Craig F. Bohren and Eugene E. Clothiaux

Fundamentals of Atmospheric Radiation

An Introduction with 400 Problems

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To my scientific sparring partners
Bill Doyle, Alistair Fraser, and Akhlesh Lakhtakia
Craig F. Bohren

To my father and mother,
they always choose kindness and curiosity over fixed notions;
to my brothers and sisters,
for sharing the wild ride;
to Jessica,
for her enduring patience and relentless support,
and to our sons Daniel and Joshua,
that they grow in the generosity of spirit of their
grandmothers and grandfathers.
Eugene E. Clothiaux

Contents

Preface

Like so many textbooks, this one has its origins in the classroom, the fruit of more than 30 years of combined experience teaching courses on atmospheric radiation to graduate and undergraduate students of meteorology. This experience has forced us to recognize that most of our students do not adequately understand the fundamentals of electromagnetic radiation and its interaction with matter. Students come to the classroom with their heads full of mantras, half-truths, or outright errors, and much of our effort has been devoted to trying to convince them that what they think are universal truths are at best approximations or simply wrong. Indeed, all theories are ultimately wrong. And a theory is just scribbles on paper, not reality. Theories can help make sense of reality but they are not reality itself.

We are careful to expose to the clear light of day all assumptions underlying theories, their limitations and ranges of validity. Nothing is intentionally swept under the rug. Because all theories ultimately break down, you must know what underlies them to have a hope of fixing them when they do. Ignorance is not bliss.

James D. Patterson, a retired physics professor, published An Open Letter to the Next Generation in the July 2004 issue of *Physics Today*. This letter is charming and refreshingly honest. Patterson does not brag about his triumphs but instead warns the next generation about mistakes he made in his career. He notes that, “We have to learn basics first, because we need them for all that follows. If we do not learn the basics, we are disadvantaged. A related sin is skipping essential details. Then we do not get to the bottom of things and are not well grounded.”

Many of the references at the ends of chapters are to original papers. Again, we quote Patterson: “When we want to know something, there is a tendency to seek a quick answer in a textbook. This often works, but we need to get in the habit of looking at original papers. Textbooks are often abbreviated, second- or third-hand distortions of the facts, and they usually do not convey the flavor of scientific research.” We go even further than Patterson and note that whenever you see in a textbook a statement of the form “Einstein [or Newton or some other scientific worthy] said…” replace “said” with “did not say” and what follows is more likely to be true. Even direct quotations are not reliable because so often textbook writers can’t be bothered to go to the library (too far to walk) and so pass on what they think they remember that some other textbook writer thinks Einstein (or whoever) might have said. The only sure way to find out what our predecessors said is to read their own words.

We present theories as a hierarchy, each level of which is more encompassing than its predecessors but each correct subject to stated limitations and approximations. Learn at a certain level secure in the knowledge that what you learn need not be unlearned. To go on to the next level is for you to decide. For example, Chapter 5 introduces multiple scattering by way of a pile of plates, which can be used to illustrate much of the physics of more complicated multiple-scattering media, such as clouds, and also is a way of introducing concepts and terms in more advanced theories. But there is nothing to be unlearned because what we say is true, subject, as always, to the stated limitations of the theory. You can then move on to the two-stream theory from which you can acquire much of the physical intuition you need to understand multiple scattering. If you wish, you can stop at the end of Chapter 5. You will have mastered something complete unto itself but not the final word (there is no final word). You need not feel ashamed for not knowing the supposedly exact (nothing is exact) equation of radiative transfer or how to solve it. There are plenty of folks who can crunch numbers using this equation but don't understand them or lack the ability to estimate them without resorting to extensive calculations (using someone else's data in someone else's computer program). Even a superficial reading of the history of science conveys the lesson that the best scientists have superb intuition. The number crunchers and formalists occupy the lower ranks. This is even true of mathematicians, who are mistakenly looked upon as logic machines. The good ones know in their bellies what is true. Proofs are needed mostly to convince others. Today, many mathematicians make their livings proving or attempting to prove the conjectures (i.e., flashes of mathematical insights) of their illustrious predecessors.

Understanding should come before number-crunching. Our aim is to give you an intuitive feel for the subject matter, a firm grasp of its foundations, and to show how theories help you understand observations and measurements. Again, Patterson's lament is apposite: "I had been more interested in getting good grades than gaining understanding".

Nowhere in this book will you find condescending and insulting statements of the form "it is trivial to show". Nothing is trivial. We had to work hard for every equation, often arguing for days about "trivial" points. The deeper you delve into a subject, the more subtleties you uncover.

It seems that textbooks are almost required by law to be boring, to be carefully purged of all traces of their human authorship. We occasionally break this law. We tell stories. Some may make you laugh. Others may make you mad (and they certainly will make your professors mad). A word of caution: Peter Pilewskie read some of the first drafts of this book, and told us that he had to be careful not to drink anything while reading because while drinking a soda he happened upon a passage that caused him to convulse with laughter and spew soda over himself and his surroundings.

In an ideal world we'd like this book to read like a racy novel. But even if we were capable of writing one, it would no doubt attract the scorn of what Sinclair Lewis in *Arrowsmith* called "Men of Measured Merriment", by whom we do not mean editors. Our experience has been that the blame for dry, lifeless textbooks lies with their authors, not with censorious and humorless editors. There is a strong sentiment within science that it should be a grim grind, that if you enjoy doing it you are not really working. Many years ago the senior author was a visitor at a university that shall remain nameless. At the time he was working long hours, seven days a week. One day, out of the blue, a red-faced professor marched into his office and blurted, "You! You think you work so hard. You don't work hard because you *enjoy* what you are doing." He was serious. This was no joke. The senior author also was attacked on the floor of the United States House of Representatives because of an article in the *National Enquirer* in which he was quoted as saying that he was having great fun doing research on green thunderstorms. It seems that if you take money from the government for doing research you shouldn't enjoy it (or if you do, pretend that it is disagreeable).

We are much more critical of demonstrable nonsense than is the norm, or even permitted, in textbooks. We reckon that there is a statute of limitations for forgiving textbook writers for errors. When books contain statements that have been known to be false for 50 or 100 years, the time has come to heap ridicule on the heads of those who continue to propagate them. For example, there is no excuse, nor has there been for about 100 years, for continuing to say that the refractive index must be greater than 1 or that there is any necessary relationship between density (mass or number) and refractive index.

In the second volume of his *Recuerdos de mi Vida*, the histologist Santiago Ramón y Cajal notes with some acerbity that "In contrast to shameful custom, the child of traditional laziness, my book was to contain, as solemnly promised in the preface, only original illustrations and conclusions drawn from my own investigations." Although we can't promise that all conclusions in this book are drawn from our own investigations, we can promise that our illustrations are original. We did not write with scissors and paste. We made many measurements solely for this book and designed figures intended to convey ideas as clearly as possible. The instrument used for all spectral measurements was a Photo Research SpectraColorimeter Model PR-650 SpectraScan, which measures radiation from 380 nm to 780 nm in increments of 4 nm with a bandwidth of 8 nm.

We hereby declare this book to be an acronym-free zone. To the extent possible we use no acronyms. They are the bane of scientific writing, making it even more boring and arcane than it would be otherwise. The anonymous author of an article in the April 16, 2005 *Economist* comments on the "delight in creating forced acronyms that plagues many branches of science." A plague acronyms indeed are, and claims that they save space are laughable given that acronym-mongers are invariably sloppy writers who could save much more space by writing more compact sentences. But aside from their ugliness, acronyms are just one more way of creating barriers between those who are in the know and those who are not, cabalistic symbols by which the initiated recognize each other. We are waiting to see a paper (maybe it already has been published) entitled "The effect of SSTs on SSTs."

Wherever possible we give the full names of authors of papers and books we cite. Most scientists do have first names, despite efforts to conceal them, and it is rumored that some even have mothers and fathers. We also spell out in full the titles of journals. Cryptic abbreviations, like acronyms, are yet another way of distinguishing between the in-group and the out-group. Do you know what MNRAS stands for? If not, you are a barbarian, not fit to eat at the same table with the lords of the universe.

A book is supposed to be a conversation between authors and readers. The best way to converse with us is to work the problems. There are almost 400. They are not acts of penance but give you the opportunity to test your mastery of the subject matter (memorization of formulas is not mastery) and they expand on topics touched on briefly if at all in the bodies of the chapters. Many of these problems are questions asked by students or correspondents. We enjoyed answering them. And if you don't enjoy solving problems, you might ask yourself why you are studying science. Scientists solve problems. So get to work. And enjoy yourselves (but frown a lot so that no one will know).

Acknowledgments

Acknowledgements are always problematical because much of what one learns comes not from books and papers but from casual conversations, often in convivial surroundings. More than 25 years ago I spent over a year in the Institute of Atmospheric Physics at the University of Arizona. At the end of each day, Sean Twomey and I, sometimes accompanied by Don Huffman and Phil Krider, would march off to the nearest tavern for a few beers. Although some of our conversation centered on horse racing, Sean's great passion, we also discussed science at length. A year spent in a barroom with Sean Twomey is equivalent to a graduate degree in atmospheric science. I do recall that one topic in this book stems from a story he told me. When he worked for the Commonwealth Scientific and Industrial Research Organization in Australia, manuscripts had to be reviewed by scientists in a division other than that from which the manuscripts originated. Sean blasted a manuscript by a radio astronomer who had committed the blunder of assuming that the sum of exponentials is an exponential. The fuming author called Sean and asked him angrily, "What the hell do you know about radio astronomy?" Sean replied, "Nothing, but I do know something about exponentials."

But my memory is becoming less reliable as I make the inevitable descent into senescence. As the story of the exponentials demonstrates I can remember almost the exact date and place where I acquired some pearls of wisdom, while others are lost in a haze. I neatly handled the problem of acknowledging three colleagues, Bill Doyle, Alistair Fraser, and Akhlesh Lakhtakia, by simply dedicating this book to them. Bill and Akhlesh have been and continue to be my sounding boards on electromagnetic theory. Both have an encyclopedic knowledge of the subject, including its history, and are aware of the many subtleties that don't make their way into textbooks. Although Bill is approaching his 80th year he still retains the enthusiasm one hopes to see in college freshmen. We continue to correspond and talk on the telephone, although now he has to call me at night to spare his office mate from having to listen to our raucous conversations. Akhlesh and I have lunch every few months and call each other with questions at all hours of the day and night.

Alistair Fraser made my 20 years at Penn State a rewarding and fruitful experience. Without him I might not have stayed. Much of what I know about atmospheric optics I learned directly from him or honed what I already knew (or thought I knew). Had it not been for Alistair my academic career almost certainly would have been different. Having such a brilliant scholar and inspiring teacher to work with made it almost inevitable that I would join forces with him. I followed in his footsteps by teaching a unique course of his design, meteorological observations, in which students photograph optical phenomena in the atmosphere and write reports on them. This is the one course that indelibly changes students. They are never the same going out as coming in. And the same can be said about the teacher. Our students were often amazed at how severely Alistair and I criticized each other. We had to explain to them that this was the best way of ensuring that our work was of the highest quality. Alistair is quick to spot logical flaws, a merciless critic of sloppy exposition, a superb interpreter of what can be seen with the naked eye.

At Penn State I also had the good fortune to learn from Herschel Leibowitz, one of the most eminent perceptual psychologists, who would teach me at the breakfast table what physicists should know, but usually don't, about how humans construct a visual world out of raw optical data.

Thanks also to Paul Kay for his criticism of our discussion of color words.

More than 30 years ago my first teacher of radiative transfer was Bruce Barkstrom with whom I collaborated on a paper on radiative transfer in snow on the ground. This was a fruitful and enjoyable collaboration that brought me up to speed on much of what I needed to know.

Although Don Huffman did not contribute directly to this book, other than to provide me with a few references, his lasting influence can be felt on everything I do.

For many years I have corresponded with Warren Wiscombe, who fires questions at me every few months, causing me to refine ideas and correct errors. And this even before email made correspondence much easier.

Ray Shaw was a guiding force behind the discussion of nonexponential attenuation in Chapter 2. Thanks also to Joe Shaw for sending me reprints and to Glenn Shaw for siring Ray and Joe.

Tim Kane directed us to references on optical heterodyning.

If computers and their programs can be “user-friendly”, users should have the right to be “computer-unfriendly”. As my colleagues know, I am outright computer-hostile. But I am grateful to Harry Henderson and Chuck Pavolski, who responded speedily and graciously to my anguished and profane cries for help when my computers, no doubt sensing my hostility toward them, rebelled against my authority.

To save Tom Kozo possible embarrassment I won’t say what he contributed, but he knows.

Manfred Wendisch had the most direct effect on this book. We sent him the first versions of most of the chapters, which he went over with a fine-tooth comb, saving us from many errors, causing us to tidy up terminology and tighten our arguments. He also caused us to take more care to make this book understandable to people whose first language is not English.

Peter Pilewskie critically commented on early versions and independently checked some of our at-first puzzling Monte Carlo calculations in Chapter 6. He also generously allowed us to publish some of his measurements, the only ones in this book we did not make.

When I had some tricky (for me) mathematical questions I turned, as usual, to George Greaves, my former climbing partner, companion on many ascents, some hair-raising, in Iceland and Scotland many years ago.

Others who contributed to this book, if only indirectly by way of the residue of mostly forgotten conversations, are Tom Ackerman, Rich Bevilacqua, Ted Staskiewicz, Tim Nevitt, Cliff Dungey, Raymond Lee, Phil Krider, John Olivero, Denny Thomson, Shermila Singham Carl Ribbing, Larry Woolf, Andy Young, Claes Beckman, Günther Können, Ken Sassen, Dick Bartels, and Fred Loxsom.

Because of my popular science books and writings on atmospheric optics, hardly a week goes by that I don’t receive email from someone, somewhere in the world, from senior scientists to elementary school students, asking me questions some of which made their way into this book. To this anonymous army of inquisitive people I am also grateful.

My many students contributed questions, which I tried to answer, and misconceptions, which I tried to dispel. At least half of the problems in this book were taken from examinations and homework problems.

The portable spectrophotometer used for the spectral measurements in this book was purchased through a grant from the National Science Foundation with matching funds from the Penn State University Department of Meteorology.

To date I have written books with three collaborators, with whom I am still friends. So although I am not easy to work with, I am not impossible. Eugene Clothiaux had the hardest row to hoe of all my collaborators. I depended on him for all the heavy work that I am no longer capable of doing. All this while he was struggling up the academic ladder and helping to raise young children. Aside from the intellectual burdens of collaboration, Eugene bore physical burdens that are perhaps unusual. Because I am retired Eugene had to make the trek to my house frequently, carrying books and papers and the latest versions of chapters. His ancient car could not make it all the way up our steep and rutted road (which he calls "the creek bed"), so in all kinds of nasty weather he would park at the house of our neighbors, then trudge up the last quarter-mile, in winter a veritable ice sheet. Now that's dedication!

As usual, my most heartfelt thanks go to Nanette Malott Bohren, my companion of more than 40 years, who had to put up with the mess and stress of yet another book but who carefully pored over draft versions ferreting out logical and typographical errors. Although Nanette has no formal scientific training, she has the amazing ability to spot errors in equations and inconsistent notation.

Craig F. Bohren
Tŷ'n y Coed
Oak Hall, Pennsylvania
July, 2005

My fortune is great in having grown up in the late twentieth century United States. Those Americans living two generations back provided the infrastructure and support that allowed my father to earn a doctorate in physics and my mother a doctorate in math and science education, even though higher education was totally lacking in their families. For a mere $4,000 of my parent's money I was able to study with Jean-Marie Wersinger, George Kozlowski, Charles Brown, Delos McKown and my father at Auburn University as an undergraduate in physics. I was able to parlay this initial investment into a graduate assistantship with Leon Cooper, Mark Bear and Ford Ebner in physics and neural science at Brown University. My luck continued into the 1990s when I received a postdoctoral research fellowship to work with Tom Ackerman, Bruce Albrecht and Denny Thomson at Penn State University. During my years as a research associate and assistant professor at Penn State University, faculty members of the Department of Meteorology were incredibly supportive, to a degree so great that I have dubbed this faculty as King Arthur's Court.

The field of atmospheric radiation is full of feisty, but kind, characters. In all of his years of research in this field Warren Wiscombe has encountered only two scientists whom he has described to me as not only feisty but also a bit nasty. Such individuals are rare in the field of atmospheric radiation and I have yet to meet them – maybe I never will. I view my colleagues much like Klaus Pfeilsticker describes his colleagues in Boulder, Colorado – as his "Boulder Family." I have my ARM Science Team Family, my MISR Science Team Family, my European Union CLOUDMAP2 Family, my Family of Wonderful Graduate Students and my Fellow Members of King Arthur's Court. I have learned, and continue to learn, a tremendous amount from all of these colleagues.

Howard Barker has influenced my thinking about many topics in this book. Ideas from Mark Miller, Pavlos Kollias, and Roger Marchand have no doubt found their way into this book. Tony Clough, during a series of enjoyable dinners dating back to the mid-1990s, has tried time and again to straighten out my thinking on topics in Chapter 2, and I am not sure I have them all straight yet. But I am certain that his and Eli Mlawer's assistance over the years has provided me the best chance of properly running their line-by-line radiative transfer model and monochromatic radiative transfer model, which we used to generate all of the high spectral resolution figures in this book. Rich Bevilacqua provided timely insights to us on retrieving water vapor profiles in the mesosphere at microwave frequencies. I first learned some Monte Carlo methods from Tom Ackerman in the early 1990s, and the first code that I ever used that could be started on one machine and then replicate itself to run on many machines was developed in a collaboration between Elizabeth Post and Tom. In the years since I have learned a great deal from Sasha Marshak (who has devoted time and patience to his many discussions with me), Anthony Davis and Frank Evans about radiative transfer in general and Monte Carlo techniques in particular. Elizabeth Post's original code has undergone radical changes as a result but she would nonetheless recognize the code that remains to this day. Discussions with Qilong Min motivated specific applications in Chapter 6. Of the graduate students I worked with at Penn State those who made a direct contribution to the radiative transfer codes I used for this book include Chuck Pavloski, Seiji Kato, Laura Hinkelman, Daniel Pawlak, Jason Cole and Jonathan Petters. All of the Monte Carlo terrestrial radiation calculations for Chapter 6 were produced by Jason Cole with a Monte Carlo code that he developed during his thesis research. Those with indirect contributions to topics in this book include Jay Mace, Chuck Long, Jim Mather, Andy Vogelmann, Ruei-Fong Lin, Xiquan Dong, Michael Jensen, Urszula Jambor, Adrian George, Kim Fineran, Manajit Sengupta, Greg Schuster and Dave Groff. Students in the atmospheric radiation courses that I taught always provided valuable feedback, with Kelly Cherrey and Jesse Stone's comments being of particular value as this book project came to an end.

For ten of my fourteen years at Penn State I had no idea who Michael Modest was even though I can see his office window from mine. He contributed ideas to Daniel Pawlak and Jason Cole during their study of radiative transfer.

I have never met our editors and technical assistants at Wiley-VCH in Germany – we did everything by email. Nevertheless, their support was wonderful. Andreas Thoss helped us in the early stages and Ulrike Werner helped us reach the end. Uwe Krieg always provided timely support with the Wiley-VCH LaTeX style sheets. I would send emails to them at the end of the day and without exception I would have my answers the following morning. They gave Craig wide latitude in determining the style of this book. After we had missed our third (or was it fourth?) deadline Ulrike told me not to worry, that if I had known what I was getting myself into with the start of this book I never would have done it. She was right, and her patience made ending this book project as pleasant as it could be. While our Wiley-VCH editors took care of our book business, Patrick Cleary's skill, flexibility, and open-mindedness was wonderful on my home front.

I owe a special thanks to Tom Ackerman and my co-author. Over the years I have learned more about atmospheric radiation from these two scientists than from any other person. My hope throughout the 1990s was that Tom and Craig would write a textbook and include me as a co-author if I could perform enough work on their behalf. Such a book might be dubbed the ABCs of atmospheric radiation, or the BAC of atmospheric radiation, but certainly not the CAB of atmospheric radiation. When Alistair Fraser suddenly retired and disappeared from Penn State University in 2001, Craig, a bit rattled from Alistair's departure, asked me to help him write a textbook in which he organized his diverse thoughts and scattered writings on atmospheric radiation. I asked him if I could get Tom to help us out. He said no – that Tom and he would have too many difficulties reaching agreement on content and style throughout the book. When I asked Tom the same question, he agreed with Craig. My hopes went up in smoke. I very much wanted to help Craig but I could not do so without Tom's blessing. I knew that whatever ideas of mine got into a textbook would partly be Tom's. Tom, as always, was amazingly gracious. He told me that I should help Craig and that I should have no worries about ideas of his that got into the book via my contributions. Over the nine years that I worked with Tom his boundless generosity towards me and the fantastic graduate students that he recruited was truly remarkable. He is second to none in this regard. I was indeed fortunate when I first crossed Tom's path in the pastry queue on the Sunday morning of April 21, 1991, during the American Institute of Physics symposium "Global Warming: Physics and Facts" held at Georgetown University.

Craig Bohren lives in a different world from the rest of us. During the course of writing this book, he has received hundreds, if not thousands, of emails from people with no real experience in science, or a bit of informal training, or plenty, or even experts in this field and that. To the best of my knowledge he has answered many, perhaps most if not all, of these emails as he tries to bring understanding to the people who write to him. My guess is that this diversity of his experience over many years has contributed to his strong and forceful statements in his discourse on science. Time and again he has energetically criticized me for writing paragraphs that he describes as incomprehensible. On a day close to the completion of this book, he called me and told me that what I had sent to him made him truly depressed – what I wrote was not clear and he could not make sense of it and it was depressing him to no end. As despair began to sink into me, I had to remember that this was Craig and he takes science communication seriously. He was being blunt because things were not clear to him and he wanted to make them clear. Over the next week he pursued cleaning up my ideas with such vigor. As he put ideas together in a logical and consistent manner I could see his mood lighten and his excitement grow. To me this is quintessential Craig – vigorously criticizing someone, me in this case, to educate as he gains clarity on a topic himself. I have come to appreciate to no end this intellectual sincerity on the part of Craig.

So, when Craig criticizes with passion something or someone in our text, he is doing so to make a point and not to humiliate. Ironically, I know that one of the first people most likely to find shortcomings in our text is going to be Tom Ackerman. I look forward to discussions with Tom in regards to aspects of the text because I know that he will be conversing with me to express his thoughts regarding some point here or there and to educate me as well. My hope is that when my colleagues, other scientists and students find an error they also let me know about it in the spirit of Tom.

Eugene E. Clothiaux
State College, Pennsylvania
July, 2005

1 Emission: The Birth of Photons

This is the first of three foundation chapters supporting those that follow. The themes of these initial chapters are somewhat fancifully taken as the birth, death, and life of photons, or, more prosaically, emission, absorption, and scattering.

In this chapter and succeeding ones you will encounter the phrase “as if”, which can be remarkably useful as a tranquilizer and peacemaker. For example, instead of taking the stance that light *is* a wave (particle), then fiercely defending it, we can be less strident and simply say that it is *as if* light is a wave (particle). This phrase is even the basis of an entire philosophy propounded by Hans Vaihinger. In discussing its origins he notes that “The Philosophy of ‘As If’ ... proves that consciously false conceptions and judgements are applied in all sciences; and ... these scientific Fictions are to be distinguished from Hypotheses. The latter are assumptions which are probable, assumptions the truth of which can be proved by further experience. They are therefore verifiable. Fictions are never verifiable, for they are hypotheses which are known to be false, but which are employed because of their utility.”

1.1 Wave and Particle Languages

We may discuss electromagnetic radiation using two languages: wave or particle (photon) language. As with all languages, we sometimes can express ideas more succinctly or clearly in the one language than in the other. We use both, separately and sometimes together in the same breath. We need fluency in both. Much ado has been made over this supposedly lamentable duality of electromagnetic radiation. But no law requires physical reality to be described by a single language. We may hope for such a language, but Nature often is indifferent to our hopes. Moreover, we accept without protest or hand-wringing the duality of sound. We describe sound waves in air as continuous while at the same time recognizing that air, and hence sound, is composed of discrete particles (molecules) in motion.

How do we choose which language to use? Simplicity. Life is short. To understand nature we take the simplest approach consistent with accuracy. Although propagation of sound in air could be described as the motions of molecules, had this approach been taken acoustics would have floundered in a mathematical morass.

In the photon language a beam of radiation is looked upon as a stream of particles called photons with the peculiar property that they carry energy, linear momentum, and angular momentum but not mass. The mass of the photon often is said to be identically zero. But given the near impossibility of measuring zero in the face of inevitable errors and uncertainties, it would be more correct to say that the upper limit of the photon mass keeps decreasing, its present value being about 10^{-24} times the mass of the electron. If it bothers you that a particle without mass can carry momentum this is because you are stuck on the notion that momentum is mass times velocity. Sometimes this is true (approximately), sometimes not. Momentum is momentum, a property complete in itself and not always the product of mass and velocity.

Photons are of one kind, differing only in their energy and momenta, whereas waves are of unlimited variety and often exceedingly complex, the simplest kind a plane harmonic wave characterized by a single (circular) frequency ω and direction of propagation (see Secs. 3.3 and 3.4). The dimensions of circular frequency are radians per unit time. You may be more familiar with just plain frequency, often denoted by ν (sometimes f), which has the dimensions of cycles per unit time. The unit of frequency is the hertz, abbreviated Hz, one cycle per second. Because one cycle corresponds to 2π radians, the relation between frequency and circular frequency is simple:

$$\omega = 2\pi\nu. \tag{1.1}$$

All electromagnetic waves propagate in free space (which does not strictly exist) with the same speed c, about $3 \times 10^8\,\mathrm{m\,s^{-1}}$. A plane harmonic wave in free space can just as well be characterized by its wavelength λ, related to its frequency by

$$\lambda\nu = c. \tag{1.2}$$

You sometimes hear it said that frequency is more fundamental than wavelength. In a sense, this is correct, but wavelength is often more useful. When we consider the interaction of electromagnetic waves with chunks of matter, the first question we must ask ourselves is how large the waves are. Big and small have no meaning until we specify a measuring stick. For electromagnetic radiation the measuring stick is the wavelength. The mathematical expressions describing the interaction of such radiation with matter can be quite different depending on the size of the matter relative to the measuring stick.

How do we translate from wave to photon language? A plane harmonic wave with circular frequency ω corresponds to a stream of photons, each with energy

$$E = h\nu = \hbar\omega, \tag{1.3}$$

where h is Planck's constant ($6.625 \times 10^{-34}\,\mathrm{J\,s}$) and $\hbar = h/2\pi$. The frequency of visible electromagnetic radiation (light) is about 10^{14} Hz, and hence the photons that excite the sensation of vision have energies around 10^{-20} J. This isn't much energy; the kinetic energy of a golf ball as it slices through air is about 10^{13} times greater.

Understanding what happens when an electromagnetic wave is incident from air on the smooth surface of glass, say, is not especially difficult if one uses the wave language. The incident wave excites molecules in the glass to radiate secondary waves that combine to form (approximately) a net reflected wave given by the law of reflection and a net transmitted wave given by the law of refraction. There is no such thing as an absolutely smooth surface, so what is meant is smooth on the scale of the wavelength.

All this makes intuitive sense and causes no perplexity. But now consider what happens when we switch to photon language. If we look upon reflection as the rebound of photons at a surface and transmission as their penetration through it, then why, if all photons are identical, are some reflected and some transmitted? This is indeed puzzling; even more so is why photons should be specularly (by which is meant mirror-like) reflected, because for photons imagined as particles of vanishingly small dimensions, all surfaces are rough.

This is not to say that one couldn't describe reflection and transmission at smooth interfaces in photon language, only that to do so would be exceedingly costly in mental effort. And the reverse sometimes is true. Many years ago one of the authors attended a colloquium entitled "The photoelectric effect without photons." By the photoelectric effect is usually meant the emission of electrons by a surface (often metallic) because of illumination by radiation (often ultraviolet). In photon language the photoelectric effect is simple to describe. When a photon of energy $h\nu$ is absorbed by the surface, the maximum kinetic energy E of the electrons thereby set free is

$$E = h\nu - p, \tag{1.4}$$

where p is the minimum energy an electron loses in breaking free of the surface. A single photon interacting with a single electron gives up its entire energy to that electron, which if sufficient enables the electron to break free of the forces binding it to the metal. According to this equation the energies of the emitted electrons are independent of the incident power whereas the photocurrent (rate and number of emitted electrons) is proportional to it, which accords with experiment. This simple equation, first written down by Einstein in 1905, is one of the keystones of the modern theory of radiation and matter. Yet the speaker at that colloquium years ago, in an effort to describe and explicate the photoelectric effect without photons, assailed the audience with dozens of complicated equations. And even at that, part way through his mathematical tour de force his mind and tongue betrayed him and he blurted out the forbidden word "photon". At that point, your author who was there leapt up from his seat and shouted, "Photons! Photons! You promised no photons."

A mirror illuminated by an incident beam gives rise to a reflected beam. Is this reflected beam redirected incident photons? Alas, we cannot do an experiment to answer this question. To determine if reflected photons are the same as incident photons would require us to be able to identify them. But photons are indistinguishable. We cannot tell one from another. We cannot tag a photon and follow its progress. Thus if you want to believe that reflected photons are the same as incident photons, you may do so. No one can prove you wrong. But you cannot prove you are right. When faced with an undecidable proposition, you may believe whatever you wish. Note that in the wave language we would not likely even ask if the reflected wave is the same as the incident wave.

It is not often acknowledged that there is a third language for talking about light, what might be called the who-gives-a-hoot-what-light-is? language. This is geometrical or ray optics, in which the nature of light isn't addressed at all. Fictitious rays are imagined to be paths along which the energy carried by light is transported, and these paths meander and bifurcate according to simple geometrical laws.

But which language is the more useful? In a letter to *American Journal of Physics*, M. Psimopoulos and T. Theocharis ask the rhetorical questions: "What new discoveries have (i) the particle or photon aspect of light, and (ii) the wave aspect of light, given rise to? Answer: (i) we are not aware of any; (ii) holography, laser, intensity interferometry, phase conjugation." To this list we add radar, all of interferometry, on which much of the science of measurement is based, and interference filters, which have many applications. The view of these authors is extreme, but they also quote the more measured words of Charles Townes, a pioneer in masers and lasers: “Physicists were somewhat diverted by an emphasis in the world of physics on the photon properties of light rather than its coherent aspects.” That is, the photon language has been the more fashionable language among physicists, just as French was the fashionable language in the Imperial Russian court. When prestigious and munificent prizes began to be awarded for flushing “ons” (electron, positron, neutron, meson, and so on) from the jungle, shooting them, and mounting their stuffed heads on laboratory walls, the hunt was on, and slowed down only with the demise of the Superconducting Supercollider.

Although the wave language undoubtedly has been and continues to be more fruitful of inventions, the photon language is perhaps more soothing because photons can be incarnated, imagined to be objects we can kick or be kicked by. Waves extending through all space are not so easily incarnated. We can readily conceive of the photon as a thing. And yet an electromagnetic wave is just as much a thing as a photon: both possess energy and momentum (linear and angular) but not, it seems, mass.

1.2 Radiation in Equilibrium with Matter

We often are told that when bodies are heated they radiate or that “hot” bodies radiate. True enough, but it is just as true that when bodies are *cooled* they radiate and that “cold” bodies radiate. *All* matter – gaseous, liquid, or solid – at *all* temperatures emits radiation of *all* frequencies at *all* times, although in varying amounts, possibly so small at some frequencies, for some materials, and at some temperatures as to be undetectable with today’s instruments (tomorrow’s, who knows?). Note that there is no hedging here: all means all. No exceptions. Never. Even at absolute zero? Setting aside that absolute zero is unattainable (and much lower than temperatures in the depths of the Antarctic winter or in the coldest regions of the atmosphere), even at absolute zero radiation still would be associated with matter because of temperature fluctuations. Temperature is, after all, an average, and whenever there are averages there are fluctuations about them.

Radiation emitted spontaneously, as distinguished from scattered radiation (see Ch. 3), is not stimulated by an external source of radiation. Scattered radiation from the walls of the room in which you read these words may be stimulated by emitted radiation from an incandescent lamp. Turn off the lamp and the visible scattered radiation vanishes, but the walls continue to emit invisible radiation as well as visible radiation too feeble to be perceptible.

We are interested in the spectral distribution of radiation – how much in each wavelength interval – emitted by matter. Consider first the simpler example of an ideal gas in a sealed container held at absolute temperature T (Fig. 1.1). When the gas is in equilibrium its molecules are moving in all directions with equal probability, but all kinetic energies E are not equally probable. Even if all the molecules had the same energy when put into the container, they would in time have different energies because they exchange energy in collisions with each other and the container walls. A given molecule may experience a sequence of collisions in which it always gains kinetic energy, which would give it a much greater energy than average. But such a sequence is not likely, and so at any instant the fraction of molecules with kinetic energy much greater than the average is small. And similarly for the fraction of molecules with kinetic energy much less than the average. The distribution of kinetic energies is specified by

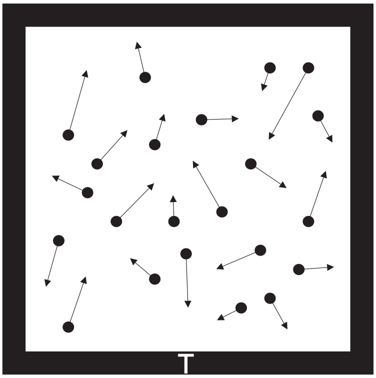

Figure 1.1: At equilibrium, ideal gas molecules in a closed container at absolute temperature T have a distribution of kinetic energies (Fig. 1.2) determined solely by this temperature.

a probability distribution function $f(E)$ which, like all distribution functions, is defined by its *integral* properties, that is,

$$\int_{E_1}^{E_2} f(E)\, dE \tag{1.5}$$

is the fraction of molecules having kinetic energies between any two energies E_1 and E_2. Note that f does not specify *which* molecules have energies in a given interval, only the fraction, or probability, of molecular energies lying in this interval. If f is continuous and bounded then from the mean value theorem of integral calculus

$$\int_{E_1}^{E_2} f(E)\, dE = f(\bar{E})(E_2 - E_1), \tag{1.6}$$

where $\bar{E}$ lies in the interval (E_1, E_2). If we denote E_1 by E and E_2 by $E + \Delta E$ we have

$$f(E) = \lim_{\Delta E \to 0} \frac{1}{\Delta E} \int_{E}^{E+\Delta E} f(x)\, dx. \tag{1.7}$$

Because of Eq. (1.7) $f(E)$ is sometimes called a *probability density*. When the limits of the integral in Eq. (1.5) are the same (interval of zero width) the probability is zero. The probability that a *continuous* variable has exactly a particular value at any point over the interval on which it is defined is zero, as it must be, for if it were not the total probability would be infinite.

A distribution function such as $f(E)$ is sometimes defined by saying that $f(E)\, dE$ is the fraction (of whatever) lying in the range between E and $E + dE$. This is sloppy mathematics because although E represents a definite number dE does not. Moreover, this way of defining a distribution function obscures the fact that f is defined by its integral properties. As we shall see, failure to understand the nature of distribution functions can lead to confusion and error. It would be better to say that $f(E)\ \Delta E$ is *approximately* the fraction of molecules lying between E and $E + \Delta E$, where the approximation gets better the smaller the value of ΔE.

You also often encounter statements that $f(E)$ is the fraction of molecules having energy *E per unit energy interval*. This can be confusing unless you recognize it as shorthand for saying that $f(E)$ must be multiplied by ΔE (or, better yet, integrated over this interval) to obtain the fraction of molecules in this interval. This kind of jargon is used for all kinds of distribution functions. We speak of quantities per unit area, per unit time, per unit frequency, etc., which is shorthand and not to be interpreted as meaning that the interval is one unit wide.

Gases within a sealed container held at constant temperature evolve to an equilibrium state determined solely by this temperature. In this state the distribution function for molecular kinetic energies is the *Maxwell–Boltzmann distribution*

$$f(E) = \frac{2\sqrt{E}}{\sqrt{\pi}(k_{\mathrm{B}}T)^{3/2}} \exp\left(-E/k_{\mathrm{B}}T\right), \tag{1.8}$$

where k_{B}, usually called *Boltzmann's constant*, is $1.38 \times 10^{-23}\ \mathrm{J\,K^{-1}}$, and f is normalized

$$\int_0^\infty f(E)\, dE = 1. \tag{1.9}$$

The limits of integration are symbolic: molecules have neither infinite nor zero kinetic energies; by zero is meant $\ll k_{\mathrm{B}}T$ and by infinite is meant $\gg k_{\mathrm{B}}T$. Because of Eq. (1.9) $f(E)$ is a probability distribution function.

The most probable kinetic energy E_{m} is that for which f is a maximum, the energy at which its derivative with respect to E is zero:

$$E_{\mathrm{m}} = k_{\mathrm{B}}T/2. \tag{1.10}$$

As the temperature of the gas increases so does the most probable kinetic energy of its molecules. Figure 1.2 shows f relative to its maximum as a function of E relative to E_{m}, a universal curve independent of temperature.

What does all this have to do with radiation? Because matter continuously emits radiation, a container with walls so thick that no photons leak from it will fill with a gas of photons (Fig. 1.3). The container is held at a fixed temperature T. At equilibrium the photons in the container, like gas molecules, do not all have the same energy (equivalently, frequency) but are distributed about a most probable value. The distribution function for the energies of photons in equilibrium with matter goes under various names and there are several versions of this function differing by a constant factor. Imagine a plane surface within the container. At equilibrium, the radiation field is isotropic, so regardless of how the surface is oriented the same amount of radiant energy crosses unit area in unit time. We consider only that radiant energy (photons) propagating in a hemisphere of directions either above or below the surface. The energy distribution function (or spectral distribution) is given by the *Planck distribution* (or *Planck function*)

$$P_{\mathrm{e}}(\omega) = \frac{\hbar\omega^3}{4\pi^2c^2}\frac{1}{\exp\left(\hbar\omega/k_{\mathrm{B}}T\right) - 1}. \tag{1.11}$$

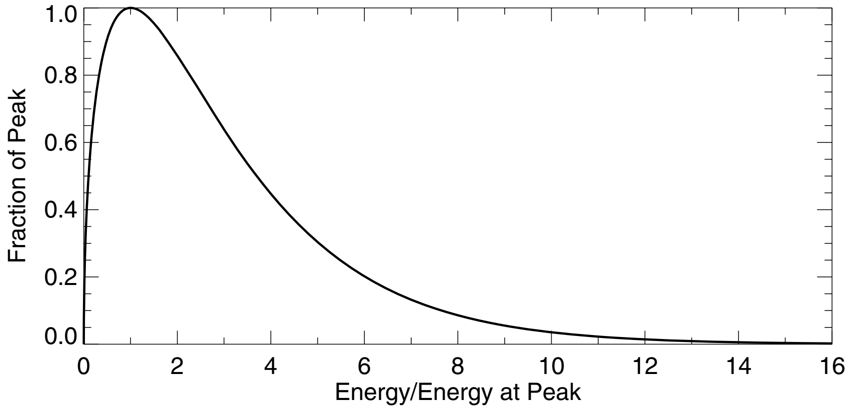

Figure 1.2: Distribution of kinetic energies of an ideal gas at equilibrium shown as a universal function independent of temperature. The kinetic energy relative to that at the peak of the distribution function, however, does depend on temperature.

The integral of this function over any frequency interval is the total radiant energy in that interval crossing unit area in unit time, called the *irradiance* (discussed in more detail in Sec. 4.2).

The Planck function is worthy of respect, if not awe, in that it contains not one, not two, but *three* fundamental (or at least believed to be so) constants of nature: the speed of light in a vacuum c, Planck's constant h, and Boltzmann's constant k_B. You can't get much more fundamental than that.

The most probable photon energy is obtained by setting the derivative of P_e with respect to ω equal to zero; the result is the transcendental equation

$$3(e^x - 1) = xe^x, \tag{1.12}$$

where $x = \hbar\omega/k_\mathrm{B}T$, the solution to which (obtained quickly with a pocket calculator) is $x = 2.819$. Thus the most probable photon energy is

$$\hbar\omega_\mathrm{m} = 2.819 k_\mathrm{B}T. \tag{1.13}$$

Note the similarity of Eq. (1.11) to Eq. (1.8) and Eq. (1.13) to Eq. (1.10), which is not surprising given that both are distribution functions for gases, although of a different kind. The most striking difference between a gas of molecules and a gas of photons is that the number of molecules in a sealed container is conserved (barring chemical reactions, of course) whereas the number of photons is not. As the temperature of the container, which is the source of the photons, increases, the number of photons within it increases. Photons are not subject to the same conservation laws as gas molecules, which are endowed with mass.

At frequencies for which $\hbar\omega \ll k_\mathrm{B}T$ Eq. (1.11) can be approximated by

$$P_\mathrm{e}(\omega) \approx \frac{k_\mathrm{B}T\omega^2}{4\pi^2 c^2}. \tag{1.14}$$

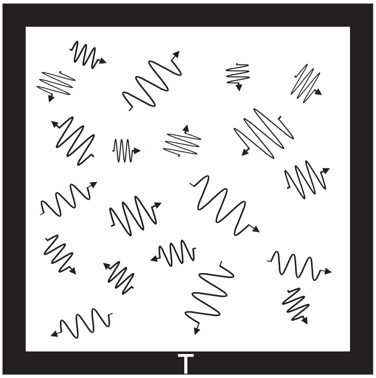

Figure 1.3: An opaque container at absolute temperature T encloses a gas of photons emitted by its walls. At equilibrium, the distribution of photon energies (Fig. 1.4) is determined solely by this temperature.

Folks interested in radiation of sufficiently low frequency (e.g., microwaves) sometimes express radiant power as a temperature. When first encountered this can be jarring until you realize that the Planck function is proportional to absolute temperature at such frequencies.

1.2.1 Change of Variable

We may express the Planck distribution as a function of frequency or wavelength. But in making a change of variables we have to be careful. The physical content of the Planck distribution is contained in its integral. According to the theorem for the change of variables in an integral

$$\int_{\omega_1}^{\omega_2} P_{\mathrm{e}}(\omega)\, d\omega = \int_{\lambda_1}^{\lambda_2} P_{\mathrm{e}}\{\omega(\lambda)\} \frac{d\omega}{d\lambda}\, d\lambda, \tag{1.15}$$

where $\omega(\lambda)$ is the transformation from circular frequency to wavelength and λ_j is the wavelength corresponding to ω_j. The derivative in the integral on the right side of this equation is called the *Jacobian* of the transformation. Equation (1.15) is *not* obtained by canceling the $d\lambda$s, which is merely a way of remembering the theorem. The notation of calculus has evolved so as to make it easy to remember theorems, but notation should not cause us to forget that they all require proofs. No theorem can be proved by purely notational tricks.

According to Eq. (1.15) the Planck function expressed in wavelength terms is

$$P_{\mathrm{e}}(\lambda) = P_{\mathrm{e}}\{\omega(\lambda)\} \frac{d\omega}{d\lambda}, \tag{1.16}$$

where we use the same symbol P_{e} for both functions even though this is sloppy mathematics. The distinction between a function and its values is often blurred. We write

$$y = f(x) \tag{1.17}$$

to indicate that y is the value the function f assigns to x. Suppose that f is the function "square it": $y = x^2$. If we transform from the variable x to $x = \sqrt{u}$, we obtain the new functional relation $y = u$. This is now a different function, and hence merits its own name (symbol). But to save having to invent more and more symbols, we are sloppy and write $y = f(x) = f\{x(u)\} = f(u)$, when we should write $y = f(x) = g(u)$. We often are even sloppier by confusing the value of the function with the function itself. That is, we write $y = y(x) = y\{x(u)\} = y(u)$. The fundamental rule of mathematical sloppiness is that you are allowed to be sloppy as long as you know how to do things correctly.

Although the Jacobian in Eq. (1.15)

$$\frac{d\omega}{d\lambda} = -\frac{2\pi c}{\lambda^2} \tag{1.18}$$

is negative, this does not mean that the radiant energy in the wavelength interval is negative. The upper limit on the right side of Eq. (1.15) is smaller than the lower limit, which by itself would make the integral negative, but the negative Jacobian makes the integral positive. So we write the Planck function as

$$P_{\mathrm{e}}(\lambda) = P_{\mathrm{e}}\{\omega(\lambda)\} \left|\frac{d\omega}{d\lambda}\right| = P_{\mathrm{e}}\{\omega(\lambda)\}\frac{2\pi c}{\lambda^2} \tag{1.19}$$

and remember to reverse the limits of integration on the right side of Eq. (1.15). The Planck function expressed in wavelength terms is therefore

$$P_{\mathrm{e}}(\lambda) = \frac{2\pi hc^2}{\lambda^5}\frac{1}{\exp(hc/\lambda k_{\mathrm{B}}T) - 1}. \tag{1.20}$$

For $hc/\lambda k_{\mathrm{B}}T \ll 1$, Eq. (1.20) is approximately

$$P_{\mathrm{e}}(\lambda) \approx \frac{2\pi c k_{\mathrm{B}}T}{\lambda^4}. \tag{1.21}$$

At temperatures around 300 K this equation is a good approximation (within about 1% or less) for wavelengths greater than about 250 μm. As we show in Section 8.1 the spectrum of skylight is approximately proportional to $1/\lambda^4$. As temperature increases without limit, therefore, the Planck function at visible wavelengths has approximately the same spectral dependence as the blue sky. So much for the notion that an exceedingly hot body is "white hot" or that blue is a "cold" color whereas red is a "warm" color.

The two forms of the Planck function presented here have the peculiar property that although the integral over any wavelength interval is equal to the integral over the corresponding frequency interval, the two functions do not peak at the same place. That is, if we find the frequency at which $P_{\mathrm{e}}(\omega)$ is a maximum and transform that frequency into a wavelength, we do not obtain the wavelength at which $P_{\mathrm{e}}(\lambda)$ is a maximum. To find this wavelength, differentiate Eq. (1.20) with respect to λ and set the result equal to zero. This yields the transcendental equation

$$5(e^x - 1) = xe^x, \tag{1.22}$$

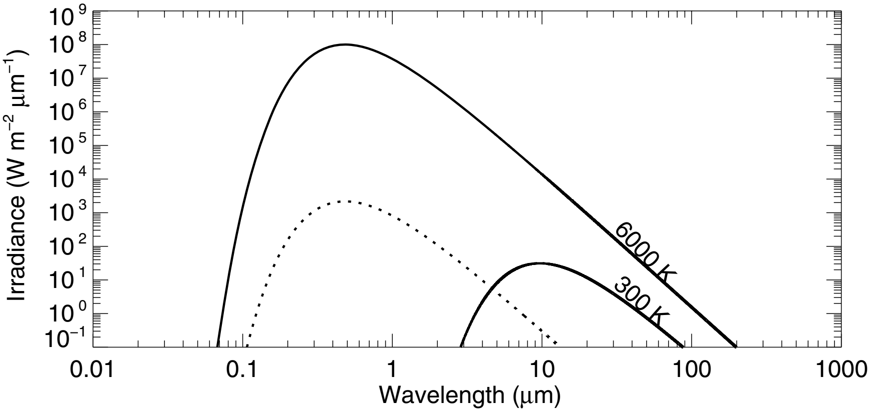

Figure 1.4: Planck function for 6000 K and 300 K. The dashed curve is the irradiance at the top of the atmosphere from a 6000 K blackbody at the Earth–sun distance, which approximates the solar irradiance.

where $x = hc/\lambda k_\mathrm{B}T$, the solution to which is $x = 4.961$. From this we obtain *Wien's displacement law* relating temperature to the wavelength λ_m at which $P_\mathrm{e}(\lambda)$ is a maximum:

$$\lambda_\mathrm{m}T = 2902\,\mu\mathrm{m\,K}. \tag{1.23}$$

For $T = 273\,\mathrm{K}\ (0\,°\mathrm{C})$, $\lambda_\mathrm{m} = 10.6\,\mu\mathrm{m}$. Equation (1.23) is called a displacement law because it determines how the Planck function is displaced as temperature increases. This displacement is evident in Fig. 1.4, which shows Eq. (1.20) for two temperatures, 6000 K and 300 K. Note also the huge difference in the amount of radiation emitted at these two temperatures.

But if we transform Eq. (1.13) into wavelength terms we obtain a different displacement law

$$\lambda_\mathrm{m}T = 5107\,\mu\mathrm{m\,K}, \tag{1.24}$$

where λ_m is the wavelength corresponding to the frequency ω_m in Eq. (1.13). And this wavelength for 273 K is $18.7\,\mu\mathrm{m}$, quite a shift from $10.6\,\mu\mathrm{m}$. Which is correct? They both are. No law requires P_e to be plotted versus wavelength. This may be the custom in some fields, but not in others. Many spectroscopists plot spectra as a function of *wavenumber* (inverse wavelength, equivalent to frequency) and would consider doing otherwise an unnatural act. There is, in general, no invariant maximum for a distribution function. This may be unpalatable but it is a fact of life, in the nature of distribution functions. And yet this seems to be a difficult idea to get across. Once, after we had carefully discussed it in class, a student asked in all sincerity, "But where is the *real* maximum of the Planck function?" He thought we knew but were withholding it from the uninitiated, a secret to be revealed only on our deathbeds.

Failure to recognize that the maximum of a distribution function depends on how it is plotted has led and no doubt will continue to lead to errors. In a delightful paper Bernard Soffer