

Probability Theory: The Logic of Science

by

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Dedicated to the Memory of Sir Harold Jeffreys,
who saw the truth and preserved it.

PROBABILITY THEORY – THE LOGIC OF SCIENCE

Short Contents

PART A - PRINCIPLES AND ELEMENTARY APPLICATIONS

Chapter 1	Plausible Reasoning
Chapter 2	Quantitative Rules: The Cox Theorems
Chapter 3	Elementary Sampling Theory
Chapter 4	Elementary Hypothesis Testing
Chapter 5	Queer Uses for Probability Theory
Chapter 6	Elementary Parameter Estimation
Chapter 7	The Central Gaussian, or Normal, Distribution
Chapter 8	Sufficiency, Ancillarity, and All That
Chapter 9	Repetitive Experiments: Probability and Frequency
Chapter 10	Physics of “Random Experiments”
Chapter 11	The Entropy Principle
Chapter 12	Ignorance Priors – Transformation Groups
Chapter 13	Decision Theory: Historical Survey
Chapter 14	Simple Applications of Decision Theory
Chapter 15	Paradoxes of Probability Theory
Chapter 16	Orthodox Statistics: Historical Background
Chapter 17	Principles and Pathology of Orthodox Statistics
Chapter 18	The A_p -Distribution and Rule of Succession

PART B – ADVANCED APPLICATIONS

Chapter 19	Physical Measurements
Chapter 20	Regression and Linear Models
Chapter 21	Estimation with Cauchy and t -Distributions
Chapter 22	Time Series Analysis and Autoregressive Models
Chapter 23	Spectrum / Shape Analysis
Chapter 24	Model Comparison and Robustness
Chapter 25	Image Reconstruction
Chapter 26	Marginalization Theory
Chapter 27	Communication Theory
Chapter 28	Optimal Antenna and Filter Design
Chapter 29	Statistical Mechanics
Chapter 30	Maximum Entropy – Matrix Formulation

APPENDICES

Appendix A	Other Approaches to Probability Theory
Appendix B	Formalities and Mathematical Style
Appendix C	Convolutions and Cumulants
Appendix D	Dirichlet Integrals and Generating Functions
Appendix E	The Binomial – Gaussian Hierarchy of Distributions
Appendix F	Fourier Analysis
Appendix G	Infinite Series
Appendix H	Matrix Analysis and Computation
Appendix I	Computer Programs

REFERENCES

PROBABILITY THEORY – THE LOGIC OF SCIENCE

Long Contents

PART A – PRINCIPLES and ELEMENTARY APPLICATIONS

Chapter 1	PLAUSIBLE REASONING	
	Deductive and Plausible Reasoning	101
	Analogies with Physical Theories	103
	The Thinking Computer	104
	Introducing the Robot	105
	Boolean Algebra	106
	Adequate Sets of Operations	108
	The Basic Desiderata	111
	COMMENTS	114
	Common Language vs. Formal Logic	115
	Nitpicking	116
Chapter 2	THE QUANTITATIVE RULES	
	The Product Rule	201
	The Sum Rule	206
	Qualitative Properties	210
	Numerical Values	212
	Notation and Finite Sets Policy	217
	COMMENTS	218
	“Subjective” vs. “Objective”	218
	Gödel’s Theorem	218
	Venn Diagrams	220
	The “Kolmogorov Axioms”	222
Chapter 3	ELEMENTARY SAMPLING THEORY	
	Sampling Without Replacement	301
	Logic Versus Propensity	308
	Reasoning from Less Precise Information	311
	Expectations	313
	Other Forms and Extensions	314
	Probability as a Mathematical Tool	315
	The Binomial Distribution	315
	Sampling With Replacement	318
	Digression: A Sermon on Reality vs. Models	318
	Correction for Correlations	320
	Simplification	326
	COMMENTS	327
	A Look Ahead	328

Chapter 4	ELEMENTARY HYPOTHESIS TESTING	
	Prior Probabilities	401
	Testing Binary Hypotheses with Binary Data	404
	Non-Extensibility Beyond the Binary Case	410
	Multiple Hypothesis Testing	411
	Continuous Probability Distributions (pdf's)	418
	Testing an Infinite Number of Hypotheses	420
	Simple and Compound (or Composite) Hypotheses	424
	COMMENTS	425
	Etymology	425
	What Have We Accomplished?	426
Chapter 5	QUEER USES FOR PROBABILITY THEORY	
	Extrasensory Perception	501
	Mrs. Stewart's Telepathic Powers	502
	Converging and Diverging Views	507
	Visual Perception – Evolution into Bayesianity?	512
	The Discovery of Neptune	513
	Digression on Alternative Hypotheses	514
	Horseracing and Weather Forecasting	518
	Paradoxes of Intuition	521
	Bayesian Jurisprudence	521
	COMMENTS	523
Chapter 6	ELEMENTARY PARAMETER ESTIMATION	
	Inversion of the Urn Distributions	601
	Both N and R Unknown	601
	Uniform Prior	604
	Truncated Uniform Priors	607
	A Concave Prior	609
	The Binomial Monkey Prior	610
	Metamorphosis into Continuous Parameter Estimation	612
	Estimation with a Binomial Sampling Distribution	613
	Digression on Optional Stopping	615
	The Likelihood Principle	616
	Compound Estimation Problems	617
	A Simple Bayesian Estimate: Quantitative Prior Information	618
	From Posterior Distribution to Estimate	621
	Back to the Problem	624
	Effects of Qualitative Prior Information	626
	The Jeffreys Prior	629
	The Point of it All	630
	Interval Estimation	632
	Calculation of Variance	632
	Generalization and Asymptotic Forms	634
	A More Careful Asymptotic Derivation	635
	COMMENTS	636

Chapter 7	THE CENTRAL GAUSSIAN, OR NORMAL DISTRIBUTION	
	The Gravitating Phenomenon	701
	The Herschel–Maxwell Derivation	702
	The Gauss Derivation	703
	Historical Importance of Gauss’ Result	704
	The Landon Derivation	705
	Why the Ubiquitous Use of Gaussian Distributions?	707
	Why the Ubiquitous Success?	709
	The Near–Irrelevance of Sampling Distributions	711
	The Remarkable Efficiency of Information Transfer	712
	Nuisance Parameters as Safety Devices	713
	More General Properties	714
	Convolution of Gaussians	715
	Galton’s Discovery	715
	Population Dynamics and Darwinian Evolution	717
	Resolution of Distributions into Gaussians	719
	The Central Limit Theorem	722
	Accuracy of Computations	723
	COMMENTS	724
	Terminology Again	724
	The Great Inequality of Jupiter and Saturn	726
Chapter 8	SUFFICIENCY, ANCILLARITY, AND ALL THAT	
	Sufficiency	801
	Fisher Sufficiency	803
	Generalized Sufficiency	804
	Examples	
	Sufficiency Plus Nuisance Parameters	
	The Pitman–Koopman Theorem	
	The Likelihood Principle	
	Effect of Nuisance Parameters	
	Use of Ancillary Information	
	Relation to the Likelihood Principle	
	Asymptotic Likelihood: Fisher Information	
	Combining Evidence from Different Sources: Meta–Analysis	
	Pooling the Data	
	Fine–Grained Propositions: Sam’s Broken Thermometer	
	COMMENTS	
	The Fallacy of Sample Re–use	
	A Folk–Theorem	
	Effect of Prior Information	
	Clever Tricks and Gamesmanship	
Chapter 9	REPETITIVE EXPERIMENTS – PROBABILITY AND FREQUENCY	
	Physical Experiments	901
	The Poorly Informed Robot	902
	Induction	905
	Partition Function Algorithms	907
	Relation to Generating Functions	911
	Another Way of Looking At It	912

Probability and Frequency	913
Halley's Mortality Table	915
COMMENTS: The Irrationalists	918
Chapter 10 PHYSICS OF "RANDOM EXPERIMENTS"	
An Interesting Correlation	1001
Historical Background	1002
How to Cheat at Coin and Die Tossing	1003
Experimental Evidence	1006
Bridge Hands	1007
General Random Experiments	1008
Induction Revisited	1010
But What About Quantum Theory?	1011
Mechanics Under the Clouds	1012
More on Coins and Symmetry	1013
Independence of Tosses	1017
The Arrogance of the Uninformed	1019
Chapter 11 DISCRETE PRIOR PROBABILITIES – THE ENTROPY PRINCIPLE	
A New Kind of Prior Information	1101
Minimum $\sum p_i^2$	1103
Entropy: Shannon's Theorem	1104
The Wallis Derivation	1108
An Example	1110
Generalization: A More Rigorous Proof	1111
Formal Properties of Maximum Entropy Distributions	1113
Conceptual Problems: Frequency Correspondence	1120
COMMENTS	1124
Chapter 12 UNINFORMATIVE PRIORS – TRANSFORMATION GROUPS	
Chapter 13 DECISION THEORY – HISTORICAL BACKGROUND	
Inference vs. Decision	1301
Daniel Bernoulli's Suggestion	1302
The Rationale of Insurance	1303
Entropy and Utility	1305
The Honest Weatherman	1305
Reactions to Daniel Bernoulli and Laplace	1306
Wald's Decision Theory	1307
Parameter Estimation for Minimum Loss	1310
Reformulation of the Problem	1312
Effect of Varying Loss Functions	1315
General Decision Theory	1316
COMMENTS	1317
"Objectivity" of Decision Theory	1317
Loss Functions in Human Society	1319
A New Look at the Jeffreys Prior	1320
Decision Theory is not Fundamental	1320
Another Dimension?	1321
Chapter 14 SIMPLE APPLICATIONS OF DECISION THEORY	
Definitions and Preliminaries	1401

Sufficiency and Information	1403
Loss Functions and Criteria of Optimal Performance	1404
A Discrete Example	1406
How Would Our Robot Do It?	1410
Historical Remarks	1411
The Widget Problem	1412
Solution for Stage 2	1414
Solution for Stage 3	1416
Solution for Stage 4	
Chapter 15 PARADOXES OF PROBABILITY THEORY	
How Do Paradoxes Survive and Grow?	1501
Summing a Series the Easy Way	1502
Nonconglomerability	1503
Strong Inconsistency	1505
Finite vs. Countable Additivity	1511
The Borel–Kolmogorov Paradox	1513
The Marginalization Paradox	1516
How to Mass-produce Paradoxes	1517
COMMENTS	1518
Counting Infinite Sets?	1520
The Hausdorff Sphere Paradox	1521
Chapter 16 ORTHODOX STATISTICS – HISTORICAL BACKGROUND	
The Early Problems	1601
Sociology of Orthodox Statistics	1602
Ronald Fisher, Harold Jeffreys, and Jerzy Neyman	1603
Pre-data and Post-data Considerations	1608
The Sampling Distribution for an Estimator	1609
Pro-causal and Anti-Causal Bias	1611
What is Real; the Probability or the Phenomenon?	1613
COMMENTS	1613
Chapter 17 PRINCIPLES AND PATHOLOGY OF ORTHODOX STATISTICS	
Unbiased Estimators	
Confidence Intervals	
Nuisance Parameters	
Ancillary Statistics	
Significance Tests	
The Weather in Central Park	
More Communication Difficulties	
How Can This Be?	
Probability Theory is Different	
COMMENTS	
Gamesmanship	
What Does ‘Bayesian’ Mean?	
Chapter 18 THE A_P-DISTRIBUTION AND RULE OF SUCCESSION	
Memory Storage for Old Robots	1801
Relevance	1803
A Surprising Consequence	1804
An Application	1806

Laplace's Rule of Succession	1808
Jeffreys' Objection	1810
Bass or Carp?	1811
So Where Does This Leave The Rule?	1811
Generalization	1812
Confirmation and Weight of Evidence	1815
Carnap's Inductive Methods	1817

PART B - ADVANCED APPLICATIONS

Chapter 19	PHYSICAL MEASUREMENTS	
	Reduction of Equations of Condition	1901
	Reformulation as a Decision Problem	1903
	Sermon on Gaussian Error Distributions	1904
	The Underdetermined Case: K is Singular	1906
	The Overdetermined Case: K Can be Made Nonsingular	1906
	Numerical Evaluation of the Result	1907
	Accuracy of the Estimates	1909
	COMMENTS: a Paradox	1910
Chapter 20	REGRESSION AND LINEAR MODELS	
Chapter 21	ESTIMATION WITH CAUCHY AND t-DISTRIBUTIONS	
Chapter 22	TIME SERIES ANALYSIS AND AUTOREGRESSIVE MODELS	
Chapter 23	SPECTRUM / SHAPE ANALYSIS	
Chapter 24	MODEL COMPARISON AND ROBUSTNESS	
	The Bayesian Basis of it All	2401
	The Occam Factors	2402
Chapter 25	MARGINALIZATION THEORY	
Chapter 26	IMAGE RECONSTRUCTION	
Chapter 27	COMMUNICATION THEORY	
	Origins of the Theory	2701
	The Noiseless Channel	2702
	The Information Source	2706
	Does the English Language Have Statistical Properties?	2708
	Optimum Encoding: Letter Frequencies Known	2709
	Better Encoding from Knowledge of Digram Frequencies	2712
	Relation to a Stochastic Model	2715
	The Noisy Channel	2718
	Fixing a Noisy Channel: the Checksum Algorithm	2718
Chapter 28	OPTIMAL ANTENNA AND FILTER DESIGN	
Chapter 29	STATISTICAL MECHANICS	
Chapter 30	CONCLUSIONS	

APPENDICES

Appendix A	Other Approaches to Probability Theory	
	The Kolmogorov System of Probability	A 1
	The de Finetti System of Probability	A 5
	Comparative Probability	A 6

Holdouts Against Comparability	A 7
Speculations About Lattice Theories	A 8
Appendix B Formalities and Mathematical Style	
Notation and Logical Hierarchy	B 1
Our “Cautious Approach” Policy	B 3
Willy Feller on Measure Theory	B 3
Kronecker vs. Weierstrasz	B 5
What is a Legitimate Mathematical Function?	B 6
Nondifferentiable Functions	B 8
What am I Supposed to Publish?	B 10
Mathematical Courtesy	B 11
Appendix C Convolutions and Cumulants	
Relation of Cumulants and Moments	C 4
Examples	C 5
Appendix D Dirichlet Integrals and Generating Functions	
Appendix E The Binomial – Gaussian Hierarchy of Distributions	
Appendix F Fourier Theory	
Appendix G Infinite Series	
Appendix H Matrix Analysis and Computation	
Appendix I Computer Programs	
REFERENCES	
NAME INDEX	
SUBJECT INDEX	

PREFACE

The following material is addressed to readers who are already familiar with applied mathematics at the advanced undergraduate level or preferably higher; and with some field, such as physics, chemistry, biology, geology, medicine, economics, sociology, engineering, operations research, *etc.*, where inference is needed.[†] A previous acquaintance with probability and statistics is not necessary; indeed, a certain amount of innocence in this area may be desirable, because there will be less to unlearn.

We are concerned with probability theory and all of its conventional mathematics, but now viewed in a wider context than that of the standard textbooks. Every Chapter after the first has “new” (*i.e.*, not previously published) results that we think will be found interesting and useful. Many of our applications lie outside the scope of conventional probability theory as currently taught. But we think that the results will speak for themselves, and that something like the theory expounded here will become the conventional probability theory of the future.

History: The present form of this work is the result of an evolutionary growth over many years. My interest in probability theory was stimulated first by reading the work of Harold Jeffreys (1939) and realizing that his viewpoint makes all the problems of theoretical physics appear in a very different light. But then in quick succession discovery of the work of R. T. Cox (1946), C. E. Shannon (1948) and G. Pólya (1954) opened up new worlds of thought, whose exploration has occupied my mind for some forty years. In this much larger and permanent world of rational thinking in general, the current problems of theoretical physics appeared as only details of temporary interest.

The actual writing started as notes for a series of lectures given at Stanford University in 1956, expounding the then new and exciting work of George Pólya on “Mathematics and Plausible Reasoning”. He dissected our intuitive “common sense” into a set of elementary qualitative desiderata and showed that mathematicians had been using them all along to guide the early stages of discovery, which necessarily precede the finding of a rigorous proof. The results were much like those of James Bernoulli’s “Art of Conjecture” (1713), developed analytically by Laplace in the late 18th Century; but Pólya thought the resemblance to be only qualitative.

However, Pólya demonstrated this qualitative agreement in such complete, exhaustive detail as to suggest that there must be more to it. Fortunately, the consistency theorems of R. T. Cox were enough to clinch matters; when one added Pólya’s qualitative conditions to them the result was a proof that, if degrees of plausibility are represented by real numbers, then there is a uniquely determined set of quantitative rules for conducting inference. That is, any other rules whose results conflict with them will necessarily violate an elementary – and nearly inescapable – desideratum of rationality or consistency.

But the final result was just the standard rules of probability theory, given already by Bernoulli and Laplace; so why all the fuss? The important new feature was that these rules were now seen as uniquely valid principles of logic in general, making no reference to “chance” or “random variables”; so their range of application is vastly greater than had been supposed in the conventional probability theory that was developed in the early twentieth Century. As a result, the imaginary distinction between “probability theory” and “statistical inference” disappears, and the field achieves not only logical unity and simplicity, but far greater technical power and flexibility in applications.

In the writer’s lectures, the emphasis was therefore on the quantitative formulation of Pólya’s viewpoint, so it could be used for general problems of scientific inference, almost all of which

[†] By “inference” we mean simply: deductive reasoning whenever enough information is at hand to permit it; inductive or plausible reasoning when – as is almost invariably the case in real problems – the necessary information is not available. But if a problem can be solved by deductive reasoning, probability theory is not needed for it; thus our topic is the optimal processing of incomplete information.

arise out of incomplete information rather than “randomness”. Some personal reminiscences about George Pólya and this start of the work are in Chapter 5.

But once the development of applications started, the work of Harold Jeffreys, who had seen so much of it intuitively and seemed to anticipate every problem I would encounter, became again the central focus of attention. My debt to him is only partially indicated by the dedication of this book to his memory. Further comments about his work and its influence on mine are scattered about in several Chapters.

In the years 1957–1970 the lectures were repeated, with steadily increasing content, at many other Universities and research laboratories.[†] In this growth it became clear gradually that the outstanding difficulties of conventional “statistical inference” are easily understood and overcome. But the rules which now took their place were quite subtle conceptually, and it required some deep thinking to see how to apply them correctly. Past difficulties which had led to rejection of Laplace’s work, were seen finally as only misapplications, arising usually from failure to define the problem unambiguously or to appreciate the cogency of seemingly trivial side information, and easy to correct once this is recognized. The various relations between our “extended logic” approach and the usual “random variable” one appear in almost every Chapter, in many different forms.

Eventually, the material grew to far more than could be presented in a short series of lectures, and the work evolved out of the pedagogical phase; with the clearing up of old difficulties accomplished, we found ourselves in possession of a powerful tool for dealing with new problems. Since about 1970 the accretion has continued at the same pace, but fed instead by the research activity of the writer and his colleagues. We hope that the final result has retained enough of its hybrid origins to be usable either as a textbook or as a reference work; indeed, several generations of students have carried away earlier versions of our notes, and in turn taught it to their students.

In view of the above, we repeat the sentence that Charles Darwin wrote in the Introduction to his *Origin of Species*: “I hope that I may be excused for entering on these personal details, as I give them to show that I have not been hasty in coming to a decision.” But it might be thought that work done thirty years ago would be obsolete today. Fortunately, the work of Jeffreys, Pólya and Cox was of a fundamental, timeless character whose truth does not change and whose importance grows with time. Their perception about the nature of inference, which was merely curious thirty years ago, is very important in a half-dozen different areas of science today; and it will be crucially important in all areas 100 years hence.

Foundations: From thirty years of experience with its applications in hundreds of real problems, our views on the foundations of probability theory have evolved into something quite complex, which cannot be described in any such simplistic terms as “pro-this” or “anti-that”. For example our system of probability could hardly, in style, philosophy, and purpose, be more different from that of Kolmogorov. What we consider to be fully half of probability theory as it is needed in current applications – the principles for assigning probabilities by logical analysis of incomplete information – is not present at all in the Kolmogorov system.

Yet when all is said and done we find ourselves, to our own surprise, in agreement with Kolmogorov and in disagreement with his critics, on nearly all technical issues. As noted in Appendix A, each of his axioms turns out to be, for all practical purposes, derivable from the Pólya–Cox desiderata of rationality and consistency. In short, we regard our system of probability as not contradicting Kolmogorov’s; but rather seeking a deeper logical foundation that permits its extension in the directions that are needed for modern applications. In this endeavor, many problems have been solved, and those still unsolved appear where we should naturally expect them: in breaking into new ground.

[†] Some of the material in the early Chapters was issued in 1958 by the Socony–Mobil Oil Company as Number 4 in their series “Colloquium Lectures in Pure and Applied Science”.

As another example, it appears at first glance to everyone that we are in very close agreement with the de Finetti system of probability. Indeed, the writer believed this for some time. Yet when all is said and done we find, to our own surprise, that little more than a loose philosophical agreement remains; on many technical issues we disagree strongly with de Finetti. It appears to us that his way of treating infinite sets has opened up a Pandora's box of useless and unnecessary paradoxes; nonconglomerability and finite additivity are examples discussed in Chapter 15.

Infinite set paradoxing has become a morbid infection that is today spreading in a way that threatens the very life of probability theory, and requires immediate surgical removal. In our system, after this surgery, such paradoxes are avoided automatically; they cannot arise from correct application of our basic rules, because those rules admit only finite sets and infinite sets that arise as well-defined and well-behaved limits of finite sets. The paradoxing was caused by (1) jumping directly into an infinite set without specifying any limiting process to define its properties; and then (2) asking questions whose answers depend on how the limit was approached.

For example, the question: "What is the probability that an integer is even?" can have any answer we please in $(0, 1)$, depending on what limiting process is to define the "set of all integers" (just as a conditionally convergent series can be made to converge to any number we please, depending on the order in which we arrange the terms).

In our view, an infinite set cannot be said to possess any "existence" and mathematical properties at all – at least, in probability theory – until we have specified the limiting process that is to generate it from a finite set. In other words, we sail under the banner of Gauss, Kronecker, and Poincaré rather than Cantor, Hilbert, and Bourbaki. We hope that readers who are shocked by this will study the indictment of Bourbakism by the mathematician Morris Kline (1980), and then bear with us long enough to see the advantages of our approach. Examples appear in almost every Chapter.

Comparisons: For many years there has been controversy over "frequentist" versus "Bayesian" methods of inference, in which the writer has been an outspoken partisan on the Bayesian side. The record of this up to 1981 is given in an earlier book (Jaynes, 1983). In these old works there was a strong tendency, on both sides, to argue on the level of philosophy or ideology. We can now hold ourselves somewhat aloof from this because, thanks to recent work, there is no longer any need to appeal to such arguments. We are now in possession of proven theorems and masses of worked-out numerical examples. As a result, the superiority of Bayesian methods is now a thoroughly demonstrated fact in a hundred different areas. One can argue with a philosophy; it is not so easy to argue with a computer printout, which says to us: "Independently of all your philosophy, here are the facts of actual performance." We point this out in some detail whenever there is a substantial difference in the final results. Thus we continue to argue vigorously for the Bayesian methods; but we ask the reader to note that our arguments now proceed by citing facts rather than proclaiming a philosophical or ideological position.

However, neither the Bayesian nor the frequentist approach is universally applicable, so in the present more general work we take a broader view of things. Our theme is simply: *Probability Theory as Extended Logic*. The "new" perception amounts to the recognition that the mathematical rules of probability theory are not merely rules for calculating frequencies of "random variables"; they are also the unique consistent rules for conducting inference (*i.e.* plausible reasoning) of any kind, and we shall apply them in full generality to that end.

It is true that all "Bayesian" calculations are included automatically as particular cases of our rules; but so are all "frequentist" calculations. Nevertheless, our basic rules are broader than either of these, and in many applications our calculations do not fit into either category.

To explain the situation as we see it presently: The traditional "frequentist" methods which use only sampling distributions are usable and useful in many particularly simple, idealized problems; but they represent the most proscribed special cases of probability theory, because they presuppose

conditions (independent repetitions of a “random experiment” but no relevant prior information) that are hardly ever met in real problems. This approach is quite inadequate for the current needs of science.

In addition, frequentist methods provide no technical means to eliminate nuisance parameters or to take prior information into account, no way even to use all the information in the data when sufficient or ancillary statistics do not exist. Lacking the necessary theoretical principles, they force one to “choose a statistic” from intuition rather than from probability theory, and then to invent *ad hoc* devices (such as unbiased estimators, confidence intervals, tail-area significance tests) not contained in the rules of probability theory. Each of these is usable within a small domain for which it was invented but, as Cox’s theorems guarantee, such arbitrary devices always generate inconsistencies or absurd results when applied to extreme cases; we shall see dozens of examples.

All of these defects are corrected by use of Bayesian methods, which are adequate for what we might call “well-developed” problems of inference. As Harold Jeffreys demonstrated, they have a superb analytical apparatus, able to deal effortlessly with the technical problems on which frequentist methods fail. They determine the optimal estimators and algorithms automatically while taking into account prior information and making proper allowance for nuisance parameters; and they do not break down – but continue to yield reasonable results – in extreme cases. Therefore they enable us to solve problems of far greater complexity than can be discussed at all in frequentist terms. One of our main purposes is to show how all this capability was contained already in the simple product and sum rules of probability theory interpreted as extended logic, with no need for – indeed, no room for – any *ad hoc* devices.

But before Bayesian methods can be used, a problem must be developed beyond the “exploratory phase” to the point where it has enough structure to determine all the needed apparatus (a model, sample space, hypothesis space, prior probabilities, sampling distribution). Almost all scientific problems pass through an initial exploratory phase in which we have need for inference, but the frequentist assumptions are invalid and the Bayesian apparatus is not yet available. Indeed, some of them never evolve out of the exploratory phase. Problems at this level call for more primitive means of assigning probabilities directly out of our incomplete information.

For this purpose, the Principle of Maximum Entropy has at present the clearest theoretical justification and is the most highly developed computationally, with an analytical apparatus as powerful and versatile as the Bayesian one. To apply it we must define a sample space, but do not need any model or sampling distribution. In effect, entropy maximization creates a model for us out of our data, which proves to be optimal by so many different criteria* that it is hard to imagine circumstances where one would not want to use it in a problem where we have a sample space but no model.

Bayesian and maximum entropy methods differ in another respect. Both procedures yield the optimal inferences from the information that went into them, but we may choose a model for Bayesian analysis; this amounts to expressing some prior knowledge – or some working hypothesis – about the phenomenon being observed. Usually such hypotheses extend beyond what is directly observable in the data, and in that sense we might say that Bayesian methods are – or at least may

* These concern efficient information handling; for example, (1) The model created is the simplest one that captures all the information in the constraints (Chapter 11); (2) It is the unique model for which the constraints would have been sufficient statistics (Chapter 8); (3) If viewed as constructing a sampling distribution for subsequent Bayesian inference from new data D , the only property of the measurement errors in D that are used in that subsequent inference are the ones about which that sampling distribution contained some definite prior information (Chapter 7). Thus the formalism automatically takes into account all the information we have, but avoids assuming information that we do not have. This contrasts sharply with orthodox methods, where one does not think in terms of information at all, and in general violates both of these desiderata.

be – speculative. If the extra hypotheses are true, then we expect that the Bayesian results will improve on maximum entropy; if they are false, the Bayesian inferences will likely be worse.

On the other hand, maximum entropy is a nonspeculative procedure, in the sense that it invokes no hypotheses beyond the sample space and the evidence that is in the available data. Thus it predicts only observable facts (functions of future or past observations) rather than values of parameters which may exist only in our imagination. It is just for that reason that maximum entropy is the appropriate (safest) tool when we have very little knowledge beyond the raw data; it protects us against drawing conclusions not warranted by the data. But when the information is extremely vague it may be difficult to define any appropriate sample space, and one may wonder whether still more primitive principles than Maximum Entropy can be found. There is room for much new creative thought here.

For the present, there are many important and highly nontrivial applications where Maximum Entropy is the only tool we need. The planned second volume of this work is to consider them in detail; usually, they require more technical knowledge of the subject-matter area than do the more general applications studied in this volume. All of presently known statistical mechanics, for example, is included in this, as are the highly successful maximum entropy spectrum analysis and image reconstruction algorithms in current use. However, we think that in the future the latter two applications will evolve on into the Bayesian phase, as we become more aware of the appropriate models and hypothesis spaces, which enable us to incorporate more prior information.

Mental Activity: As one would expect already from Pólya's examples, probability theory as extended logic reproduces many aspects of human mental activity, sometimes in surprising and even disturbing detail. In Chapter 5 we find our equations exhibiting the phenomenon of a person who tells the truth and is not believed, even though the disbelievers are reasoning consistently. The theory explains why and under what circumstances this will happen.

The equations also reproduce a more complicated phenomenon, divergence of opinions. One might expect that open discussion of public issues would tend to bring about a general consensus. On the contrary, we observe repeatedly that when some controversial issue has been discussed vigorously for a few years, society becomes polarized into two opposite extreme camps; it is almost impossible to find anyone who retains a moderate view. Probability theory as logic shows how two persons, given the same information, may have their opinions driven in opposite directions by it, and what must be done to avoid this.

In such respects, it is clear that probability theory is telling us something about the way our own minds operate when we form intuitive judgments, of which we may not have been consciously aware. Some may feel uncomfortable at these revelations; others may see in them useful tools for psychological, sociological, or legal research.

What is 'safe'? We are not concerned here only with abstract issues of mathematics and logic. One of the main practical messages of this work is the great effect of prior information on the conclusions that one should draw from a given data set. Currently much discussed issues such as environmental hazards or the toxicity of a food additive, cannot be judged rationally if one looks only at the current data and ignores the prior information that scientists have about the phenomenon. As we demonstrate, this can lead us to greatly overestimate or underestimate the danger.

A common error, when judging the effects of radioactivity or the toxicity of some substance, is to assume a linear response model without threshold (that is, a dose rate below which there is no ill effect). Presumably there is no threshold effect for cumulative poisons like heavy metal ions (mercury, lead), which are eliminated only very slowly if at all. But for virtually every organic substance (such as saccharin or cyclamates), the existence of a finite metabolic rate means that there must exist a finite threshold dose rate, below which the substance is decomposed, eliminated,

or chemically altered so rapidly that it has no ill effects. If this were not true, the human race could never have survived to the present time, in view of all the things we have been eating.

Indeed, every mouthful of food you and I have ever taken contained many billions of kinds of complex molecules whose structure and physiological effects have never been determined – and many millions of which would be toxic or fatal in large doses. We cannot doubt that we are daily ingesting thousands of substances that are far more dangerous than saccharin – but in amounts that are safe, because they are far below the various thresholds of toxicity. There is an obvious resemblance to the process of vaccination, in which an extremely small “microdose” of some potentially dangerous substance causes the body to build up defenses against it, making it harmless. But at present there is hardly any substance except some common drugs, for which we actually know the threshold.

Therefore, the goal of inference in this field should be to estimate not only the slope of the response curve, but *far more importantly*, to decide whether there is evidence for a threshold; and if so, to estimate its magnitude (the “maximum safe dose”). For example, to tell us that a sugar substitute is dangerous in doses a thousand times greater than would ever be encountered in practice, is hardly an argument against using the substitute; indeed, the fact that it is necessary to go to kilodoses in order to detect any ill effects at all, is rather conclusive evidence, not of the danger, but of the *safety*, of a tested substance. A similar overdose of sugar would be far more dangerous, leading not to barely detectable harmful effects, but to sure, immediate death by diabetic coma; yet nobody has proposed to ban the use of sugar in food.

Kilodose effects are irrelevant because we do not take kilodoses; in the case of a sugar substitute the important question is: *What are the threshold doses for toxicity of a sugar substitute and for sugar, compared to the normal doses?* If that of a sugar substitute is higher, then the rational conclusion would be that the substitute is actually safer than sugar, as a food ingredient. To analyze one’s data in terms of a model which does not allow even the possibility of a threshold effect, is to prejudge the issue in a way that can lead to false conclusions however good the data. If we hope to detect any phenomenon, we must use a model that at least allows the possibility that it may exist.

We emphasize this in the Preface because false conclusions of just this kind are now not only causing major economic waste, but also creating unnecessary dangers to public health and safety. Society has only finite resources to deal with such problems, so any effort expended on imaginary dangers means that real dangers are going unattended. Even worse, the error is incorrigible by current data analysis procedures; a false premise built into a model which is never questioned, cannot be removed by any amount of new data. Use of models which correctly represent the prior information that scientists have about the mechanism at work can prevent such folly in the future.

But such considerations are not the only reasons why prior information is essential in inference; the progress of science itself is at stake. To see this, note a corollary to the last paragraph; that new data that we insist on analyzing in terms of old ideas (that is, old models which are not questioned) *cannot lead us out of the old ideas*. However many data we record and analyze, we may just keep repeating the same old errors, and missing the same crucially important things that the experiment was competent to find. That is what ignoring prior information can do to us; no amount of analyzing coin tossing data by a stochastic model could have led us to discovery of Newtonian mechanics, which alone determines those data.

But old data, when seen in the light of new ideas, can give us an entirely new insight into a phenomenon; we have an impressive recent example of this in the Bayesian spectrum analysis of nuclear magnetic resonance data, which enables us to make accurate quantitative determinations of phenomena which were not accessible to observation at all with the previously used data analysis by fourier transforms. When a data set is mutilated (or, to use the common euphemism, ‘filtered’) by processing according to false assumptions, important information in it may be destroyed irreversibly. As some have recognized, this is happening constantly from orthodox methods

of detrending or seasonal adjustment in Econometrics. But old data sets, if preserved un mutilated by old assumptions, may have a new lease on life when our prior information advances.

Style of Presentation: In part A, expounding principles and elementary applications, most Chapters start with several pages of verbal discussion of the nature of the problem. Here we try to explain the constructive ways of looking at it, and the logical pitfalls responsible for past errors. Only then do we turn to the mathematics, solving a few of the problems of the genre to the point where the reader may carry it on by straightforward mathematical generalization. In part B, expounding more advanced applications, we can concentrate from the start on the mathematics.

The writer has learned from much experience that this primary emphasis on the logic of the problem, rather than the mathematics, is necessary in the early stages. For modern students, the mathematics is the easy part; once a problem has been reduced to a definite mathematical exercise, most students can solve it effortlessly and extend it endlessly, without further help from any book or teacher. It is in the conceptual matters (how to make the initial connection between the real-world problem and the abstract mathematics) that they are perplexed and unsure how to proceed.

Recent history demonstrates that anyone foolhardy enough to describe his own work as “rigorous” is headed for a fall. Therefore, we shall claim only that we do not knowingly give erroneous arguments. We are conscious also of writing for a large and varied audience, for most of whom clarity of meaning is more important than “rigor” in the narrow mathematical sense.

There are two more, even stronger reasons for placing our primary emphasis on logic and clarity. Firstly, no argument is stronger than the premises that go into it, and as Harold Jeffreys noted, those who lay the greatest stress on mathematical rigor are just the ones who, lacking a sure sense of the real world, tie their arguments to unrealistic premises and thus destroy their relevance. Jeffreys likened this to trying to strengthen a building by anchoring steel beams into plaster. An argument which makes it clear intuitively *why* a result is correct, is actually more trustworthy and more likely of a permanent place in science, than is one that makes a great overt show of mathematical rigor unaccompanied by understanding.

Secondly, we have to recognize that there are no really trustworthy standards of rigor in a mathematics that has embraced the theory of infinite sets. Morris Kline (1980, p. 351) came close to the Jeffreys simile: “Should one design a bridge using theory involving infinite sets or the axiom of choice? Might not the bridge collapse?” The only real rigor we have today is in the operations of elementary arithmetic on finite sets of finite integers, and our own bridge will be safest from collapse if we keep this in mind.

Of course, it is essential that we follow this “finite sets” policy whenever it matters for our results; but we do not propose to become fanatical about it. In particular, the arts of computation and approximation are on a different level than that of basic principle; and so once a result is derived from strict application of the rules, we allow ourselves to use any convenient analytical methods for evaluation or approximation (such as replacing a sum by an integral) without feeling obliged to show how to generate an uncountable set as the limit of a finite one.

But we impose on ourselves a far stricter adherence to the mathematical rules of probability theory than was ever exhibited in the “orthodox” statistical literature, in which authors repeatedly invoke the aforementioned intuitive *ad hoc* devices to do, arbitrarily and imperfectly, what the rules of probability theory as logic would have done for them uniquely and optimally. It is just this strict adherence that enables us to avoid the artificial paradoxes and contradictions of orthodox statistics, as described in Chapters 15 and 17.

Equally important, this policy often simplifies the computations in two ways: (A) The problem of determining the sampling distribution of a “statistic” is eliminated; the evidence of the data is displayed fully in the likelihood function, which can be written down immediately. (B) One can eliminate nuisance parameters at the beginning of a calculation, thus reducing the dimensionality of a search algorithm. This can mean orders of magnitude reduction in computation over what

would be needed with a least squares or maximum likelihood algorithm. The Bayesian computer programs of Bretthorst (1988) demonstrate these advantages impressively, leading in some cases to major improvements in the ability to extract information from data, over previously used methods. But this has barely scratched the surface of what can be done with sophisticated Bayesian models. We expect a great proliferation of this field in the near future.

A scientist who has learned how to use probability theory directly as extended logic, has a great advantage in power and versatility over one who has learned only a collection of unrelated *ad-hoc* devices. As the complexity of our problems increases, so does this relative advantage. Therefore we think that in the future, workers in all the quantitative sciences will be obliged, as a matter of practical necessity, to use probability theory in the manner expounded here. This trend is already well under way in several fields, ranging from econometrics to astronomy to magnetic resonance spectroscopy; but to make progress in a new area it is necessary to develop a healthy disrespect for tradition and authority, which have retarded progress throughout the 20'th Century.

Finally, some readers should be warned not to look for hidden subtleties of meaning which are not present. We shall, of course, explain and use all the standard technical jargon of probability and statistics – because that is our topic. But although our concern with the nature of logical inference leads us to discuss many of the same issues, our language differs greatly from the stilted jargon of logicians and philosophers. There are no linguistic tricks and there is no “meta-language” gobbledygook; only plain English. We think that this will convey our message clearly enough to anyone who seriously wants to understand it. In any event, we feel sure that no further clarity would be achieved by taking the first few steps down that infinite regress that starts with: “What do you mean by ‘exists’?”

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E. T. Jaynes
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