle like its mass, its lifetime, its size and internal structure (if it has any), or certain quantum numbers. If then all those properties can rightly be interpreted to give us the real structure of those microentities and if we have good reasons to believe in Pauli's theorem, we will have to include the statistical type of the particles in the real constitution of things. In other words: statistical patterns of behaviour do not just figure as a general feature of the microworld, but more specifically they occur in different variants for different kinds of microparticles. Hence, whoever is inclined to suspect that the general feature is somehow due to our epistemological relationship to small dimensions, will find it much harder to maintain this view with respect to quantum statistics.