Wiley Series in Probability and Statistics

Understanding Uncertainty

REVISED EDITION

Dennis V. Lindley

WILEY

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Understanding Uncertainty

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Preface

There are some things that you, the reader of this preface, know to be true, and others that you know to be false; yet, despite this extensive knowledge that you have, there remain many things whose truth or falsity is not known to you. We say that you are *uncertain* about them. You are uncertain, to varying degrees, about everything in the future; much of the past is hidden from you; and there is a lot of the present about which you do not have full information. Uncertainty is everywhere and you cannot escape from it.

Truth and falsity are the subjects of logic, which has a long history going back at least to classical Greece. The object of this book is to tell you about work that has been done in the twentieth century about uncertainty. We now know that uncertainty has to obey three rules and that, once they are understood, uncertainty can be handled with almost as much confidence as ordinary logic. Our aim is to tell you about these rules, to explain to you why they are inevitable, and to help you use them in simple cases. The object is not to make you an expert in uncertainty but merely to equip you with enough skill, so that you can appreciate an uncertain situation sufficiently well to see whether another person, lawyer, politician, scientist, or journalist is talking sense, posing the right questions, and obtaining sound answers. We want you to face up to uncertainty, not hide it away under false concepts, but to understand it and, moreover, to use the recent discoveries so that you can act in the face of uncertainty more sensibly than would have been possible without the skill. This is a book for the layman, for you, for everyone, because all of us are surrounded by uncertainty.

However, there is a difficulty, the rules really need to be written in the language of mathematics and most people have a distaste for mathematics. It would have been possible for the book to have been written entirely in English, or equally in Chinese, but the result would have been cumbersome and, believe me, even harder to understand. The presentation cries out for the use of another language; that of mathematics. For mathematics is essentially another language, rather a queer one, that is

unfamiliar to us. However, you do not, for this book, need to understand this language completely; only a small part of it will be required. It is somewhat like an English speaker needing about six characters from Chinese out of the many thousands that the language uses. This book uses part of the language of mathematics, and this part is explained carefully with, I hope, enough motivation for you to be convinced of its advantages. There is almost no technical use of mathematics, and what there is can be appreciated as easily as ordinary arithmetic.

There is one feature of our uncertain world that may either distress or excite you, I hope the latter, in that it does not always behave like common sense might suggest. The most striking example is Simpson's paradox, in Chapter 8, where a medical treatment appears to be bad for both the men and the women, but good for all of us. We will apply the ideas about uncertainty to the law, to science, to economics, and to politics with sometimes surprising results.

The prologue tells something about how this book came to be written. The final version owes a great deal to José Bernardo, Ian Evett, and Tony O'Hagan who read a draft and made many constructive proposals, almost all of which have been eagerly incorporated. In addition, Jay Kadane read the draft with a keen, critical eye, made valuable suggestions, and persuaded me not to ride too vigorously into fields where I had more passion than sense. The final version is much improved as a result of their kind efforts.

PREFACE TO THE REVISED EDITION

The principal change from the original edition is the inclusion of an additional Chapter 14, describing the impact the ideas of this book have on statistics, betting, and finance. The treatment of one problem (§§12.4 and 12.5) has been enlarged because of developments between the two editions. Efron's dice have been discussed because some readers have queried an important assumption. Minor changes have been made in the interests of clarity, several kindly suggested by Mervyn Stone. I would like to thank my daughter, Rowan, for help with the logistics, without which this new edition would not have been possible; and Steve Quigley at Wiley for persuading me to undertake the revision.

Prologue

Almost all my professional life has been spent in academe as a statistician. In my first appointment in Cambridge, I was required to lecture for six hours each week during half of the year and personally to supervise some students. Admittedly, the preparation of new lecture courses took a lot of time, one occupying the whole of the 4 month summer vacation, but these duties did not constitute a reasonable workload. To fill the gap, one was expected to do exactly what I wanted to do, conduct research. As I moved to become Professor and Head of Department, first in Aberystwyth and then at University College London, other duties, principally administrative, crowded in upon me and there was less time for research. But still it got done, because I wanted it to get done, often in conjunction with good, graduate students.

Research, at least in my case, consists of taking questions that interest one and to which you feel you might, given enough time and effort, be able to find an answer; working on them, producing an answer, which often turns out to be quite different from the form originally anticipated, and publishing the results for others to read. There are many aspects to this creative work but the one to be emphasized here is that the questions I chose to answer were selected by me. There was no superior, as there would have been in industry, posing me problems and expecting answers. There was no deadline to be met. This was freedom of thought in its true sense, requiring little more than a comfortable office, a good library, and, most important of all, time in which to think deeply about what interested you. Good answers produce rewards in promotion and more money but that is not the real motivation, which comes instead from the excitement of the chase, to explore where no one has been before, to think deeply, and to come up with something that is genuinely new. And all this free from the interference of others except those you wish to consult. That is true academic freedom that dictators hate so much.

At least during the first 20 years of my researches, I do not recall ever asking myself or being asked by others, whether what I was doing

was worthwhile. Society paid me a salary that provided a comfortable living for myself and my family, giving me enough time to think and write, yielding appreciation from the few people who bothered to read my answers. I suppose if someone had asked me to justify my salary, I should have mumbled something about the training in statistics I had given to many students and the value of statistics in society. But nobody did ask and my conscience did not bother me; it was the chase that mattered. Later, however, as I began to sit on committees and come into more contact with life outside the university, I did wonder about the relevance to society of the answers I had given to questions I had chosen and, more widely, about the value of statistical ideas and methods produced by others. When I thought about this, the answers were not terribly encouraging, for admittedly the discovery of the harmful effects of smoking was mostly due to statistical analysis, and statisticians had played an important role in the breeding of new plants and animals, but I had had little to do with these activities and few had attempted to use the answers my research had provided, let alone succeeded. It had been a good life for me, but had it been a worthwhile one from the viewpoint of society?

Research, especially in disciplines that use a lot of mathematics, is a young person's game and after early retirement I did little research but began to read more widely and consider problems that had not seriously entered into my comfortable research world. And I made a discovery. There were people out there, like politicians, journalists, financiers, lawyers, and managers, who were, in my opinion, making mistakes; mistakes that could have been avoided had they known the answers to the questions pondered in my ivory tower. In other words, what I had been doing was not just an exercise in pure thought, but appeared to have repercussions in the world that could affect the activities of many people and ultimately all of us. This is a phenomenon that has been observed repeatedly; namely that if people are given the freedom and opportunity to use their reasoning abilities to explore without any application in mind, what is termed pure research, they often come up with results that are applicable. Ivory towers can yield steel and concrete, produce food and shelter. This book is an attempt to explain in terms that motivated, lay persons can understand, some of the discoveries about uncertainty

made in academe, and why they are of importance and value to them, so that they might use the results in their lives. In a sense, it is a justification for a life spent in academe.

The preceding paragraphs are too personal and for clarification it is necessary to say something more about scientific research. Research is carried out by individuals and often the best research is the product of one person thinking deeply on their own. For example, relativity is essentially the result of Einstein's thoughts. Yet, in a sense, the person is irrelevant, for most scientists feel that if he had not discovered relativity, then someone else would; that relativity is somehow "out there" waiting to be revealed, the revelation necessarily being made by human beings but not necessarily by that human being. This may not be true in the arts. For example, if Shakespeare had not written his plays, it would not follow that someone else would have produced equivalent writing. Science is a collective activity, much more so than art, and although some scientists stand out from the rest, the character of science depends to only a very small extent on individuals and what little effect they have disappears over time as their work is absorbed into the work of others. There are two lessons to be learnt from this as far as this book is concerned. First, my contribution to the results described herein is very small and is swamped by the work of others. It is as if I had merely added a brick or two to the whole building. Second, I have not thought it advisable in a book addressed to a general audience to attribute ideas to individuals. Our concern with individual scientists is often misplaced, because it is the collective wisdom that is important. The situation is made worse by the fact that the ideas are often attributed to the wrong individual. The ideas with which this work is usually associated are termed Bayesian, after Thomas Bayes, who had hardly anything to do with them. Generally, there is Stigler's law of eponymy that says that a scientific notion is never attributed to the right person; in particular, the law is not due to Stigler. Some scientists are named in the book because results are universally named after them—Bayes rule, for example, or de Finetti's theorem.

Here is a book about uncertainty, showing how it might be measured and used in your life, especially in decision making and science. It tells the story of great discoveries made in the twentieth

century that merit dispersal outside the narrow community where they were developed. New ideas need new forms of exposition, so after a collection, in Chapter 1, of examples of where uncertainty impinges on our lives, Chapter 2 is concerned with certain stylistic questions, including the thorny subject of mathematics; it is only in Chapter 3 that the discoveries really begin.

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