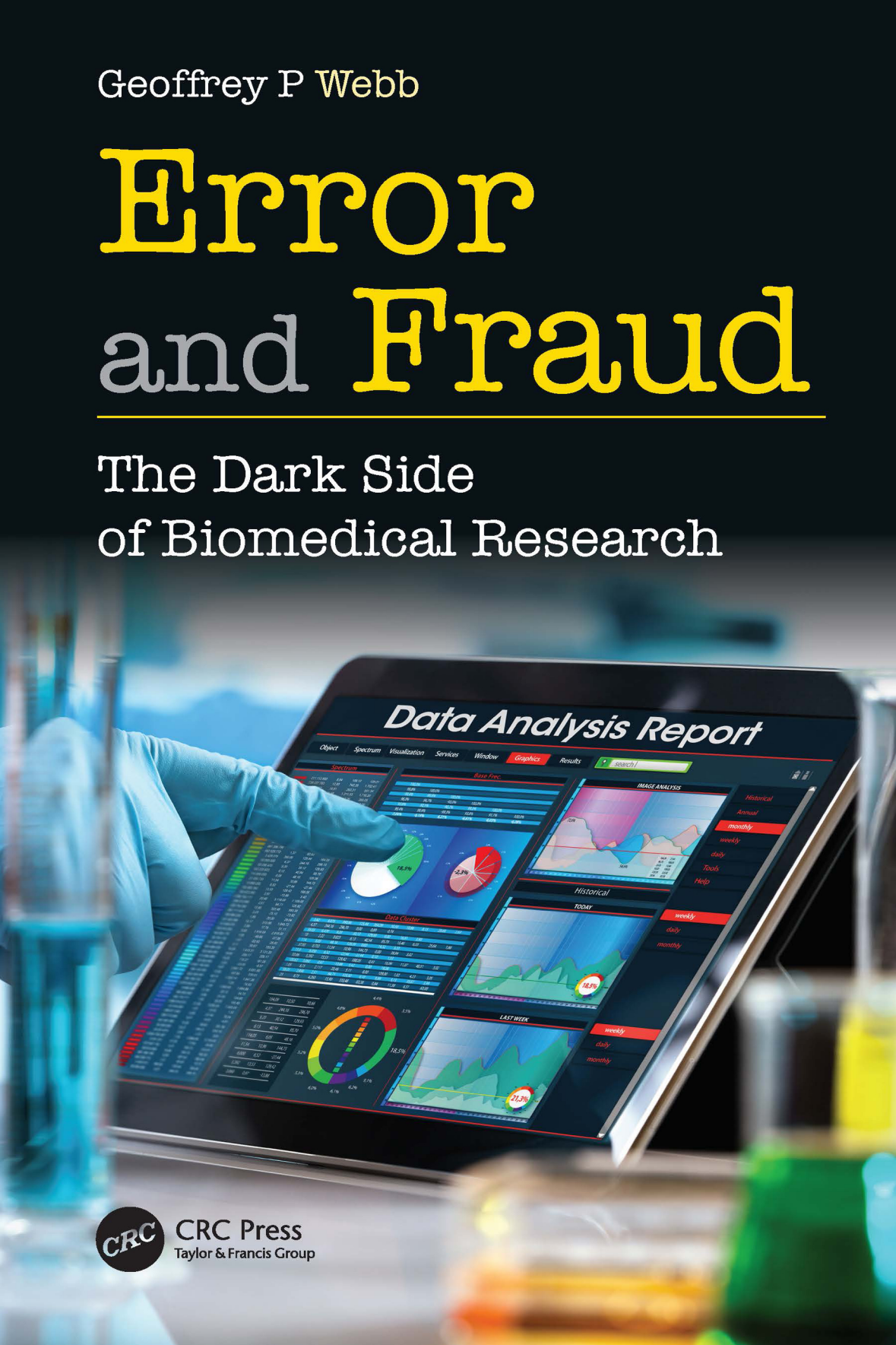
Geoffrey P Webb

Error and Fraud

The Dark Side of Biomedical Research



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The Dark Side of Biomedical Research

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For Kate and Lucy Webb

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Preface

This book has had a very long gestation period and I have probably spent more time researching and writing it than any of my other books, including my 650-page nutrition textbook. I started to make serious efforts towards researching this book in 2011 and I have had an almost complete draft in my files for over 5 years. I have had difficulty in finding a publisher willing to take the risk of publishing it; it seemed to fall into the gap between an academic text and a popular science book that is often perceived to have limited sales potential. I seriously considered resorting to self-publishing so that my efforts were accessible to readers and not wasted. I am therefore particularly grateful to my commissioning editor, Joanna Koster, for persuading Taylor & Francis Group to agree to publish this book. My daughter Kate also helped in the dissemination of my ideas and research by setting up a blog site for me (https://drgeoffnutrition.wordpress.com/) which I have used to post many articles relating to scientific error and research fraud including many detailed accounts of the fraud and error case studies that are summarized in Chapters 2 and 4. This blog also contains many opinion pieces and educational articles. I have cited these blog articles many times in this book as fuller accounts of issues and cases summarized here and as a route to find the many and varied original sources that are too numerous to list here.

The first half of this book is concerned with the flaws and limitations of the research methods used by biomedical scientists and some of the scientific errors that have resulted from misuse or misinterpretation of these methods. I have been writing about research methodology and research errors in my articles and books for over 30 years. My interest in scientific mistakes was first triggered by finding out that the *protein gap* that was so prominent in nutrition teaching and research during the 1950s and 1960s was an illusion (see case study in Chapter 2). The problem of meeting human protein needs was a major topic on my undergraduate course and was a key element of the rationale for my PhD thesis. During my undergraduate and postgraduate studies at Southampton University (1967–1973), my department was heavily involved in work on a potential new protein source known locally as the "Rank mold" being developed by the company then called Rank Hovis McDougal. This research eventually led to the marketing of a mycoprotein preparation as a meat replacement product for affluent Western vegetarians. This research was one of many expensive projects

around the world looking for novel sources of protein-rich food that might help to alleviate the perceived critical shortage of protein supplies for the Third World where most children were considered to be at very high risk of protein deficiency.

After finishing a year's study leave at King's College London in 1987, I started to do some library research into the past overestimation of the protein needs of children that led almost all nutritional scientists to the mistaken belief in a large and rapidly increasing shortfall in world protein supplies. My researches led to publication of a review article about the protein gap in an education journal. This research also left me with an abiding interest in the flaws and limitations of biomedical research methods and their interpretation and ultimately led to the research upon which this book is based and to critiques of these methods in my papers and books, and to accounts of the error case studies discussed in Chapter 2.

Despite writing extensively about research errors and despite having served for about 8 years on the editorial board of the *British Journal of Nutrition (BJN)*, up until around 2010, I had never really thought seriously about professional scientists deliberately fabricating research data. I had heard of the *Piltdown man*, of course but considered it a one-off aberration; the exception that proves the rule. The possibility of data fabrication or falsification was not something that I considered when reviewing papers for the *BJN* and other journals or when choosing sources to rely on for my own books and papers. My eyes were first opened to this possibility when I read that an author that I had cited many times and who was regarded as a leading authority in his field of nutritional immunology, RK Chandra, had been openly accused of publishing fabricated trial data. Then in November 2010, I read in the *Times Higher Education Supplement* that a colleague, Jatinder Ahluwalia, had had a paper retracted from *Nature* and been accused in a report by University College London of multiple acts of research misconduct in generating the data for this paper. I was once again particularly shocked by these allegations because I had cited this *Nature* paper several times in my books and articles. These revelations triggered the start of my research into fraud and misconduct in scientific research. As soon as I started to look for information about research fraud, I found numerous other examples of individuals who had been accused of research fraud, and a large sample of my case studies of these individuals is summarized in Chapter 4. I have always tried to understand and explain the background to each of these cases of fraudulent research and its impact on that research field as well as details of how the perpetrator was exposed and his or (rarely) her subsequent fate. My case studies include botanists, zoologists, psychologists, dentists, paleontologists, geneticists, clinicians, cancer biologists, immunologists and nutritionists. I have thus had to delve into many areas of science that are outside my own areas of expertise; despite the negative stimulus that prompted this research it has been an enjoyable and rewarding experience. I have largely avoided the physical and mathematical sciences in my investigations because I did not think that I would have the background to fully explain the science behind these cases and their impact.

The momentum of my research into scientific fraud was maintained and increased when my daughter chose research fraud as the subject of her MA dissertation. Her work on this dissertation gave me an entry into the wider literature

about research fraud and the efforts being made to detect, quantify and control it. Without the stimulus of working with her for those few months, I doubt whether this book would ever have been completed. Despite being an English graduate who had a very limited science background, she produced a report that I have found scientifically useful and still refer to today.

I have given many invited talks to students and academics about research error and fraud. The reaction of my audiences has convinced me that, despite publicity about recent high-profile cases like Andrew Wakefield and the MMR vaccination, I was not alone in rarely thinking about data fabrication or falsification as a major issue. Many scientists think as I did, that research fraud is a rare aberration that has little impact on scientific progress or clinical practice. In Chapter 5, I have tried to quantify the extent of research misconduct and highlight the sometimes quite disproportionate harm that it can cause. Whilst fraudsters may be uncommon, they are often prolific and prolonged offenders; faking exciting, high-quality data is much quicker than generating it honestly. The faked data of these serial offenders have sometimes done considerable harm and wasted precious research time and resources. In Chapter 3, I similarly try to give an indication of the extent of false and irreproducible conclusions in the scientific literature and the reasons for this frequent lack of reproducibility. The four error case studies discussed in Chapter 2 may represent the tip of a large iceberg, and some have even claimed that the conclusions of most scientific papers are wrong and that most research expenditure is wasted. The pace of scientific progress has been rapid despite this waste of resources, but if the effort and expenditure directed towards unproductive and erroneous research could be reduced, this would amount to a free boost to the resources available for useful research.

One of the major reasons for writing this book and for the numerous articles about error or fraud that have preceded it, is to raise awareness and consideration of these problems by scientists and science students. Awareness and understanding of a problem is a necessary first step in its reduction.

If we spent more time analysing and discussing the causes of past mistakes and how they achieved general acceptance, then perhaps we would be less likely to make similar mistakes in the future. I have suggested that extrapolation of evidence that is towards the bottom of the evidence hierarchy and making premature interventions based upon that evidence has been a common element in several of these past mistakes. If NICE had existed in 1970 and had graded and evaluated the evidence in favour of front sleeping for infants, would it ever have become the norm recommended by most health professionals and baby care writers?

If scientists and particularly journal editors and referees had been more conscious of the possibility that data might have been fabricated or falsified, then might questions about the authenticity of the data of some career fraudsters have been raised sooner and their impact reduced? In several cases, questions and doubts were raised about serial fraudsters long before they were finally exposed but no action was taken. In the final three chapters of the book, I have discussed some of the features of fraudulent data and fraudulent authors that might raise suspicions and allow those suspicions to be tested.

Consideration and discussion of research error and research fraud should start early and be more overtly present in the undergraduate and especially the postgraduate programs of science students. There should be even more emphasis on guiding students towards good practice and ethical behaviour and encouraging them to avoid cutting corners to get another paper under their belt. Students should also be given guidance on how to recognize poor practice and the possibility that the data of others has been fabricated or falsified. There should be more discussion of past mistakes and the reasons why they occurred and why false beliefs flourished for so long. Students should discuss past cases of research fraud and the ways in which fraudulent practice was detected. As I say later in the book:

Today's students are tomorrow's referees and editors.

Prologue

AIMS, ORIGINS AND STRUCTURE OF THIS BOOK

For many years, I have been writing about major scientific mistakes such as the four examples below which are discussed more fully in Chapter 2:

- The mistaken belief that protein deficiency was the most important cause of world malnutrition because of a massive deficit in world protein supply, the so-called *protein gap*.
- The promotion of front sleeping for babies that led to large worldwide increases in cot death rates in the 1970s and 1980s.
- The belief that a defect in the heat-generating capacity of brown fat was a major cause of human obesity and that drugs that stimulated brown fat might be a viable treatment for human obesity.
- The belief that antioxidant supplements, when taken by well-nourished adults, would reduce cancer and heart disease and so increase life expectancy.

A common feature of these errors has been an uncritical and unjustified extrapolation from findings at a low level in the evidence hierarchy. Such as:

- Assuming that an epidemiological association is due to a cause and effect relationship
- Assuming that a favourable change in some biochemical risk marker will inevitably lead to reduced disease risk or increased life expectancy
- Prematurely applying the results from small animal studies to people
- Extrapolating suggested benefits for a small high-risk group to the whole population

In recent years, evidence-grading hierarchies have been developed. Normally changes in clinical practice or health policy should only be made if there is clear supportive evidence at the highest levels of the evidence hierarchy or pyramid. Rigorous application of this system would have prevented most of the practical consequences of these past errors. In Chapter 1, I briefly review the observational and experimental methods available to scientists in the biomedical sciences and

discuss the strengths and limitations of these various lines of enquiry. I also discuss how results from this variety of investigative approaches can be integrated and graded to optimise the chances of making correct scientific, clinical and policy judgements. Meta-analysis has become a very popular technique for trying to get a consensus from similar studies with common outcomes, and meta-analyses of controlled trials are at the top of the evidence hierarchy. Meta-analysis involves a weighted amalgamation of similar studies, so it is prone to distortion by large or multiple fabricated trials; this reinforces the importance of identifying false or fabricated data and removing it from the scientific record.

IS THERE A SYSTEMIC PROBLEM WITH THE SOUNDNESS OF PUBLISHED RESEARCH?

In Chapter 3, I discuss claims that most published research is wrong and that as much as 85% of research expenditure wasted, i.e. that the four error case studies discussed in Chapter 2 are not just isolated cases but are symptomatic of a more general problem with the soundness of much scientific research. I review some of the problems with the design, execution and analysis of scientific studies that may increase the likelihood that their results and conclusions will be unreliable. A major factor undermining confidence in published research is the lack of reproducibility or lack of any attempt to reproduce most of it. Despite this lack of reproducibility, many scientists still have great faith in the traditional belief that incorrect or fraudulent science will be quickly detected when other scientists are unable to reproduce it; the evidence suggests that this faith may not be wholly justified.

The drive to generate research papers has led to an avalanche of research papers that are largely unread. Many of these papers are of low-quality and published in low-quality journals with low or very low thresholds of acceptance. Much of this research seems to have little obvious potential for improving scientific understanding or little chance for improving healthcare or health advice. An improbable claim based upon statistically weak or flawed evidence may generate a succession of similar papers oscillating between supporting and refuting the original claim. For example, weak evidence of an association between eating dairy products such as yogurt and ovarian cancer risk has helped to spawn scores of follow-up papers over several decades that have not advanced our understanding of the causes of ovarian cancer or our ability to make recommendations to reduce it. This is discussed more fully in Chapter 3 where I come to the conclusion that further similar research is unlikely to change that conclusion.

MY PERSONAL JOURNEY FROM ERROR TO FRAUD

Error and fraud may seem like two quite distinct issues:

> *Largely honest production of flawed data or misinterpretation of data to support a false hypothesis.*

As opposed to:

> *Wilful fabrication of data or manipulation of real data to convince others of the correctness of a hypothesis.*

My interest in both error and fraud was sparked by personal involvement. My doctoral research project was part of a programme to develop an alternative fungal protein source that could be produced industrially and so contribute to alleviating the perceived large and increasing shortage of protein for human consumption – the "protein gap". I was shocked when I later discovered that this protein gap had probably never existed. My later research involved the use of genetically obese mice. The notion that a defect in the heat-generating system in brown fat might be the primary cause of obesity in these mice and that defective thermogenesis might be an important cause of human obesity became briefly fashionable at this time (late 1970s/early 1980s). Our observation that mice could lower their body temperature and become torpid when fasted led us to suggest that the well-documented persistent mild hypothermia of these mice and their intolerance to sudden cold exposure was not a failure of thermogenesis but manifestations of an adaptive energy conserving response to perceived starvation. Their genetic defect is now known to leave their brains unable to detect their huge fat stores and so they respond as if in a permanent state of starvation; they respond by entering a permanent semi-torpid state. The defective brown fat theory of human obesity was the result of an incorrect interpretation of research observations in mice and its inappropriate application to people.

I first became conscious of research fraud when I discovered that Ranjit K Chandra, whose publications I had cited in several of my books and papers, had been accused of fabricating his data. I had also cited a *Nature* paper of Jatinder Ahluwalia to support my case against the likely benefits of antioxidant supplements. Ahluwalia had become a colleague by the time news first broke that he had been accused of research fraud and I subsequently became involved in efforts to persuade my employer to take action against him. At around this time, my daughter Kate was taking an MA in publishing at University College, London and because of my frequent discussions (ranting?) about research fraud she chose to write about an aspect of research fraud for her dissertation. It was her research and her discussions with me that helped to convert what had been a general interest with sporadic bouts of reading about individual cases to more systematic research about fraud and its causes and consequences that eventually led to this book. She made me aware of many more cases and showed me that there is a substantial body of academic literature dealing with various aspects of research fraud. She also made me aware of organisations that deal with research fraud like the Office of Research Integrity (ORI) in the USA and the UK Research Integrity Office (UKRIO).

Scientists have often failed to fully consider flaws in the logic and gaps or inconsistencies in the evidence for the theories that generated major scientific mistakes. With hindsight, some of these gaps and flaws now look fairly obvious. The high protein requirements of rapidly growing laboratory animals was used

as evidence for the high protein needs of slow growing human babies despite the low protein content of human milk. The largely speculative benefits of front sleeping were deemed sufficient to justify promoting a change in sleeping position for babies that subsequently led to increased risk of cot death. Analysis of past "mistakes" made me very aware of the fallibility not only of individual scientists but of the scientific establishment in accepting flawed and poorly substantiated theories and translating them into practical health advice or treatment. It was thus no surprise to me that individual scientists who cheat and generate false data may similarly be able to escape detection by the scientific community. Papers based upon fabricated or falsified data may go largely unchallenged and be cited for decades despite glaring flaws or anomalies that, in hindsight, now seem obvious. The focus of the second half of this book is on scientists who try to deceive others by the use of fabricated or improperly manipulated data.

Error and fraud both flourish when there is a collective suspension or suppression of critical evaluation by the rest of the scientific community. Some of the evidence used to support mistaken theories now looks weak or seriously flawed. This makes it difficult to understand how they became so firmly established and sometimes persisted as the accepted *textbook* belief for decades. It is even more alarming that major policy decisions were made and implemented on the basis of these inadequately substantiated theories.

Was there ever enough evidence to justify programmes costing the equivalent of billions of pounds/dollars to develop new or improved protein sources to close the protein gap?

Was there ever any substantial evidence to support recommendations for parents to adopt the front sleeping position for their babies? Maybe the change in sleeping position was casually adopted because of the illogical assumption that even if it did no good then such a simple change in behaviour could not do any real harm, but surely any behaviour change considered worthwhile and significant enough to do some good must also have the potential to do harm.

The history of medicine is littered with well-intentioned practices that have turned out to do more harm than good. The phrase *evidence-based practice* has become the mantra of health professionals and health policy makers, but has there always been proper and critical evaluation of the evidence that underpins current practice? Once a scientific theory or mode of treatment has been widely accepted it becomes accepted as the truth and is presented as such to students in their textbooks and lectures. In Chapter 3, I discuss efforts to challenge many current practices and weed out those that are ineffective or even harmful. Once a theory becomes accepted *fact,* further attempt to question or test the fundamental basis of the theory often ceases. Research may be focused upon aspects of the theory or its application under the assumption that it is proven fact. Sometimes a theory may become so much a part of the research fabric in an area that careers, research grants and even whole research programmes and organisations may be dependent upon continuation of the theory.

For example, so much money, scientific effort and political capital had been invested in measures to increase protein availability that it was very difficult to persuade people to look critically at the rationale that underpinned this effort

and expenditure, i.e. to question the belief that there really was a crisis in world protein supplies. The existence of a protein gap was the raison d'etre of an agency of the United Nations, the Protein Advisory Group. In his watershed 1974 *Lancet* paper questioning the existence of "a protein gap", Donald McLaren suggested that many scientists had privately expressed sympathy with his opposition to the existence of a protein gap and the expensive efforts to close it. He claimed that they were unwilling to support him for fear of damaging their careers and research funding. He even suggested that there were attempts to cynically suppress critical re-evaluation of the protein gap concept.

The antioxidant theory of disease and aging prevention has been seriously undermined in recent years, and it is becoming increasingly difficult to argue that indiscriminate use of antioxidant supplements in basically well-nourished adults will prolong life. This theory has generated tens of thousands of research papers, dozens of books and has been the justification for countless research grants and programmes. Many scientific reputations and careers have been built upon it. This theory has also had considerable wider commercial impact; it has been used to promote the sale and manufacture of many foods, drinks and supplements that are "high in antioxidants". New exotic fruit and vegetables with high antioxidant content have been introduced to the market and promoted on the basis of their antioxidant content; new varieties of existing fruit and vegetables with higher antioxidant levels have also been developed. If it is confirmed that extra antioxidants given to normally nourished people have no long-term benefits, then these products would lose the prestige and marketability they have gained from their high antioxidant activity.

A similar suspension of fundamental critical analysis also characterises many of the examples of deliberate fraud that are discussed in the second part of the book. In Chapter 4, there are summaries of selected case studies of major research fraudsters, some of whom have had decades of success and have been become very influential by using fabricated or falsified data. These cases have been selected in part to act as a reservoir of examples to illustrate points made in the last four chapters. There now seem to be obvious flaws and inconsistencies in their fraudulent data, some of their claims now seem outrageous and unbelievable and the sheer volume and scope of work said to have been done by some fraudulent researchers seems beyond belief.

With the benefit of hindsight, one might question:

- Why some obvious and major statistical flaws in a person's research output were overlooked: impossible standard errors/deviations, regression coefficients that remained constant to 3 decimal places despite large increases in sample size, the same number of people with the same side effect in multiple clinical trials, baseline distributions that could not have occurred if trial subjects were randomly assigned as claimed by the authors.
- How a single individual could generate data and research papers at a phenomenal rate; sometimes publishing as sole authors, papers that would normally involve multiple co-authors and an army of assistants with differing areas of expertise.

- How it was possible for someone to get away with using totally fictitious collaborators on their papers.
- Why data that is completely out of line with the results of other groups remained largely unchallenged for decades.
- How claims to have used impossibly large samples of fictitious subjects with narrow or rare selection criteria went unchallenged.
- How claims to have used drugs, databases or other materials that were not then available were not noticed.
- How someone who cannot be traced and probably does not exist can publish papers or be listed as a co-author.
- How someone can publish studies using many laboratory animals when the institution's animal house has not supplied them.
- How someone can report experiments using radioactive chemicals when there is no record of their use in the legally required log of their use and safe disposal.

Scientists may be trained to criticise the methodology of other scientists and the way they analyse and interpret their data, but they generally trust the honesty of what has been written or said. Referees look for flaws in the design of a study, question whether the correct statistical analyses have been done and whether the interpretation of the results is sound. However, they generally assume that the methods have been honestly described and the data submitted has been generated and analysed in the way described by the authors. They would not expect the authors to be deliberately trying to deceive them. This trust acts as a powerful shield for the deliberate fraudster because referees do not actively look for indications of fraud. Colleagues and managers at the host institution may be so blinded by the reflected glory generated by their star researcher that they do not question how their prolific output was achieved.

In some cases, the revelation that a prominent scientist has been faking data for years comes as an apparent surprise to the scientific community. In other cases, suspicions about a scientist's work may have been present long before the general acceptance of misconduct—suspicions that have been largely ignored or suppressed for years.

After an accusation against Canadian scientist Ranjit Chandra, a Memorial University panel decided in 1994 that he was almost certainly guilty of misconduct:

> With respect to the allegations, the committee is, therefore, led to conclude that scientific misconduct has been committed by Dr Chandra.

This report was not released, and Chandra continued to work at the university until around 2002, when further public accusations of fraud were made. He published prolifically during these intervening years including many influential scientific reviews and keynote lectures at scientific conferences. He became an acknowledged expert about how diet and dietary supplements affect the immune

system, and he was regarded by some as the father of nutritional immunology. Chandra was awarded the Order of Canada, was on the 1995 honour roll of Canadians who *had made a difference* published by Maclean's magazine and was said to have been twice nominated for the Nobel Prize. Several of his major papers have only recently been retracted and his Order of Canada revoked.

In 2012, the Japanese anaesthesiologist Yoshitaka Fujii was proven to have faked at least 172 scientific papers including many clinical trials. Suspicions about the veracity of his work had been first raised in a letter published in April 2000 when it was pointed out that the incidence a side effect appeared to be almost exactly the same in all the groups in his 21 published clinical trials. It was statistically almost impossible that these reported identical incidences could have occurred by chance and some underlying influence must be responsible for this "incredibly nice" data, i.e. a subtle way of suggesting that the results were not genuine. Despite this warning signal he continued to publish prolifically for another decade.

Why were these and other serial fraudsters allowed to pollute the scientific record with fabricated data for so long especially when suspicion preceded their formal unmasking by many years? If flaws in a scientist's work that suggest misconduct are highlighted then how can they carry on obtaining research grants, holding on to senior academic positions and continuing to publish further flawed data? Did colleagues and particularly senior colleagues at the host institution not question how these individuals were able to publish so prolifically without creating a whirlwind of research activity in their laboratories and clinics? Were senior academics and managerial staff reluctant to question the integrity of the data of their star performer who was generating research income and publications to enhance the standing and prestige of their institution? Did they fear that any attempt to investigate the suspected offender would risk scandal and damage their institution's reputation? Were research sponsors happy to accept favourable data even though there might be some suspicion that there had been irregularities in its generation? Were junior colleagues too in awe of the eminent professor to question their integrity or were they concerned that their own career might be blighted by any attempt at whistle blowing? Did the tradition of trust amongst scientists and a failure to consider the possibility of data fabrication help them to remain unexposed for so long?

Do the libel laws in particular so frighten employers, authors, editors and journal owners that they become very reluctant to do anything that could be construed as questioning the integrity of an author? Journal editors may be reluctant to retract papers unless requested to do so by the other authors; until Ranjit Chandra lost a libel case against the Canadian Broadcasting Corporation in 2016, only one of his papers had been retracted even though clear evidence of data fabrication had been around for over two decades and a paper had been retracted for research fraud in 2005.

In Chapter 5, I discuss efforts to estimate the prevalence of research fraud. It is sometimes argued that research fraud is not a major problem because the numbers of people involved are small, but some fraudsters have caused major and lasting harm with their fraudulent publications, and there is discussion of some of the adverse consequences of research fraud; in some cases, research fraud has almost certainly been responsible for increased patient deaths. As noted earlier,

the popular technique of meta-analysis will be distorted by large or multiple fabricated data sets and there is discussion of how data from some prolific fraudsters changes the outcome of meta-analyses.

Chapters 6, 7 and 8 look at the ways in which the scientific literature might be kept relatively free of fraudulent and incorrect research under the three umbrella headings of prevention, detection and disinfection. Peer review is seen as the primary safeguard against publication of poor quality research. Peer review may identify many studies that are poorly designed, poorly executed or inappropriately analysed and interpreted. However, because reviewers usually trust that a paper is an honest account of what was done and what was found, it is relatively ineffective where authors fabricate their findings and deliberately aim to deceive the editor, peer reviewers and journal readers. Some editors have been found to by-pass the peer review process in order to fill their journal and they have accepted deliberately and obviously flawed spoof papers sent to test their peer review process. The growth of open access publishing, where publication fees from authors are the main source of the journal's revenue, may have encouraged some less scrupulous editors to accept almost anything submitted provided that the publication fee is forthcoming. Some notorious fraudsters have published much of their fraudulent work in journals that they edit and have thus able to control or bypass peer review of their own papers.

Chapter 7 is primarily concerned with the detection of fraudulent or incorrect research after publication. As noted earlier there is a traditional belief that fraudulent or incorrect data that gets through the peer review process will be identified when other researchers try to reproduce it. Despite this faith, peer review and lack of reproducibility have rarely been responsible for unmasking high profile cases of research fraud. Most fraudsters have been identified when colleagues have acted as whistle blowers and reported their suspicions about the behaviour of a colleague or collaborator. Other fraudsters have been unmasked by vigilant readers who have spotted signs of data fabrication in published work. Whistle blowers render an important service to the scientific community by helping to uphold scientific integrity and reduce the distracting and distorting effects of fraudulent published data. However, rather than being lauded and rewarded for their service to science, some whistle blowers have suffered as a consequence of their vital service and examples of these harmful impacts upon whistle blowers are highlighted. Some of the characteristics of papers and data that have led readers to suspect that publications contain fraudulent data are also discussed in this chapter.

Chapter 8 deals with the ways in which the total output of a fraudulent researcher should be investigated so that the literature can be disinfected of all their suspect work. In some cases most of the published output of known fraudsters remains in the literature, but in other cases, expensive investigations by employers or professional bodies have led to mass retractions of a fraudster's past papers. The importance of removing fraudulent data from the scientific record has been increased by the increased amalgamation of published data into meta-analyses. Finally there is discussion of some practical measures that might reduce the amount of fraudulent material that reaches and remains in the scientific literature.

WHAT DO I HOPE TO ACHIEVE BY WRITING THIS BOOK?

One of my main motivations for writing about past mistakes was to encourage open discussion of why they had occurred, and how we could learn from them and thus be less likely to make similar mistakes in the future. In my opinion, there has been too little analysis and open discussion of these past mistakes despite their major impacts, e.g. billions of dollars/pounds in wasted research and development projects to close the protein gap and tens of thousands of extra cot deaths caused by the promotion of front sleeping for babies. There seems to be a reluctance of some in the scientific community to acknowledge just how important and damaging these mistakes were and to fully recognise some of the fundamental flaws in the way research was conducted and interpreted which caused them. The first part of the book aims to increase awareness and discussion of past, present, and likely future errors in research. It considers why so much research effort and expenditure is thought to be wasted and why most published papers are unread and/or incorrect in their conclusions.

A code of relative silence and an apparent reluctance to openly address serious problems in scientific research has also characterised the way in which research fraud has sometimes been handled. This book aims to increase awareness of the impact of research fraud, some of its tell-tale signs and characteristics and of the great damage that it can cause. This increased awareness should make life harder for the dishonest researcher, increase the chances of exposure and maybe increase the chances of their being quickly identified and severely penalised for their dishonesty. In addition to the relatively small number of cases of outright fabrication, there is suggestive evidence and considerable suspicion, that selecting or improperly presenting data is more common, e.g. the clustering of reported probabilities just below the 0.05 value taken to indicate significance is one indication of inappropriate data selection or manipulation.

Perhaps all science courses in universities should include direct coverage of research fraud as a curriculum topic. Students could be given analysis of individual cases of proven fraud and made more aware of the possibility of fraud and shown some of the characteristics of fraudulent papers:

> *Today's undergraduates and postgraduate students are tomorrow's journal referees and editors.*

This more open discussion and awareness may make it more likely that other cases will be detected more quickly and dealt with effectively once allegations have been made or suspicion aroused. One recurring comment made after a fraudster has been exposed is that referees, editors, co-authors and employers had not even considered the possibility that they were being deceived and so they unquestioningly accepted the authenticity of data provided by the fraudster; they looked for error and misjudgement but did not consider the possibility of deliberate deception. In a lead article in *The Times Higher Education Supplement* (August 23rd 2012), John Gill, in an appeal for more openness in the way allegations of research fraud are dealt with, wrote:

> *"Sunlight is the best disinfectant, and the UK's institutions and researchers must be fearless in shining a light on misconduct".*