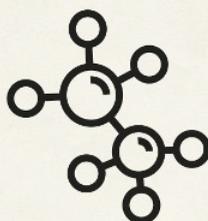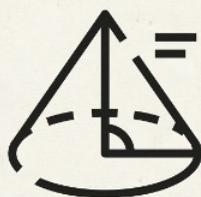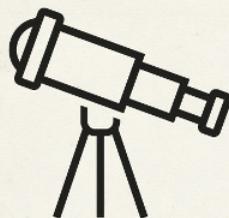# ROBERT STEWART

HOW TO DO
RESEARCH

and How to Be a Researcher

How to Do Research

How to Do Research

And How to Be a Researcher

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With thanks to my academic line managers over the years (Anthony, Martin, Graham, Matthew) and to everyone I have worked with and supervised, for teaching me most of what I know.

With thanks to Trevor for kicking the tyres.

And with gratitude beyond expression to Ange, Tom, and Luce for putting up with all of this.

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Image Credit Lines

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Introduction

For better or worse, our history has been shaped by our ability to develop and share knowledge. This is not simply a matter of learning about the world around us—animals can do that perfectly well. Where we depart from other species is in the communication of that learning: we no longer have to repeat the same experiments and make the same mistakes all over again; the many can benefit from the work of a few. Fire, the wheel, and bread-making allowed early civilizations to grow because of our ability to pass on (or steal) knowledge and skills. Later, the development of steam engines and telescopes depended on earlier discoveries from working with iron and glass, and the rediscovery of America by Europeans was the result of centuries of progress in ship building and navigation. In turn, the way we view that rediscovery of America and what happened afterwards is shaped by historians' research and the theories or frameworks applied to the evidence from source materials. The accumulation of knowledge is more than just a series of inventions—it's about understanding how the world works. Knowing about micro-organisms allows us to understand diseases, knowing about atmospheric pressure allows us to predict the weather (to some extent), knowing about atomic structures allows us to develop nuclear power, and knowing about how wars have started in the past ought to help us prevent them in future. Most of what we take for granted in our daily lives has come about because people have made observations or deductions, developed ideas, and passed these on to others.

The trouble is that any old notion can be communicated and shared—I could spend an afternoon on the Internet wading through wild conspiracy theories—so how do we sift the truth from the nonsense?[1] This book puts the principles of research alongside some of the diverse people who have shaped them, and goes on to cover the more mundane, day-to-day components of a research career—writing, networking, looking for funding, dealing with institutions and their vagaries. Along the way, it also considers how research, and therefore what we know or take for granted, is directed by

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[1] And incidentally, why are conspiracy theories so attractive in the first place? And why is it a good thing for a scientist to be sceptical about what we think we know, but a bad thing for the general public to be sceptical about scientists?

external forces—by money, politics, influence, and the sheer eccentricity and bloody-mindedness of those involved. There is a tendency for academics[2] to portray the quest for knowledge as a high and noble endeavour, and perhaps it's useful to hold this up as an aspiration in our better moments. However, in reality a lot of advances come from accidental findings (e.g. Alexander Fleming leaving his petri dish lying around when he went on holiday and discovering penicillin) or else they take a long time to be recognized (e.g. the forty-plus years it took for the Royal Navy to supply sailors with citrus juice after it was proposed as a treatment for scurvy by James Lind in 1753). If we don't understand the messiness, we will have a very poor picture of the way we move forward.

A mission statement

I do hope that this book is easy to read. There's actually nothing particularly complicated about the principles underlying research; if it does get complicated, this tends to be because of the more specific challenges of the discipline in which you work.[3] If so, you may need to supplement this with something more specialized—there's plenty of material around. What I intend to cover here are the considerations that are common (more or less) to any field of research. Whether you're thinking seriously about research as a career or just dipping your toe in the water through a student dissertation, I hope that you'll be able to see how your project fits into the general scheme of things. If you're already well into a research career, I hope this book provides some lighter reading than the turgid academic output you're probably wading through at the moment, and that you can sit back a bit on a sunny afternoon and remind yourself why you chose to belong to this slightly strange world in the first place.

I also hope that this book makes sense to you regardless of your field of interest; perhaps this is over-optimistic but I've tried my best. Research principles and techniques are described most often in relation to the sciences, and I'm bound to be biased in that direction from my own professional

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[2] I'm going to use 'academic' and 'research' quite loosely and interchangeably in this book, although I'm aware that there are differences—not everything described as academic is or has been focused on research, and not all research is carried out by people who would describe themselves as academics. Apologies if you find this a problem; however, neither research nor what would be called 'academia' today have retained very much connection with the mythical Academus or Plato's Academy founded on his land in the fourth century BCE (see Chapter 15), so I think this allows some freedom with the term.

[3] And, even then, the complexities are usually around the logistics of measuring something (getting laboratory conditions correct) or having the right equipment. The objective of the study itself is often a much more straightforward matter.

background.[4] However, everything I've read or heard about from humanities research suggests that we're all following similar paths and that what we do is underpinned by the same basic principles. In a similar vein, most of the discussion over the years has concerned what is called 'empirical' research—that is, derived from observations of the world around us—so there might again be a tendency for material to drift in this direction; however, the research fields involving a more 'rationalist', deductive approach (e.g. pure mathematics, a lot of humanities, and more so-called empirical sciences than you might believe) have commonalities and I hope that these are also adequately covered.

The general principles of research are covered in Chapters 2–12, followed by shared experiences in the job of a researcher and some of the challenges you're likely to encounter along the way if you take up (or continue) this career, covered in Chapters 13–17. Again, I hope I've covered the main issues, but there's always plenty more to find out from conversations with colleagues and other, more detailed sources. The research world, with everything else, has been going through some strange times recently[5] and life as a researcher could become quite different quite quickly—it always has been changeable, as you'll find out if you choose to read further, and there's no reason why this shouldn't continue to be the case. I draw some tentative conclusions in the final chapter, although I wouldn't be a researcher if I promised definitive answers.

A tale of Victorian London

I'm going to begin with the story of John Snow and the 1854 cholera epidemic. I feel an obligation, as he's the 'father of epidemiology', my specialty, and his example frames this discipline. In my early days as a researcher, I used to have to explain to my family and friends what epidemiology was; after March 2020 this became less of an issue and it's amazing how many people view themselves as epidemiologists nowadays. The story of John Snow and cholera is an 'old chestnut' that is wheeled out repeatedly to describe 'the scientific method', helped no doubt by the whiff of nineteenth-century social reform and an age of impressive facial hair (on men, at least). And yet what's actually most interesting is what's usually kept out. Yes, at the start of the nineteenth century, cholera was believed to be a disease spread through bad air. And yes, by the end of the century, everyone

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[4] Not that epidemiology is considered a particularly hard-core science
[5] Meaning the 2020+ COVID-19 pandemic if you're reading this in some blissful utopian future.

accepted that it was spread through contaminated water. But how did this change in knowledge actually occur? John Snow is now widely credited as the person responsible, as someone whose research most obviously tipped the balance, but was he really so successful (and was he really the model, dispassionate observer)? The change in opinion about cholera only really happened after he had died, which is so often the way with pioneers, and it's worth trying to understand why.

London in the late summer of 1854 plants us firmly in the mid-Victorian era. Britain was nearly a year into its Crimean War—the Light Brigade would charge pointlessly but poetically into the valley of death that October. In Parliament, Palmerston as Home Secretary was passing reformist legislation to reduce child labour in factories and making vaccination compulsory, which would all sound very worthy if he wasn't at the same time fighting hard to prevent voting by the urban working class. Charles Dickens was at the height of his creative powers, having finished *Bleak House* the previous year and serialized *Hard Times* in the intervening period. Up in Yorkshire, Charlotte Brontë, recently married, had outlived her siblings, although had only seven months left herself before she died of pregnancy complications. Queen Victoria, on the other hand, was between her eighth and ninth childbirth, her oldest son Albert (the future Edward VII) was approaching his thirteenth birthday, and the magnificent Crystal Palace, which had accommodated her husband's labour of love, the Great Exhibition, was in the process of being moved from Hyde Park to its new home in Sydenham.

Only a quarter of a mile to the east of Hyde Park, Soho was not a good place to be. Urban migration was in full flow as more and more people arrived to scrape out a living in the Big Smoke. London had been the world's largest city since the 1830s, the first since ancient Rome to top a million inhabitants. Its population grew threefold between 1815 and 1860, and the 1851 census showed that nearly forty per cent of its residents had been born somewhere else. Soho's inhabitants lived in cramped unsanitary accommodation and there was no sewage system to speak of. The only way to dispose of human waste was in a pit beneath the cellar floorboards. These cesspools were rarely adequate and frequently overflowed—often into the wells that had been dug to provide public water supplies. Those households who noticed their cesspool filling faster than it was emptying might pay to have their waste collected and dumped in the River Thames, although this was just another place from which drinking water was sourced. The devastation inflicted by infectious diseases is entirely understandable now—so why did it take so long to work out?

King Cholera

Oddly, cholera tends not to be the first disease that comes to mind when we think about the nineteenth century, and yet it was probably the most devastating. Perhaps this is down to the art and literature of the time. 'Consumption' (tuberculosis) is more suited to the plots of novels and operas, with its slow decline, the accentuation of an already fashionable pallor, and the visual image of fresh blood on a white silk handkerchief. Even today, all an actor in period costume has to do is cough and you know what's going to happen next. Perhaps a disease has to fit in with its times to be remembered. Bubonic plague certainly chimes harmoniously with our impressions of the mediaeval world: all those grinning skeletons in paintings and sculptures. And the ravages of syphilis in the eighteenth-century illustrations by Hogarth and others were helpful for portraying immorality and decadence. Cholera, as a messy and squalid disease, would probably not have put off an early novelist like Henry Fielding, or an illustrator like James Gillray, but fashions had changed by the time it arrived in Europe. It is 'bad air' (as well as typhus and consumption) which causes the deaths we are meant to care about in *Jane Eyre*, poor nutrition (and consumption) in *Nicholas Nickleby*, and smallpox (and consumption) in *Bleak House*. In Victorian times people died from cholera very rapidly, providing little opportunity for noble speeches between the violent bouts of diarrhoea and vomiting. So, a disease that killed in the millions barely receives a mention in what has been passed down to us, and we can easily forget how much it must have shaped that century.

The cholera outbreak in Soho alone in the late summer of 1854 killed 127 residents in its first three days and over 600 by the time it subsided. It was actually just part of what is now known as the 'third cholera pandemic'—a pandemic, as we now know only too well, describes an infection that is continental rather than national in scale. The cholera pandemics of the nineteenth century were devastating waves that swept across the world in the same way that plague had done earlier, and influenza would do later. Cholera (named after the Greek word for 'bile' for obvious reasons) had probably been present in the Indian subcontinent since ancient times, only spreading more widely as trade routes expanded. The first pandemic in 1816–1826 mainly affected India and east Asia and didn't extend west of the Caspian Sea; however, the second struck Europe with a vengeance, killing six thousand Londoners, for example, in 1832 and a further fourteen thousand in 1849. Russia was the country worst hit by the third pandemic, with at least a million deaths, but the effect was widespread and over ten thousand lives were lost in London alone during 1853–1854. Sitting in the twenty-first century city, it's hard to

take in the magnitude of the carnage or to imagine the disease's impact, particularly considering the smaller population at the time. Would our ancestors have felt the same way as we would now about something that wiped out over one in ten people in some areas within the space of a few months? Or was the city so used to successions of diseases and disasters over the centuries, and all the other hardships of nineteenth-century life, that it just gritted its teeth and carried on? After the 1832 outbreak, the disease was known as 'King Cholera', which suggests a mixture of respect and black humour not very different from London attitudes today.

On the difficulty of persuading anyone to change their mind

The problem with scientific breakthroughs is that, once they happen and are accepted, it becomes hard to imagine the old worldview. Why on earth would the intelligent folk of the 1850s not consider that perhaps it might be a good idea to keep raw sewage out of the drinking water? However, we shouldn't be too hard on our ancestors—we don't have to travel back too far ourselves to a time when the health benefits of cigarettes were being widely touted in advertising, or when the smoke from factories was felt to be a useful defence against disease, or when people were hand-painting watch hands to display the exciting new substance, radium, that glowed in the dark. It's important to realize that the 'old view' is often held for entirely logical and understandable reasons; most ideas are. What it takes first of all is someone with vision who can think outside the prevailing viewpoint and consider it objectively. Next, they need to compare the evidence for competing viewpoints and, crucially, find some way of clinching the argument. Finally, they have to get people to listen to them. These three steps are at the heart of research.

   'Miasma' was the predominant explanation for infectious disease in the 1850s and had been popular since ancient times. The word itself is simply the Greek for pollution, having been written about by Hippocrates way back in the fourth century BCE, and the belief was that diseases like cholera, bubonic plague, and even chlamydia were spread by 'bad air'.[6] Indeed, prominent and influential supporters of miasma theory included Florence Nightingale (who used it as an argument for freshening up hospitals) and William Farr (the founding father of modern statistics who was instrumental in shaping the national census and recording of causes of death). Miasma as an idea

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[6] And it wasn't just diseases—there were theories around that you could put on weight by smelling food.

is entirely logical—disease tends to smell, areas with bad smells tend to be those with highest levels of disease, so therefore the bad-smelling air causes the spread of disease. Of course, 'smell' and 'risk' have powerful unconscious connections from way back in our evolutionary past, and there is a natural tendency for us to look first to the air around us when we think about illness—whether this involves pollution, radioactivity, or the common cold. A passenger on a packed train in winter worries much more about picking up a virus from the shared air than a shared handrail.

You know nothing, John Snow

John Snow (Figure 1.1) was the son of a labourer, born in a poor area of York that was prone to flooding. Having worked as a surgeon's apprentice (and having witnessed the devastating impact of the 1833 cholera outbreak in Newcastle), he graduated from the University of London and became a member of the Royal College of Physicians in 1850 at the age of 37. He was a true nineteenth-century polymath. As well as his work on cholera, he pioneered the use of anaesthetics and personally administered chloroform to Queen Victoria for the births of her final two children. He was also one of the first people to propose that rickets might be caused by diet rather than general poverty. Snow was already sceptical of miasma theory by 1854 and was considering contaminated water as an alternative explanation—for example, noticing that the usually healthy and clean-aired town of Dumfries was badly affected by cholera in 1832 and that it might not be a coincidence that its water supply from the River Nith was notoriously contaminated with sewage. He had also compared infection rates between areas of London drawing their water from different parts of the Thames (i.e. upstream or downstream of the worst pollution) during the 1848 outbreak. So, when cholera broke out again in Soho, he must have been determined to clinch the argument.

His approach was a simple one—he just took a street map and, with the help of a local curate, drew dots on it where cases of cholera had occurred (Figure 1.2). These clearly showed a cluster around a particular water pump on Broad Street, roughly in the centre of Soho.[7] Some cholera cases were closer to a neighbouring water pump, but it turned out that these people used the Broad Street pump because they preferred the water or were children who went to school there. None of the workers in the nearby brewery

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[7] Now Broadwick Street—much more up-market these days, and a haunt of well-heeled media types.
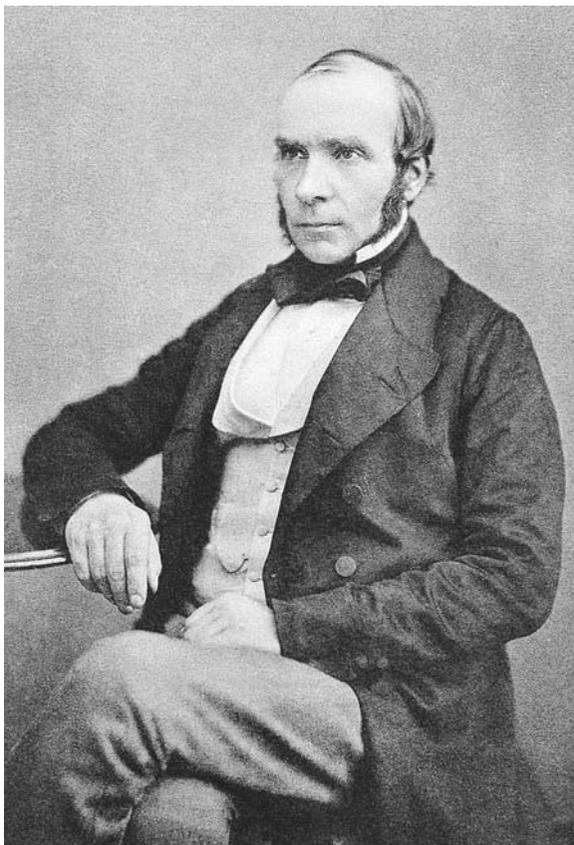
Fig. 1.1 John Snow in his early 40s, not long after he mapped out the 1854 cholera out-
break in Soho. Described as frugal and a health enthusiast (an early adopter of a vegan
diet for a while), he dressed plainly and remained a bachelor. His idea that cholera was
waterborne was not at all popular at the time and, unfortunately, he did not live to see
his proposals vindicated, dying from a stroke around two years after this picture was
taken.
Autotype of John Snow (1856; published 1887). Thomas Jones Barker, Public domain, via Wikimedia
Commons.

contracted cholera, but these were given an allowance of beer every day
(which contains boiled water) and apparently didn't drink anything else. On
7 September, Snow met with local officials, removed the handle of the pump,
and the cholera epidemic subsided. Sometime later it was found that the pub-
lic well had been dug far too close to an old cesspit which had probably been
contaminated with nappies from a baby who had picked up cholera from
somewhere else.
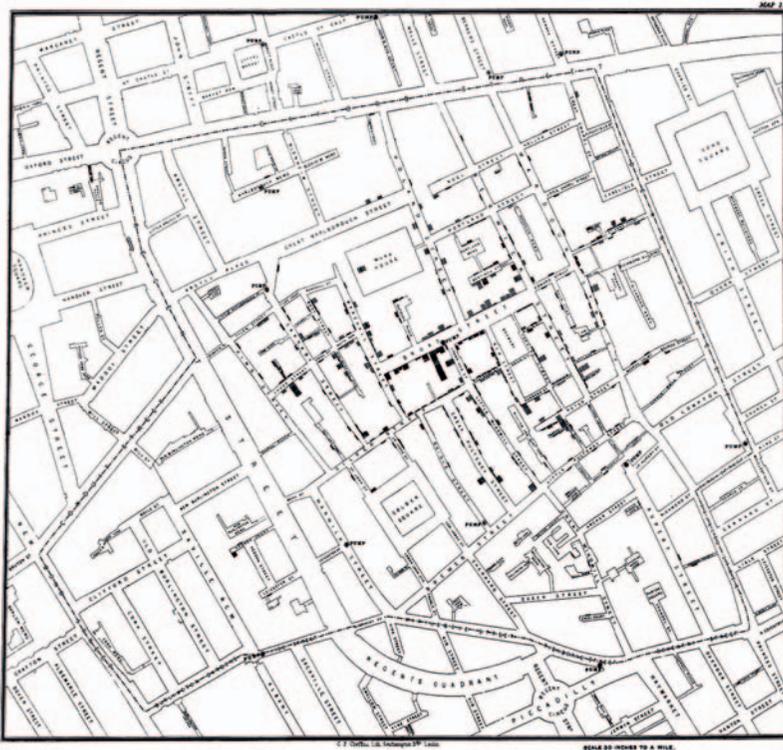
Fig. 1.2 John Snow's map of cases of cholera during the 1854 outbreak in Soho. Broad Street is in the centre and the offending pump is in the centre of Broad Street. John Snow kept on trying to persuade his medical peers that this looked like a disease that was spread in the water rather than the air, but still without success.

From Snow J., *On the Mode of Communication of Cholera*, CF Cheffins (Lith.), Southampton Buildings, London (1854). Public domain via Wikimedia Commons. https://en.wikipedia.org/wiki/File:Snow-cholera-map-1.jpg

The easy explanation—careful, empirical research

The story of John Snow works on many levels as an illustration of how research works. It begins with a pre-existing idea (miasma), which may be popular but hasn't really been put to the test. And it begins with someone who has thought long and hard enough to be sceptical. And that person might well be sceptical because they have been taking careful observations and finding that they didn't really fit with the current theory. But the next stage is not a blinkered attempt to marshal evidence in the opposite direction; rather, it is an objective investigation designed to test the competing theories one way or another. If a disease is transmitted through the air, then you would imagine

quite a diffuse pattern of cases radiating out from an area or following the prevailing wind direction (as seen, for example, in the aftermath of the 1986 Chernobyl radiation leak). Diseases transmitted by water should instead cluster around the offending water sources, so a simple mapping exercise ought to be able to distinguish the two, as it did for Snow. Finally, there is the actual experiment—in this case, the removal of the pump handle—taking an action and observing the consequences (the epidemic subsiding).

So, there are important principles here: of observation that is careful and methodical, objective, and unbiased, and of putting ideas to the test. This 'scientific method' is not just confined to medicine or even to physical science more broadly; it is fundamental to all worthwhile research. A good scholar in any field will collect evidence and consider objectively whether it supports a prevailing theory or suggests a new one. For example, a historian might be weighing up whether Elizabeth I really planned never to marry or was forced into that position; an English scholar might be considering how much Mary Shelley's work was influenced by recent experiments in electricity; an economist might be investigating whether state intervention is a good thing or not at a time of recession; a sociologist might evaluate whether living in a tight-knit community is supportive or stifling. These processes of assembling and testing evidence are essentially how we have accumulated shared knowledge as a species. But is this really the way things work? For example, why can it take so long for knowledge to be adopted, why are 'experts' often viewed with suspicion, and why does it seem that some controversies are never resolved?

The messier explanation—politics and public opinion

Even John Snow's story is not a clear-cut example. The Broad Street pump handle was replaced as soon as the cholera epidemic had resolved, there was no attempt to correct the underlying contamination, and it was 12 years (and several more outbreaks) before William Farr (the 'father of modern statistics' mentioned earlier and one of Snow's chief opponents) accepted that water was the problem and, having collected some supporting evidence of his own, recommended that water be boiled. In the meantime, it was the 'great stink' of the summer of 1858 (Figure 1.3)—an unpleasant combined effect of sewage, industrial pollution, and hot weather on the river Thames—which led to the construction of London's still-monumental sewage system, and even this was mainly done to remove the smell rather than improve the cleanliness of the wells or river water. So, does that mean that it was miasma theory making

Fig. 1.3 An 1858 lithograph on the state of the River Thames in London. The 'Great Stink' occurred in the summer of that year when extended high temperatures caused a fall in the water level, exposing the raw sewage on the riverbanks. Queen Victoria and Prince Albert were forced to curtail a pleasure cruise and Members of Parliament debated whether they would have to conduct business elsewhere. All of this resulted in a massive sewer-building project, sorting out the health effects of water pollution by applying miasma ('bad air') theory. John Snow's alternative waterborne theory for cholera transmission wouldn't become accepted until 10–20 years later.

The Silent Highwayman (1858); Original: Cartoon from *Punch Magazine* 35: 137; 10 July 1858 This copy: City and Water Blog. Public Domain, https://commons.wikimedia.org/w/index.php?curid=4465060

the difference in the end, even though it was incorrect? And was Snow really the unbiased, objective observer if he was already writing articles suggesting water-borne transmission? Either way, he didn't live to see the public health improvements he had argued for, dying himself in 1858 at the age of 45 just a month before the 'stink'. He is buried in Brompton Cemetery, and the John Snow pub and replica water pump can still be found on Soho's Broadwick Street, close to where the original outbreak occurred.

It would be nice to think that this worthy piece of research really made the difference, but you do wonder whether it was only honoured retrospectively once others had come around to the idea via other paths. How much was Snow's research ignored at the time because prim Victorian society wasn't prepared to discuss faecal contamination of drinking water? How much was

this compounded by cholera's non-appearance in contemporary novels? How much did William Farr hold out against John Snow's theory simply because it was someone else's research—needing to make his own observation? Or how much was a change in understanding delayed because 'miasma' is such an ingrained idea, dating back beyond Hippocrates to principles of medicine from ancient India and China?

Stories are important and old beliefs persist for powerful, often instinctive, reasons. The bacterium responsible for cholera was identified by an Italian anatomist in the same year as the Broad Street epidemic, but this finding was also ignored because it didn't fit in with current thinking. Indeed, it wasn't until the mid-1870s that miasma theory died, and germ theory took its place as the main explanation for infectious diseases, and this was mainly because of discoveries in a different disease area (anthrax). Even the story of John Snow is sometimes exaggerated for effect—the epidemic on Broad Street was probably already subsiding by the time he removed the pump handle. So actually, research is not just a matter of careful observation and experiment; it is also determined by the power of the story, by politics, by money and power, by personalities, and perhaps (above all) by the *zeitgeist*, as well as the natural tendency we have to hang on to the beliefs we have grown up with, and a scepticism (perhaps sometimes healthy; other times less so) of self-appointed bodies of experts. An abstract description of scientific theory and knowledge acquisition, without considering these eccentric and complicating influences, may be fine for researchers embarking on their first project, but it does a disservice to anyone interested in what we think we know now and the many journeys we have taken to get here.

2
Origin Stories

We all do research in one way or another, whether we mean to, or not—how else are we to find out about the world? If a young child comes across a switch that turns a lamp on, she won't be satisfied with one trial; she will switch it on, then off, then on, then off . . . until an adult realizes what she's up to and unplugs it. Understanding cause and effect is best achieved by repeated experiments and early life is a time for extensive experimentation—making faces at your parents until you accidentally smile and find them smiling back; making any old noise with your mouth until you find that it coincides with the words that you're hearing (and gets results in the form of praise or that toy on the shelf you can't reach); working out how hard you have to poke your younger brother until he cries; hitting a window with a plastic hammer until you crack it and find out exactly how disappointed your parents are.[1]

As you grow older, the experimentation dies down a little—you've figured out the basics of the world around you and are getting used to being human; however, the research still continues, just in a more complex way. If you meet a person for the first time, you form an initial impression of them—a hypothesis, you might say, and something that's tied in with expectations and assumptions (if you do/say X, how will they respond?). Then you get talking to them and your initial impression might need to change (refining your hypothesis), and so on as you get to know them better. You might even have a few stock questions that help accelerate your understanding of the other person (politics, football, music, whatever you're into) and which you can drop into the conversation among the more mundane and less informative chatter about work, the weather, the price of coffee, etc.

Clearly the location or context for these complex personal experiments is going to influence their accuracy, hence the risk of speed dating, or of over-commitment following an encounter at a crowded nightclub (a poor-quality set of observations resulting in an incorrect conclusion only too evident the morning after). And of course, so many problems of the world stem from

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[1] Sorry, close to home, that one, but then childhood vandalism is perhaps easier to deal with if you re-frame it as the beginnings of a potential research career.

us getting our individual research wrong—all sorts of misunderstandings, disagreements, fights, and wars. If you've been on unconscious bias training (and, if you haven't, you ought to), you'll know about the problems with that all-important initial impression: how you may hold all sorts of false assumptions based on the other person's skin colour, gender, clothing, accent, etc. You might struggle to recognize many of these assumptions (hence the 'unconscious' label), but they'll result in inappropriate and unfair comments or actions unless you try to regain objectivity—or, if you want to put it that way, unless you attain the dispassionate perspective of the researcher. So, academic institutions have no excuse at all if they're found to have discriminatory career structures; everyone working there ought to be an expert in forming unbiased objective opinions. If only that were the case . . .

On the health benefits of the research perspective

Taking this a step further with a more specific example, cognitive behavioural therapy (CBT), the most common talking treatment for depression, is essentially based on self-examination and adopting a researcher's perspective. The idea behind the 'cognitive' part of CBT is that a lot of our anxiety and depression stems from 'negative automatic thoughts'—false assumptions about ourselves and our experiences. So, if you're at a low ebb when your boss looks irritable one morning, it's easy to slip into assuming you've done something to make them feel that way. But, if you force yourself to think about it objectively, there may be all sorts of other explanations. So, CBT places a strong emphasis on keeping diaries, recording thoughts like this, and weighing up the evidence for any underlying assumptions. Working with a good therapist (it's hard to be objective on your own), you might remember that your boss did mention having a bad weekend, so perhaps that's a more likely reason for their irritability. And you might remember that you were praised for your work the previous week (and it's easy to forget praise when you're feeling low). Or perhaps you didn't receive praise but everyone else in the office thinks the boss is grumpy and there's nothing unique about your own situation. Whatever the alternative explanation, the more you work on these thoughts, the quicker you're able to catch them when they occur next time and the less likely they are to bring you down. But really the process is rather like becoming the objective researcher looking in on yourself, making a careful observation (the incident, the thoughts you were having at the time, and the feelings that accompanied them), identifying the different possible conclusions, and weighing up the evidence for each. The process isn't necessarily easy (it's very hard to be unbiased) but then, neither is research.

What is research, anyway?

Of course, this is all very well, but are we really talking about 'research' here? Well, perhaps. The Oxford English Dictionary defines research as the *careful study of a subject . . . in order to discover new facts or information about it*. The 'careful study of a subject' part of that definition does tend to imply something broader than a child learning what a light switch does, although it doesn't absolutely exclude it. We like to think of research as seeking universal truths that, in time, will become accepted and contribute to new knowledge and a change in the way people view the world. However, we don't have to look very far to find examples where that sort of widely held robust evidence comes up against personally held truths and it all gets rather messy. We'll revisit this later (Chapters 16, 17), but it's important to begin by acknowledging that the definition of research is actually rather vague and that what we now call research is a process that has emerged gradually and almost imperceptibly. At some (also rather hard to define) point, it began to become aware of itself and its need for an identity, and it wouldn't be unreasonable to say that it's still finding its feet, whatever the 'experts' say.

To begin with, experimentation and discovery aren't even exclusively human activities. Bees fly out to explore the world around them, discover the best flowers, and return to dance in the hive and communicate their findings to the rest of the workers. Thrushes find a convenient rock to break open snails and octopuses collect coconut shells to use as shields. Chimpanzees make sticks into weapons and moss into sponges—again, not research as we would normally define it, but you can't really watch animals working on a problem and not think of it as careful study to discover new facts. All that we can say is that at some point in the last 300,000 years or so, *Homo sapiens* became better at research than other species, or at least better at finding ways to pass on knowledge and technology so that the next generation could advance without waiting for genetic evolution to happen.

Of course, the origins of human research are lost in prehistory because a lot of the passed-on knowledge would have been verbal or pre-verbal. At some point someone discovered a way to make fire, but even that discovery might have been lost a few times before it became widespread knowledge—there may have been any number of our ancestors who were as gifted as Einstein but whose knowledge died with them and their tribe. Thor Heyerdahl, the Norwegian ethnographer, carried out a series of voyages in primitive boats to show that prehistoric intercontinental travel was at least feasible, and argued reasonably that a society's lack of sophistication can't be assumed just because it didn't leave a written record. Supporting this, it's highly likely that advances

were made independently in several places. For example, the transition from hunter-gatherer to agrarian societies (i.e. the discovery of cultivation and all the knowledge and technology that needs to be in place to make it work) occurred in a variety of world locations (Mesopotamia, the Indus valley, China, Central America) within quite a short period of time and with no likelihood of any one society learning from another. Simultaneous discovery has also been quite frequent throughout the history of science—Hooke and Newton for gravity, Darwin and Wallace for evolution by natural selection, Scheele and Priestley for oxygen, among others. There's definitely something about the time being right for strands of knowledge to come together, so that it's almost accidental who happens to make the advance, although it may also depend on who's devious and self-promoting enough to ensure their legacy (more about Isaac Newton later in Chapter 16). What we have to assume with prehistory is that there were enough researchers around in the world to make the same discoveries when the time was right: for example, at the end of the last ice age 10,000 years ago when there was time to settle down. Of course, it doesn't always follow that an advance will be universal—the Inca civilization famously reached the sixteenth century with sophisticated civil and hydraulic engineering, but with no carts or other wheeled vehicle. They appeared to know about the wheel as a concept; they just didn't feel the need to take it any further.

Mathematics—where it all started

Mathematics has as good a claim as any for being the oldest academic discipline—or at least the oldest one to be written down. Once you start settling in one place to grow crops, you also tend to start trading with neighbours. This requires a number system and the ability to add and multiply; lengths and weights start to be used in manufacture; anticipating seasonal events like temperature and rainfall changes requires calendars and astronomy; any significant building or engineering work requires geometry. As your civilization develops you need to communicate larger numbers, so have to decide on a 'base' system for this. The decimal (base 10) system we use today is a logical extension of finger counting and seems to have been present in early Egyptian and Indus Valley civilizations from at least around 3000 BCE, but there were alternatives, and it wasn't inevitable that we followed that route. A sexagesimal (base 6) system divides up in more convenient ways than a decimal system (e.g. into thirds and quarters) and was favoured in Mesopotamia and Babylon; this persists today in the way we measure time

(60 seconds in a minute) and angles (360 degrees in a circle). The system we have today of repeating the same symbols for multiples of our numbers (e.g. making the number ten out of a 1 and a 0 rather than its own new symbol) also has its origins in the Sumerian civilization in Mesopotamia. Numbers in Egypt at the time were more like what would become Roman numerals, but you can see that doing a long multiplication is a lot easier when it's written out as 62×19 than as LXII $\times$ XIX, so it's not surprising that the Sumerians were more advanced in mathematics than the Egyptians—and that the Romans had little use for it when their empire came about. In turn, this illustrates the way in which research has been closely tied to the civilizations or societies that foster it. Early advances in mathematics seem to bounce around between Greece, Babylon, India, and China—presumably spread and exchanged along old trade routes—and certainly, a great deal of progress had been made by the time it started becoming an academic discipline with a surviving written record. For example, the theory attributed to Pythagoras (about the square on the hypotenuse) was probably well-known in Babylon long before the Greek civilizations got going and a thousand years before Pythagoras was born, even if the proof of the theory did come later.

Knowledge for the sake of it

When your civilization is evolving, most research is driven by immediate needs—that is, working out how to grow new crops, put up better buildings, develop irrigation systems, or make new weapons. And there's likely to be a certain amount of natural selection favouring the civilizations with these research-driven advances. For example, the Hittites developed chariots and smelted iron (rather than bronze) weapons, which helped them carve out an early empire in Turkey around 1300 BCE.[2] At some point, however, a society emerges where there's a bit of free time to start thinking more conceptually—that is, without the need for relentless application of research.

Ancient Greece was a good, if short-lived, example, although this may be just because its records and legacy have been more accessible. In mathematics research this relative freedom was reflected in knottier problems being considered for their own sake, rather than because the government was banging on the door wanting better designed ships or chariots. Zeno, for example,

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[2] Although this itself was quite a recent discovery, as the Hittites as a people were only mentioned cursorily in surviving texts such as the Old Testament or Egyptian accounts, and they weren't known to Greek historians. In the late nineteenth century, a hypothesis was proposed about an early empire in central Turkey which was subsequently supported by archaeological investigation (see Chapter 9).

was a philosopher living in the Greek-speaking city of Elea in southern Italy around 450 BCE (apparently visiting Athens at one point when the philosopher Socrates was a young man), and was particularly known for setting paradoxes, which came to be considered as mathematical challenges for subsequent generations, even if he never viewed them that way himself. One of the more famous ones concerns a race between the hero Achilles and a tortoise, where Achilles is clearly much faster, but every time he reaches the point where the tortoise was when he started out, the tortoise has moved a finite distance on from that point; he gets to where the tortoise is next, but the tortoise has moved further, and so on. Therefore, from a certain way of looking at it, Achilles never reaches the tortoise. It's all bound up with finite and infinite segments and these sorts of paradoxes have been used over the years as a way of challenging mathematicians to improve their calculus. However, it's a good early example of how an academic subject can be pursued for its own ends with no thought of it being of any practical use, helped by a period in a society that allowed space and time for thought—in this case, by relatively wealthy men from a slave-owning aristocracy.

Unfortunately, these periods of tranquillity didn't tend to last very long, and aristocrats with time to think tended to come into conflict with their rulers in the end. As the story goes, Zeno was implicated in a conspiracy to overthrow a tyrant; after being tortured to name his fellow conspirators, he claimed to have an important secret, persuaded the tyrant to lean in closer, then bit off his ear (or nose) as a final act of defiance.

Many homes

The rest of the early history of mathematics follows the rise and fall of empires in its progress. Zeno and colleagues are succeeded fairly soon afterwards by the Pythagoreans, following the rather obscure figure of Pythagoras who gives his name to a number of ideas that probably weren't his own.[3] This school, in turn, has a strong influence on the Athenian philosophers Plato and Aristotle (about whom more later) and then the first substantial textbook, *Elements*, is written by Euclid at Alexandria in Egypt, around 300 BCE. During the Roman Empire, despite little interest in mathematics by the power base itself, relative peace and ease of travel allows the East–West exchange of ideas to continue, fortuitously coinciding with a similarly tranquil period in China under its Han dynasty. Alexandria continues as a

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[3] Mystical as well as mathematical—the migration of the soul after death and a rather nice theory that planets and stars move in a way that produces inaudible music: the 'harmony of the spheres'.

centre of learning, supporting this exchange. Nothing lasts, however, and the Christianization of Europe results in the suppression of mathematics because of its close associations with paganism. Old texts are kept in Constantinople at the heart of the Byzantine Empire, but not particularly developed. In the meantime, the centre of activity moves to India in the hands of the mathematician-astronomers Aryabhata (around 500 CE) and Brahmagupta (around 625 CE), although possibly borrowing a lot from China. Around this time the decimal position system becomes standard, as does the use of the number zero. Later, the field is taken up in the Islamic Sassanid empire and the 'Baghdad House of Wisdom'. The mathematician Muhammed ibn Musa al-Khwarizmi (around 825 CE) ends up translated and his latinized name gives us 'algorithm' (Figure 2.1); similarly, the translated title of his *Hisab al-jabr wal-muqabala* (science of equations) gives us the word 'algebra', as well as bringing Indian numerals to the West. Other key figures are Omar Khayyam (around 1100 CE), who works on cubic equations in northern Persia as well as writing his *Rubaiyat* and other poetry, Ibn Al-Haytham (around 1000 CE), who writes influentially on optics in Cairo, and Al-Zarquali (around 1060 CE), who makes major advances in astronomy in Cordoba. Further east, there's a lot of mathematical activity during the Song (960–1279 CE) and Yuan (1271–1368 CE) dynasties in China (Figure 2.2), although not all information comes west. For example, the mathematician

Fig. 2.1 The title page from *The Compendious Book on Calculation by Completion and Balancing* by Muhammad ibn Musa al-Khwarizmi. Written in early ninth-century Baghdad, it not only gave the world 'algebra' as a term (derived from the Persian *al-jabr* representing the 'completion' or 'restoration' in the book's title), but also laid down many of the principles for that discipline, including solutions for linear and quadratic equations. About half-way down the page is the prayer, 'May God benefit righteous ones with his knowledge and deeds'.

Source: John, L. (ed) (1999). *The Oxford history of Islam*. Oxford University Press, Oxford. Al-Khwarizmi, Public domain, via Wikimedia Commons.

Fig. 2.2 Zu Chongzhi was an astronomer and mathematician who worked as a politician for the Liu Song dynasty in fifth-century China. His main text, *Zhui Shu* (Methods for Interpolation), was lost in the subsequent centuries, and might have contained solutions for cubic equations. He is believed to have calculated the number pi to six decimal places, holding the record for accuracy, until this was exceeded by the Persian Jamshid al-Kashi in the fifteenth century.

Statue of *Zu Chongzhi*. 中文(简体): 祖冲之铜像。(2016). Attibution: 三猎, CC BY-SA 4.0 https://creativecommons.org/licenses/by-sa/4.0, via Wikimedia Commons.
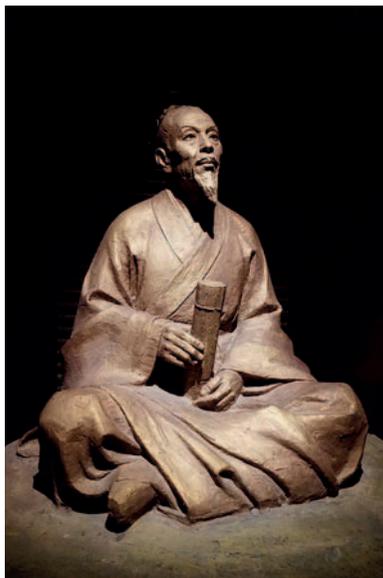
Qin Jiushao finds solutions to complex equations in 1278, while working as a (rather corrupt) government official in Shangong province, that won't be solved in the West until 1819, by the British mathematician and schoolteacher William George Horner, unaware of the much earlier Chinese method. Similarly, Zu Chongzhi, a fifth-century politician, astronomer, mathematician, and inventor, estimates the number pi to an accuracy that isn't exceeded for nearly a millennium.

Back in mediaeval Europe, mathematics research is less impressive. Hypatia (Figure 2.3), a gifted philosopher, astronomer, and mathematician is widely respected in Alexandria, but she falls on the wrong side of a political intrigue and is killed by a Christian mob in 415, possibly lending her pagan story to the Christian legend of St. Catherine (of Catherine wheel fame). Boethius, a philosopher in Rome under Gothic rule, writes mathematical texts in the early 500s that are considered as standards in the West for a millennium; like Hypatia, he also ends up executed for political reasons, but his Catholicism allows a martyrdom story to emerge and help ensure his legacy. Alcuin, the Northumbrian polymath at the court of the Frankish emperor Charlemagne in the late 700s, writes a series of logical problems that can just about be called mathematical. And then there's interest from some of the monastic orders and early Christian philosophers (e.g. Origen, St. Augustine, Thomas Aquinas), but nothing to rival the activity in the East. In time, the translation (often by Jewish interpreters) of Islamic texts from
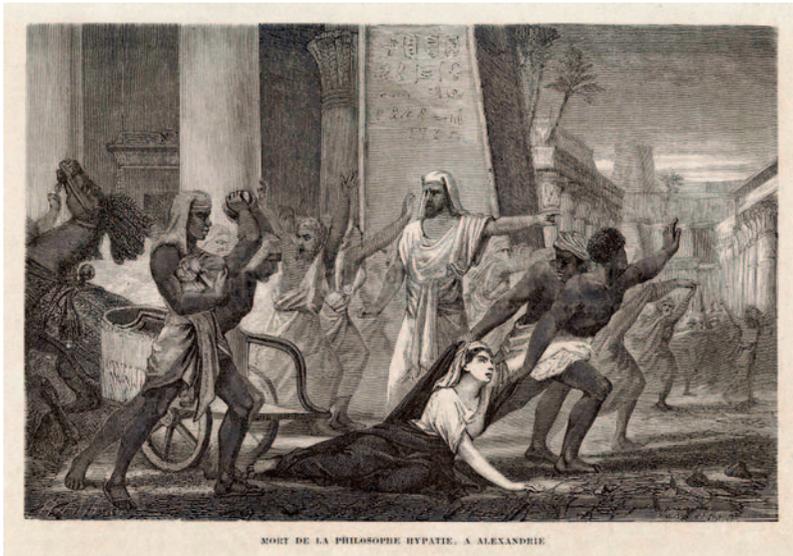
Fig. 2.3 Hypatia was a philosopher and mathematician living in fourth- to fifth-century Alexandria. Unfortunately, her reputation and wisdom meant that she was sought as an advisor to the Roman prefect of Alexandria at the time. Her influence, and the fact that she wasn't a Christian, set her at odds with Cyril, the bishop of Alexandria. As a consequence, was murdered by a Christian mob in 415 CE. She was re-invented in more recent centuries as a martyr for philosophy/science and as a proto feminist; however, this nineteenth-century image of her 'martyrdom' clearly shows the dangers of re-invention with its blatant (and borderline-absurd) racist overtones. Perhaps it's better simply to accept that Hypatia was a gifted intellectual who got caught up in turbulent politics just as anyone else might have been, and who just happened to be female and non-European.

Mort de la Philosophie Hypatie. From Figuier, L. (1872). *Vies des savants illustres, depuis l'antiquité jusqu'au dix-neuvième siècle*. Hachette, Paris. Public domain via Wikimedia Commons. https://commons.wikimedia.org/wiki/File:Mort_de_la_philosophe_Hypatie.jpg

reconquered Spain leads to refamiliarization with Greek and Middle Eastern learning, as do the developing trade routes between Italian cities and the Arabic world, and the growth in banking and early capitalism. Leonardo of Pisa (also known as Fibonacci) writes his *Liber Abaci* in 1202, firming up the use of the Hindu–Arabic numerals we use today, although this takes a while to catch on. Then, the Byzantine Empire starts to collapse, and Greek scholars and manuscripts from Constantinople begin travelling westwards to Italy, where rival city states are setting up centres of learning and keen to kick-start the European Renaissance.

Why 1500?

So, we've reached the point now where histories of science, and research more generally, tend to start—the Western European Renaissance and the 'Enlightenment' period that followed. But it's worth stopping to consider exactly *what* started around that time. As discussed, research itself has probably been going on throughout humanity's development, and it seems reasonable to assume that all the major discoveries along the way—fire, the wheel, agriculture, bronze, iron, glass, etc.—were the result of at least some sort of systematic enquiry and study. In the emergence of mathematics as a discipline, we know that there were also centres of thought, learning, and the sharing of information, as well as some level of communication between these centres—albeit a little fragile and dependent on short periods of peaceful trading in between the wars and revolutions. So, what's special about the Renaissance? Most importantly, when textbooks imply that everything starts off in the fifteenth and sixteenth centuries, are they just propagating a Eurocentric historical viewpoint, ignoring all the foundations of knowledge 'borrowed' from earlier Middle Eastern, Indian, and Chinese work? There are historians much better qualified than I to judge. However, most people like to start a story somewhere and there is certainly something that changes significantly from the Renaissance onward. For example, although a mathematician might be able to read Euclid with a sense of fellow-feeling and appreciation, most other academic disciplines would struggle to find very much of direct relevance before 1500.

What fundamentally changes from around 1500 onwards is an understanding of how the world works, and a new way of developing that understanding. Early on, this is in astronomy and the painful process of accepting that the Sun, rather than the Earth, is the centre of the solar system. At around the same time, people are beginning to investigate the workings of the human body, particularly the circulation of blood. Telescopes lead to microscopes and the beginnings of biology. Further application of mathematics to astronomy leads to gravitational theory, laws of motion, and the emergence of physics. Vacuum flasks enable the composition of the air to be investigated as the beginnings of chemistry. Geological researchers start to question the age of the Earth. Naturalists (prototype botanists and zoologists) begin the process of identifying species and think about how they might have emerged over time. And so on. These happen to be a list of scientific disciplines, but the change can be seen in other fields of research as well—for example, in historians moving from simply recording events as they hear about them towards considering them more analytically and taking the quality of evidence into

consideration. Archaeology emerges as a means of gathering and analysing new information for historians, just as palaeontology does for natural scientists. Other humanities disciplines start emerging with their own research activity, applying approaches developed by historians, at least to begin with. And alongside it all, mathematics continues to progress, problem by problem, and it increasingly finds itself applied to other emerging specialties.

Of course, there are pre-existing ideas already explaining a lot of natural phenomena—it's just that a lot of them are just plain wrong (or 'inconsistent with evidence' to be more polite). And it isn't generally until after the European Renaissance that the alternative viewpoints we take for granted today begin to emerge. What most of these have in common is the search for an understanding of cause and effect.

3

God or Clockwork?

The Importance of Ignorance

In her weighty 1870s classic *Middlemarch*, novelist George Eliot presents a fairly scathing portrait of an academic, the Rev. Edward Casaubon—a bone-dry, emotionally impoverished, and increasingly embittered scholar. He can't bring himself to submit his work for publication, so ends up accumulating volumes upon volumes of research that will ultimately die with him and probably rightly so. For anyone 'in the trade' it makes for salutary and uncomfortable reading. Casaubon's failed magnum opus is called *The Key to All Mythologies* in which he is trying to find commonalities in the multitude of stories and belief systems, a task that overwhelms him and allows Eliot to muse uncompromisingly on personal failure and a wasted life. However, within 20 years of *Middlemarch* (or, strictly speaking, within 60 years of the novel's setting in the 1830s), James Frazer achieved just what Casaubon couldn't, publishing a comprehensive account of magical and religious beliefs that was anything but unnoticed.

Frazer (Figure 3.1) was born in Glasgow in 1854 but spent most of his working life at Trinity College, Cambridge pursuing his research in the emerging field of social anthropology. He carried out very little direct observation, travelling no further than Italy and Greece; instead, he collected information by reading widely and corresponding with missionaries and officials across the British Empire of the time, assembling whatever stories and traditions he could lay his hands on. From all of this came *The Golden Bough*, running in the end to twelve hefty volumes, putting even Casaubon in the shade; despite this, it was considerably influential.

The work begins with a description of a tree growing in a grove sacred to the Roman goddess Diana. From this and the sacrificial rites described in ancient texts, Frazer goes on to consider at great length the themes that underlie religions, customs, and superstitions all over the world. His work presents a variety of material ranging from the magical properties of things that don't touch the ground (e.g. mistletoe for druids) to days when social norms are overturned for a limited time (e.g. Carnival), as well as stories of kings or gods who die and come back to life—which understandably generated some
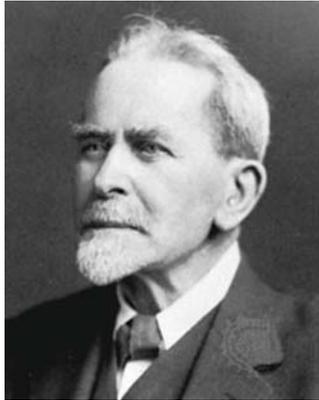
Fig. 3.1 James Frazer, the Scottish anthropologist, set himself the fairly formidable task of compiling and making sense of all the traditions and superstitions he could lay his hands on without travelling much. The resulting 12 volumes of material were influential beyond his specialty (e.g. Sigmund Freud was, unsurprisingly, interested in his examples of totemism). His successors in anthropology weren't so keen on his methods, but the fact remains that these early beliefs of how the world might work as an internally controlled system are the origins of what we now call Science.

Contemporary photograph of James Frazer, 1933. http://www.britannica.com/EBchecked/art/ 10799/Sir-James-George-Frazer-1933#tab=active~checked%2Citems~checked,   Public   Domain, https://commons.wikimedia.org/w/index.php?curid=4250992

controversy at the time. As a work of research, it's not the finest—while it inspired the emergence of anthropology, it became quite rapidly disowned by that discipline. One of the problems is that Frazer's conclusions don't feel particularly dispassionate or objective; instead, there's a distinct impression of someone trying to squeeze observations together to fit in with preconceived ideas. On the other hand, it's not a bad read (the abridged single volume version at least) and I wouldn't be surprised if at least some of his critics were just jealous of his influence, particularly on twentieth-century literature and as a bit of an atheist's poster boy. It's not uncommon for academics to be suspicious of anything that's readable and popular.

The first decision

So, what has *The Golden Bough* got to do with causation? Well, if research has been going on since prehistory, and if working out causes and effects has been a continued preoccupation (after all, how else do we learn to understand the world?), then the same quest for understanding might be seen in customs and

rituals. Early on in his book, Frazer asks us to imagine a situation where we're in a primitive society with no knowledge of the world but a pressing need to understand it. For example, we've begun growing and relying on crops and need to know about the changing of the seasons. If the rains come, everything's fine; if they don't, we starve. So, it seems sensible to try to work out whether there's anything we can do about it. Frazer's proposition is that we make a fundamental choice at this point between two routes of enquiry. One possibility is that the cause of the rainfall is something external to the world—for example, a god who can turn the taps on and off in the sky. The other possibility is that the cause is internal, some sort of system within the physical world. The choice we make here determines our course of action. If we think an external being controls the rainfall, then it's obvious that we need to get in a right relationship with him/her; hence, this takes us down the route of religion. On the other hand, if we think that there's some internal process involved, then we ought to be working out what it is because we might be able to influence it—after all, if we've already started diverting water courses, creating paddy fields, or building fences to keep out predators, why not figure out a way to control rainfall?

Frazer proposes that this second choice of an 'internal system' gives rise to what he calls 'magic' and is reflected in the wide variety of customs and superstitions compiled in his book. All you have to do is try to find the root from which they've arisen. I'm not sure how original this idea was, and Frazer rather over-eggs his arguments at times, but it does make sense as a general principle. For example, quite a lot of traditional rain dances have involved people beating the ground with sticks and there's an obvious logic underlying this. If you notice that the rains tend to fall after the ground has been dry and parched for a while, and if the ground looks like it's in pain when it's dry and parched, then it's reasonable to assume that the ground's pain might influence the rains arriving. Therefore, if you inflict even more pain on the ground by hitting it, perhaps the rains will arrive earlier. The explanation might be wrong, but it's a reasonable one if you don't know anything else about how the weather works. Like any superstition, the action (the rain dance) is followed by the consequence (the rain) often enough to reinforce it, and once the ritual is set up, it's hard to break. It may be a noble idea to try missing out the dancing for a year and see whether the rains still arrive, but who's going to risk it if you might starve as a consequence? And how many actors and sporting celebrities freely admit to their own personal rituals for similar reasons before a performance (and reluctance to experiment by omitting them)? And how many of us haven't done the same before an examination, or don't have some superstition we're aware is irrational but follow,

nonetheless? Also, if the rain dancing becomes part of a comforting seasonal routine, a chance to get together, to drink alcohol, and conduct business, why worry about dubious evidence for whatever cause–effect assumption started it off in the first place?

Looking to the skies

The point is that this internally controlled system (the 'clockwork' of this chapter's title) is what research also focuses on, and its task from 1500 onwards has been to emerge from a world explained by magic and super-stition, or at least by 'any old idea'. For example, the way we understand the planets and stars didn't start from scratch in the European Renaissance; it started from a very old belief that the Earth was at the centre of a concentric series of orbiting bodies and that this explained what humans had been care-fully observing for thousands of years in the night skies—which, of course, were a lot more visible and personal in the days before light pollution.

In Dante's *Paradiso*, written around 1320, the author ascends into heaven through its nine spheres (Figure 3.2).[1] Starting with the Moon, Mercury, and Venus, Dante passes the Sun as the fourth sphere, then moves on to Mars, Jupiter, and Saturn, before the known planets end and are followed by a sin-gle sphere containing all the stars.[2] The final ninth sphere beyond the stars is called the *Primum Mobile*, the mechanism that keeps the heavens in motion, and beyond this is the *Empyrean* where God lives. This widely accepted sys-tem was drawn up by Claudius Ptolemy, a man of many talents living in Alexandria in the early second century. Ptolemy claimed to be drawing on over 800 years' worth of observations and calculations by Babylonian and Greek astronomers and produced tables to compute past and future positions of the planets, as well as an updated chart of stars in the northern hemi-sphere. Having been preserved in Arabic, his *Almagest*[3] was translated into Latin in 1175 and became highly sought after. Despite a veneer of religion, Europeans were very fond of astrology—the idea that stars and planetary alignments had influences on world events. Therefore, anything that pre-dicted the positions of heavenly bodies was destined to be popular and several famous pioneers of modern astronomy had to earn their living by writing horoscopes. Anyway, Ptolemy's system worked, more or less; the problem

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[1] Conveniently matching the nine circles of Hell going down to the centre of the Earth, through which he'd travelled only a week previously in his *Inferno*—it was an eventful month for him.
[2] And the fixed sphere of stars is fair enough—the stars act most of the time like smaller and larger points on a single sphere and it was centuries before interstellar distances became apparent.
[3] *Almagest* means 'the greatest'—one way of describing a best seller.

Fig. 3.2 The mediaeval belief of an Earth-centred planetary/solar system appears with all its splendour in *Paradiso*, the third part of Dante's Divine Comedy completed in 1320. Here, Dante and his guide Beatrice climb up past the seven planetary/solar spheres and the fixed stars to reach God's home, the Empyrion, on the other side. When Copernicus and Galileo started chipping away at the Earth-centred system, they came into conflict with the Church (both Lutheran and Catholic), which was perversely but doggedly following a pagan model drawn up by Claudius Ptolemy in second-century Egypt. However, you can understand why Dante might have been disappointed.

Gustave Doré. (1892). Canto XXXI in H. Francis (ed), *The Divine Comedy by Dante, Illustrated, Complete*. Cassell & Company, London/Paris/Melbourne. Public domain via Wikimedia Commons. https://upload.wikimedia.org/wikipedia/commons/d/d2/Paradiso_Canto_31.jpg

was that it had to incorporate some rather clumsy corrections to account for inevitable deviations in planetary and stellar movements. Furthermore, there were inconsistencies in the Moon's movements, which everyone did their best to ignore. So, Ptolemy can sit reasonably with any other scientist or

researcher—he made observations (or at least reproduced other people's) and produced a theory to explain them. When Nicolaus Copernicus published his alternative theory, all he was saying was that a Sun-centred model offered a simpler explanation without the need for all the workarounds.

Making a start

Copernicus is often cited as the first modern researcher, although it's fair to say that he doesn't quite fit the mould. Yes, he took a rather problematic popular view of 'how the world works' and proposed a better solution as an alternative, but he didn't try to test this or make any observations of his own. Also, the idea of the Sun as the centre of the solar system wasn't new[4] —it had been brewing for a while in the period running up to the Renaissance, so would probably have emerged anyway around that time. Copernicus also didn't put much effort into communicating his ideas; he had probably formulated most of them over 30 years before publishing his snappily titled *De Revolutionibus Orbium Coelestium* ('on the revolution of the celestial spheres') in 1543, so it's rather a matter of chance that his name is so prominent; not that it made a difference to him—he died the same year.

One of the reasons for him taking so long to publish was that he was a busy man with lots on his mind—he was the canon of a cathedral in Poland, and for a while the commander of a castle, seeing off an invasion of Teutonic knights from Prussia. There was no such thing as a research career in those days and an income had to come from somewhere. Another reason for the publication delay was that there were still problems with the idea that the Earth was in motion rather than sitting stationary in the middle of everything else—for example, if your hat was blown off when you leaned out of a carriage window, why wouldn't the Earth (moving much faster) result in hurricanes? Why weren't the oceans being whipped up into tidal waves? Why did objects still fall straight down to the ground rather than backwards as you'd expect from a moving platform?[5]

It's also possible that Copernicus sat on his findings because he was worried about controversy and the views of the Church, although this has generally been overplayed because of the later hostility experienced by Galileo. At the time Copernicus was beginning to communicate his theories privately, these

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[4] It had been proposed, for example, by the Greek astronomer Aristarchus around 250 BCE.
[5] This was before anyone had actually tried dropping things off heights while in motion. It wasn't an easy experiment to carry out, although was eventually managed from the masts of boats moving on very calm water.

were discussed with interest at the Vatican and a cardinal there wrote back, urging him to publish (a letter he included in the finished work). It was actually the early Protestants, beginning with Martin Luther himself, who kicked up the most fuss. The Catholic objections only came later on because of a heretical sect, Hermetism, emerging around that time. This cult was keen on some supposed mystical teachings from Ancient Egypt and was therefore understandably enthusiastic about the idea of a semi-deified Sun at the centre of the universe. Giordiano Bruno, a prominent member, was caught by the Inquisition and burned at the stake in 1600, and Copernicus' *De Revolutionibus* had joined the Vatican's list of banned books by 1616, lingering there until someone saw sense in 1835.

The old order of things

If Renaissance astronomy had a struggle to achieve a turnaround in thinking, at least it was up against a single competing model, and at least there was an acceptance of the mathematical principles governing the observations (e.g. equations for ellipses). Chemistry, on the other hand, had to strip itself of layers of mysticism and diverse traditions before it could start calling itself a science. The world of alchemy is a fascinating glimpse into what happens when imagination runs riot, and the psychoanalyst Carl Jung theorized at length on what it revealed about the human subconscious.[6] However, you don't get the impression that much of this early chemistry was based on dispassionate or objective experimentation. The accepted knowledge that had to be challenged included theories that gold was something that could be found if impurities were removed from other metals (obviously a potent economic driver behind alchemy), and that matter was composed of mixtures of the four elements: earth, air, fire, and water (popularized by Aristotle in Ancient Greece but a common belief across the ancient world). Later blind alleys for the sciences, mostly originating in the seventeenth century, included 'phlogiston' (a new element proposed to explain weight gain during combustion and corrosion before oxygen was discovered), or the view of the philosopher Descartes that there was no such thing as a vacuum and that planets moved in a fluid, or the idea of 'caloric' as an invisible liquid that allowed heat transfer between objects. And then of course there was the miasma theory of disease transmission we considered in Chapter 1, which went back to Ancient Greece, and probably even earlier.

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[6] Jung's 1944 *Psychology and Alchemy* has similarities to Frazer's 1890 *Golden Bough* in trying to identify underlying themes, although it's a considerably harder read.

Although religion is often spoken of as the source for the competing explanations that research disciplines have had to counter over the years, this doesn't hold very much water. Science deals with internal, rather than external, explanations for the way the world works, so has mostly had to distinguish itself from other 'clockwork' theories and perhaps shouldn't have much to say about anything else (although more on science vs. religion in Chapter 6). Renaissance astronomy was competing against theories passed down from Ancient Greece and Rome (Aristotle as well as Ptolemy). Early understanding of the human body had the task of challenging the writing of Galen (an influential Greek medic and philosopher working in Rome around 200, not long after Ptolemy) as well as the older theories of Hippocrates (around 400 BCE) about health being controlled by the four humours of the body: blood, phlegm, and black and yellow bile. Early chemistry, as we've seen, had to distinguish itself from alchemy and general mysticism. Early research suggesting that light might behave as a wave had to compete with Isaac Newton, who had decided that it must be made up of particles. As Newton was held in such high esteem for his laws of motion, there was a reluctance to accept that he might not always be right (Einstein had a similar battle to fight over his special relativity theory).

An example of where science *did* have to compete with religion was in growing evidence from geological research that the Earth might be really quite old. However, this was largely up against the writings of a few sixteenth-century bishops who had decided to date creation (at around 4000 BCE) from the genealogies of the Old Testament, and quite what any Old Testament writer would have had to say about this use of their work is anyone's guess. Also, the biblical Earth-dating exercise wasn't just dreamt up by bishops but involved luminaries such as Johannes Kepler (the great astronomer and pioneer in optics) and Isaac Newton himself. And then of course there's Darwinism vs. intelligent design, creationism vs. the Big Bang, and others— debates that haven't quite settled down yet, whatever either opposing side would like to argue.

The point is that the advances in knowledge we hope to achieve through research are invariably replacing (or at least testing) some other pre-existing theory. And whatever has come before will generally be an idea that's popular and accepted. European mediaeval explanations of the world had been around for over a thousand years, and it's not particularly surprising that people weren't rushing to change them in 1500.[7] The church was certainly a source of conservatism, which didn't help matters, but many of the concepts

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[7] And to be honest, a lot of the older theories are simply more *fun*, or at least psychologically appealing (as Jung argued for alchemy, but this could be claimed for Dante's world as well).

it was holding on to had distinctly pagan origins. Therefore, Renaissance research had to make its case from the get-go, to distinguish itself in some way from the way ideas had been generated up till that point. So, what changed?

Acceptance of ignorance—the empty spaces

Not so long ago, in 2014, Yuval Noah Harari, an Israeli historian, wrote *Sapiens: A Brief History of Humankind*, which I believe sold rather well. It was certainly very visible in airport shops and seemed to be on a lot of people's bookshelves at the time (and probably still adorns the backgrounds of more recent pandemic-era video calls). Anyway, along with a variety of transition points along our species' history, Harari considers two maps drawn either side of 1500. The first, from 1459, is a typical mediaeval-era circle filled with detail; if there's a place where information is missing (e.g. southern Africa), something is made up to go there. The second map is from 1525 and shows the eastern coastline of parts of the newly discovered American continents along with a whole lot of empty space—the acceptance of the unknown. This does coincide quite conveniently with a shift in attitude that began to favour the new research fields, because you can't begin exploration without the idea of a map to complete, and you can't really make any progress in advancing knowledge unless you accept your ignorance, or at least your uncertainty.

There's nothing new in this, of course; there's rarely anything original about any of the principles underlying research. Back in the early days of philosophy, Socrates was held up as someone who was aware of, and honest about, his lack of knowledge, and this awareness was considered as a first step towards wisdom—'I neither know nor think I know'. Rather fittingly, we don't know very much ourselves about Socrates directly. If he wrote anything, it hasn't survived, and all the details of his life and teaching come from his successors. We do know that he was put on trial in Athens and sentenced to death in 399 BCE (we're not sure what he was accused of), that he was given exile as a choice but declined this (perhaps because he was already an old man), deciding to drink the poison hemlock instead. A lot of what's attributed to Socrates comes from the writings of his pupil Plato, and it's not always clear what's Socrates and what's Plato. It doesn't really matter for our purpose.

Acceptance of ignorance is certainly fundamental to research in the sciences, and I think it has to be just as important in the humanities. Why would you be enquiring about anything if you felt you knew the answer? If you're just looking for evidence to bolster a particular viewpoint then that's surely

propaganda, not research. The trouble is, self-awareness is a high ideal and I suspect all researchers fall short of it at least some of the time, becoming bound up with pre-conceived ideas and failing to keep objective. This seems to be ingrained from quite an early career stage—in my own research field, I'm often supervising students who are investigating a hypothesis (e.g. X and Y are associated) by analysing a database, and there's a near-universal disappointment if they carry out the analysis and find no support for the new idea. However, a robust negative finding ought to be as important as a positive one if it's the question you're interested in, rather than a pre-conceived answer. It would perhaps help if academic journals felt the same way and were less swayed by exciting new associations that will play well with news media, although I don't know whether my students are thinking that far ahead. I think it's more likely that they have just absorbed a deeper-seated culture where positives are all that's important.

On the importance of staying objective

Unfortunately, objectivity doesn't get any easier with age or experience. A spirit of dispassionate enquiry might be possible in a bright and enthusiastic young researcher. However, sooner or later you start getting a reputation for a particular discovery, and maybe funding and career security follow on from this. How is any of that going to help matters? What will you really do if your findings (or those of others) start to support some different conclusion? I guess the only way to judge (approximately) is to consider the senior academics in whatever field you're working or interested in and have a look into how many times they've changed their mind. This may also be a reason why many of the major advances in knowledge have come from researchers at relatively junior stages of their careers. Michael Faraday was near-unique as a scientist in contributing relatively little before the age of 30 and doing his best work later in life—but then his personality and career are interesting and unusual to say the least (Chapter 16). What's sad about this sort of conservatism is that most of the fun of research lies in the 'finding out'. Pushing out publications that relentlessly support a cherished idea might be enough to keep you in employment (or, more altruistically, to keep your research group in employment) but it doesn't feel like a recipe for job satisfaction. And it's not really research, even if, like Frazer's *Golden Bough*, it makes for a good story and ends up better read than a properly academic treatise.

The discovery of a new continent and the design of new maps with all the blank spaces must have had a powerful influence on European society

Reihen	Gruppo I. — R'O	Gruppo II. — RO	Gruppo III. — R'O³	Gruppo IV. RH⁴ RO⁴	Gruppo V. RH³ R'O⁵	Gruppo VI. RH² RO³	Gruppo VII. RH R'O⁷	Gruppo VIII. — RO⁴
1	H=1							
2	Li=7	Be=9,4	B=11	C=12	N=14	O=16	F=19	
3	Na=23	Mg=24	Al=27,3	Si=28	P=31	S=32	Cl=35,5	
4	K=39	Ca=40	—=44	Ti=48	V=51	Cr=52	Mn=55	Fe=56, Co=59, Ni=59, Cu=63.
5	(Cu=63)	Zn=65	—=68	—=72	As=75	Se=78	Br=80	
6	Rb=85	Sr=87	?Yt=88	Zr=90	Nb=94	Mo=96	—=100	Ru=104, Rh=104, Pd=106, Ag=108.
7	(Ag=108)	Cd=112	In=113	Sn=118	Sb=122	Te=125	J=127	
8	Cs=133	Ba=137	?Di=138	?Ce=140	—	—	—	
9	(—)	—	—	—	—	—	—	
10	—	—	?Er=178	?La=180	Ta=182	W=184	—	Os=195, Ir=197, Pt=198, Au=199.
11	(Au=199)	Hg=200	Tl=204	Pb=207	Bi=208	—	—	
12	—	—	—	Th=231	—	U=240	—	— — — —

Fig. 3.3 Dmitri Mendeleyev was born in Siberia, educated in St. Petersburg, and worked for a couple of years on the Crimean Peninsula by the Black Sea to help with his tuberculosis before returning north again to develop St. Petersburg University as an academic centre for chemistry. The genius of his c1870 periodic table of elements lies in the blank spaces—where there ought to be elements that haven't yet been found (sooner or later the gaps were filled in). At the heart of all research there has to be an idea of something unknown (the blank space) that's there to be discovered. If you spend your time trying to prove that you're right and shore up your theories, then you've rather lost your way. Dmitrij Mendeleyev's Periodic Table (1869). Original uploader: Den fjättrade ankan at Swedish Wikipedia, Public domain, via Wikimedia Commons.

around 1500, and the impetus for geographic exploration to fill in the spaces must have spilled over into other areas of enquiry. In a slightly similar way, a stroke of genius in the 1869 periodic table of elements drawn up by Dmitri Mendeleyev was that it left blank spaces (Figure 3.3) to be filled in—he included known elements up to that point but predicted new ones with given properties. Sure enough, those gaps were filled over time—for example, gallium, scandium, and germanium were added within the subsequent 20 years. Over the centuries, a number of theories have been proposed that have lain dormant for a long time, waiting for the technology to emerge to fill in the gaps. Neutrino particles were predicted as an explanation of atomic beta decay in the 1930s but had to wait until the 1950s for their detection. The continental drift hypothesis had to wait until the military mapping of seabeds after the Second World War. Fermat's last theorem famously took over 350 years to prove. Possibly the best way to start any research career is to find a field and try to identify the empty spaces. If it already seems full and crowded, is it going to be the best use of your time? Might it be better to look for a quieter, less explored area?

The impossible third choice

Of course, the analogy of empty maps in sixteenth-century Europe has uneasy connotations. The extensive exploration and conquest that followed Columbus hardly had glorious outcomes for the rest of the world. And it's fair to say that advances in knowledge achieved through research have often been driven forward with little consideration of the consequences. But then that's progress for you. Another point Harari makes in his *Sapiens* history is that hunter-gatherer societies might well have been considerably happier and healthier than the agriculture-based civilizations that followed—or at least happier and healthier as a whole; civilizations do allow some luxury for the select few at the top, but not for the vast majority of people keeping them there. This is important and salutary. If the book of Genesis in the Bible can be viewed as a source of orally transmitted ancestral memory (as Thor Heyerdahl proposed, amongst others), then the Garden of Eden story might be a poignant attempt to describe that transition to agriculture—from a life remembered as idyllic and self-sufficient to a life recognized as grim and laborious—and all precipitated by that nagging desire for apples from the tree of knowledge.[8] It's not the place of this book to comment on whether human history has been a good or a bad thing (Harari does this much better), but we did begin this chapter with the idea of our ancestors beginning to figure out a way to understand the world and deciding between externally or internally controlled systems. Of course, a hypothetical third option would have been to leave things as they were, and not to try to understand or manipulate the world at all. It's probably not a feasible choice because of the way we seem to have been configured as a species—the curiosity and drive to 'find out' that are hard-wired in our makeup, the stark truth that even if we decide something might be best left undiscovered, someone else will look into it sooner or later. If we're to take on Socrates' acceptance of ignorance as a basis for academic integrity, then it's probably worth adopting the same objectivity when it comes to thinking about what we call 'advances' and 'progress' and to consider whether these are always the right words to describe what we do.

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[8] And it's probably no coincidence that John Milton published *Paradise Lost*, his epic about Adam and Eve, in 1667 as the Renaissance moved towards the Enlightenment. In Book 8 of Milton's poem, Adam is specifically told off by the Angel Raphael for asking about celestial motions. However, both the angel in the book and Milton, the author, are swimming against the tide.

4

Careful Observation

As researchers, we reach a point where we decide that something is worth investigating because we think there's an intrinsic element in its system to understand. Furthermore, we accept our lack of knowledge as a basis for carrying out research in the first place, and try to avoid preconceived assumptions. Perhaps there also has to be a *need* for knowledge as an impetus, and maybe that was another change in Europe around 1500, as the Church's dominance began to wane. Yuval Noah Harari gives the hypothetical example of someone in mediaeval times working out how spiders weave webs but having nowhere to go with that knowledge because of there being no reason to pursue it; if religious scriptures (of whatever source) have no opinion on the matter, then it can't be worth knowing. I have reservations about this. Some areas of natural science (e.g. biology) might have remained dormant for longer than necessary because of a belief that what we needed to know was 'covered' by God's providence. However, a lot of knowledge generation tends to happen for its own sake, driven by curiosity rather than necessity, and without a particular end in mind (or even consideration of adverse consequences). So, I suspect the acceleration in research after 1500 might have been much more to do with better systems of communication between interested parties—particularly printing and relative freedom of distribution. It is also possible that the political situation in Europe at the time was more favourable than elsewhere—the growth of a relatively free-thinking and curious (and literate) middle class after the ravages of the Black Death swept away feudal systems; the beginnings of capitalism (i.e. private money to support research so that it was no longer quite so dependent on heads of state); small nations looking for something to compete over and setting up academic institutions as markers of prestige; maybe even a brief period when monarchs (and hence their courts) were expected to be intelligent and well-read.

Induction

Francis Bacon was certainly a product of those times (Figure 4.1). Born in London in 1561, the son of an aristocrat and the nephew of William Cecil,

Fig. 4.1 Francis Bacon made a living as a politician, but his position enabled him to write widely on a variety of topics, including emerging scientific theory. His political career was always precarious, as I suppose was only to be expected in the late six-teenth/early seventeenth century. Also, he was always incurring debts, which didn't help matters. His key works on empirical science were published at the height of his influence, just before his political downfall in 1621, charged with 23 separate counts of corruption. All things considered, he was lucky to be allowed a quiet retirement.
Francis Bacon by Paul van Somer I (1617). pl.pinterest.com, Public Domain, https://commons.wikimedia.org/w/index.php?curid=19958108

chief advisor to the well-educated Queen Elizabeth I, he rose to political importance towards the end of her reign and was Lord Chancellor for her successor, the similarly well-educated King James (sixth of Scotland, first of England and Ireland), although in the end, corruption and rivals got to him and brought about his disgrace and retirement. Concurrently, and as well as laying down foundations for legal reforms, library classification systems, and British expansion in North America, Bacon was a profoundly influential scientist and philosopher. Nowadays we would probably call him a philoso-pher more than a scientist—his works are mainly focused on ideas about knowledge and research methods rather than containing actual new find-ings; however, the difference between science theory and science practice was a little blurred back then, and Bacon's ideas were probably responsible for more progress than any amount of experimentation at the time. Also, the (possibly apocryphal) story about his death from pneumonia at age 65 is that

it was caused by his being out in the snow while he was investigating whether it could be used to preserve meat—so perhaps he really was a researcher at heart.

Francis Bacon is particularly associated with the theory of 'induction' or 'inductive reasoning'. This proposes that we derive knowledge from careful observations, coming up with theories supported by these findings. Induction therefore results in a conclusion that's a probability, as opposed to 'deduction' where a conclusion is more definite and derived from logical argument (think Sherlock Holmes or Hercule Poirot). As a rather broad generalization, deduction has its origins in mathematics and lends itself to knowledge that can be generated from first principles, put together by 'if this is true then this must follow' type arguments. On the other hand, induction belongs to the philosophy of empiricism, which is about deriving knowledge from experience—whether observation or experiment. In Bacon's time, what we now call the 'scientific method' had not yet evolved, but the principles he put together can be said to have begun that process—or at least he assembled and systematized ideas that were already knocking around sixteenth-century Europe, and that were likely to come together anyway.

A renaissance in observation

The importance of careful, methodical observation is key to a lot of research. If observations are faulty then we're not going to make much progress. Therefore (as we'll consider in Chapter 12), research progress has often relied on technological advances that improve the quality, accuracy, and sometimes the simple availability of observations on which more fundamental theories are built. However, the acceptance of ignorance has to come first. If observations are faulty because we think we know the answer (and are perhaps deliberately or inadvertently skewing them in a particular direction), then that's even worse, because no amount of improved accuracy is going to help matters. Natural curiosity and, crucially, open-mindedness, are pre-requisites to successful research, whatever field you're in.

Before we even think about inductive reasoning and the generation of new theories, good observation simply tells us how things *are*. European researchers after 1500 weren't the first to make careful observations, but they were certainly having to cope with over a millennium of lapsed attention to detail. The maps with the empty spaces needed to be filled in, which, in turn, required surveyors and cartographers to draw coastlines and river courses.

In a similar way, all the accreted false assumptions about the natural world needed to be replaced with demonstrable facts.

The emergence of zoology and botany is a good example of this. If you take the twelfth-century bestiary translated in T. H. White's *Book of Beasts* (1956), quite a lot of it contains fairly uncontroversial, if slightly eccentric, observations about animals like ravens ('this bird goes for the eye first when eating corpses'), horses ('they exult in battlefields'), and dogs ('he has more perception than other animals and he alone recognises his own name'), alongside some rather strange pictures. But then there's a whole lot of fabrication-presented-as-fact about known animals—for camels, 'if they happen to be sold to a stranger, they grow ill, disgusted at the price'; for mice, 'the liver of these creatures gets bigger at full moon'; for hedgehogs, 'when a bunch of grapes comes off the vine, it rolls itself upside down on top of the bunch, and thus delivers it to its babies' (if only that were true). And then the descriptions of genuine creatures are intermingled with similarly authoritative facts about unicorns, griffins, satyrs, etc.

Animals had been systematically described for almost as long as humanity had been making written records, particularly by the philosopher Aristotle, who worked in Athens until, in 343 BCE, he went to Macedonia to tutor Alexander the Great. Among his many achievements, Aristotle listed and categorized around 500 creatures and was known for deriving his ideas from observation (unlike his Athenian predecessor Plato, whose philosophy was more based on first-principles argument). Some of Aristotle's work was beginning to filter into Western Europe via Arabic translations from the eleventh century (although probably not his animal classifications, so a twelfth-century bestiary would have drawn on other material). The problem was that knowledge derived from observation was being jumbled up with knowledge derived from any old story (mermaids and all the rest of it) in the same way that the old maps were completing their outer, lesser-known regions with the products of sailors' fables in order to fill in the space. So, just as the new maps had to be constructed by the long, slow process of surveying landscapes, zoology and botany had to begin with a long, slow process of describing and logging facts about the living world. Botany, as a scientific discipline, presumably had a simpler task than zoology, as there would have been fewer fanciful or mythological descriptions clogging up the literature and at least it could draw on a more unbroken monastic tradition of cultivating and describing plants for medicinal use, as well as accumulated knowledge from agriculture.

Maria Sibylla Merian

Although long and slow and methodical, the process of describing as many as possible of the plants and animals in the world was at least feasible and didn't require substantial advances in technology. Along with travel opportunities, all anyone needed was a personal income and to be the sort of person who's happy to give some attention to detail. It also helped to be good at illustration. Maria Sibylla Merian (Figure 4.2), for example, was born in Germany in 1647 and fortuitously grew up in an artistic family—her father was an engraver, and her subsequent stepfather was a still-life painter who encouraged her training. Combining these skills with a lifelong interest in insects (cultivating silkworms as a teenager), she began publishing illustrated descriptions of

Fig. 4.2 Maria Sibylla Merian combined artistic skills and training with an interest in zoology, producing important early descriptive research, not least of the life cycles of insects. In the early days of science, before the committees and institutions took over, opportunities were a little more equal for women if you had the means to support yourself, which Merian did by selling her paintings. She carried on her work while bringing up her family and was supported by her home city of Amsterdam to travel to Suriname for further investigations in entomology. This portrait in her 50s unapologetically shows her scientific output (the books) and travel (the globe).

Maria Sibylla Merian by Jacobus Houbraken / Georg Gsell (c. 1700). Public domain via Wikimedia Commons. https://upload.wikimedia.org/wikipedia/commons/b/b3/Merian_Portrait.jpg

caterpillars, butterflies, and their life cycles from her early thirties. This work was original and revolutionary at the time, as no one had given much thought to insects and where they came from—they were generally thought to have associations with the Devil and to be born spontaneously from mud. After the end of an unhappy marriage, Merian learned Latin (the language of science at the time), moved to Amsterdam, and was allowed to travel in her fifties to the Dutch colony of Suriname in South America, where she worked for two years and published hugely influential work on plant and animal life, setting the standard for naturalist illustrators to come. A human rights and ecology campaigner before her time, she also spoke out against the treatment of slaves and the short-sightedness of relying on sugar plantation monoculture.

The invention of the microscope

Art and science haven't always had the easiest of relationships, but high-quality illustration is obviously essential if you are to assemble and communicate accurate observations from life, in much the same way that a good-looking graph makes the world of difference to the impact of a data-based research paper. Research therefore benefited from the improvements in draughtsmanship that accompanied the artistic side of the European Renaissance, as the differences between twelfth- and seventeenth-century illustrations clearly show. In natural sciences, perhaps the most profound revolution came with the microscope—a technology that followed swiftly on from the development of the telescope, and that was made possible by advances in lens-making. Robert Hooke is now known for many areas of scientific work (including optics, astronomy, and physics), but his collection of illustrations from the microscope in *Micrographia* (1665) was an immediate best-seller and must have been quite something to read at the time (a page-turner famous for keeping the diarist Samuel Pepys awake until the early hours of the morning). As well as presenting a new world that had previously been invisible to its readers, the book, with the now-familiar images of small insects like fleas and lice (Figure 4.3), was able to describe previously unknown micro-anatomy (a fly's compound eye, the structure of a feather) and to establish that fossils might really be the remains of once-living things, rather than rocks that just happened to look that way. Through this approach, the simple observations started to shift towards new ideas—commonalities and diversities between species and the fact that species have appeared and disappeared over time—taking a while to sink in, perhaps, but ultimately culminating in the natural selection theories of Darwin and Wallace around 200 years later.
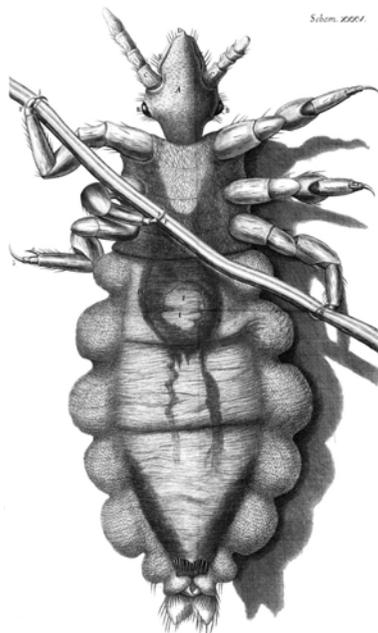
Fig. 4.3 One of the famous illustrations from Robert Hooke's 1665 work, *Micrographia*, showing a louse holding onto a human hair. Both telescopes and microscopes opened up new universes every bit as game-changing for Europeans as the discoveries from world exploration. Robert Hooke was a man of many talents (e.g. he might have been getting close to laws of gravity at the same time as Isaac Newton) and all he was really doing here was drawing something seen under a lens. However, visualization can be a powerful element of research and it's easy to see why it created so much excitement in the seventeenth century. I suspect the new Royal Society in London, set up by Charles II only five years earlier, was relieved to have this book as its first major publication.

Robert Hooke. Schem. XXXV – *Of a Louse*. Diagram of a louse. In Hooke, R. (1665). *Micrographia or some physiological descriptions of minute bodies made by magnifying glasses with observations and inquiries thereupon*. National Library of Wales. Public domain via Wikimedia Commons. https://upload.wikimedia.org/wikipedia/commons/1/10/Louse_diagram%2C_Micrographia%2C_Robert_Hooke%2C_1667.jpg

Making sense of observations

When translating observations into new ideas, there often needs to be a consolidation process. Describing and illustrating thousands of animals and plants is in itself a considerable improvement on the 'make it up as you go along' approach to mediaeval collections, but it's not much help if the thousands of observations can't be systematized somehow. In biology this required a classification system. Known as taxonomy, it was the quite formidable—if a little bizarre—achievement of Carl Linnaeus. Linnaeus was

born in Sweden in 1707, the son of a curate and a rector's daughter. He inherited his father's amateur interest in botany. While he seemed to struggle at most other school subjects, sponsors helped him to complete medical qualifications—a route taken by a number of researchers in the eighteenth and nineteenth centuries as a way of continuing investigations in the absence of an established career pathway. Whether these academic stars were any good as medical doctors isn't clear; Linnaeus practised in Sweden until he was appointed as a chair of medicine in Uppsala University at the age of 34, but he managed to switch to a chair of botany a year later—an interesting academic manoeuvre. This sort of move is not common nowadays, but it seems to have been the right step for him; Linnaeus held the position until he died aged 70. Over his lifetime he described around 7,700 plants and 4,400 animals across multiple publications and came up with a hierarchical system to classify them all from Kingdom at the top to Genus and Species at the end—a structure which hasn't required very much subsequent modification.

I don't think minds like Linnaeus' occur very often, and it was just as well that he found something useful to do with his. It's quite possible that the 'careful observation' element of research is what attracts the stereotype of the academic, who displays a high tolerance of repetitive work, an enjoyment of routine, and an eye for detail. Linnaeus certainly seems to fit that model, organizing meticulously timed outings (and even uniforms) for his students, and he was definitely a maker of lists. He was also precocious: many of his core ideas were formed quite early in life and he had already published one of his major works, *Systema Natura*, by the age of 28. However, he was clearly affable and popular, inspiring considerable loyalty and affection in his students. Seventeen of these he called 'apostles' and sent or encouraged them off on expeditions all over the world to collect samples. Several of these expeditions had fatal consequences—travel was associated with significant risks back then—and he switched to sending out younger and unmarried students after his first 'apostle', a 43-year-old pastor, died of a tropical fever, and the man's widow complained that her children had been left fatherless. Whatever an HR department might have had to say about this sort of research team mentality, Linnaeus' followers certainly helped publicize work that could easily have been lost in obscurity if it had come from a less likeable person; he ended up highly acclaimed by both the scientific and literary communities, with enthusiastic praise from writers such as Rousseau and Goethe.

The process started (or at least accelerated) by Linnaeus caught on in a major way and could perhaps be said to be an early example of crowdsourcing. Once the idea of observing and cataloguing the natural world took hold, it provided an obvious purpose in life for anyone with a methodical mindset

and solitary enthusiasms who had enough of a personal income (and prefer-ably some travel opportunities) to indulge them. There was work to be done and academic territory to claim. Some of the titles for posterity are impres-sive, such as 'Grand Old Man of Lichenology' held by Edvard August Vainio, a Finnish researcher who published more than 100 works on the subject over the late nineteenth and early twentieth centuries.

Not all contributions were adequately recognized. Maria Sibylla Merian has a reasonable claim as a founder of entomology, as we've seen, although William Kirby, a rector living a quiet life in Suffolk is often credited with this, even though his first major work, on bees, was published in 1802, over a century after Merian's two-volume series on caterpillars. And Merian never received a mention from Linnaeus in his classifications, even though he used her illustrations. He did at least mention Jane Colden, a botanist working in North America in the late eighteenth century, who compiled descriptions of over 400 plant species from the lower Hudson River valley. She is the only female researcher cited by Linnaeus, and perhaps it helped that she used his classification system.

Transformations in geology

The acceleration in observation across the seventeenth and eighteenth cen-turies wasn't confined to plants and animals. Geology likewise provided readily available opportunities for amateur (and then increasingly profes-sional) research by those who wished to take a little rock hammer with them on their travels. As with botany and zoology, geological research was clearly going on in prehistoric times when early settlers were beginning to discover the location and uses of tin and iron, or when building projects started to become more ambitious, requiring stone quarrying and an appreciation of the difference between building on rock or sand. Writings on geology date back at least to ancient Greece, and it was one of the many interests of Abu al-Rayhan al-Biruni, the eleventh-century Iranian scholar, as well as Shen Kuo, the eleventh-century Song dynasty Chinese politician and polymath. However, the first geological maps were drawn up by William Smith in Som-erset around 1800 and were more akin to the description and categorization going on in biology. Smith began his life working as a surveyor for mines and canals, allowing him to collect source materials as they were excavated. From these, he observed that different rock strata contained distinct fossils, and used these observations to map rocks across regions. He went through some rather hard times when his geological map of Great Britain in 1815 failed to

sell, and he had to spend some time in a debtor's prison; however, eventually he gained some overdue recognition and a comfortable enough retirement.

From observation to theory-building—palaeontology

Geology and biology coincide in palaeontology: not only in the observations made of animal and plant fossils in rock layers, but also in the way in which the implications of these discoveries (species diversity, interrelatedness, emergence, and extinction) contributed to evolutionary theory. Again, the field wasn't new but there was a definite acceleration, this time from around 1800. Ancient Greeks had noticed fossil seashells and had concluded that what was now land had once been underwater, and the above-mentioned Shen Kuo was interested in the origins of fossilized shells and bamboo, proposing long periods of time over which these changes must have occurred, along with climate changes and alterations of landscape. Leonardo da Vinci, among his many activities, had taken an interest in fossils and distinguished those that were the direct remains of earlier life and those that indicated their presence, such as worm burrows. Robert Hooke included microscopic observations of fossils in his *Micrographia*—for example, showing that petrified and living wood were the same substance.

Hooke began the suggestion that fossils might represent animals and plants that had become extinct, a revolutionary view in a Western European society that believed that everything had been created in its present form a few thousand years previously. However, this older belief was increasingly undermined by further discoveries, such as the plesiosaur skeletons uncovered on the Dorset coast by Mary Anning (Figure 4.4) in the early 1820s—creatures that could not be explained except by extinction. Anning wasn't the first to discover a dinosaur (a giant marine reptile had been described in 1808 in Holland), but the collections that she amassed and investigated were major contributions to an acceptance that there were indeed animals that had once existed and were now extinct, coupled with geological findings suggesting that current species hadn't always been present. Unfortunately, most of Anning's life was spent in fairly unrewarding circumstances. Like the geologist William Smith, she didn't have the luxury of financial independence (she had to make a living by selling the fossils she found), and this was compounded by exclusion from the British scientific community through being female and non-Anglican. Unlike William Smith, she could not publish or even attend scientific meetings. Instead, she had to be content with being known and admired by a few individual male academics, relying on

Fig. 4.4 Mary Anning was an expert fossil collector, working on the Dorset coast in the early nineteenth century. Recognized now, if slightly over-romanticized, for her important work in palaeontology, she led a hard, impoverished life. As a woman, she was barred from academic institutions, although she did have friends in the scientific community, such as the geologist Henry de la Beche, who painted a prehistoric scene based on Anning's fossils and sold lithographs to friends and colleagues for her benefit. He became president of the Geological Society of London shortly after Anning died and published a eulogy to her, typically reserved for society fellows.

Mary Anning. Credited to 'Mr. Grey' in Tickell, C. (1996). *Mary Anning of Lyme Regis*. Public domain, via Wikimedia Commons https://upload.wikimedia.org/wikipedia/commons/e/e7/Mary_Anning_painting.jpg

them to communicate her findings and conclusions, which they sometimes did without even mentioning her name. Besides the plesiosaur and other skeletons, Anning was able to demonstrate that fossilized ink sacs were an important commonality between extinct and living squid-like creatures. In the end, she did receive a small pension for contributions to geology, and a eulogy at the Geological Society after her early death from cancer aged 47. However, sadly, it was quite some time before her contributions were fully recognized.

So, we can see a revolution in systematic observation occurring over a few hundred years in these four disciplines: zoology, botany, geology, and palaeontology. We also can see the beginnings of inductive reasoning happening almost by necessity—that is, in the dawning realization that

processes such as fossilization and laying down of rock strata must have taken place over quite some time—certainly a lot longer than seventeenth-century assumptions of a few thousand years since creation. Induction therefore goes from the specific observation (a fossilized plesiosaur skeleton), draws logical inferences from the observation (this creature is nothing like anything alive today and must have lived a long time ago in order to become fossilized) and translates the inferences into a general theory (species extinction/non-permanence). Although fundamental, it's also rather simple and intuitive—most research is, at heart. The difficulties lie in the practicalities of obtaining the measurement (e.g. digging up a good quality fossil) and then, more importantly, coming to the correct conclusion.

Smallpox, part 1

A final example of observation and induction comes from the work of Edward Jenner and the development of the smallpox vaccine. Jenner was born in Berkeley, Gloucestershire in 1749, the son of a vicar. Although he became a physician and is known as the 'father of immunology' for his work on small-pox, he also happened to be the first person to describe the life of the cuckoo and that it was the newly hatched chick that got rid of other eggs in the host bird's nest, rather than the parent cuckoo (the previous assumption); he was elected a fellow of the Royal Society in 1788 for this work, which might have been enough for most people. If you think about it, the level of careful obser-vation required to make this discovery, as well as the slight oddity of having that sort of interest in the first place, did mark Jenner out as an ideal person for scientific enquiry and it was a continued zoological interest that led him on to study the various animal versions of smallpox. In particular, taking the common observation that milkmaids had very low levels of smallpox, Jenner inferred that this might be because they contracted cowpox, the bovine ver-sion of the disease which could be passed to humans but in a much milder form than smallpox. Therefore, before anyone knew about antibodies or its other components, Jenner was using inductive reasoning to propose acquired immunity. He also understood the implication that giving people exposure to cowpox might prevent them developing smallpox. The name he gave to cowpox was *Variolae vaccinae* (literally smallpox of the cow) which gives us the word 'vaccine', and his theory was published in 1798, just ten years after his observations of cuckoo chicks. Almost 200 years later, in 1979, smallpox was certified as eradicated as a result of Jenner's theory of immunity and the vaccines that followed.

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Ideas under Pressure

Smallpox, part 2

The story of Edward Jenner in Chapter 4 didn't stop with him deriving his theory; he went on to test it. Having decided that the pus in cowpox blisters might be providing the immunity to smallpox in milkmaids, he needed proof for his theory. Jenner took some fluid from the blisters on the hand of a milkmaid and inoculated the eight-year-old son of his gardener (Figure 5.1), going on to challenge him with various sources of infection.[1] This is clearly not the sort of study design that would get past a research ethics committee today, but thankfully the boy was unaffected, and Jenner's theory was supported. He then went on to test this on 23 more people, including his own 11-month-old son. Remarkably quickly after Jenner's 1798 publication (many discoveries take a lot longer to translate into action, as we've seen with John Snow and cholera; Chapter 1), the vaccine roll-out began. For example, a Spanish expedition in 1803–1806 inoculated millions of people in South America and East Asia.[2] On a literary note, the plot of Charles Dickens' novel *Bleak House* involves one of the principal characters developing smallpox.[3] Published in instalments from 1852 to 1853, *Bleak House* coincided with the 1853 introduction of compulsory infant vaccination in England; whether that was a coincidence or not, I don't know, but it is difficult to imagine that it wasn't knocking around in conversations of the time.

 If the vaccine had been ineffective, the world would have known soon enough from subsequent cases (smallpox was a common and very visible disease) and the theory would have drifted into obscurity. However, vaccination wasn't appearing as a completely new concept. People knew that if you survived certain diseases then you didn't get them again, and the prevention of smallpox prior to Jenner's experiment involved 'variolation'—trying to

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[1] It wasn't actually the first smallpox vaccination, although was the first to be described properly in a publication.

[2] A worthy gesture to South America, albeit a little belated after the decimation it had suffered from smallpox, a disease that was brought over by European settlers in the first place.

[3] Probably smallpox, at least—it isn't named in the book but it's similarly disfiguring. Like cholera, smallpox is another common nineteenth-century disease that doesn't make it into literature very often.

Fig. 5.1 A rather lurid nineteenth-century French depiction of Edward Jenner administering the smallpox vaccine to eight-year-old James Phipps, the son of his gardener, on 14 May 1796. The stoical young lady with the bandage on the right is presumably Sarah Nelmes, the cowpox-affected milkmaid from whose hand Jenner had taken the fluid for the vaccine. Phipps lived to see his 65th birthday, attended Jenner's funeral, and was buried in the same churchyard. Smallpox vaccination had a rapid international impact despite European political turbulence, hence the French interest here. Napoleon, for example, vaccinated his entire army and awarded Jenner a medal, despite being at war with Britain at the time. Jenner asked instead for the release of two British prisoners, which was granted.

The inoculation of James Phipps by Edward Jenner. Lithograph by Gaston Mélingue (c. 1894). Public domain, via Wikimedia Commons.

induce a mild (but still potentially fatal) infection by injecting pus from affected people into a scratch in the skin. This had been carried out since at least the tenth century in China, was common practice in the Ottoman Empire and was communicated to Western Europe in 1717 by Lady Mary Wortley Montagu, the enterprising wife of the British ambassador there who had already inoculated her five-year old son by that method. Jenner's new vaccine was accepted rapidly as a much more preferable solution (since it resulted in nothing more than a mild fever at worst) and variolation was banned in Britain by 1840. The point is that smallpox vaccination wasn't just

a theory Jenner derived from observation—it was a theory that had been put to the test.

The problem with induction

Although the ideas of Francis Bacon and his contemporaries about inductive reasoning were influential and an important starting point for a lot of modern research, it wasn't long before other thinkers were picking holes in them. Leading this charge was David Hume, the eighteenth-century Scottish philosopher. Hume worked as a librarian at the University of Edinburgh, publishing extensively but getting repeatedly turned down for academic positions because of his suspected atheism. It was only when he produced a best-selling history of England that he finally achieved some financial security from the royalties. Hume was unhappy with inductive reasoning as a way of understanding the world, particularly because *induction* (drawing a conclusion that can only be said to be probable) could never be the same as *deduction* (drawing a conclusion that can be argued as a logical and certain consequence). Instead, he proposed that repeated observations of past behaviour (e.g. paracetamol has relieved my headaches previously) are assumed to predict future events (e.g. paracetamol should help the headache I have now), but it's best to view these as beliefs, rather than proofs, and to remain sceptical. After a whole lot of development of research in the intervening period, these ideas eventually matured into 'falsifiability', a principle underlying a lot of research nowadays and worth spending some time on.

Falsifiability

Falsifiability as a concept is particularly associated with the science philosopher Karl Popper (Figure 5.2), whose first work on the subject, *Logik der Forschung* (The Logic of Scientific Discovery) was published in 1934 while he was working as a secondary school teacher in Vienna. The key principle of Popper's theory of falsification is that you can never prove a theory; you can only say that it's consistent with observations up to now (in this respect, broadly what David Hume had been saying in the eighteenth century). However, a single inconsistent observation can show that theory to be false. For example, you can claim that all swans are white, and this will hold as a theory until you see a black swan; then you have to change your idea. Or you can say that water always boils at 100 degrees Celsius, and this will be the case
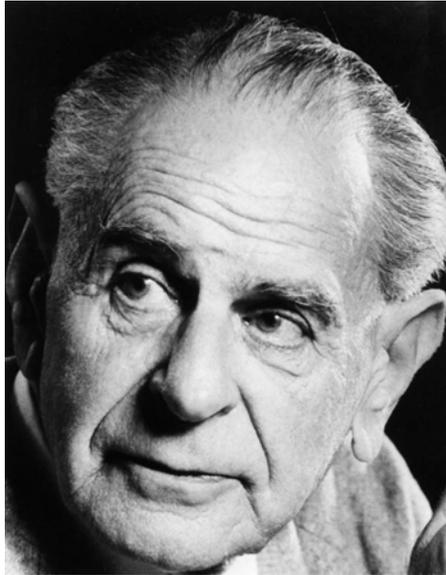
Fig. 5.2 Karl Popper—the science philosopher particularly associated with empirical falsification. Getting his first publication was an urgent deadline for Popper, as he needed this to obtain an academic position in a country that was safe for someone of Jewish descent. In 1937 he managed to get out of Europe, taking up a philosophy lecturer post at the University of New Zealand in Christchurch, as far away as possible from the Nazis and the Second World War. After the war, Popper worked in the UK at the London School of Economics as a professor of logic and scientific method and ended up living to the ripe old age of 92.
Photograph of Karl Popper (c. 1980). LSE Library/imagelibrary/5.

with repeated experiments at sea level until you take your kettle to the top of a mountain and find that it boils at a lower temperature.

In Chapter 2, we considered the sort of everyday 'research' we do as part of growing up and learning about the world. As discussed there, the idea of the repeated experiment is a powerful and intuitive one. Popper's theories have been extremely influential in the way that today's research is conducted, in scientific disciplines at least, and in the standards expected of researchers. However, like a lot of research principles, they are not very different to natural human behaviour. We spend most of our early lives repeating experiments of one sort or another to understand the world around us, and the truths we obtain from others via textbooks and other media are tips of the iceberg compared to those we have (often unconsciously) found out for ourselves. If one day you were to clap your hands and make no sound, or if you were to let go of a stone and see it fall upwards, the shock and disturbance might almost

be sanity-threatening—because the extent to which rules of sound and gravity have repeatedly been observed in the past makes it near inconceivable that they won't continue to occur in the future. However, the philosophy of Hume and Popper would hold that both are falsifiable—that we would have to amend the theory of gravity wholesale, for example, as soon as a stone was observed to fall upwards. It may be very unlikely, but it's still possible.

Science or magic?

Thinking again about the post-1500 acceleration in research and knowledge generation, inductive reasoning alone was never going to be enough, whatever Francis Bacon said. In part, the problem stems from our inability to conclude cause and effect from observation—if the cockerel always crows before the sun rises, does the cockerel cause the sun to rise? Also, it doesn't really distinguish 'science' from the magic and superstition accreted since ancient times. The people planning their rain dance might carefully observe the 'pain' of the ground before the rains arrive, and they might observe this year after year. And they might carry out their rain dance, beating the ground year after year and find that the rains arrive eventually. So, this false idea of cause and effect may arise from a process of perfectly logical inductive reasoning. In Chapter 4, we discussed how there was definitely a deficiency in pre-1500 Europe with faulty or absent observations, and the careful cataloguing of the world over the following 300–400 years provided a lot more knowledge from which to draw conclusions. However, the process of drawing conclusions still has to be sound, and there's nothing about the general principle of inductive reasoning that makes one conclusion better than another from a given set of observations. Bacon was certainly quite critical of Aristotle and keen to distinguish the Renaissance/Enlightenment era[4] from what he saw as old and outmoded ways of thinking. As far as I can see, Bacon's criticisms seem to be more about Aristotle's quality of observation and the way he drew conclusions, rather than the general principle of inductivism (NB: comparing philosophers is well outside my comfort zone and you may want to consult an expert). For our purposes here, inductive reasoning is an important component of research, but in the days when the sciences were emerging as disciplines, it wasn't really showing them up as any different to

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[4] The beginning of the Enlightenment era is a bit vague. Some date it from 1637 when the philosopher Descartes wrote 'I think, therefore I am'; some date it from 1687 when Newton published his *Principia*, or from the death of the French king Louis XIV in 1715.

earlier learning. Perhaps this is why the old mystical beliefs took quite a while to disappear. For example, among his more scientific work, Issac Newton was an enthusiast for alchemy and for predicting the date of the world's end from a careful reading of the Bible.

What *does* start to distinguish modern from ancient research is the principle of putting ideas under pressure. If you stop dancing, the rains still arrive. If you remove the cockerel, the sun still rises. If you take your kettle up the mountain you have to revise your theory about the boiling point of water and bring atmospheric pressure into the equation. Jenner took his theory of acquired immunity from cowpox and developed the first vaccine. John Snow tested his emerging suspicion that cholera might be a water-borne disease by mapping cases of the outbreak in Soho.[5]

The 'study hypothesis'

Not all research is about cause and effect, but quite a lot is, particularly in the sciences, and nowadays the 'hypothesis' is a central component, arising directly from Popper's falsification principles. If you write an application for research funding, the chances are that your study's hypothesis will be requested as one of the first pieces of information. Similarly, higher impact journals will expect this prominence early in a research paper, usually shortly after an introductory summary of the rationale for the study. The hypothesis is a distillation of the broader theory you are bringing to the project, and a prediction arising from the hypothesis is what your study will be testing, putting the idea under pressure. Figure 5.3 summarizes this experimental 'cycle' (which is really quite straightforward). Having formulated a hypothesis, a prediction is made from it (if X is true, then we should observe Y). An experiment is then carried out to test that prediction. If the findings are supportive then the hypothesis holds, but another prediction and experiment might then be planned to test it further—back around the cycle, keeping the idea under pressure. If the findings are not supportive then the hypothesis needs to be refined, further predictions made, further experiments carried out to test these. And so on . . .

If you've decided on a field of research, you'll tend to find that the textbooks on your subject focus on the design of the experiments and the conclusions you might draw from your findings. They may well acknowledge the importance of the central hypothesis, but there doesn't tend to be much written

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[5] And chaining up the offending water pump as an experimental intervention, although as mentioned in Chapter 1, that part of the story is unfortunately a little too good to be true.

Observation

↓

Hypothesis

↓

Prediction

↓

Experiment

Supported   Not supported

↓   ↓

Re-tested   Refined
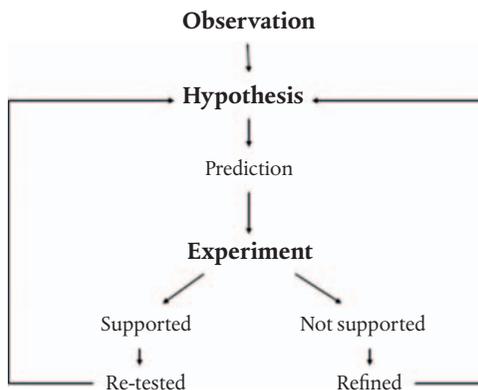
Fig. 5.3 The hypothesis cycle. Note that hypotheses have to come from somewhere in the first place—it's all very well for falsification enthusiasts to criticize induction, but both principles are required. It has been said that the 'health' of a research field can be judged by the extent to which new hypotheses are being developed (induction) and tested (falsification). If you find yourself in a field that's not really going anywhere, you might want to have a think about this—is it a lack of new ideas (in which case, perhaps you should work on conducting better observations), or is it because new ideas are not being adequately put to the test (in which case, perhaps work on designing better experiments)?

Author's own work.

about the hypothesis itself, and this is because there isn't very much more to say. It's possibly the most important element of many fields of research, but it's hard to teach. In the end, we have the scientific method that has emerged over the last 500 years—simply the way things are done and the way that many fields of research have emerged and justified themselves. Perhaps someone will come along one day with a different framework to Popper's and perhaps that will change the way research happens. I'm not so sure, because Popper's falsification theory isn't actually a huge leap forward from the earliest principles of knowledge generation. 'Socratic dialogue' in the old Athenian marketplace (attributed to Socrates but it could as well be Plato's) involved one person proposing an idea and the others around him trying to knock it down with argument, putting the idea under pressure in the same way that scientists are putting their own hypotheses under pressure around 2,500 years later.

So, once again, nothing much is new. Although accounts of the scientific method today tend to reference Popper's work, his falsification principle was simply describing what had emerged anyway, just as Bacon was describing a way of thinking that was already being adopted in the sixteenth century.

William Gilbert was the physician to Queen Elizabeth I of England and therefore a near-contemporary of Bacon; however, he was already well on the way to methods that were more 'scientific' than induction alone. Aside from coining the term 'electricity', he described principles of magnetism which weren't added to until electromagnetic research over 200 years later. In the preface to his *De Magnete* masterwork (published in 1600, two years before he died of bubonic plague), Gilbert writes:

> In the discovery of secret things, and in the investigation of hidden causes, stronger reasons are obtained from sure experiments and demonstrated arguments than from probable conjectures and the opinions of philosophical speculators.

Gilbert's suggestion sounds very close to putting ideas under pressure and not relying on induction alone. Bacon published most of his theories around 20 years later and was not very complimentary about Gilbert—he was sceptical about magnetism and didn't like Gilbert's support for a Sun-centred planetary system. Or it's possible that they'd just fallen out at court at some point; Bacon was rather prone to making enemies.

Constructing a hypothesis

There are at least some features of the hypothesis that are important and can be taught. For example, it helps if a hypothesis is expressed in as clear and simple a way as possible, because you want to make sure that your readers actually understand what it is you're testing—because if a finding can't be communicated and picked up by someone else, is it really worth anything? And ideally, other researchers should be able to take that idea and test it with their own experiments—because who's going to believe something that only comes from a single group? Also, it's best practice if the hypothesis is for-mulated and recorded before the research project is carried out—sometimes called an *a priori* hypothesis. The alternative is a *post hoc* hypothesis that is formulated after the findings have emerged—a possibly valid but weaker situation. This simply reflects the two approaches to research that we've dis-cussed. Inductive reasoning is when a study is carried out and a theory is derived from the observations made, which we have seen is fine in itself, but potentially flawed because the process of deriving that theory doesn't make it true. However, taking a theory and testing it puts the study in the realm of experimental falsification and makes the results a lot more credible. If you're looking at other people's research findings, do consider this one carefully.

Sometimes the wording isn't clear on a research paper, and you have to do some reading between the lines to work out whether they really were testing a preconceived idea or simply carrying out a project and seeing what happened. There's a difference.

When testing a hypothesis, you do sometimes have to put some thought into what it is you're investigating; this is because it's often hard to narrow down an experiment so that it puts only one theory under pressure. For example, around 150 years after Isaac Newton proposed fundamental laws of motion and gravitation in his 1687 *Philosophiae Naturalis Principia Mathematica* (usually abbreviated to *Principia*), astronomers were observing the orbit of the planet Uranus (discovered much later in 1781) and found that it wasn't fitting in with what was predicted from the positions of all the other planets and the carefully calculated Newtonian gravitational forces. If you had observed this, you might view it as a test of Newton's laws and a reason to go back and refine them. However, the alternative possibility is that you don't yet have all the necessary observations for that calculation. Looking again at the mathematics, another undiscovered planet was proposed, with its gravitational effects providing an explanation for the observations. Sure enough, Neptune was discovered in 1846. The fact that a previously unknown and invisible object could be predicted mathematically in this way was clearly a major support for Newtonian theory, just as Edmund Halley (who played a major role in helping persuade Newton to publish his *Principia* in the first place) was able to apply Newton's laws to the comet he observed in 1682 and predict that it would return in 1758. He was vindicated (and the comet was named after him) when it duly did so, 16 years after his death.

From observation to experiment

For a figure so prominent, it's ironic that Isaac Newton's personal approach to research was quite far away from what would later become the scientific method. Newton says:

> The best and safest method of philosophizing seems to be, first to inquire diligently into the properties of things, and to establish those properties by experiences and then to proceed more slowly to hypotheses for the explanation of them. For hypotheses should be employed only in explaining the properties of things, but not assumed in determining them; unless so far as they may furnish experiments.

I'm not completely sure about the last few words, but most of this sounds like Bacon's inductive reasoning—to make observations (on 'properties') and derive hypotheses that explain these. Newton wasn't really in the business of putting his ideas under pressure by experimentation; he was primarily interested in deriving principles from mathematics that explained what was already being observed. The hypothesis testing was left to his successors, and perhaps it's an advantage that the verification came from independent sources and clear predictions that could be tested, as with Halley's comet and the discovery of Neptune.

Important further testing and development of Newton's theories came particularly from France, although with a bit of a delay because of national enmities and the reluctance of French academics to embrace theories (or anything else) from Britain. The first French translation of *Principia* was carried out by Émilie du Châtelet (Figure 5.4), although wasn't published until 1756,

Fig. 5.4 Émilie du Châtelet, the eighteenth-century French mathematician, philosopher, and general polymath. As mentioned for Maria Sibylla Merian (Figure 4.2), there were times when scientific respect and reputation were relatively gender neutral if you had the means to support yourself, as can be seen in this assertive portrait. Du Châtelet was fortunate to have parents who were well connected with writers and scientists and who encouraged her precocious abilities with extensive and broad-based tuition. In turn, she argued strongly, ahead of her time, for gender equality in secondary education. *Madame Du Châtelet at her desk* (detail) by Maurice Quentin de La Tour (eighteenth century). Public domain, via Wikimedia Commons.

seven years after she died in childbirth. Her translation included a personal commentary and additional principles of her own around conservation of energy, which Newton had missed. Du Châtelet was much more of an experimentalist, investigating energy and momentum by dropping balls onto soft clay from different heights. She wrote her own widely read physics textbook, as well as completing an analysis of the Bible and sustaining a long-standing relationship with the philosopher Voltaire, among other achievements.

Pierre Simon Laplace was born in 1749, the year of Du Châtelet's death, and is sometime referred to as 'the French Newton'. He seems to typify the quiet, reserved researcher, recognized to have a prodigious mathematical ability that he applied particularly to astronomy, working out why the Solar System wasn't going to collapse in on itself,[6] solving observed irregularities in the movement of Jupiter and Saturn, developing equations for tidal forces, and considering the likelihood of black holes.[7] Interestingly, Laplace managed to become celebrated in his country just for being self-evidently gifted and without much of a need to press his cause, and this may be why he survived within quite high-ranking positions across all the upheavals of the French Revolution, from monarchy to republic to empire and back to monarchy again—his survival almost as much of an achievement as his mathematics. It also seems to have helped that he was an excellent communicator, with his 1796 *Exposition du Système du Monde* account of physics theory at that time being famously readable as well as comprehensive. He certainly wasn't a modest man and was quite happy to be seen as the best mathematician in France; however, he did look after younger researchers and fostered talent. Presumably he also survived because he didn't play aggressive political games and got along with his peers (for example, he collaborated on chemistry experiments with Antoine Lavoisier (Chapter 10), who didn't fare so well in the Revolution).

Even though some of Newton's theories were corrected or developed over the years by Du Châtelet, Laplace, and others, the general assumption seems to have been that knowledge inexorably advanced and that Newtonian principles had 'sorted out' this corner of his field. So, when Albert Einstein proposed his theory of special relativity in 1905, it was viewed as revolutionary for highlighting and solving a situation where Newtonian models did not work (to do with the speed of light being constant, no matter how fast you're travelling towards or away from the light source). For this reason, it tends to

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[6] Whereas Newton had assumed that God intervened every few hundred years to stop this—yes, seriously, that's what he thought.

[7] Or at least bodies so massive that light could not escape. Black holes technically were coined later as a potential inference from Einstein's general relativity theory.

be cited as one of the most important examples of falsification when people are talking about Popper's theories and advances in science, even though Einstein was assembling the theory to explain the observations of others, rather than carrying out his own experiment. Famously, his paper only names four other scientists besides Newton (Einstein wasn't known for his background reading; like Newton, he preferred to work from first principles via deduction). Either way, a model of scientific progress starts to emerge with waves of experimentation continuously putting ideas under pressure and refining hypotheses—Newton's theories are all that's necessary for over 200 years, then Einstein comes along and points out that they're an approximation and don't work in all circumstances. It then follows that special relativity needs general relativity theory in strong gravitational fields, and so on (e.g. continuing difficulty combining quantum theory and general relativity).

On the advantages of aggressive hypothesis testing

Of course, in the process of hypothesis-testing it helps if the testing involves independent researchers, just like Halley's comet and the discovery of Neptune. It's even more persuasive if a theory is supported by someone genuinely trying to knock it down. As discussed in Chapter 3, one of the problems with Newton was that he was held in such high regard for his mechanics that everything else he said was felt to have some absolute truth.[8] One theory strongly held by Newton was that light was composed of particles travelling through a medium he called 'ether'. When later researchers started to observe that light might be better described as behaving like a wave, this was met with considerable scepticism, simply because it was contradicting a Newtonian theory. When Augustin Fresnel, a civil engineer with an amateur interest in optics, developed an experiment to demonstrate the wave theory of light, he put it forward in a competition at the French Académie des Sciences and found himself up against a formidable panel of judges, all Newtonians. In brief, Fresnel's experiment shines light through a very small slit, puts a narrow obstacle (e.g. a needle) in its path and shows that the light bends around the obstacle in a way that can be best explained by viewing it as a wave. One of the judges, the eminent mathematician Poisson, didn't like this and proposed that a circular obstacle would end up with a bright spot in the middle of its shadow if wave theory was correct—an idea he believed was absurd. However, they agreed to the experiment and the bright spot was confirmed—compelling support for

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[8] Well, almost everything; I think his wilder ideas about alchemy and predicting the end of the world might have been quietly set to one side.

the wave theory, given the hostile attempt to knock it down. It would be nice to say that the panel had a 'road to Damascus' moment and were instantly convinced, but history is a little more complicated. All the same, the theory of light as a wave had taken hold and gained acceptance within a reasonably short period of time.

Interestingly, another example of support for a theory from hostile examination comes in the opposite direction because, of course, light can be described both as particles and as a wave. One of Einstein's papers in his 1905 *annus mirabilis* (the same year as his paper on special relativity and two other ground-breaking reports) described light as quanta of energy. This was as revolutionary as wave theory had been and, understandably, it had its critics. One of these was the physicist Robert Millikan working at the University of Chicago who carried out a series of experiments trying to prove Einstein wrong. He kept at it for ten years but had to give up in the end and accept the truth. However, at least he was awarded a Nobel prize for his efforts (this work and calculating the charge of an electron) in 1923, only a year after Einstein's award for the theory he was trying to refute.[9]

In conclusion

It's perhaps worth finishing by emphasizing that the practice of experimental falsification doesn't negate in any way the other principles we have been discussing. The hypothesis under investigation has to come from somewhere in the first place and is often drawn from careful observation and inductive reasoning. And the falsification process has to come from a scepticism and awareness of uncertainty—Socrates's statement that 'I neither know nor think I know'. In many ways, the objectivity in the experiment is harder to achieve, because the idea you are putting under pressure might be the product of your own (or, worse, your supervisor's) inductive reasoning. Just remember: no one ever said research was easy.

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[9] Einstein was awarded his 1921 prize in 1922.

6
Choosing a Solution

Common ground between science and history

Thus far, we've been considering research drawn from observations and experience—deriving theories through inductive reasoning and then putting them to the test by experiment. Although mostly described for the sciences, the principles seem common enough ones to me and eminently applicable to humanities. History research is obviously focused on working out cause and effect, just as much as any science discipline, and a lot of research in art and literature will be drawing on principles and practices derived from history. For example, one of the things you don't expect when embarking on *War and Peace* is the amount of space (in that already-lengthy novel) devoted to Tolstoy's musings on the turbulent events of the Napoleonic era and on what caused them—was it just the actions of single men like Napoleon himself, or was it the product of populations and many smaller actions? The theorizing is fine in itself, although it does rather interrupt the story.

Like the research fields we've been thinking about so far, 'historiography' (the methods used by historians) had its beginnings in the ancient world, suffered a bit of a drop in quality during mediaeval times in Europe, and then developed steadily increasing rigour throughout the last 500 years. For history, the mediaeval problem tended to be the inclusion of any old story without much checking—very similar to what the bestiaries were doing for zoology. An example of this would be the uncritical incorporation of legends about King Arthur and other speculative tales into *Gesta Regum Anglorum* (Deeds of the English Kings), finished by William of Malmesbury in 1125, around the same time as the bestiary discussed in Chapter 4. Again, as with the bestiary and as with the old maps, there didn't seem to be too much concern back then whether a given fact or story had been proven to be true or not; it was just thrown in anyway—good for readability perhaps, but not really serving the development of knowledge.

In the eighteenth-century 'Age of Reason', it's not surprising that the objectivity and rigour developing in the sciences were also being adopted in history writing—all academic disciplines would have been strongly influenced by the philosophy of the time. David Hume wrote on both science and history (as well as economics), pushing for precision and scepticism in thinking and

ideas, regardless of the academic discipline. As mentioned in Chapter 5, after struggling career-wise because of his suspected atheism, he finally managed to achieve some financial independence by writing his own history of England.[1] At the same time, his philosopher contemporary Voltaire was arguing the same for historical writing in France, presumably while debating Newtonian mechanics with Émilie du Châtelet.[2] There was only a small European academic community in those days, so it was ripe for ideas crossing between disciplines before they specialized and drifted apart. By the time Edward Gibbon (Figure 6.1) completed his epic six-volume *History of the Decline and*

Fig. 6.1 An engraving of the historian Edward Gibbon based on his 1779 portrait by Sir Joshua Reynolds. In the eighteenth century, the emerging scientific method was being shaped by intellectuals like Hume and Voltaire, who were writing and thinking about history just as much as science. When he compiled his magnum opus *The History of the Decline and Fall of the Roman Empire*, Gibbon was following a trend towards more careful consideration of primary sources of information, akin to the systematic observation and inductive reasoning being applied in the sciences. However, he also wasn't averse to throwing in an opinion or two, including some quite scathing comments on the early church (which, unsurprisingly, got him into the eighteenth-century equivalent of a Twitter storm).
Edward Gibbon, by John Hall, after Sir Joshua Reynolds (1780). Public domain, via Wikimedia Commons.

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[1] So presumably his beliefs didn't come in the way of book sales, just university appointments.

[2] Voltaire, incidentally, had attended Isaac Newton's funeral and had clearly made some rather personal enquiries with the attending physicians, establishing to his satisfaction that Newton had never 'had any commerce with women', a characteristic far removed from his own experience.

Fall of the Roman Empire in 1776, there was at least an expectation that a historian would consider information critically and be cautious about conclusions drawn from secondary and potentially biased sources (i.e. inductive reasoning). Also, academic communities were emerging where ideas could be debated and at least put under some pressure, even if history, as a discipline, doesn't lend itself to experimentation.

Passing the baton—research in mathematics

Not all discoveries are drawn from observations and not all research uses the types of reasoning we've been considering so far. Chapter 2 discussed how mathematics emerges out of prehistory based around fundamental truths and then develops as a field through deductive, rather than inductive, reasoning—working out what logically follows as a consequence of a given truth and steadily building up discoveries by proof, rather than experiment. Sometimes mathematics research is carried out with a specific end in mind, particularly when it's required to solve problems in other research fields (such as the inverse square law of gravitational force explaining the movements of planets); other times, the research is carried out for its own sake with no particular end in mind.[3] And sometimes the research carried out for its own sake turns out to have unforeseen applications further down the line.[4]

We've discussed an acceleration since 1500 in applied research, along with some of the context that might explain why this particularly happened in and around Western Europe for a large part of that period (change in worldview, discovery of old learning, acceptance of blank spaces on the maps, etc.). I rather suspect that progress in mathematics would have continued its development regardless of the European Renaissance; after all, it seemed to have been progressing well enough up to that point outside Europe, and perhaps there are always people who are gifted in that way and who will inevitably push the pure field forward. Printing undoubtedly helped to connect the research community, rather than relying on unstable Eurasian trade routes for the exchange of ideas. Also, the growing need for applied mathematics in disciplines such as engineering and economics presumably provided some impetus for more theoretical developments, or at least supported university

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[3] 'Pure' mathematics—as if there's something unsavoury about the rest of it.
[4] Which I suspect is the argument that's rolled out when a department's funding is about to be cut or when students are looking bored, a little like the way Latin gets justified at school.

departments so that they could accommodate these external demands while they were pressing ahead with advances in the pure field.

After Isaac Newton's work in the early eighteenth century, mathematics activity shifted to France, Germany, and Russia (in particular). Although there was an Industrial Revolution going on in Britain, there also seemed to be a sense that Newton had sorted everything out and there wasn't much left to do; either that, or national self-satisfaction got in the way of progress (as sometimes happens). There appears to have been an important role for knowledge transfer between successive generations of leading mathematicians, which might also explain the fall in British activity after Newton (who doesn't sound like the easiest of men to get along with, and who was rather preoccupied in his later years with politics and civil service work instead of fostering a new generation of researchers). An example of this baton-passing began with Gottfried Wilhelm Leibniz working in Hanover. Leibniz was Newton's approximate contemporary, although their relationship was less than cordial—they argued about which of them had invented calculus (probably they both did, independently of each other). Leibniz trained the Bernoulli brothers in mathematics—Jakob (who worked in Basel) and Johann (who worked in Groningen and later in Basel). Johann Bernoulli then went on to train Leonhard Euler who was widely recognized as the leading mathematician of the eighteenth century, giving us the symbols π and e, for example, as well as what is now standard graphical notation. Euler was born in Basel but began his academic career at St. Petersburg, moving to Berlin because of political instability in Russia. After things settled down, he returned when he was offered a high-paying position. His eyesight steadily deteriorated over his working life but this, if anything, increased his academic output: with the help of scribes, he managed one paper per week in 1775 when he was 68 years old and totally blind.

The Göttingen dynasty

A later succession of leading mathematicians was hosted by the University of Göttingen in the state of Hanover, central Germany. This started with Carl Friedrich Gauss, who was recognized as an unusually precocious young man, produced ground-breaking work from his early twenties, and continued into his old age working quietly away at the university. He may or may not have come up with non-Euclidean geometry—János Bolyai, the son of a friend of his, published on this in 1832 and Gauss congratulated him by saying that he had been thinking of it himself for the previous 30 years. It's possible that this

was true—Gauss wasn't a prolific writer and didn't like to publish anything until he was quite sure about it; however, it put understandable strain on the relationship. Not that it mattered too much, as Gauss was highly influential in many other ways—for example, developing the 'method of least squares', fundamental to the regression models in statistics that many of us have to grapple with, although used at the time to predict the movements of planetoids. As well as these achievements, Gauss managed to persuade junior colleague Bernhard Riemann to abandon theology in favour of mathematics. Riemann importantly further developed the non-Euclidean geometry of objects on curved surfaces—a necessary knowledge base upon which Einstein would later build his general theory of relativity. The sad end of the University of Göttingen's prominence came with David Hilbert who, beyond his own work, was particularly known for presenting in 1900 a list of current problems or challenges for mathematics at that time. He retired in 1930 but lived to see the Nazi purges of universities and the emigration of most of his colleagues and successors, who were replaced with government-appointed staff. When Hilbert was later asked by Hitler's Minister of Education whether his Mathematical Institute had really suffered so much from 'the departure of the Jews', he replied that it no longer existed. He died in 1943 and the epitaph on his tombstone is as good a mission statement as any: *Wir müssen wissen. Wir werden wissen* (We must know. We will know).[5]

So, it does seem that pure mathematics thrives with active mentoring, as well as the personal gifts of those involved. For example, if Gauss hadn't been working at Göttingen, Riemann would presumably have gone on to become a theologian (he remained a devout Christian throughout his life and viewed his academic work as service to God). Across its wider history, mathematics does seem to have flowered in particular places at particular times. This would be understandable for a research field that depends on the necessary equipment and facilities being available, or the skills of laboratory staff, so it's interesting that the same need for the right environment is present in a subject that depends so much on thinking and logic. In the eighteenth century, the centres of excellence were particularly oriented around the courts of monarchs who might be called 'enlightened despots'—Catherine the Great (St. Peterburg), Frederick the Great (Berlin), and Louis XV–XVI (Paris, where Laplace worked). By the nineteenth century, the universities had taken over as the main employers and therefore the sustaining soil for local research expertise, exemplified by the succession at Göttingen. This continued into

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[5] Hilbert also seems to have got quite close to general relativity theory before Einstein got there first—a little like Locke for Newton, or Wallace for Darwin.

the twentieth century, broadening from Europe to the US, Japan, and wider international settings, as well as moving beyond universities to encompass commercial enterprises. In theory, problem-solving in pure mathematics might be the sort of research that could be done by anyone at home with the right type of mind, time to spare, and access to the relevant work of others. It therefore remains to be seen whether Internet-based connectivity might provide the necessary community for future progress and bypass the limitations of academic career structures.

The need for challenges

Pure mathematics thrives on problems to be solved. In 1900, David Hilbert issued a list of 23 problems needing solutions, and this list was influential in directing twentieth-century research. The tenth problem on his list is one of the best-known challenges, Fermat's last theorem, which was posed around 1637 by Pierre de Fermat, a French lawyer and mathematician. This is relatively simple on the face of it—just asking for an explanation why you can have positive integers where $a^2 + b^2 = c^2$ (one example being $3^2 + 4^2 = 5^2$), but why this doesn't work for any higher power than two (e.g. $a^3 + b^3 = c^3$, or $a^4 + b^4 = c^4$, etc.). Fermat wrote in the margin of another book that he had a proof but that it needed more space. Whether this was true or not, he never wrote it down, leaving a long-standing puzzle that took until 1994 to solve. This first complete proof was provided by Andrew Wiles, working between the universities of Oxford and Princeton; however, it's fair to say that there were a number of steps along the way that made this possible. An important example was the work on the theorem carried out by Marie-Sophie Germain in the early nineteenth century—a French mathematician who (because she wasn't a man) had to make her way in the field independently and through correspondence with contemporaries such as Gauss and Laplace. Gauss did recommend her for an honorary degree at Göttingen, which was decent of him, although the inclusion of the words 'even a woman . . .' in his argument does rather detract from this. Perhaps it helped pave the way, however, as the University of Göttingen awarded a doctorate in 1874 to the Russian mathematician Sofja Wassiljewna Kowalewskaja (Figure 6.2), who went on to become the first woman to hold a full university professorship (at Stockholm University ten years later) and one of the first to edit a scientific journal.

The eighth problem on Hilbert's list originates from Riemann (Hilbert's predecessor at Göttingen) and remains unsolved to this day. Hilbert himself said that if he slept for a thousand years, his first question on waking would

Fig. 6.2 The Russian mathematician Sofja Wassiljewna Kowalewskaja. While she did benefit from parental support and a good private education at home, she had to work hard and relentlessly to make her way in the male-dominated world of nineteenth-century mathematics, impressing professors at a number of universities and cadging what private tuition she could negotiate when she was not allowed in lectures. After five years of quite an itinerant life between European centres of excellence, she presented three papers to the University of Göttingen and became the first woman to be awarded a doctoral degree in her subject.

Photograph of Sofja Wassiljewna Kowalewskaja (unknown; 1888). Public domain, via Wikimedia Commons.

be whether the Riemann hypothesis had been proved—that's how much these things matter if you're in the field. There have been several similar problem lists compiled since Hilbert's, a prominent one being the Millennium Prize Problems posed by the Clay Mathematics Institute, New Hampshire—just seven challenges this time (still including the Riemann hypothesis), but with an added reward of USD 1 million for any correct solution. So far, one of them has been solved, by Grigori Perelman from St. Petersburg, who creditably refused to take the prize money because he felt it should be shared with another contributor, Richard S. Hamilton from Columbia University, New York.

For a research field based on proof rather than opinion, you would expect that pure mathematics would be relatively peaceful, apart from the usual academic arguments over who thought of something first. However, there do seem to be some quite strong words exchanged at times. One of David Hilbert's achievements was to promote and help establish some ideas developed originally around 1874–1884 by Georg Cantor, a mathematician who worked at the University of Halle, not far from Göttingen. Cantor's set theory provided an underpinning link between several areas of mathematics and

has ended up accepted as a standard, but his ideas did cause quite a stir at the time. Leopold Kronecker, Cantor's counterpart in Berlin, was particularly exercised, describing Cantor (of all things) as a 'scientific charlatan' and a 'corrupter of youth'.[6] It does seem a little odd, to someone on the outside, why sceptics didn't just read the theory and work out whether the proofs were robust; perhaps the building of knowledge through deduction is more complicated than it sounds. Cantor also proposed the concept of 'transfinite numbers' (larger than finite numbers but not necessarily infinite), which got him into trouble with the philosopher Wittgenstein and some theologians (to do with a divine monopoly on the infinite), even though Cantor was a devout Lutheran and believed that his theory had been communicated from God. All of this disagreement might have contributed to the bouts of severe depression Cantor suffered, although he had received considerable recognition and awards by the time he died in 1918.

Rationalism vs. empiricism

Mathematics and philosophy have had a fairly close relationship over the centuries, perhaps understandably. One of the long-running debates they have shared in their different ways has been around the idea we're considering here of deriving knowledge from 'first principles', sometimes called rationalism, and whether this is preferable to the 'knowledge from observation', or empiricism, that has been the theme of previous chapters. Francis Bacon, for example, was a Renaissance empiricist, although this way of thinking can be traced back to Aristotle and even earlier (to 600 BCE India if you want). Therefore, as we've seen, he was a firm believer in drawing ideas from observations. His near-contemporary, the philosopher René Descartes, on the other hand, tends to be seen as laying the foundations of rationalism (although that can also be traced way back—e.g. to Plato and earlier). Descartes was French born but mainly worked in the new Dutch Republic. He is best known for his 1637 'Cogito, ergo sum' (I think, therefore I am) statement, and for his 1641 *Meditationes de Prima Philosophia*, in which he starts by stripping away everything that is not absolutely certain (presumably down to something like 'I think, therefore I am'), then tries to build on that foundation with certainties and logical reasoning. The full title of the work translates as *Meditations on first philosophy, in which the existence of God and the immortality of the soul are demonstrated*, so you can see where he's going with his argument. Aside from

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[6] As well as blocking all his attempts to secure an academic position in Berlin—a depressingly common practice over the years and a good reason why researchers are best kept away from positions of power.

his mathematics (analytic geometry in particular), Descartes' contribution to research, like Bacon's, is more in the philosophy of knowledge generation than actual knowledge itself. Indeed, his ideas were often less than satisfactory and sometimes counterproductive. For example, he was convinced that a vacuum wasn't possible and proposed that the planets therefore moved in a fluid, which held up progress for a while. Also, in Western health research 400 years later, the separation between things of the mind and things of the body ('mind-body dualism') is recognized to have created all sorts of problems and tends to get blamed on Descartes, whose philosophy was very emphatic on the matter.

This isn't really the place to go into the lengthy to-and-fro between empiricist and rationalist philosophers over the centuries, not least because it would require picking apart the thoughts on the subject by Immanuel Kant, which are frankly beyond me.[7] I think it's fair to say that most research involves a mixture of the two processes—to some extent, drawing conclusions from observations; to some extent, building up knowledge through logical reasoning. The need for ideas to be put under pressure and for researchers to remain sceptical, continually questioning their assumptions, is common to both approaches. Although assessing conclusions derived from pure rationalism should be just a matter of checking the logic or algebra, the example of Georg Cantor and his set theory does sound like an idea being put under peer pressure in the more conventional sense, just like Socratic dialogue in ancient Athens—although it would be nice if academics could be a little more polite and less vindictive in their disagreements.

The simplest solution

At the heart of its problem solving, the task of mathematics is to get from one piece of knowledge to another. There might well be more than one possible path to take and conventionally the simplest is chosen. This is sometimes referred to as the principle of parsimony or 'Occam's razor'.[8] William of Ockham (Figure 6.3) was a Franciscan friar living around 1300 who wrote on the process of reasoning, which gave his name to the metaphorical razor, although he never used that phrase himself. Again, there was nothing particularly new here, as Aristotle had already proposed that 'Nature operates in the shortest way possible'. The 'simplest is best' principle is worded more precisely as 'entities should not be multiplied beyond necessity'. So, if you can

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[7] And I've tried!
[8] Variably spelt; sometimes Ockham or Ocham.

Fig. 6.3 Possibly a portrait (or self-portrait) of William of Ockham—or at least it's on a fourteenth-century manuscript of one of his works and says 'frater Occham iste'. Is it just me, or does the face look like a Picasso? Brother Occham, an English Franciscan friar, expressed his thoughts a little too independently at times, which got him repeatedly into trouble, and he was sent to defend himself from unorthodoxy in Avignon, southern France. A few years later he had to escape to Bavaria when the Franciscan belief that you shouldn't own property came into conflict with Pope John XXII (whose church owned rather a lot). Brother Occham wrote on a range of topics including what would now be called philosophy, mathematics, and politics, but it was his arguments for parsimony (i.e. of opting for an explanation with the fewest possible factors) that gave his name to 'Occam's razor'.

Sketch labelled *frater Occham iste* (1341). In *Ockham Summa Logicae*, MS Gonville and Caius College, Cambridge. Public domain, via Wikimedia Commons.

get away without extra material in your theory, do what you can to get rid of it and strip the theory down to what's essential. Going back to Copernicus and the discussion in Chapter 3, Ptolemy's pre-existing model of planetary movements placed the Earth at the centre, which mostly worked but needed extra tweaks to account for irregularities. Copernicus and his successors proposed that a model with the Sun at the centre was a simpler one that didn't need the tweaks (or at least not so many of them).

All of this sounds very neat and it's a strong tradition in mathematics where there's generally some way of quantifying the size and complexity of equations and assumptions in order to work out what's simplest. However, mathematical tradition doesn't actually claim that one approach is true and that another is false—it just says that a more complicated pathway is unnecessary because you have a simpler one. It depends on what you want to do with the model. All objects in space are relative to each other, so

there's nothing fundamentally incorrect with working on Earth-centred relative movements—and this might actually be more helpful, for example, if you're launching a space probe (i.e. rather than trying to work out everything in relation to the Sun). It's just that the Sun-centred model is a simpler and better one if you're trying to explain the way our solar system operates as a whole. Similarly, general relativity theory is considerably more complex than Newtonian theory; it's just that it's needed to explain certain things, like the deflection of starlight. On the other hand, it can be argued that general relativity theory does have elegance in the principles underlying its equations. Sometimes there is more than one pathway to knowing something—for example, working out the age of the Universe can be approached via the theory of general relativity (i.e. the physics of very large objects) or by calculating the age of the stars with quantum theory (i.e. the physics of very small objects). If they didn't agree, it might be a tricky process working out how to test which one was right. As they do seem to agree, there's nothing much that Occam's razor can tell us about which approach to choose (it would be hard to argue which is simpler)—you can just take your choice.

Beyond neatness being an attractive idea for those of us with busy lives and cluttered offices, there is some justification for avoiding complexity, in that greater numbers of assumptions bring in higher chances of error. Some comparisons between simpler and more complicated predictive models do bear this out. Also, the Universe, as far as we can tell, does seem to be a perfect system—for example, Earth and the planets never deviate from their orbits—which can be said to support simpler rather than more complex theories. However, this still leaves us with 'simpler is better' as little more than an assumption; also, it does tend to drift towards a further assumption that 'simpler is always closer to the truth'. Moving beyond pure mathematics to wider application of this principle in science, does this therefore mean that more complex explanations are always wrong? If we're researchers at heart, then Occam's razor should perhaps remain a theory of convenience that we happen to hold at the moment but that is as subject to experimental falsification as anything else (even if not particularly easy to put to the test). And if we're researchers at heart, we should be adopting cautious scepticism in the meantime and not rushing to think we know the answer.

Science and faith

Casting your mind back to Chapter 3, you might remember that we considered the hypothetical situation James Frazer proposed of a person not

knowing anything about the world but trying to understand it and having to make a choice between an externally or internally controlled system— God or clockwork. We've followed the second choice so far and the way in which science has sought to differentiate itself from other clockwork theories (planetary spheres, alchemy, unicorns, and the rest of them). Perhaps unsurprisingly, over the centuries some people have felt that the remit should be broader and that we should be testing clockwork against God. I'm not going to say very much because tempers tend to run high in this particular argument and I could do without the aggravation; however, equally I don't think the question can be ignored entirely because God has featured in research for as long as it has been written about and the bickering doesn't show any sign of going away.

Back in the early days of science, God was very much part of the clockwork. As mentioned in Chapter 5, Isaac Newton hadn't quite solved the problem of why the Solar System didn't collapse in on itself, so he simply proposed that God intervened every now and then to stop this. Around 100 years later, Pierre-Simon Laplace solved the problem and removed the requirement for divine intervention as an explanation. The story goes (quite possibly mythical) that Napoleon was talking with Laplace about his work and asked him where God fitted in; Laplace replied that he had 'no need for that hypothesis'.

In the days when the Christian Church wielded political power in Europe (including in university appointments), science was often quite wary about picking a fight. For example, in relation to the age of the Earth,[9] the evidence against the '6000 years since Creation' hypothesis had stacked quite high before a challenge was made. This was left to Georges-Louis Leclerc, Comte de Buffon—a French aristocrat and socialite who was also a natural scientist, when he found the time. He wrote his *Histoire Naturelle* in 44 volumes from 1749 until his death in 1788 just before the Revolution (which probably wouldn't have treated him well; they guillotined his son). In amongst a wealth of other material, he ran with the idea that the Earth had been thrown out of the Sun, calculated how long it might take to cool down and estimated its age to be at least 75,000 years—a little short of 4.5 billion, but a step in the right direction. You get the sense that Buffon was the sort of person who was quite capable of side-stepping a bit of controversy, although even he presented his ideas as speculations. He was also an early proponent of evolution, a theory which formed the next battleground with the Church. Nervousness about this conflict might also explain the notoriously long time Charles Darwin sat on

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[9] Which, as previously mentioned, doesn't seem to have much to do with religion, being more about some odd seventeenth-century calculations involving Kepler and Newton, as well as a few bishops.

his natural selection hypothesis without publishing; however, if so, it seems more likely out of concern for the views of his devout wife Emma rather than worry about what the Church might do to him.

The Science vs. God debate is as polarized as every other disagreement seems to be these days. From a research perspective, the key arguments for those who consider themselves the Science side are that clockwork theories have won,[10] that the Universe, its history, and our place in it can be fully explained and, to paraphrase Laplace, that there is no need for any other hypothesis. However, the fault in the argument seems (to me) to lie in the 'simple is best' assumption and poor old Ockham (who would probably be wishing now that he'd kept his razor to himself). The problem is that there's no arbiter in that debate—who decides what's a simpler hypothesis? And how do we know in the first place that 'simple is best' means 'simpler is always true'? Even the field of pure mathematics (the products of which underlie a lot of the 'clockwork' theories for the origins of the Universe) doesn't claim Truth as underlying its choice of the most straightforward equation—it's just a pragmatic solution that seems reasonable and has become standard practice.

Applying the principles of Karl Popper doesn't really help either. You could start on the one hand with a 'God exists' proposition—fair enough, but it's only worth having as a scientific hypothesis if there's some way of putting it to the test, and I've yet to hear of a way in which the hypothesis could be feasibly falsified experimentally, beyond chipping away at rather peripheral sub-beliefs (e.g. God doesn't appear to live in the clouds and there doesn't appear to be an Empyrean beyond the fixed stars). Alternatively, you could start with a 'God doesn't exist' hypothesis and decide that it's consistent with your observations, but then you'll have plenty of people coming along and saying that in their view that hypothesis has been falsified (because of an answered prayer, a particularly powerful religious encounter, whatever). You can decide whether or not you accept their falsification argument, but then you're drifting into making subjective judgements on personal faith and experience, and scientific reasoning is on the back foot again. As a middle way, you could settle with a 'God probably doesn't exist' proposal, which was an advert that appeared on the side of London buses a few years ago and certainly seemed singularly pointless at the time—the sort of meaningless, woolly compromise that perhaps would only be possible in England.

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[10] Although not quite 'clockwork' anymore—quantum mechanics rests on probabilities and uncertainties, albeit subject to mathematical laws. Despite being a contributor, Einstein was never happy with this, saying that 'God does not play dice'. It's also worth noting that general relativity predicts singularity, a point of infinite density, as a result of gravitational collapse. A quantum theory of gravity might resolve the paradox, but this hasn't quite been worked out.

To be fair to Science, the God side in the debate (or at least the Western Christian Church) has hardly covered itself in intellectual glory, from the days of the sixteenth-century Vatican hierarchy hanging on grimly to an Earth-centred planetary system that came from pagan Alexandria in the first place, or seventeenth-century Protestant bishops deciding that the Bible was written as an instruction manual for dating Creation. And rationalism hasn't fared well either from attempts to prove the existence of God by first-principles reasoning, from Descartes in the seventeenth century to C. S. Lewis in the twentieth.[11] The argument that certain fields of knowledge, like the origins of the Universe, somehow 'belong' to theologians rather than scientists also seems particularly vacuous.

The problem, in my (very humble and inexpert) opinion, is that both sides are arguing over what can't be argued and that there are many better things to be doing with limited time and salary. Any religion that depends on (or is tempted to think it might be enhanced by) scientific proof, seems a contradiction in terms. And quite what business scientists have in attempting to falsify what can't be falsified, I've no idea. Worse still, there's no sense of dispassionate objectivity on either side of the argument—which I'm afraid comes down worse on the Science side, because if there's one thing you should take pains to avoid in research, it's testing a question when you've already decided the answer. The arguments have been running since 1500 at least and there's still no end in sight, so if there's anything appropriately sixteenth-century to say about the debate, it's 'a plague on both your houses'.

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[11] To be fair, C. S. Lewis, the Christian essayist and Narnia author, repented of his unwise decision to prove God by argument; I don't know about Descartes.

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The Ideal and the Reality

As discussed, the acceleration in knowledge as a result of 'modern' (post-1500) research came about, at least partly, because of the development of new (or rediscovered) ways of thinking. These are important because they continue to shape research today. First, there has to be the acceptance of ignorance, of there being something 'out there' to discover, and worth discovering—an objectivity in the way we think about the world (Chapter 3). This provided an impetus for observation and description (Chapter 4) which can be seen in both the sciences (e.g. astronomy, botany, zoology, geology) and, arguably, in the more rigorous approaches taken to information collation in wider subjects like history, art/literature, and social sciences. Building on this improvement in the quality of observation has been the philosophy of inductive reasoning as a basis for drawing conclusions, or at least theories, usually related to causation. What best accounts for the movement of the planets? What might explain the diversity of species, and how they appear to have emerged or become extinct from fossil evidence? What explains rock structures that look like they were once connected but are now on either side of an ocean? Was Richard III of England really an evil man, or was that just propaganda made enduringly popular by Shakespeare? Why did the poet Percy Bysshe Shelley decide to embrace a meat-free diet and what impact did it have on vegetarianism as a lifestyle (and/or on his poetry)? All of these require good-quality source information coupled with a clear and transparent process for drawing conclusions. Sometimes the conclusions are drawn from the observations (empiricism), sometimes they might be argued through logic and deduction (rationalism), perhaps most of the time there's a mixture of the two processes involved (Chapter 6).

We also considered the principle of falsification and the need to keep ideas under pressure (Chapter 5). This is slightly different in that it seems to have been something that science, at least, has had to learn for itself. The mindset of the European Enlightenment was a little overoptimistic at times—a sense that this 'Age of Reason' would sort out all that was wrong in the world, that simple mathematical laws like gravity would explain the world and result in lives and societies that could be as well-ordered as the geometrical gardens of Baroque stately homes. Clearly things didn't turn out this way, so what went wrong?

The problem with induction

To begin with, inductive reasoning isn't enough (Chapter 5). It's nice to imagine clarity of thought applied to meticulous observations resulting in elegant mathematical models like Newton's laws of motion. The problem is that this relies on the clarity of thought happening to come up with the right answer and with observations that are sufficiently generalizable to all circumstances. To be fair, Newtonian mechanics received no particular challenges for over 200 years, which isn't bad going, even if some of his other theories held back progress (e.g. for wave theories of light). And to be fair, the laws still apply in a lot of circumstances—quantum theory is only needed as a replacement at the subatomic level. However, Einstein's theory of special relativity was still quite something for early twentieth-century scientists to get their head around, because it demonstrated that Newton's laws were not universal. It was Einstein's theory that indicated you couldn't take anything for granted indefinitely, that the dream of the Age of Reason had finally passed, and that even the strongest-held hypotheses were fallible and needed to be kept continually under scrutiny. Hence, Popper's principle of falsification in the 1930s was a description or codification of what had already been discovered, rather than proposing an entirely new direction.

The limits of falsification

The difficulty with falsification is that it's all very well for the basic sciences—physics, chemistry, etc.—where processes can be tightly controlled. However, its application is difficult in fields where observations are necessarily messier and subject to error, and where experiments are more difficult (or impossible) to design. If you're working on particle physics at the Large Hadron Collider (Figure 7.1), the 27-km circumference tunnel deep under the French–Swiss border, you can fire one particle into another and be fairly sure that what you observe happening afterwards is a result of that collision and nothing else. If your theory predicts one thing and you find something else, then your hypothesis is falsified and you can move rapidly along to refining it—or at least that's the way it sounds; perhaps it's more complicated.[1] Because particle physics theories can be tested so robustly in this way, I'm sure that a sizeable number of *Science* and *Nature* papers have accumulated as a result. If, on the other hand, you're a health researcher who happens to find that

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[1] For example, I assume there's some probability theory involved in the falsification.
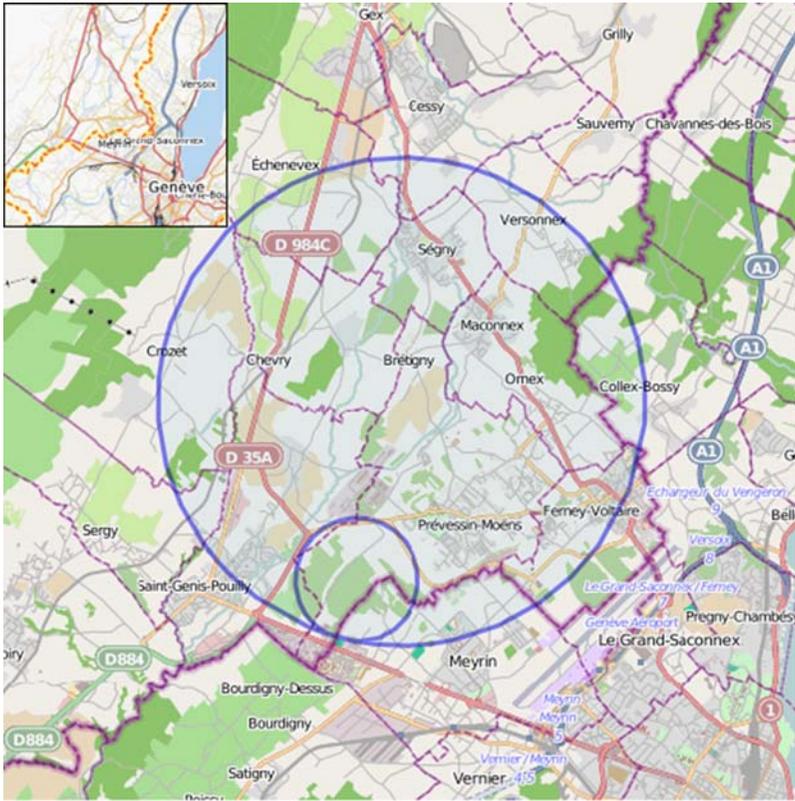
Fig. 7.1 A map of the Large Hadron Collider (LHC) built under the French–Swiss border by the European Organization for Nuclear Research (CERN) over a ten-year period and operational since 2010 (the smaller circle is the Super Proton Synchrotron, also built by CERN). The LHC was built for particle physics research; 'hadron' particles include protons and neutrons, as well as more recently discovered pions and kaons. The Higgs boson particle was identified there after a 40-year search in 2012, and the world didn't end as some had predicted. Particle physics may be hard work, but at least you know (or ought to know) what's going on in a vacuum—that is, what's cause and what's effect. The trouble is most research fields can't isolate and control their observations and experiments to that extent and so have had to find other solutions.

Location map of the Large Hadron Collider and Super Proton Synchrotron. OpenStreetMap contributors, CC BY-SA 2.0 <https://creativecommons.org/licenses/by-sa/2.0>, via Wikimedia Commons.

a high-cholesterol diet isn't a risk factor for heart disease in a particular population, it will be highly unlikely that you can proceed immediately to a publication in *JAMA* or *The Lancet* claiming that you have falsified that particular hypothesis. Even if the study is well designed, there will always be the question whether someone adopting the same sort of approach might find something different in another study. Research in the natural world, with all

the random 'noise' in the system, is a far cry from the carefully controlled environments in laboratories or particle colliders.

The challenge of 'real world' research

So, there's a problem here. A fundamental issue for any hypothesis or wider theory is that you can't claim it's true (Chapter 5); you can only say that it's consistent with observations. An inconsistent observation means that you have to refine your hypothesis—for example, taking your kettle up a mountain to show that water doesn't always boil at the same temperature. However, as we've just seen, outside basic science an inconsistent observation may not be enough to falsify a hypothesis. So, if we can't prove that something is true (as a principle) and we can't prove that it's false (because of the messy world around us), how can we hope to advance knowledge?

I'm going to focus on health research in this chapter, because it's had to face up to this challenge for most/all of its history, so quite a lot of thought has been put into solutions, or at least workarounds. As will hopefully be obvious, many of the principles are intuitive and apply more widely. We've come across the issue already in Chapter 1 when we thought about John Snow's work. Quite clearly there was a prevailing current theory—that diseases were spread by bad air—and Snow's findings challenged this for the 1854 cholera outbreak in Soho, but they weren't enough for the hypothesis to be refined. It took quite a lot longer for water to be considered as a carrier of disease and for the necessary measures to be put in place to prevent this—which subsequently saved millions of lives and might save millions more if clean water was more equitably available around the world. Health research is mostly having to deal with cause–effect relationships (what causes a disease and how can it be prevented or treated), but most of these need to be investigated in the natural world. In other words, investigations must consider people with all their varying lifestyles and behaviours, where one risk factor is likely to cluster with plenty of others, where treatments don't always work (because they just don't work the same way in everyone, or because people don't always take them properly), and where disease outcomes themselves are many and varied, influenced by all sorts of things besides medical care. So, it's complicated . . .

Judging causality

Sir Austin Bradford Hill (Figure 7.2) was an epidemiologist and a successor to John Snow, although he worked around a century later, setting up some of

Fig. 7.2 Sir Austin Bradford Hill was Professor of Medical Statistics at the London School of Hygiene and Tropical Medicine in the 1950s and 1960s. He was prevented from studying medicine because of lengthy treatment for TB as a young man, so took a correspondence degree in economics instead, after which he developed a career as a statistician, and then as an epidemiologist. Like many disciplines, health research has to rely on studies carried out in 'real world' situations that exist outside carefully controlled laboratory environments. Working out cause and effect is therefore complicated, and Bradford Hill formulated many of the approaches that are now taken for granted. Medicine's loss was medicine's gain.

Photograph of Sir Austin Bradford Hill, unknown date, unknown author. CC BY 4.0 <https://creativecommons.org/licenses/by/4.0>, via Wikimedia Commons. https://upload.wikimedia.org/wikipedia/commons/e/eb/Austin_Bradford_Hill.jpg.

the early studies that indicated the connection between tobacco smoking and lung cancer. As we know from the old adverts, smoking was viewed in a very different way back in the 1950s than it is now. Pollution from domestic and industrial smoke was at high levels (visible, for example, in the regular London smog) and cigarettes were actually thought to provide some protection against this; indeed, so many people smoked that it was sometimes hard to find non-smokers as a comparison group. Bradford Hill worked at the London School of Hygiene and Tropical Medicine, which was, and remains, a key training ground for epidemiologists in the UK, and he wrote influentially on the emerging discipline. Popper strongly influenced his thinking, and Bradford Hill was preoccupied with how we might provide evidence for causal relationships in health, given all the challenges mentioned about imprecise experiments in the natural world. In 1965, he came up with nine criteria that he felt provided evidence for a causal relationship. These are mostly quite intuitive, but they have been influential, and it's worth considering them in turn.

Strong associations

The first of Bradford Hill's 'causal criteria' is strength. This is simply say-
ing that very strong associations are more likely to be causal. There is a type
of cancer, for example, called mesothelioma, which affects the lining of the
lungs. It's thankfully quite rare and when it occurs, it's nearly always in people
who have been exposed to asbestos—a fibrous material that used to be incor-
porated in building materials, particularly for fire prevention, until its risks
became recognized. Asbestos isn't used any more, but it's important to check
old buildings carefully before any walls are knocked down or interfered with
(in case the fibres are released into the air), so exposure still crops up from
time to time. The fact that mesothelioma is so strongly linked with asbestos
exposure makes the causal relationship very likely—it's hard to think of any
alternative explanation for the observation (although you might want to take
things like cigarette smoking into account before drawing conclusions from
a study of this). There are quite a few other strong relationships, like asbestos
and mesothelioma, particularly with certain exposures in the environment
and/or from occupations. It's fairly obvious, for example, that radiation expo-
sure causes thyroid cancer, simply from its recorded occurrence in people
working with radioactive material without sufficient protection, or in the
high levels seen in those who survived the nuclear bombing of Japan or the
Chernobyl nuclear disaster in the former Soviet Union. Quite a few of the old
industries carried specific health risks that were well-recognized without the
need for research—for example, the Mad Hatter character in Lewis Carroll's
Alice in Wonderland is believed to be based on common symptoms observed
in that occupation because of the use of mercury in the manufacture of felt
hats and the poisoning that resulted (which causes slurred speech, memory
loss, and tremor, among other effects). Again, all of these strong associations
between an exposure (the toxin) and an outcome (the disease) are almost
self-evidently causal.

Consistent associations

The second criterion is consistency. This simply refers to similarities between
findings observed in different groups of people and different places. It under-
pins the need for multiple studies coming to similar conclusions rather than
relying on the results of a single study. So, a trustworthy researcher report-
ing their new finding, however excited they are, will tend to say 'this needs
replicating'—meaning that they'll be hoping other people find the same

thing. The risk factors for diseases that we take for granted—smoking and cancer, high cholesterol levels and heart disease, pollution and asthma, heavy alcohol consumption and liver disease—have all been found repeatedly in many different studies, and usually from fairly varied populations around the world.

Specific associations

Specificity is the third criterion, which states that an association of a particular exposure with a particular outcome is more likely to be causal. This clearly applies to some of the industrial exposures we've just considered—asbestos with the cancer mesothelioma, as well as more commonly with a particular pattern of lung scarring and inflammation called asbestosis. Another causal relationship exists between radiation and blood disorders, such as the anaemia that killed Marie Curie at the age of 66 as she pioneered research into radioactivity without fully appreciating its health effects.[2] Identifying new infections often relies on this specificity criterion—for example, the emergence of AIDS as a new disease in the early 1980s was followed shortly afterwards by the identification of the HIV virus as the likely cause; it was a virus that hadn't been seen before and was always present in people with the collection of AIDS-related diseases—a highly specific association. However, not all diseases look like infections to begin with, and sometimes it takes a while to look for and recognize that cause—for example, recognizing that a lot of stomach ulcers are potentially caused by the *Helicobacter pylori* bacterium, which meant that antibiotics started to be used in the 1990s as part of the treatment, rather than surgery; or recognizing that cervical cancer is particularly associated with human papilloma virus (HPV) infection, resulting in the introduction of vaccination programmes in the mid-2000s.

Cause before effect

Temporality, the fourth causal criterion, is self-explanatory. This is simply the principle that the cause should be observed first and the effect later on. For this reason, evidence is viewed as stronger when it comes from a study carried out over time (sometimes called 'longitudinal' or 'prospective') rather than

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[2] Or the leukaemia that caused at the age of 58 the death of her daughter, Irène Joliot-Curie (who also worked extensively with radioactive materials).

a snapshot or 'cross-sectional' study. If you wanted to investigate whether stress in men causes hair loss, you could do a quick and easy study where you interviewed men with different degrees of baldness and asked them how stressed they'd been feeling. However, you can imagine that it might be better (if more expensive and time consuming) to recruit a group of men with similar hair coverage, ask them about current stress, and then follow them up over the next few years to see who lost the most hair. The length of follow-up in health research may sometimes be very long. For example, there were a number of studies in the late 1990s interested in whether the conditions babies are exposed to in the womb might set them on course for higher or lower risk of heart disease as adults many decades later. That's clearly a hard hypothesis to test but was made possible because of old birth or pregnancy records that could be linked to current health. There are also studies that have been following people up for a very long time indeed. A British example of this started with examinations of infants born in 1946 and continued to keep in contact with participants over the years, who are now in their 70s and still kindly participating in interviews and examinations, providing very valuable information across a lifetime.

Graded relationships

The fifth criterion is called 'biological gradient', or sometimes 'dose-response relationship', and is easier to explain by example. If you were interested in whether high blood pressure was a risk factor for having a stroke or heart attack, you could follow up people with high blood pressure and normal blood pressure and compare the two groups, and you would probably find a higher risk in the high blood pressure group. This would be OK; however, you might be much more persuaded about causation if you could look across the range of blood pressure levels (from low, to normal, to slightly high, to moderately high, to severely high, etc.) and see a gradual increase in risk across those gradually increasing levels of exposure—which, indeed, is what happens. So, it's useful to measure different levels of exposure if you can, in order to capture this sort of relationship. For example, risk of lung cancer is higher in people who smoke a lot than those who smoke a little. The risk then drops a great deal if you stop smoking (and the longer you've quit, the better), although it doesn't ever quite reach the lowest risk in people who have never smoked. Again, this all adds strong support to the hypothesis that smoking is a cause of lung cancer, rather than just a marker of some other risk factor.

Some limitations

Up to this point, Bradford Hill's causal criteria are fairly straightforward and intuitive and there are only a few limitations. Strength and specificity are fine as criteria but have probably become less relevant, as we may well have found most of the strong and specific associations in health. Although it would be nice if there was a single cause 'out there' for something like Alzheimer's disease or schizophrenia, it's becoming increasingly likely that it would have been picked up by now if life was so simple. So, we're left having to assume combined effects of multiple weaker risk factors which are less easy to establish as definitely causal. Temporality is also unarguably important—the main thing is not to assume that causation always occurs in a single direction. For example, it would be straight-forward in a longitudinal study to show that people with worse physical health have a higher risk of developing depression. However, it would also be straight-forward to show in another longitudinal study that people with depression are more likely to develop physical health conditions. A lot of things work both ways. For biological gradients, these observations are helpful although not always seen—for example, while some conditions like lung cancer increase in risk both with smoking and with the amount smoked (higher risk in heavy smokers than light smokers), bladder cancer has a higher risk in smokers than non-smokers but doesn't seem to depend on the amount smoked.

Consistency is sometimes a tricky one, as inconsistency can often be illuminating. For example, if you were to think about multiple sclerosis (MS)—a disease of the brain and spinal cord where the insulating coverings of nerve cells are damaged—you might imagine it's the sort of condition that happens randomly to anyone, anywhere. However, from at least the 1920s it started to be recognized that MS was almost unheard of in tropical countries and became more common the further you were from the equator, north or south. This obviously alters the way that you think about the condition, making it less likely to be a random or purely genetic illness and more likely to be triggered by something in the environment (still unclear, as yet, although there are a number of possibilities). When you further examine the situation by looking at people who move from one part of the world to another, the risk seems to depend more on where someone grows up than where that person lives as an adult. Also, there are some exceptions to the rule—for example, low risk in Inuit populations very far north, and higher than expected risks in people living in Sardinia or Palestine. The inconsistency between populations here is therefore helpful in providing clues to the causes of this disease. A similar situation occurred with Alzheimer's disease in sub-Saharan Africa,

when reports began to emerge that it was hardly ever seen in older people from these countries. A research group at the University of Ibadan, Nigeria, therefore investigated this further, led by the neurologists Oluwakayode Osuntokun and Adesola Ogunniyi, and working together with colleagues at the Indiana University School of Medicine. Through careful and painstaking fieldwork, the two groups established that Alzheimer's disease was present in Nigeria, but at considerably lower levels in given age groups than was observed for African-American communities in Indianapolis. Again, this demonstrates the value of inconsistent findings in thinking about causation, as well as the importance of research carried out in a properly international context and not just in Western Europe or North America.

The last four criteria

After the first five criteria, there are a few added by Bradford Hill that overlap a bit. The sixth 'plausibility' criterion requires that there is a reasonable explanation for the observation, the seventh 'coherence' criterion requires that the observation fits in with evidence from other fields (for example, with laboratory science), and the ninth 'analogy' criterion requires broader similarities between different associations. On the topic of smoking and lung cancer again, there is clearly a plausible mechanism involved (components of the cigarette smoke reaching the parts of the lung where cancers form) and coherence with other research (biological evidence showing that components of cigarette smoke have the effects on cell DNA that are recognized to underlie cancer). There are also analogies to be drawn between the cancers arising in the lung, throat, and oesophagus where direct contact with cigarette smoke occurs. In the middle of these, the eighth (and perhaps most important) criterion is experimental evidence, which will be discussed at greater length in Chapter 10.

Nothing surprising, nothing new, nothing specific

As with every other principle we've discussed, there's nothing particularly surprising about any of Bradford Hill's criteria—they're rooted in human behaviour and the way we understand the world—that is, they're intuitive and widely used. If we go back to the process of a child's learning and experimentation (Chapter 2), clearly strong, specific, and consistent associations are

going to be ranked as more likely to be causal—for example, the particular light that continues to turn on every time a particular switch is flicked (and that ideally happens in a similar way for other similar switches in other people's houses). And we are naturally going to pay attention to 'dose response relationships'—the harder I hit a window, the more likely it is to break. Temporality is also important—if I wake up with diarrhoea and vomiting, I'm going to be thinking quite hard about what I ate the night before; if I find myself with an itchy rash on my hands, I'm going to be thinking about what I've been in contact with over the last few days that might cause an allergic reaction. Plausibility, coherence, and analogy are likely to be influencing what I'm thinking about cause and effect. If I've been lifting heavy objects and notice a pain in my lower leg the next day, I might wonder whether I've pulled a muscle there; if I know a bit more, I might recognize that disc trouble in the lower back can cause pain that's felt in the lower leg, so I might think about it as a back problem instead.

The principles apply more widely as well, whenever the knowledge being sought relies on observation of the natural world and when tightly controlled experiments aren't possible. For example, one of the strands of research into global warming has been the investigation of temperature fluctuations over the Earth's history and relationships with atmospheric greenhouse gases. The theory had been around for a while that higher carbon dioxide levels might act to increase global temperatures; however, evidence was needed that this was a causal relationship. Like health research, this is not an exact science that can be carried out in a laboratory, and we have to rely on what information we can find from the world around us. Also, like a lot of health research, there's no feasible experiment to be done, unless you count the rather unnerving experiment the world is currently carrying out—of letting greenhouse gases continue to rise in order to see what happens (and whether it will be too late to do anything now that the effects are being felt). Among other evidence, carbon dioxide and temperature measures were taken from columns of ice drilled by the Russian Vostok station in one of the remotest parts of Antarctica (Figure 7.3). These measures provide information over the last 400,000 years or so (four glacial periods) from a 3,500-m column of ice. Graphs taken from these show peaks and troughs of carbon dioxide regularly coinciding with peaks and troughs of temperature—an association, you might say, that shows strength, consistency, temporality, and graded relationships. Also, if you add in another time-varying measure like dust levels, the findings show specificity, in that temperature levels primarily follow the carbon dioxide changes and aren't obviously related to changes in dust.
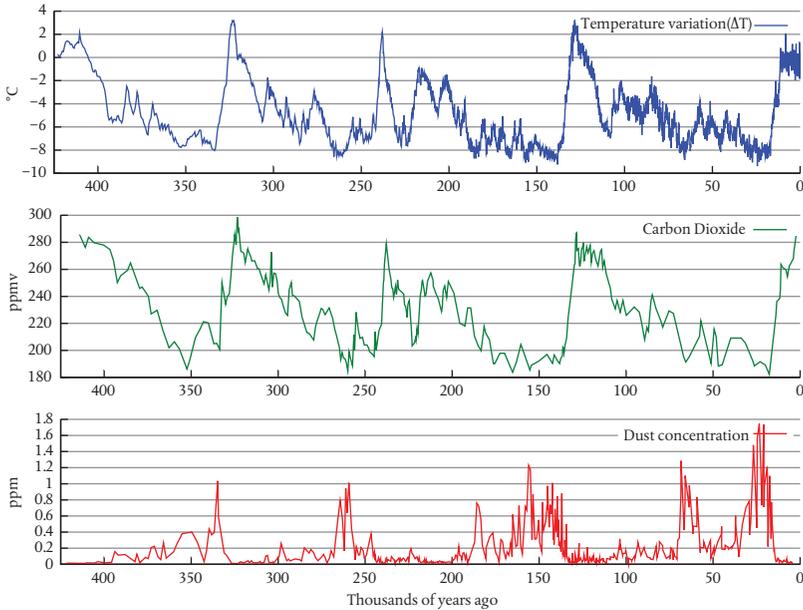
Fig. 7.3 Graphs of temperature, atmospheric carbon dioxide, and atmospheric dust over the last 400,000 years, estimated from ice cores drilled and extracted at the Russian Vostok Station in Antarctica. Like health research, climate change theory is not something that can be readily tested in a laboratory, so has to rely on this sort of information. The coinciding peaks of temperature with carbon dioxide, but not with dust, concentrations fulfil the strength, consistency, gradient, and specificity criteria for a causal association. So, they do look very suggestive, but they remain observations. Anthropogenic climate change is not something that can be demonstrated in a controlled laboratory environment, so inevitably has had to rely on findings that are supportive, but not 100% conclusive. The trouble is, as we know only too well, any residual uncertainty will be exploited by those with vested interests to argue otherwise.

In conclusion

In conclusion, we're faced with the fact that experimental falsification is all very well in theory, but it is harder to achieve in practice in many fields of research. However, these causal criteria and other features of observed relationships do at least help to rank findings in importance. They are therefore worth considering when designing studies, because you want your work to be as impactful as possible. There's probably not much you can do about the strength of an association (it is what it is), but you can certainly consider

consistency by making your study as easy for someone else to replicate as possible, or by trying to keep your measurements or sources of information as consistent with those used by others in your field (unless there's a good reason not to) so that your findings can be more directly compared. For specificity, in the greenhouse gas example we've just considered, it might have been problematic if only carbon dioxide had been measured against temperature because perhaps everything varies in the same way. The fact that dust levels were measured as well helped to show that this wasn't the case—so it's worth thinking about taking measures of other factors besides the one you're interested in. For temporality, consider whether you can separate in time the measurement of the proposed cause and the proposed effect, and finally, consider whether you can measure different levels of the proposed cause so that you can look at graded relationships with the proposed effect.

8

Consensus

Although experimental falsification seems nice and clear the way Popper puts it, it doesn't sound likely that a single person's finding has made the difference very often. Even the famous examples are more like a tipping point—the change in viewpoint happens and one person's work is credited with achieving this, but then when you look into it more closely, the change was probably going to happen anyway and often there's more than one person involved, even if the fame is unevenly distributed. Copernicus is credited with kick-starting modern science, but the understanding of planetary movements and the model of the Sun at the centre was as much a product of the subsequent careful observations made by Danish astronomer Tycho Brahe (quite possibly with his sister Sophia playing a significant role) alongside the mathematics of Kepler in Germany, and the understanding and development of the theory by Galileo working in Venice. A lot of the work on gravity and astronomy published by Newton in his 1687 *Principia*, might well have been completed or at least enabled by his senior colleague Hooke; however, they had a particularly fractious relationship (see Chapter 16) so Newton didn't credit Hooke and did what he could to obscure his legacy from history after he died. Darwin delayed publication of this theories on evolution by natural selection for around 20 years until he heard that Wallace was coming to the same conclusions and was persuaded to rush them out. Einstein's work built heavily on the preceding achievements of James Clerk Maxwell, whose theories brought together light, electricity, and magnetism into single unifying models.

The principle of falsification may therefore be best thought of as a process, rather than a single event. Yes, the idea is that a single inconsistent observation ought to be enough to falsify a hypothesis and require it to be refined; however, historically there don't seem to be many examples of single inconsistent observations that have been enough on their own to force the issue. Even in clear examples of experimental falsification, such as Fresnel's demonstration of light behaving as a wave (contrary to the Newtonian theory of particles; Chapter 5), it still took a while for new ideas to take on. When it comes to it, no one likes to have to change their mind. Academics ought to be better at it than everyone else, given the principles we've discussed of objectivity and acceptance of ignorance; however, unfortunately, the forces of conservatism are as strong in research communities as elsewhere, if not

stronger. So, we're still stuck between a high ideal and a messy reality—once again, how do we make this work? How do we advance knowledge if we can't prove something or disprove it either?

Bayesianism—avoiding the question altogether

One rather niche option is not to think in terms of binary truth/untruth in the first place. This is the approach taken by Bayesian statistics, named after Thomas Bayes, a Presbyterian minister living in Kent in the mid-eighteenth century who was a mathematician in his spare moments. He published very little in his lifetime, but his theories were promoted after his death by a fellow minister-mathematician Richard Price (Figure 8.1). Bayes' theorem is
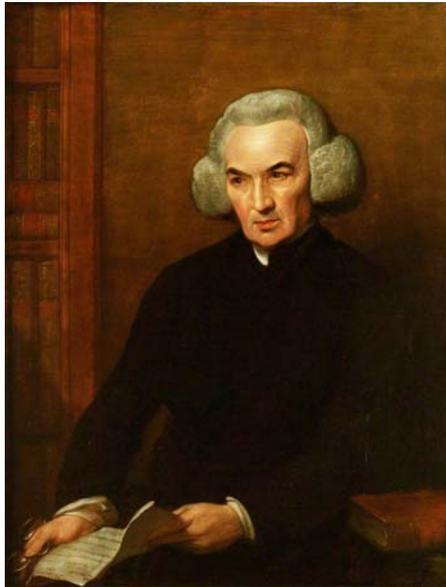
Fig. 8.1 A portrait of Richard Price, a philosopher, mathematician, Nonconformist minister, and political activist. Research is as much about those who facilitate and publicize as it is about the originators. Without his friend Price, the statistical techniques of Thomas Bayes would have gone unrecognized—important here because of the way in which they allow the issue to be ducked of hypotheses being true or not. Price is better known for being an influential supporter of the American independence movement— he was friends with Benjamin Franklin, whose letter is being displayed in this portrait. He was also a mentor and supporter of Mary Wollstonecraft as she developed her 1792 *Vindication of the Rights of Woman.*
Portrait of Dr Richard Price by Benjamin West (1784). Public domain, via Wikimedia Commons. https://upload.wikimedia.org/wikipedia/commons/1/1d/Richard_Price_West.jpg

an important milestone in probability theory, and Bayesianism describes an approach to hypotheses that views them not as being true/false, but as having a 'strength' or 'confidence' that exists along a scale and can be influenced by emerging evidence as this is brought into the picture, along with its own probabilities of truth. As you can imagine, it's a complicated process and probably not for the faint-hearted or non-specialist; however, it does at least provide one answer to the problem of how to deal with accumulating, rather than individually decisive, evidence. Whether Bayes intended his theories to be used in this way isn't clear. A lot of the legwork on developing and applying the principles was carried out by Pierre-Simon Laplace in Paris, the great mathematician and political survivor who we've come across in Chapters 5 and 6.

Consensus—verdicts of causation and paradigm shifts

In practice, consensus is a more important process. When he drew up his causal criteria in the 1960s (Chapter 7), Sir Austin Bradford Hill spoke about passing from an observed association to a 'verdict of causation'. This draws an analogy with a court case in which the facts of the matter are carefully considered, the evidence weighed up and a group ('jury') decision arrived at—which all sounds a bit like the Bayesian option without the probability theory. Either way, we're still left with the principle that findings build up and viewpoints change gradually—that each individual piece of research can be viewed as a smaller or larger 'brick in the wall'.

Thomas Kuhn was a science philosopher who considered this matter at length, publishing his major work, *The Structure of Scientific Revolutions*, in 1962 while working at the University of California, Berkeley. His theories promote the idea of the 'paradigm', a set of shared assumptions of truth, and phases of what he called 'normal science' where support for this truth is firmed up and its remit expanded. Contrary to Popper's principle, Kuhn proposed that inconsistent findings are viewed on their own as mistakes rather than falsifications; however, if they keep being found, they can build up to a crisis (a phase that he called 'revolutionary science') resulting in a new paradigm ('paradigm shift'). There was a fair amount of debate following these theories, and it looks as if Kuhn himself dropped the idea of the paradigm later in life.[1] I'm also not sure whether it can be applied to research as a whole—as we've considered over the previous chapters, some

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[1] Which sounds good to me—never trust an academic who doesn't change their mind.

changes in thinking have happened quicker than others (i.e. more like Popper's falsification principle), particularly when there were fewer researchers in the world. In other cases, the 'paradigm' seems to have shifted already before the research happens that might be labelled as revolutionary.

Kuhn's theories are particularly applied in accounts of developments in social sciences and in economic and political thinking. An example sometimes cited of a paradigm shift is the sizeable change in economics theory influenced by the academic work of John Maynard Keynes (Figure 8.2) in the 1930s. Keynes was an English economist whose reputation had earned him a senior position in the Treasury during the First World War, but he fell out with the government over the punitive conditions imposed on Germany

Fig. 8.2 The economist John Maynard Keynes (right) photographed in 1946 with Harry Dexter White, a senior US Treasury department official who was a key player in directing American financial policy during and after the Second World War (albeit while also suspected of acting as a Soviet informant). Keynes' work underpinned what can be seen as a 'paradigm shift' in economic thinking—possibly influential on US policy during and after the Great Depression of the 1930s, more probably influential on a number of governmental responses to the more recent 2007–2008 financial crisis. Keynes died of a heart attack about six weeks after this photograph was taken.

Photograph of John Maynard Keynes greeting Harry Dexter White. International Monetary Fund (1946). Public domain, via Wikimedia Commons. https://upload.wikimedia.org/wikipedia/commons/0/04/WhiteandKeynes.jpg

after the war and supported himself as a journalist writing generally anti-establishment economic tracts during the 1920s.[2] The 'Keynesian revolution' that bears his name was a particular result of his *General Theory of Employment, Interest and Money*, published in 1936 at the height of the Great Depression. The prevailing economic theory up to that time was broadly that there was nothing governments or treasuries could or should do to alleviate unemployment. Keynes argued otherwise and influenced a lot of subsequent government interventions during recessions. The influence becomes clearer as time goes by. It would be nice to say that it had an immediate impact on President Roosevelt's interventions in the 1930s US, as Keynes and Roosevelt clearly respected each other; however, it seems to have taken longer than that to filter through into orthodoxy. Either way, Keynes' revolution is a fairly clear example of a paradigm shift—revolutionary research[3] resulting in a change in thinking—although clearly views have swung around a fair bit since then. Keynesianism fell substantially out of favour during the 1970s and 1980s, when monetarism was on the ascendancy within the Reagan and Thatcher regimes; however, there was some revival of interest after the 2007–2008 financial crisis, and a lot of current pandemic-era policies look distinctly Keynesian, as if we're back in the 1930s all over again.

The mechanics of consensus

So, if consensus is important in advancing knowledge, how should this work? Within health research, there's a whole industry devoted to it. Systematic reviews seek to bring together all the research on a particular question in order to come to a common answer. The idea at the end of it all is therefore to pool the evidence, rather than rely on any one specific study—a mechanization of consensus, essentially. There are even techniques in statistics, 'meta-analyses', that help this process of pooling individual findings, trying to combine all individual studies into one big one, although clearly dependent on source measures being consistent enough for this.

The systematic review industry has had political as well as academic effects. Beyond trawling what are expanding and increasingly sophisticated digital databases of published work, there has been a growing realization that a lot of relevant material for consideration may be unpublished and require

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[2] Winston Churchill was Chancellor at the time and very much in Keynes' firing line.

[3] 'Research' in this sort of economics work involving, I'm guessing, a certain amount of induction (deriving theories from observations) and a certain amount of rationalism (building theories by logical argument and deduction). We consider the idea of the 'experiment' in economic theory in Chapter 11.

more active extraction than simply searching journals. Because most of the reviewing activity tends to focus on the results of medication trials,[4] there is also suspicion around selective publication by agencies with a vested interest (primarily, but not exclusively, pharmaceutical companies) and therefore stronger regulations to promote 'open science',—that is, ensuring that if a research study has been carried out, its findings can be found. For example, it's now expected for all medication trials to be pre-registered to stand any chance of subsequent publication, which at least provides some chance of discovering what's out there, even if the reviewer may still have to write around requesting unpublished results. The Cochrane collaboration and library were set up as a charitable organization to provide a repository for volunteer systematic reviews of healthcare interventions and diagnostic tests in order to make this combined evidence publicly available, rather than relying on the vagaries of medical journals and their editorial decisions. However, the initiative has its critics and there are concerns whether this repository and the wider reviewing culture might act to obscure or suppress findings that run counter to prevailing opinion (which is the whole point of falsification in the first place).

The challenge in consensus—anthropogenic climate change

Perhaps one of the most important, but rather fraught, consensus processes recently has been in the field of global warming and climate change. Like health research, the disciplines involved (e.g. meteorology, geology) are dealing with information from the natural world: information that is subject to any amount of random error and that can be quite difficult to measure directly. Also, there's no recourse to experimental data unless the course of world events over the last century or so can be viewed as one large experiment to see what happens (Chapter 7). Moving the field forward, and identifying the priorities for intervention, has required a consensus to be achieved—not easy by any means when there are so many voices, as well as quite a sizeable amount of misinformation linked to agencies with vested interests in opposition to the process. Even attempting to include it as an example here feels risky, but on the other hand it's simply too important a topic to ignore.

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[4] That is, answering the questions of whether a given medication works, how well it works, and (sometimes) how frequent are its side-effects.

A consensus on climate change is challenging to pull together to begin with because it involves several interrelated questions: first, whether global temperatures are rising, and second, whether these rising temperatures are due to greenhouse gases generated by human activity. After these fairly basic considerations, there are other elements where consensus is needed on actions that could be taken to arrest the process or mitigate the consequences. There is also no clear starting point because current climate change theory arises from a much longer period of research to work out the age of the Earth and the wide variation in its past climate over the time in which life has been developing and evolving. For example, Jean-Pierre Perraudin in 1815 observed very large boulders strewn across valleys in the Alps and proposed that they had been moved there by prehistoric glaciers. This piece of inductive reasoning (an observation generating a hypothesis)[5] was met with considerable scepticism at the time, but past ice ages began to be talked about within 20–30 years, drawing on further evidence, taken up by the geographically well-located Swiss scientists, Jean de Charpentier, Ignaz Venetz, and Louis Agassiz (the last of whom coined the term 'ice age' in 1837 before emigrating to the US and Harvard). Much later, by the mid-1960s, the recurring recent pattern of ice ages had been more accurately measured and a theory proposed and accepted relating these to subtle changes in the Earth's tilt over the right sort of periods which at least showed that climate was sensitive and could change very rapidly.

Over the same period in the nineteenth and twentieth centuries, the wider palaeontology community was mapping out the more distant changes in the Earth's climate since life began in its oceans. This included epochs such as the Eocene, which lasted from 56 to 34 million years ago and began with a rapid increase in atmospheric carbon, possibly due to volcanic activity, accompanied by mass extinctions and global temperature rises, resulting in subtropical regions at the poles and no ice sheets.

The idea of greenhouse gases began in the late nineteenth century. At Stockholm University, Svante Arrhenius (Figure 8.3), who was working on quite wide-ranging research in physical chemistry, proposed a theory in 1896 to explain the ice ages, suggesting that carbon dioxide could increase the Earth's temperature through a greenhouse effect. The result was that both carbon dioxide and water vapour were used as explanations for the Sun's heat

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[5] Which wasn't wholly novel—similar ideas can be attributed to Shen Kuo, writing much earlier in eleventh-century China. It seems a straightforward conclusion to make—that there have been demonstrable changes in the landscape (e.g. something that was once a seabed that's now miles away from any sea) that must have taken a very long time to occur. Chinese thinking of course wasn't constrained by seventeenth-century Christian bishops who had decided that the world was 6000 years old, but on the other hand, there isn't a sense of these sort of abstract eleventh-century ideas in China being taken forward as something worth developing further.

Fig. 8.3 Svante Arrhenius, a Swedish scientist and one of the founders of 'physical chemistry', applying this to investigating the relationship between atmospheric carbon dioxide and global temperature. Arrhenius' earlier doctoral work on electrolyte conduction had not impressed his doctorate examiners at Uppsala University, who awarded him a third-class degree. Undaunted, he extended this and ended up winning the 1903 Nobel Prize in Chemistry—an encouragement, surely, for anyone encountering a difficult PhD examination.

Photograph of Svante Arrhenius. Photogravure Meisenbach Riffarth & Co., Leipzig (1909). Public domain, via Wikimedia Commons. https://upload.wikimedia.org/wikipedia/commons/6/6c/Arrhenius2.jpg

being retained on Earth, but not on the Moon. However, at the time there was very little human carbon dioxide production, so Arrhenius thought that any appreciable warming was a long way off and might actually be a good thing.

A few years later, in 1899, University of Michigan geologist Thomas Chrowder Chamberlin published *An Attempt to Frame a Working Hypothesis of the Cause of Glacial Periods on an Atmospheric Basis*, which pulled together evidence available that explicitly linked climate in the past with atmospheric carbon dioxide. These ideas weren't immediately adopted, as the limited experiments of that time struggled to distinguish effects of carbon dioxide from those of water vapour, and it was assumed that carbon dioxide would be rapidly absorbed by oceans. In the early twentieth century, changes in solar activity were thought to be a more promising explanation for climate fluctuations, although this fell out of fashion after predictions failed. Carbon dioxide was taken more seriously from the 1950s after evidence showed there was very little water vapour in the upper atmosphere—thus removing it as a competing explanation. Improved understanding of ocean chemistry

also showed that absorption of carbon dioxide was much more limited than previously assumed. By 1959, scientific presentations were beginning to propose that global temperatures would rise, and polar icecaps melt, as a result of human carbon dioxide production. Improvements in spectroscopy and computer modelling allowed development of Arrhenius' original theory and more precise relationships calculated between carbon dioxide levels and temperature.

From the mid-1960s through the 1970s, popular opinion was reversed for a while. A survey of temperatures from weather stations indicated an increase from 1880 to 1940 but then a decrease after that point. At the time, there was parallel concern about increasing use of (coolant) aerosols, as well as a theory that the world was heading for a new ice age, based on previously observed recurring patterns. The majority of scientific opinion was still in favour of warming rather than cooling, but this was obscured at times in popular reporting.[6] By the early 1980s, levels of aerosol pollution were beginning to drop—there has even been some suggestion that these might have masked the warming caused by greenhouse gases for a while. Also, there was increasing acceptance that the survey results on post-1940 cooling had been overly influenced by a few severe winters in northern hemisphere countries and weren't adequately reflecting global temperatures. Evidence from ice core studies, such as the one carried out at the Antarctic Vostok Station (Chapter 7) (Figure 7.3), provided better long-term measures of temperature, as well as of carbon dioxide levels. Methane also joined carbon dioxide as a gas of concern in the 1980s and the Intergovernmental Panel on Climate Change was set up in 1988 to take the process forward.

Translating consensus into influence

Broadly speaking, therefore, the process has been one of growing consensus on climate change, although clearly the formidable challenges in the source research (geology, palaeontology, meteorology, chemistry) have sometimes impeded this process, or given rise to distractions and variability in opinions. This is hardly surprising—researchers investigating the health hazards of tobacco smoking similarly had to deal with the fact that many of those affected by its cancer and heart disease outcomes were living in cities with high levels of domestic and industrial smoke pollution, eating high cholesterol diets, and experiencing levels of work stress and malnutrition that

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[6] I certainly remember hearing more about cooling than warming when I was growing up in the 1970s.

are much less common today; at the time, living beyond the age of 70 was considered quite an achievement. Finally, both fields of research issued recommendations that impinged on personal lifestyles (reduced carbon footprint and smoking cessation) and required governments to do things they would rather avoid if at all possible. Governments are persuadable but tend to have a high level of inertia, and this inertia will be compounded by any hint of a lack of 'expert' consensus. All government ministers will have had their fingers burned at some point because of following the wishes of one well-meaning pressure group, only to come under fire from another group wanting the opposite—so they're understandably cautious.

And this is just at the altruistic end of human behaviour. It assumes that the research community ultimately wants what's best, but just needs to make sure that it's acting on a genuine consensus (rather than one group of the community simply shouting louder than the others). However, climate change is clearly a lot more complicated, and is unsavoury as a topic because implications of the theory collide head-on with the vested interests of certain sectors that have large amounts of money to spend on protecting these interests. Furthermore, unfortunately, it takes a lot less effort to undermine consensus than to build and maintain it. This is, of course, not new. Any researcher working on climate change needs only to glance at healthcare research in the late twentieth century to see similar examples of overt or covert financial support for activities to undermine consensus. The tobacco industry was the first to come under fire for this in the days when smoking-related harms were still under debate. More recently, certain sectors of the food and drink industry have also been scrutinized as public health attention has swung in their direction. This has been accompanied by concerns about the sometimes less-than-satisfactory vigilance of the pharmaceutical industry to adverse events and problematic off-label use, and then you can extend it (if you want) to ongoing debates around the mental health impacts of the dieting industry, the gambling industry, the social media industry, and so on. The Christian Church may come in for justified criticism over its inhibition of research at times, but I can't think of any action of the Church that has brought as much coordinated power and money to bear as some commercial sectors on unwanted research findings.

I don't feel that there's any need to name names for the players involved in climate change research—those parties interested in disrupting a consensus that threatens their interests know very well who they are. And, if it hasn't started already, the number of sectors with vested interests under threat is likely to multiply, as what is being asked of governments and populations moves beyond energy use to dietary choices and farming practices. Of course,

now that we're in the lovely twenty-first century, the debate has become as polarized and acrimonious as ever, magnified by social media's echo chambers. One side categorizes the other position as 'climate change denial'; those on the other side don't like this because it sounds too much like 'Holocaust denial'—they'd prefer the term 'climate change sceptics' because it sounds more like traditional scientific scepticism. The first side feel that this is inappropriate when pronouncements equate to the denial of established scientific facts, and then of course there's the question of where the dirty money's coming from to foment the argument in the first place.

What is academic consensus and what is oppressive conservatism?

I'm skating on thin ice here—even an attempt to describe two sides of a debate is to imply that there is some genuine debate and then to fall into the trap of being seen to belong to one of the parties involved (and the one to which I would really rather not give any time). I guess it's just an illustration of what consensus can mean nowadays, when the financial and societal stakes are so high. Galileo and Darwin were no strangers to controversy, but I'm sure they never dreamed it could become as poisonous. Historically, the process of consensus has broadly reflected how many researchers and other interested parties there were at the time. From 1500 to 1800 there weren't that many people with any influence, so achieving consensus just involved being a little politically assertive and ideally publishing a well-written book that caught the interest of a small, literate public (as Hooke, Newton, Laplace, and others managed to do). By 1900, academia had become more of a profession and the numbers of active researchers had expanded considerably. Unfortunately, this meant that the community had become more conservative, and it was more difficult for new ideas to filter through, particularly those that really were new and required a change in direction. It also probably meant that you were a lot less likely to exert any influence if you were outside the university system. Nowadays, it's reasonable to feel unsure about our position, and to wonder where we'll go next. Researchers work in industry nowadays as much as in universities, and perhaps increasing numbers are working in their own homes with no affiliation at all, connected as communities online, rather than as academic departments. Publication submissions are spiralling beyond what's possible to peer review, fuelled (at least in my sector) by an equivalently spiralling number of journals touting for business, particularly now that the profits are shifting towards authors paying for publication rather

than relying on sales to university libraries. Whatever the world is, it's very different from what it used to be.

As mentioned, consensus can be quite an aggressive process at times. While there may be very good justification for this, it doesn't feel quite right. Leaving climate change well alone and switching back to the less-charged territory of health research, if I was to carry out a robust study and find a disease where tobacco smoking actually had some benefits,[7] I might find myself in difficulties. For a start, I might feel reluctant to publish in the first place, because of the worry about giving the wrong public health message after all the years spent by my colleagues and forebears achieving a consensus that smoking is universally bad. Then, even if I *did* attempt to publish, I would expect a fairly hard time at the hands of journal editors and reviewers, who probably wouldn't appreciate the message and the implied challenge to a hard-won consensus. Or perhaps not—perhaps people are no longer so concerned about a massive tobacco lobby campaign to persuade everyone again to buy cigarettes. In fact, from what I remember, one of the last pro-tobacco stands in the health debate was the possibility that it might be protective against Alzheimer's disease (which was based on a reasonable hypothesis to do with stimulation of nicotine receptors boosting cognitive function). I'm not sure how much industry funding went into it; my recollection is that it turned out to come from studies that hadn't been particularly well designed, and the hypothesis faded without a fight.[8] Anyway, hopefully you can see the point—when it starts becoming too assertive, consensus risks drifting away from the dispassionate position we're supposed to occupy as researchers, and I think it can sometimes be taken too far, no matter how noble the reasons. We'll revisit this when we think about lessons learned from the COVID-19 pandemic (Chapter 17).

So, what can we do? Does consensus necessarily run against Popper's principle of falsification? I think there are a few points worth making. First, it's fine to be sceptical but perhaps keep it to yourself unless you've got some decent evidence of your own to the contrary (i.e. put up or shut up). Second, if you really have to criticize someone else's findings, try to suggest a realistically better way of carrying out the research, rather than just carping about poor study design when there wasn't really an alternative—better still, do it yourself. Third, try to avoid taking dodgy money—that is, funding that might influence the way you carry out or report your research. Fourth, if

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[7] Unlikely, I know, but let's remember that we're trying to be open-minded like Socrates, so anything's possible.

[8] Although it continues to crop up in the occasional 'I thought smoking was protective' comment from members of the public, so maybe it's still around somewhere.

you really have to take the money, be up front about it and make sure it's public so that others can make up their own minds about you. And try to direct your research away from what might be influenced (i.e. don't carry out a study if you want the findings to turn out one way rather than another). Finally, be polite to each other—we are supposed to be living in civilized societies, after all.

That's enough about academic bickering. A more fundamental problem with consensus as an answer to the original problem (how to work with gradual accumulation of knowledge) is the 'whose consensus?' question. I don't know anything about Sir Austin Bradford Hill as a person—he might have been a very amiable and liberal-minded fellow. However, my strong suspicion is that when he spoke about a verdict of causality in the 1960s, he was thinking about a jury full of relatively old white men. The 'scientific community' that people have in mind when they talk about consensus nowadays might be a little more diverse than it used to be in the mid-seventeenth century, when the Royal Society and Académie des Sciences were founded by Charles II and Louis XIV in London and Paris, or in the mid-eighteenth century, when the American Philosophical Society was founded by Benjamin Franklin in Philadelphia. Regardless, the scientific community is still elite. Perhaps it's right that it's up to those actually *doing* the research to determine their own consensus—it's just that it's a rather difficult concept to communicate nowadays when the public's patience has run a little thin with 'experts'. The conversation continues . . .

9

Designing Research

From Description to Theory-Building

We've spent quite a long time thinking about the principles underlying research in the last few chapters, so we now move on to actually *doing* research. The next few chapters consider some of the ways in which research projects are designed and put together. Clearly what follows is inevitably an overview. While there are things to be said that do cut across many research fields, there is also a wealth of material out there that's more tailored to individual disciplines and you're likely to need to read more widely for detail on your particular field of interest.

On that subject, if you're here as someone just starting research, I'm actually not sure how much you need to read up on research design in advance, at least initially. It's a little like pouring over the Highway Code and expecting to be able to drive a car or ride a bike. Instead, the textbooks tend to be more useful as a reference when you're underway, just as you might leave the Highway Code aside until you've at least figured out how to work the clutch and accelerator. Early in a research career there's really no better learning experience than actually carrying out a study, talking to other researchers, and getting good supervision. If they can be taught at all, discipline-specific research methods are best learned from a course with a strong emphasis on practical exercises. However, do try to apply what you learn as soon as possible, as memory fades very quickly.[1] The trouble is that courses and specialist textbooks don't tend to cover the wider context for research very much, which I guess is why I ended up writing what you're reading now.

Getting underway

How do you start a piece of research? The first step must be determining the reason for your study in the first place. What is it trying to achieve? What have other researchers done in that area? How have they done it? Could it

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[1] Statistical methods for data analysis are particularly prone to being forgotten if you don't apply them quickly, although I suspect it applies more widely.

be done better? All of this goes back to Socrates' 'I neither know nor think I know' statement and the idea of blank spaces on sixteenth-century maps (Chapter 3). Research begins with the idea that there's something to be discovered and it's important to put a little thought into exactly what that is. As I've mentioned before, 'acknowledged ignorance' and a genuinely objective and enquiring attitude are rare states of mind in practice—difficult to achieve and even more difficult to sustain. It's not going to get any easier as you immerse yourself in your project, so this period at the beginning is the best time to have a go, even if you only achieve enlightenment briefly over a cup of coffee one particular morning. Where exactly are those blank spaces on the map? Which particular shoreline are you going to explore? And what resources are you going to need to explore it?

Again, if you're beginning a research post at the early stages of your career, you're likely to be joining a study that's already been designed. That's fine— you can still try to get a feel for the context. Ask your supervisor what exactly it is that you're trying to find out. If they're any good as a supervisor (and if you catch them at the right moment) they'll get enthusiastic at this point, because 'finding out' is what really makes research fun and the reason why most academics put up with its more wearying aspects (who ever said that exploration was easy?). The learning curve may be steep but do try to become familiar with what's out there and how others have approached similar challenges in the past. You're hopefully going to do something different and move the field forward a little (otherwise why do the study at all?) but it's still important to understand the audience you'll be talking to when you've finished everything and are preparing your papers and presentations. Some people like Einstein have the gifts to match their arrogance and can take a research field forward on thinking alone with minimal reliance on the knowledge and experience of others. However, most of us are not Einstein.

Induction or deduction?

To begin with, it also helps to be clear whether you're in a rationalist or empiricist camp, or somewhere in between. This may sound rather daunting and philosophical, but it's not actually that complicated. To recap Chapter 6, the rationalist approach is to work towards a solution from first principles, applying logic and deduction to build up an argument. On the other hand, the empiricist approach derives knowledge from observation—drawing conclusions, proposing theories, and putting them to the test. We've tended to focus on the two extremes here: pure mathematics as an example of rationalism

and the natural sciences for empiricism. However, life is rarely that simple, and you won't be surprised to learn that most research sits in murky territory somewhere in the middle. For example, you may find that your field is labelled as primarily empirical, and you may find yourself carrying out individual pieces of research that involve taking observations and drawing conclusions. However, beyond your own conclusions from your own particular findings, if you're bringing your observations together with those from others, and trying to think more broadly, you might well find yourself deploying quite rationalist arguments (e.g. if X and Y seem to be true from observation then Z ought to follow from logical deduction). This is particularly the case if you end up carrying out any review of the literature with a narrative component—that is, one that goes beyond simply pooling observations from other studies but is actually trying to use the pooled findings to propose new knowledge. It's also almost inevitable that an individual research project will require you to consider your own findings in relation to those from past research, and you'll then probably end up making an argument about implications and what further research might be most useful. These sorts of mental steps forward are going to sound more convincing if they're carefully considered and argued. In other words, it's best for them to employ logical, deductive, rationalist principles, just as if you'd been putting mathematical constructs together to solve Fermat's theorem.

So, although most of the next few chapters on methods focus on the empirical end of research, the rationalist approach can't be ignored entirely. The two strands become particularly entangled in humanities. If, for example, you were wanting to research the author Charles Dickens and the rather odd and unsatisfactory way he tended to portray young women in his novels, your research is obviously going to build on 'observation' (i.e. a close reading of the novels and the characters in question, assembling material for case examples). Angelic, dutiful, rather bland, disturbingly childlike, and profoundly unrealistic female characters are not difficult to find. You could begin with obvious ones like Florence Dombey (Figure 9.1) (*Dombey and Son*), Ada Clare (*Bleak House*), Amy Dorrit (*Little Dorrit*), and Lucie Manette (*A Tale of Two Cities*), and then move on to consider earlier prototypes like Little Nell (*The Old Curiosity Shop*) or Kate Nickleby (*Nicholas Nickleby*), or less typical examples like Lizzie Hexam (*Our Mutual Friend*), Dora Spenlow (*David Copperfield*), and Esther Summerson (*Bleak House*).[2] However, although drawing

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[2] Esther Summerson, as a first-person narrator in *Bleak House*, is at least given a voice, albeit to rather unsatisfactory effect. Incidentally, I'm referring here to the characters portrayed by Dickens in his novels. Adaptations for film and television nowadays try their best to reinstate some agency in his heroines, although clearly sometimes it's a bit of a struggle.

Fig. 9.1 An illustration from Charles Dickens' *Dombey and Son*. Florence Dombey (pictured on the right) is one of Dickens' commonly occurring two-dimensional angelic young heroines, who pose a bit of a challenge for modern readers and adaptations. So, one hypothesis might be that Dickens simply couldn't write young female characters. On the other hand, another hypothesis (not necessarily contradictory) might be that these characters were primarily written as a means of showing up their inadequate fathers, who are the real focus (e.g. the emotionally impoverished Mr Dombey, pictured here), and that the subject was too close to home for Dickens to portray in any other way (he certainly steers well clear of father–son relationships). Addressing this in research is likely to require a mixture of empirical observation (systematically assembled source materials), inductive reasoning from these observations, and rationalist deduction (arguments developed from first principles).

Illustration by Hablot Knight Browne, from Dickens, C. (1848). *Dombey and Son*. Public domain, via Wikimedia Commons. https://upload.wikimedia.org/wikipedia/commons/f/f9/P_595--dombey_and_son.jpg

on observations, the approach being taken in this type of study is rationalist because you're unlikely to be coming to the material for the first time, 'discovering' these characters and then trying to find out what they mean. Instead, the examples are likely to be assembled as components of an argument and logical progression of ideas. Similarly, biographical facts about Dickens and evidence of his relationships with women might be drawn on as further components—observations, but ones that feed the logic and the

overall argument. For example, problematic fathers abound in Dickens's work and quite a few young women in the novels seem to exist primarily to show up their father's deficiencies—or else they're deployed more generally as mirrors on the world or menfolk in general (Little Nell or Ada Clare perhaps), but fathers are definitely a recurring target. So, you might therefore construct an argument that these particular two-dimensional female heroines might have more to do with Dickens's inability to get over or express the unsatisfactory relationship he had with his own father than the more simplistic view that he couldn't write parts for women.

Whatever side you come down on, the research process here is building its theories by logic and reasoning, not dissimilar to the building up of a case in a law court, although ideally in a more open-minded way—seeing where the logic goes and what can be defensibly argued in a particular direction, rather than having to work for the prosecution or defence. The legal analogy does make me wonder about Sir Austin Bradford Hill's 'verdict of causality' principle discussed in Chapter 8—whether it is actually taking research findings towards rationalism (achieving consensus through logic-based argument) when pure empiricism (inductive reasoning and falsification) has not been able to provide the answers. On the other hand, the apparent importance of consensus in more rationalist research fields (as discussed for pure mathematics research in Chapter 6) does suggest that (at least some) empirical processes are brought to bear, even if these models of Socratic dialogue (putting ideas under pressure) are not quite Popper's idea of experimental falsification.

But back to your research study … you can see by now that it's easy to muddle the most straight-forward thinking, so it's helpful to consider carefully what your own project is going to involve. How much is it akin to surveying uncharted territory—taking observations and filling in the map from these? Or how much is it akin to building up a case from source evidence, investigating whether it's possible to travel by logical argument from one piece of knowledge to another? If it's a bit of both, can you separate the strands a little? The deductive, rationalist approach will require you to work out, at an early stage, what evidence, arguments, or equations are watertight, and to think about how they might be combined. The inductive, empirical approach focuses on observations and accurate measurements, and an awareness of whether you're developing or testing a hypothesis. Both rationalism and empiricism can start from a dispassionate, objective position of ignorance, but they diverge early on in the approach taken to develop knowledge. It helps to know the difference.

Description *or* hypothesis-testing?

Another early issue to clarify is whether you're aiming simply to describe what's there or whether you're going to build a theory or test a hypothesis. Again, there can sometimes be confusion over this, and it helps to be clear because you want to know where you stand among the research principles that we've discussed. Your more discerning readers will definitely appreciate a recognizable framework when you come to report your findings. Also, trying to change objectives or the overall ethos of a research project mid-stream tends to be messy and unsatisfactory, so it's best to sort everything out at the start.

Having talked about the importance of the hypothesis, remember that not all research involves hypothesis testing, and this doesn't necessarily detract from its value. Within the more empirical disciplines, some studies are simply descriptive and that's fine. For example, when Hooke was writing his 1665 *Micrographia*, he was just aiming to describe what was present when you looked at living organisms and tissue through the first microscopes. It was an important step forward in knowledge, but it wasn't testing any particular theory. When Linnaeus was cataloguing all known plants and animals in his 1735 *Systema Naturae*, there was also no theory being tested—his classification system happened to work satisfactorily and was later adopted as a template for understanding evolution, but it wasn't set up to imply an underlying process; rather, it was just a convenient means of organization and communication.

Descriptive science continues to this day. If you wanted to know how common diabetes is, you would carry out a survey to measure this—trying to capture a representative sample of the population, then applying some technique to screen for and ascertain diabetes (maybe just asking people, maybe taking a blood test if they're willing). Alternatively, rather than going out and examining lots of people, you might make an estimation from pre-existing information—for example, health service records. The first option is better because some cases of diabetes go unrecognized and therefore don't appear in health records (the source material for the second option). However, population surveys are clearly more expensive and time consuming, so you'd need to justify the effort and funding involved. The usefulness of health surveys never goes away—disease frequencies change (e.g. diabetes is becoming more common in many countries), and you can't assume that information from the 1980s, for example, is still applicable today. Surveys are obviously not restricted to health topics; people are regularly being asked questions about all sorts of things: attitudes, lifestyle, and behaviours, for example. Similarly,

descriptions are repeatedly being taken of the natural world in botany, zoology, geology, meteorology, astronomy, and others. All of these are simply measuring how things are at a given time (or changes over time, or variations between locations/regions). The knowledge is useful in itself, and there doesn't need to be a theory or hypothesis that's under evaluation. However, it's helpful to be clear about this when you're embarking on such a study.

Description *and* hypothesis testing?

The next step along the path (if you can view it that way) takes us to studies that involve observation but are beginning to think about cause and effect (i.e. moving on from just describing what's there to understanding why it's there or what it's doing). In my field of epidemiology, we talk about 'descriptive' vs. 'analytic' studies to draw this distinction, but the boundaries can be quite blurred. For example, John Snow set out to describe the distribution of cholera cases in the 1854 Soho outbreak (Chapter 1), but he was using this description to test whether it fitted an air-borne or water-borne model of infection (and he had ideas about this from earlier studies suggesting that the miasma theory might need to be refined). Similarly, the ice columns drilled at the Antarctic Vostok Station have been used to describe temperatures during and between the last few ice ages; however, they have also been used to estimate methane and carbon dioxide levels, among other measurements, and the interrelationships between proposed greenhouse gases and global temperatures are obviously key to testing climate change hypotheses.

Research in social sciences in particular, involves both describing and seeking to explain a situation. The studies involved often carry out what is called qualitative analysis (i.e. using recorded information from open-ended interviews with relevant people or groups to see what themes emerge from the responses and describing that narrative[3]). A commonly used framework for qualitative research is 'grounded theory', which was originally developed in the 1960s by the US sociologists Barney Glaser and Anselm Strauss. In many respects, the approach is pure description. If you carry out research in this way, you are encouraged to keep yourself deliberately free of preconceptions in order to improve the quality of the information you obtain. You will definitely need a supervision process that challenges you to stay objective and avoid introducing personal bias, and the general rule is not to review any background literature in advance of the study for the same reason. The idea

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[3] As distinct from 'quantitative' analysis that might come from surveys administering pre-prepared questionnaires with checkboxes, allowing results to be analysed numerically and via statistical methods.

of a prior hypothesis here is almost a contradiction in terms because it might influence your approach and produce self-fulfilling results.[4] On the other hand, your observations as they emerge might well be allowed to influence the study design as it is carried out—for example, you may need to change the questions you're asking or the people you're approaching for interview in order to improve on the knowledge you've acquired so far. At the end of it all, you'll be organizing the information obtained and beginning to categorize it according to the themes that have emerged, not unlike Linnaeus finding a categorization system for plant and animal species—still very descriptive. However, you might then start integrating your own findings with models that other researchers have previously proposed and decide how well they fit one model or another. At this point, it therefore starts to become more analytic—testing out theories, putting ideas under pressure—although opinions and practices start to diverge on what to do here, including the more recent views of Glaser and Strauss, who founded the theory in the first place.[5]

The emergence of history ('historiography' technically) as an academic discipline might also be seen as a gradual shift from the descriptive to the analytic. As we discussed in Chapter 4, the eighteenth century saw developments that were mostly focused on what might be called pure 'measurement'—that is, a recognition of the importance of primary sources by the time Gibbon published his *Decline and Fall of the Roman Empire* in 1776. In addition, there was a broadening out of source material in the histories of Voltaire and David Hume, who were no longer just covering monarchs and battles, but were considering entities like societies, culture, and customs. However, although a historian like Gibbon had plenty of opinions to express (not all of them particularly savoury nowadays), his main objective was still to put events together and describe what happened to the Roman Empire and its successor states. Later on, through the nineteenth century in particular, history writing started to drift closer to philosophy and even scientific methods in places—moving on from just describing the past to formulating these descriptions into broader theories. For example, in the 1840s, Thomas Carlyle was arguing for the importance of 'great men' in shaping history, while Thomas Babington Macaulay was proposing a rather optimistic account of the progressive development of civilization towards freedom of belief and expression—neither of them particularly fashionable views nowadays, nor borne out by the disasters of the twentieth century. Meanwhile, a young Karl Marx was hooking up

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[4] That is, if you're desperate enough to find something, you'll probably end up finding it, but that doesn't make it true.

[5] Not a problem—as mentioned before, it might be messy to have differing views and a bit of debate, but you should also be cautious about too close or cosy a consensus.

with Friedrich Engels in Paris and developing a rather different outlook as he drew his own conclusions from history, applied them to economics and politics, and put it all together in *Das Kapital* 20–40 years later.

Therefore, nineteenth-century history writing might possibly be squeezed into an analogy with the sciences in its movement from simple improved description in the eighteenth century (e.g. Gibbon and his search for primary sources) towards inductive reasoning and theory-building (e.g. Marx and Engels). If we follow this analogy, twentieth-century history writing might be said to have moved further still by taking preconceived ideas and putting them under pressure. In a very concrete sense, the relationship between history and archaeology is often one of direct testing of hypotheses. For example, the Hittites (Figure 9.2) as a people were very little understood up to 1870, with no contemporary Greek records, only brief mentions in the biblical Old Testament, and a reference in Egyptian records to a war with the 'Hati' people. The historian Archibald Sayce carried out extensive study of scattered
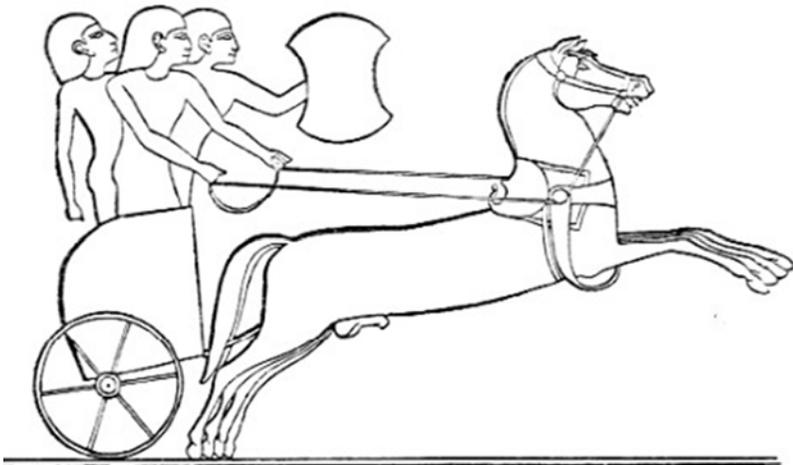
Fig. 9.2 The Hittite Bronze Age civilization was a classic 'lost empire'. Until the late nineteenth century, all that was known about them came from a few references in the Old Testament, and some Egyptian mentions, like the picture here of a Hittite chariot. Research in this case follows an approach more akin to scientific theory: initial observations (similarities between as-yet-unreadable hieroglyphs across a wide area of central Turkey), inductive reasoning and hypothesis generation (that there had been a sizeable kingdom there, as yet unknown), followed by further enquiry and support for the hypothesis through new archaeological findings. The result was a transformation in our understanding of that place and time.

Drawing from an Egyptian relief from Volz, P. (1914). *Die biblischen Altertümer*, p. 514. copied from de:Hethitischer Streitwagen.jpg. Public domain, via Wikimedia Commons

hieroglyphs across Turkey and proposed in 1888 that they might indicate the presence of a hitherto-unknown empire. This wasn't viewed with favour at the time because he couldn't find a way to decipher the language. However, it led to interest in excavations in that region and around 20 years later, archaeologists Hugo Winckler and Theodore Makridi discovered a stockpile of clay tablets in the Hittite language around Boğazkale, central Turkey, which were enough to allow a translation process to begin. This, and further excavations, established that this site was indeed the capital of a major empire in the fourteenth and thirteenth centuries BCE, supporting Sayce's hypothesis.

Rewriting history—but whose consensus?

Another way in which history moved towards putting ideas under pressure from the twentieth century onwards was through taking known evidence and rereading it with a fresh perspective. This was a particular concern for W. E. B. Du Bois (Figure 9.3), a sociologist and civil rights activist as well as a historian, and, in 1895, the first African-American to earn a doctorate at Harvard. Later in 1935, while he was working as a professor at Atlanta University, Du Bois published what was perhaps his most important work, *Black Reconstruction in America*. This took on a widely held account of the 1865–1877 Reconstruction period after the American Civil War that portrayed the universal Black voting rights at the time as being a failure, collapsing into chaos until it was 'saved' by White southern Democrats.[6] Clearly this accepted history underpinned a great deal of inequality and frank racism, mixed together with a bit of Social Darwinist pseudo-science to suggest that Black people couldn't/shouldn't provide political leadership. Du Bois, in response, went back to the source material and retold the story, refuting the idea that the post-emancipation South had descended into chaos, citing examples of public health, education, and welfare measures, as well as the continuation of many post-Reconstruction policies by Democrats for a long time afterwards. This different narrative took a while to take hold nationally (the original 'Dunning School'[7] view was still taught into the 1960s) but became in time a key element of the Civil Rights movement.

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[6] Interestingly portrayed as a battle against the reforming liberal Republicans in those rather different political days.

[7] William Archibald Dunning, working at Columbia University, New York, in the late 19th and early 20th Centuries, was by all accounts an influential and supportive teacher who was much respected by his students, many of whom went on to become leading scholars themselves in Southern US universities, hence the 'Dunning School'. The trouble was that the theories he so supportively propagated were very

Fig. 9.3 W. E. B. Du Bois, sociologist, historian, and civil rights activist, particularly known for the challenges he raised against accepted narratives of the time about African-American contributions to the Reconstruction era after the American Civil War. The competing hypotheses can be said to be derived from inductive reasoning based empirically on source observations. The prevailing theory was that Reconstruction had been a failure, leading to an inference that African-American people were not fit to hold power. Du Bois' contribution involved a reappraisal of the source historical records, demonstrating that the pre-existing observations were unsatisfactory (they were selective and biased) and that the conclusions drawn from them were therefore unsound. Research in history can be as much about putting ideas under pressure as it is in the sciences, although the challenge of adjudicating between competing theories (e.g. according to whose consensus?) is likewise fraught with difficulties.
Photograph of W. E. B. DuBois by James E. Purdy (1907). National Portrait Gallery Public domain, via Wikimedia Commons. https://npg.si.edu/object/npg_NPG.80.25

The process of revisiting and retelling history from new perspectives is a common and familiar enough research technique, widely used today. Like all observational investigations, this sort of approach depends on processes of consensus at the end of it all, which in turn can range from polite debate to shouting and abuse. And there's still the vexed question of 'whose consensus'? At one extreme, there are countries rewriting history for their own ends,

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weighted towards a narrative of Southern Confederates and plantation owners being oppressed by Northern Republicans and freed slaves (a view of US Civil War history that was further popularised in the 1930s by the novel and film of *Gone with the Wind*). I guess this illustrates the potential downside of effective academic mentoring.

and individuals who, for reasons best known to themselves, attempt to undermine well-attested atrocities and claim that this is just academic scepticism, wilfully ignoring (at best) the way this is used to fuel racism and its attendant intimidation and threat. That's the unsavoury end of things but easier to counter because it's clearly visible. In more balanced debates about conflicting theories (i.e. each with their own proponents and each with bodies of evidence to back them up), you would be forgiven for wondering where 'academic consensus' ends and where 'the fashionable position' begins. Perhaps there needs to be a little more acceptance that conflicting theories can coexist, without getting angry about it, just as light can be usefully described as behaving like waves in some respects and like particles in others.

In summary, many fields of research begin with description—the collation and organization of relevant information (Chapter 4). Next, there are the theories built from description (Chapter 4). And then there may be competing theories built from more description that require some process to decide which theory to hold. In most academic fields the decision between theories relies on consensus and a rather nebulous process of acceptance by some community of experts and fellow researchers (Chapter 8). Sometimes this consensus process is civilized, and sometimes it's acrimonious; sometimes logic and rationalism can hold sway if the equations or theories are watertight; sometimes there isn't that luxury and it's as easy to argue one way as another; sometimes the consensus is plain wrong—for example, just following a fashionable way of thinking, or swayed by an individual or group with the loudest voice or the strongest political influence.

On the importance of staying curious

What can you do about all of this when you design your studies? Well, smoothing the path to knowledge is everyone's responsibility. At any stage in your research career, you can try your best to maintain integrity:

1. Take your observations as carefully and as accurately as you can manage.
2. Try to separate the observations you're reporting and the conclusions you're drawing, so that there's a clear distinction between the two.
3. Don't be selective in your reporting—if observations disagree and don't all support your theory, be up-front about this.
4. Be careful about personal bias when you're drawing conclusions—if you find yourself wanting the results to go one way or another, there may be

no point fighting this, but keep it uppermost in mind and try to stay objective and truthful, nonetheless.

If there are arguments to be made in drawing conclusions, make sure they're robust and think carefully about any alternative explanations. If your arguments don't hold, change them, and this is vital: don't be afraid to alter your opinion and follow the logic or the evidence. There's a lot of satisfaction to be had in a well-conducted research study, feeling that you're genuinely discovering something, however small, and regardless of whether the findings (or the conclusions you draw) are positive or negative.

Finally, getting to know others working in the same field is an important part of a research career because you need to understand the community who'll provide the consensus at the end of it all. Most disciplines are moving away from isolated university structures to global networks, and the more you get to know your colleagues, the less tricky any differences in opinion are likely to be further down the line. Also, it's good to be part of a community and there are plenty of genuinely nice people out there. Academic debate is vital if knowledge is to be based on robust and well-tested theories, but there's no reason to make it personal or vindictive. We're all ultimately working towards the same goals if we're serious about meaningful research and if we expect the rest of the world to respect expertise. Acrimony in academia reflects a failure of the research field or institutions affected, but we can all play a part in fixing it.

10

Designing Research

Experiments

If you're trying to work out causes and effects, there's really nothing better than an experiment involving an intervention (and I'm going to use evaluations of interventions and 'experiment' interchangeably here).[1] We've talked a lot so far about researchers taking observations and deriving theories (Chapter 4), even putting those theories to the test by repeated studies with more observations (Chapter 5). However, at the end of it, you're left feeling not completely sure what's true, even with the best observational studies—there's still a lingering uncertainty whether the conclusions have been drawn correctly, or whether there might be alternative explanations aside from the theory you're proposing or testing. On the other hand, if you *do* something, make a change, and then see what happens, you've got a much better chance of working out a cause and effect. It goes back to Chapter 2 and the little child playing with a light switch. The child might notice adults using the switch to turn the light on, but she's not going to be convinced about the connection between the cause and consequence until she's had a few tries herself (i.e. has moved from observation to experiment). A successfully run experiment is the closest you can get to actual proof, and for some questions it may only take a single experiment by a single researcher to make the difference (although it helps if others can repeat it to make sure).

As mentioned, not all research fields have recourse to experiments, and we consider some of the alternative approaches in Chapter 11. However, the message remains that if you can test something experimentally then you should try to do so. The trick in designing an experiment is being sure at the end of it all that you actually did what you were trying to do. You're starting with a situation, changing it in some way, and seeing what happens. And you're hoping that whatever you observe at the end is a consequence of your intervention and there aren't any alternative explanations. So, you

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[1] Some people have a broad view of 'experiment' as encompassing any new study, including observational research, that's set up to test a hypothesis; however, I think that most would restrict the term to studies involving an intervention of some sort, so that's what I'm implying here.

need to be sure what the conditions really were at the start, you need to know what exactly was introduced (i.e. actually changed) in your intervention, and then you need to know what exactly happened as a consequence. The design of an experiment is partly a matter of keeping the conditions under tight control—so that you know it was your intervention that changed things and not some contaminating factor—and it is partly a matter of accurate measurement—making sure you can identify what happened as a result of the intervention.

Experiments have, of course, been carried out for as long as we've been a species and go back even further to learning in animals. Conditioning behaviour (e.g. Pavlov's dog salivating to the sound of the bell because it expects the food when the bell goes off) is essentially all about learning by the repeated pairing of cause and consequence. However, the pairing of cause and consequence won't occur unless the animal has done something new in the first place (i.e. has carried out an experiment, of sorts). This in turn requires the attribute of curiosity, and it's reasonable to argue that *Homo sapiens* has come to its current level of dominance as a species through a particularly high level of curiosity,[2] accompanied by conceptual thinking to accelerate learning, and by the communication skills to bypass the need for repetition of the same learning with every generation (and I suppose opposable thumbs have helped as well).

Understanding pressure—Robert Boyle

So, we've always learned from experimentation. Presumably fire-making was discovered by someone seeing what would happen when they rubbed sticks together or struck flints. And agricultural science as a discipline clearly emerges, like mathematics, out of prehistory as a result of any number of experiments in crop production and animal husbandry, helping us to adapt to settled lifestyles and feed civilizations. Metallurgy follows soon after, with developments in bronze and iron, also presumably underpinned by experimentation. Skipping forward to the seventeenth century, the invention of vacuum pumps was certainly an important step forward in what we would call modern science. If you're beginning to study chemistry and want to understand how substances behave, you need to be able to do this in isolation from everything else in the atmosphere. Robert Boyle, often credited as the founder

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[2] We certainly seem to be singularly unable to stop finding things out, even when the consequences are potentially species-threatening.

of modern chemistry, was one of the pioneers in the field and laid a lot of the groundwork with his painstaking experiments—showing, for example, the relationship between the pressures required and the volume of air compressed or expanded (the law that bears his name), as well as demonstrating that life and combustion depended on the presence of air (or whatever it was that a vacuum pump was removing).

Boyle was also able to show the relationship between air pressure and the boiling point of water, providing the experimental evidence to explain why this changes at the top of a mountain. I'm not aware that there was a relevant body of observation in advance of Boyle's findings, but you can imagine that simply showing that water has a different boiling point at the top of a mountain than at sea level doesn't provide any single underlying theory—because it's just an observation combined with speculation, however inductive the reasoning. On the other hand, clearly there's the opportunity to carry out an experiment here—you could be heating water in an identical location, applying different air pressures, and seeing what happens to its boiling point, knowing that nothing else has changed. The difficulty lies in how to design that experiment practically with limited seventeenth-century equipment. The water needs to be heated in a sealed container, because you need to be able to vary the air pressure, but you also need to be able to measure the temperature of the water at boiling point, as well as the actual pressure of the air in the container. So, Boyle's experiment involved creating a quite complicated apparatus incorporating a mercury barometer and thermometer inside a sealed glass vessel—a sort of ship-in-a-bottle construction (Figure 10.1). This is indicative of the experiments of the time, which were as much about having someone good and inventive to manufacture your glassware and other equipment as it was about the study design. Boyle was able to put together his own team of assistants (including a young Robert Hooke) and essentially construct his own research institute, thanks to the income from his late father's extensive estates in Ireland.[3] Experiments do tend to need funding.

As one of the first members of the British Royal Society in 1662, Boyle was at the heart of the early revolution in Western European thinking that laid the foundations for modern science. He was an avid admirer of Bacon's writing, and Boyle's 1661 masterwork *The Sceptical Chymist* became widely credited as beginning the slow shift from alchemy and mysticism to chemistry and Enlightenment thinking. The opening sentences of his first chapter are worth

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[3] Boyle therefore belonged to the hated class of absentee landlords in Ireland, although was personally benevolent and philanthropic as far as we can tell.
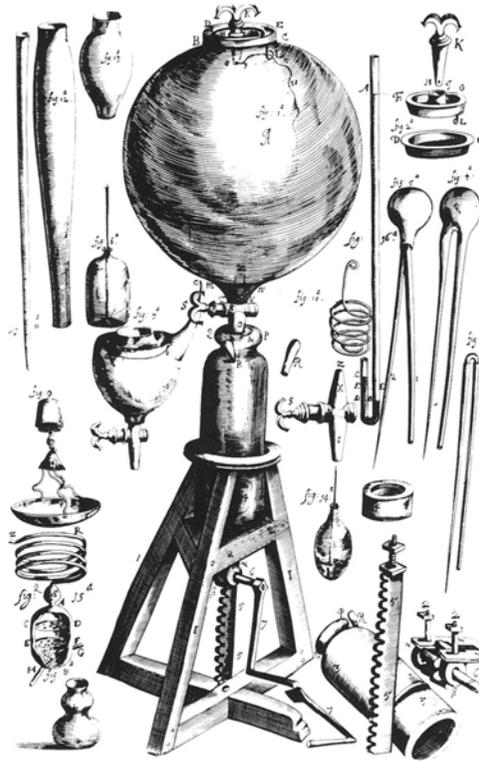
Fig. 10.1 Early science is as much about its equipment as its practitioners. Robert Boyle was interested in things that were taken for granted like air, atmospheric pressure, and the boiling point of water. In order to be able to investigate these, he needed an apparatus that would allow temperature and atmospheric pressure to be varied, hence his air pump pictured here. In those early days, nothing came very close to achieving a complete vacuum—a 'low air pressure chamber' would be a better description of his apparatus, but you can see the beginnings of an environment being assembled to allow experimentation, which depends on being able to impose the cause, measure the effect, and keep everything else as constant and unaltered as possible.

Drawing of Robert Boyle's air pump. From Boyle, R. (1661). *New experiments physico-mechanical: touching the spring of the air and their effects*. Public domain, via Wikimedia Commons. https:// upload.wikimedia.org/wikipedia/commons/3/31/Boyle_air_pump.jpg

a quote. Although he was demolishing the 'earth, air, fire, and water' theory of matter popularized by Aristotle, you can almost sense Socrates nodding away in the background:

> I perceive that divers of my Friends have thought it very strange to hear me speak so irresolvedly, as I have been wont to do, concerning those things which some take to be the Elements, and others to be the Principles of all mixt Bodies. But I

blush not to acknowledge that I much lesse scruple to confess that I Doubt, when
I do so, then to profess that I Know what I do not.

Discovering gases—Priestley and Scheele

Experimentation, rather than simple observation of the natural world, was at
the heart of Boyle's work, underpinned by the principles we've already cov-
ered of acknowledged ignorance and theory building. Although Boyle didn't
get much further than proposing that matter was composed of particles in
motion and their collisions, you can trace a fairly direct line between exper-
iments with vacuums to experiments with purified gases, both individually
and in combination. Around 100 years later, Joseph Priestley (Figure 10.2),
while working as a minister in Leeds, had already been investigating 'fixed
air' (carbon dioxide), detecting it initially in a local brewery as a layer above
the vats of fermenting beer. He then worked out how to produce it from chalk
using sulphuric acid and invented soda water as a by-product. This develop-
ment of fizzy drinks was a great success in eighteenth-century society and
attracted the attention, in turn, of Lord Shelbourne, who provided Priestley
with a laboratory of sorts on his estate in Wiltshire. Priestley produced oxygen
there in 1775 by heating mercuric oxide in a jar, using sunlight and a lens to
create the temperature required. He then carried out a series of what are now
well-known experiments to demonstrate the properties of this new gas—for
example, a candle flame flaring up when immersed in it, or a mouse surviv-
ing for twice as long in a sealed oxygen-filled vessel than in one containing
air alone.

Priestley was the first to publish this new finding, although like many
apparent discoveries, it had already been made a little while earlier—in this
case independently by Carl Scheele, working in Uppsala in 1772, who hadn't
got around to submitting his findings and was beaten to it. This doesn't seem
to have mattered too much, as Scheele went on to discover quite a range of
other elements and compounds (including barium, tungsten, and hydrogen
cyanide). Unfortunately, safety procedures weren't considered back then, and
Scheele's early death aged 43 was probably a result of exposure to any number
of toxins. Priestley, meanwhile, drifted into radical nonconformism and rev-
olutionary politics, which became increasingly inconsistent with a settled life
in Britain (he narrowly escaped a nationalist riot in Birmingham). In the end,
he emigrated with his family and settled in Pennsylvania, where he became a
good friend of President Jefferson. Priestley continued to write and publish
avidly until his death in 1804.
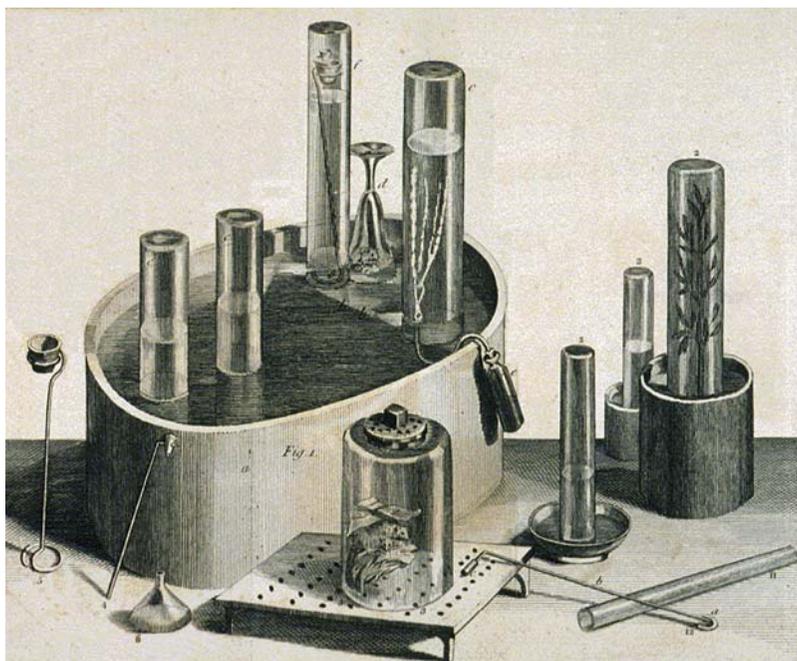
Fig. 10.2 By the time Joseph Priestley was carrying out his investigations in the 1770s, the quality of apparatus had advanced considerably to the extent that different gases could be isolated, and their effects investigated on combustion and life (hence the two unfortunate mice in the foreground). Priestley was able to obtain the necessary environment for his work on oxygen through aristocratic sponsorship following his earlier discovery of carbon dioxide and invention of soda water. Unfortunately, he fell out with his sponsor for reasons unknown and his later life, although productive, was dominated by a disappointing scientific conservatism, unable to accept much of the subsequent progress in his field.

Drawing of Joseph Priestley's equipment. From Priestley, J. (1775). *Experiments and observations on different kinds of air*. Public domain, via Wikimedia Commons. https://commons.wikimedia.org/w/index.php?curid=7123539

Figuring out molecules—Cavendish and Lavoisier

Once the constituent components of air were discovered, it wasn't long before researchers started looking at them in combination, developing the knowledge we now take for granted about elements and molecules. However, this still required access to funding in order to build and maintain the equipment required—a frequent challenge with experimentation (which is rarely possible on a small budget). Henry Cavendish, like Boyle, was a gifted British scientist who also luckily happened to have a sizeable inherited

personal fortune.[4] Unlike Boyle, he was a shy and retiring individual who was clearly more enthused by the process of discovery than by telling the world about it; consequently, although he did publish occasionally and was widely respected, a lot of his discoveries weren't appreciated until much later. One of these arose from experiments combining the gases that we now know to be hydrogen and oxygen to make water, using a technique and apparatus developed by Alessandro Volta (the Italian physicist and chemist who also invented the electric battery and founded the field of electrochemistry). This involved a sealed copper or glass vessel into which mixtures of gases could be introduced and then ignited with an electric spark—therefore ensuring that it could be weighed before and afterwards with nothing escaping and nothing introduced (as might have happened if a flame had been used). By carrying this out repeatedly with different ratios of hydrogen and air, and measuring the pure water that resulted, Cavendish was able to demonstrate the H_2O formula (two atoms of hydrogen combined with one atom of oxygen). Others were engaged in similar activity at around that time (notably James Watt, the inventor of the steam engine) and tending to get into print more rapidly, but Cavendish's prominence in the discovery became generally acknowledged.

To complete the story of the beginnings of modern chemistry, one of the scientists simultaneously publishing with Cavendish (and not always giving him the credit he deserved) was Antoine-Laurent Lavoisier (Figure 10.3) working at the Académie des Sciences in Paris. Lavoisier was particularly interested in heat and one of his experiments showed that the volume of ice that melted as a result of the body heat of a guinea pig was equivalent to the ice melted by burning enough charcoal to make the same amount of carbon dioxide. This demonstrated that animal respiration could be considered as a form of combustion, combining carbon and oxygen to produce energy (heat) and carbon dioxide. Lavoisier coined the names of oxygen and hydrogen (among other substances), developing the language of chemistry in much the same way as Carl Linnaeus (Chapter 4) had defined the language of biology, moving it finally away from its origins in alchemy[5] and setting it up for Mendeleyev's Periodic Table around 70 years later (Chapter 3).

It's important to note that a significant component of Lavoisier's achievements was made possible by his wife, Marie-Anne, who worked with him in his laboratory and made valuable translations for him of works by his

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[4] Cavendish was described at the time as 'the richest of the wise, and the wisest of the rich'.

[5] As well as disproving phlogiston theory—a seventeenth-century idea that there was a fire-like element released during combustion, accounting for the visible changes in burned materials. This fell into disuse once oxidation was understood.
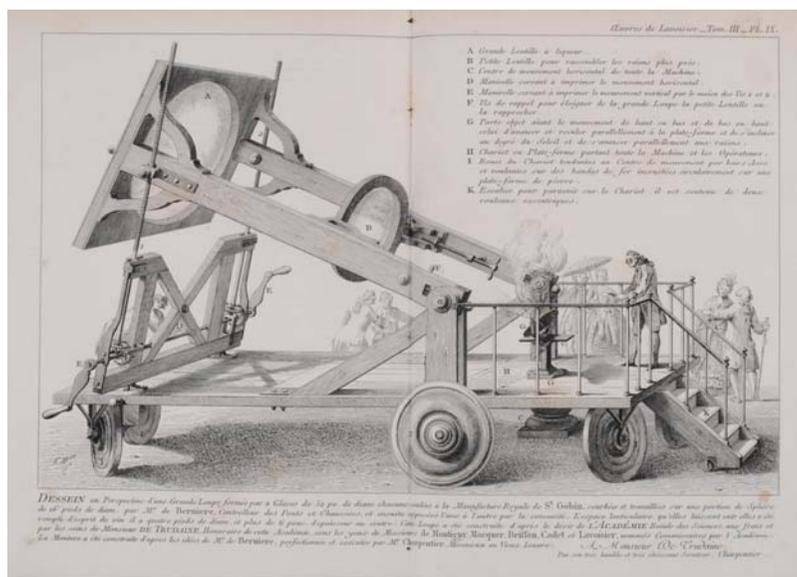
Fig. 10.3 Sometimes the apparatus for experiments simply has to be big and expensive. Antoine Lavoisier (pictured in goggles) took Priestley's discoveries forward but needed to produce heat and combustion in his sealed containers without introducing extra material so that he could demonstrate the conservation of mass. The apparatus here uses solar energy for this purpose. Lavoisier, like Priestley, found himself on the wrong side of politics in his home country for reasons that were nothing to do with his research. Priestley escaped into exile whereas Lavoisier was executed.

Antoine Lavoisier with his solar furnace. From Lavoisier, A. L. (1862). *Oeuvres de Lavoisier. Publiées par les soins de son excellence le minister de l'instruction publique et des cultes*. Paris, Imprimerie impériale. Science History Institute. Public Domain. https://commons.wikimedia.org/w/index.php?curid=64707215

contemporaries Priestley and Cavendish; she may also have contributed what would now be considered co-authorship (although uncredited at the time) on his widely read 1789 *Traité Élémentaire de Chimie* (Elementary Treatise on Chemistry). Unfortunately for Antoine-Laurent, although his family fortune helped his academic career, it drew him into unwise political connections, so that he fell afoul of the French Revolution and was guillotined in 1794.[6] However, Marie-Anne continued his legacy to the best of her ability, publishing his memoirs and, once the worst of the troubles were over, ensuring that his confiscated notebooks and apparatus were recovered for posterity.

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[6] As we've seen (Chapter 5), unlike his colleague and sometime collaborator Pierre-Simon Laplace, who more prudently kept out of Paris when times were tricky.

Science and its equipment

This potted history of early chemistry is, I think, a reasonable illustration of the way in which experimental techniques developed—the creation of controlled-enough environments and precise-enough measurements to detect what turn out in the end to be quite subtle phenomena, like the properties of sealed chambers before and after the combustion of gases within them. Certainly, the ingenuity involved in designing some of the equipment matched the thinking behind the experiments themselves. The same story could be told of other fields advancing through experiments, although perhaps a tightly controlled environment mattered more to early chemistry than, say, to electromagnetism research at the time. Once chemistry and physics started combining in atomic and subatomic research, the quality of experimental conditions became more universally paramount, and it's possible to draw at least some sort of line of succession from Boyle's vacuum flasks to the CERN Large Hadron Collider or to the pristine, dust-free laboratory environments that might be used in cell biology today. However, not all experiments can be carried out in a vacuum or laboratory, and it's worth considering the challenges faced outside these rarefied settings.

Experiments in vivo

Perhaps the most important questions facing modern medicine are around treatments and whether they work or not. Consequently, a sizeable clinical trials industry has built up, along with a broad consensus on standards for study design and conduct. The issue here, again, is one of cause and effect. If a potential new drug treatment has been developed and been found to be safe in early-stage evaluations then we need to be absolutely sure whether it works (and how well it works) before it starts getting prescribed and has to be paid for (whether via taxation, health insurance, or personally).

   The observational approach to this question would be to start the new drug and compare people who happen to be receiving it or not, following them up to see who gets better and looking at the difference between the two groups. However, the problem is that if you did find a difference (e.g. that people receiving the new drug were more likely to get better), you would still be left unsure whether it was really the effect of the drug. There might be all sorts of other differences between people in the comparison groups—for example, perhaps physicians are more likely to give the new drug to people with milder illness or some other reason that gives them a better outcome. Even if you 'adjusted' for differences between the groups, you could only adjust for the

things you'd measured and there might be other unmeasured factors at play. Finally, it could be quite an expensive undertaking to do all of this follow-up, so you would end up spending a lot of money and still not be sure whether you'd answered the question. Hence the development and routine use of the randomized controlled trial (RCT), which still tends to be expensive, but at least gives a more accurate answer.

Part of the advantage of the RCT is simply that it's an intervention—that is, you're not just observing people who happen to be taking one treatment or another. Instead, you're actually assigning the treatment groups. Intervention studies go back quite a long way in medicine. For example, as discussed in Chapter 5, having formed the theory that cowpox might be protective against smallpox, Edward Jenner went straight on to an intervention study—inoculating his gardener's eight-year-old son and exposing him to sources of smallpox infection in order to demonstrate the effect (hardly ethical, but it was the late eighteenth century, and William Wilberforce was still having to argue that the British slave trade wasn't a good thing). Perhaps more ethical, but still unwise, interventions were going on in chemistry around that time, with researchers freely exposing themselves to new substances to see what happened. Humphry Davy, for example, was a prolific discoverer of new elements and compounds, including sodium, potassium, and calcium, going on to give rather flamboyant public displays at the newly established Royal Institution in London. He also took an interest in nitrous oxide (laughing gas) which had been synthesized in 1772 by Priestley. Davy inhaled it copiously as a research subject, making notes and trying it out for different uses (including as a cure for hangovers) before considering it as a potential anaesthetic. He less sensibly inhaled nitric oxide and carbon monoxide to see what would happen and was lucky to survive each of those experiments.[7] These are all intervention studies of sorts, although before–after comparisons can't really be said to advance the methodology from what had been carried out in medicine since ancient times. After all, presumably traditional herbal remedies, leeches, and other old treatments had been adopted along the way because someone had tried them out and felt that there was a benefit.

The first controlled trial—citrus for scurvy

Probably the first controlled trial in medical research was carried out by James Lind in 1747. Scurvy, now known to be caused by a deficiency of vitamin C

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[7] He described himself 'sinking into annihilation' following the carbon monoxide inhalation and had enough presence of mind to take his pulse and describe its quality (fast and 'threadlike') in his laboratory notes.

in the diet, was a major problem for the expansion of sea travel at that time. For example, in 1740–1744 a squadron of eight ships were sent off to sail around the world as part of a conflict between Britain and Spain. It resulted in a popular written account of the voyage and all the various adventures along the way, but one of the stark facts was that only 188 men survived from the original 1,854 who set sail—most of the losses being due to suspected scurvy. Citrus fruit in the diet had been suggested as a possible means of preventing the disease but this was not widespread practice. Lind was a ship's surgeon on board HMS Salisbury while it was patrolling the Bay of Biscay and decided to test the intervention by taking sailors suffering from scurvy and assigning groups of two each to receive one of six dietary supplements— cider, vitriol, vinegar, seawater, barley water, and citrus (two oranges and a lemon)—all added to an otherwise identical naval diet. The two sailors who received citrus had nearly recovered within six days (when the supply of fruit ran out) and, of the other groups, only the sailors who received cider showed any improvement at all. Lind published his findings in 1753 but the medical establishment was conservative in those days, and his discovery took a while to become established. Eventually, a successful 23-week voyage to India in 1794 took place with lemon juice issued on board and no scurvy outbreak reported; this created enough publicity to change practice. Vitamins, incidentally, weren't proposed as entities until 1912, and vitamin C was isolated from 1928 to 1932—named 'ascorbic acid' after the Latin word (*scorbuticus*) for scurvy.[8]

The 'controlled' part of the clinical trial is a necessity because the intervention is being administered in the natural world, outside a laboratory environment. If you're dealing with chemicals, biological samples, or subatomic particles, you can design an environment where nothing happens to them, apart from your intervention—by doing so, you know that the outcome is definitely a result of the intervention. When you're intervening in living creatures, or in any less predictable system, you can't impose that sort of experimental environment. A simple comparison of the situation before and after an intervention isn't enough—a person might have got better anyway without your medicine. Therefore, in order to draw conclusions, you have to compare groups receiving an intervention with those who aren't. Furthermore, you need *groups*, rather than *individuals*, because interventions in the natural world are often shifting the probability of an outcome, rather than determining it absolutely. Jenner was lucky that his smallpox vaccine was

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[8] Two groups were particularly involved in this discovery, and their leads won Nobel prizes: Albert Szent-Györgyi from the University of Szeged, Hungary, and Walter Norman Haworth from the University of Birmingham, UK.

highly effective. If the vaccine had only been 80% effective, and if his gardener's son had fallen in the wrong 20%, then Jenner would have drawn the wrong conclusion, and an important opportunity might have been missed (and an eight-year-old boy needlessly given a potentially fatal illness). Lind was also lucky that vitamin C is so effective against scurvy and so specific to citrus fruits (although it is contained in cider as well, hence the partial improvement in the two sailors who received this).

The importance of randomization

Thus, having a control arm (or arms) to a trial is important, but it's not enough in itself, because there might still be other differences between the groups accounting for the outcome. Lind's groups of sailors, for example, might well have had differences in their background health or some other factor—again, he was lucky that he was dealing with a vitamin deficiency that is very easily and effectively corrected and that results in a rapid and visible return to good health. Most diseases are not so amenable. The next step forward, the RCT, was actually pioneered by Sir Austin Bradford Hill, whose 'causal criteria' we came across in Chapter 7 and 'verdict of causality' in Chapter 8. The idea of randomly assigning an intervention wasn't a completely new one—it was being used in agricultural science, notably at the Rothamsted Experimental Station in Hertfordshire.[9] However, Bradford Hill was the first to apply it in medical research in an evaluation of the drug streptomycin as a treatment for tuberculosis (TB) in 1948. It's not entirely clear that the choice of random treatment allocation at the time was made for the methodological reasons nowadays acknowledged—the records are a little unclear on this. The randomization might have been adopted partly because of a limited supply of the drug and a desire to ensure that people with TB had a fair and equitable chance of receiving it. However, there also does appear to have been a wish to keep the comparison groups as similar as possible. Nevertheless, the design became popular, and it is now a requirement for any new drug to have RCT evidence for its efficacy before it can be approved for prescription in most medical settings.

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[9] Under the direction of Ronald Fisher, a pioneering and highly influential statistician and experimentalist who laid the foundations for many methodologies still in use. He would be more celebrated and better known today (and get more than a footnote here) if he hadn't also held and publicized some frankly unsavoury views on eugenics and racial superiority. He was also an early vocal sceptic about health hazards associated with smoking—this might have been simply because he liked to smoke, but it doesn't help his reputation that he was employed by the tobacco industry as a consultant. Even if he only opposed the implications of research findings because he personally enjoyed the odd cigarette, it hardly suggests a dispassionate scientific attitude open to new ideas.

The key advantage of a randomly allocated intervention is that the comparison groups are identical in all other respects, differing only in the fact that one group has received the intervention and the other group hasn't. There might be chance differences, but large enough groups means that any effects of chance alone are negligible. Therefore, if the other parts of the study work successfully and if there turns out to be a better outcome in the group receiving the treatment, there really isn't any plausible explanation apart from a treatment effect—why else would the groups be different after the study if they were near-identical beforehand? Of course, as mentioned earlier, if you hadn't randomly assigned the treatment (i.e. just allocated it like Lind did for scurvy), there might have been differences between comparison groups that could have been 'adjusted for'—but you can only adjust for something you can measure and there might be other differences that can't be measured. Random allocation importantly deals with everything, measured and unmeasured, which is what makes it so powerful a design—taking experimentation in the natural world as close as it can get to vacuum tubes and the laboratory environment.

In conclusion

Although randomized trials are mostly associated with health research, there's no particular reason to limit their applications here. For example, trials have been long-standing in agricultural science, as we've discussed, as well as being very often used in biological research involving laboratory animals, and they are sometimes considered in fields such as criminology and education. In the corporate world, large companies, particularly those at the more technical end of the market, may sometimes randomly introduce new developments in customer engagement, which provides a better way of self-evaluating than introducing a policy across the whole organization. This then merges into trials that move beyond the individual level and evaluate interventions that have to be applied to groups. For example, if you're considering a new way of delivering healthcare to a community and want to investigate whether it works, it may not be possible to allocate the intervention randomly to different individuals within the community. Instead, you might possibly randomize different geographic units to receive the new service or not. Sometimes there may be sufficient numbers of geographic units to treat them (i.e. their different outcomes) like individuals in a trial. However, this may not always be possible, and you might find yourself falling back on a combination of approaches—for example, comparing outcomes between areas with

different interventions, while also carrying out before- vs. after-intervention comparisons within the areas, and then pulling together these strands of information together in order to draw conclusions. As you can see, this moves towards observational, rather than experimental, techniques—something we consider further in Chapter 11.

11
Designing Research

Alternatives to Experiments

We discussed in Chapter 10 how an experiment is the best way to establish a cause–effect relationship. The problem is that this isn't always possible in all circumstances. In general, the reasons against carrying out an experiment are either that it's not feasible or that it's not ethical. We'll consider the feasibility question first.

As covered in previous chapters, not all research is interested in establishing causes and effects in the first place. Pure mathematics, for example, may be interested in relationships between constructs or explanations for truths, but you wouldn't talk about causation. Also, some research is simply descriptive and is more interested in 'what?' or 'when?', rather than 'why?'. Other research *is* interested in causation, but experiments are simply out of scope. For example, most of history research is about events and their interrelationships, and I assume that any introductory lecture to a history degree course is going to say something about learning from the past in order to shape the future. However, the theories in this research field are drawn from reasoning applied to past events, rather than by experimentation in the present. The same might be said for other humanities research—for example, in art, literature, theatre studies, music, or theology. There's a lot of interest in causation, but the academic approach is one of reasoning and deduction.

Experimentation of a sort—political science

On the other hand, you could say that experimentation (of a sort) does come into the picture when theories derived from historical research are put into practice, particularly when they shape government policies. Although policies can be considered as interventions, the trouble with working out cause and consequence is that you're dealing with a situation beforehand, a situation afterwards, something that happened in between, and a whole lot of reasoning at best (guesswork at worst) when you try to put the connections together. At the end of Chapter 10, we considered group-level interventions

that could be applied differently in different areas and subjected to quasi-experimental study designs. However, because government actions tend to cover whole populations, there isn't generally any opportunity to include an intervention-free comparison group, as would be the case in a controlled trial. Also, there's no means of controlling everything else going on in the environment as a laboratory experiment might do. Finally, the intervention itself is likely to be quite diffuse and difficult to define. So, the policy is imposed, and future historians[1] are left to draw conclusions as to its consequences, perhaps debating whether the intervention was really an adequate test of the theory in the first place. For example, you might ask whether twentieth-century communism can be viewed as an evaluation of applied Marxist theory—did the collapse of the Soviet Union in the 1990s indicate the failure of 'the Marxist experiment' as a whole, or was it simply the evaluation of one attempted application? Similarly, can the deaths from the 1845–1852 Great Famine in Ireland be viewed as a general evaluation of 'laissez-faire capitalism' as a principle (i.e. the theory, adopted by the British government at the time, that market forces would somehow sort the problem out and that intervention would make matters worse)? Or was it just basic incompetence? Or genocide? The questions certainly keep historians busy enough.

Understandably, a lot of this sort of evaluation relates to economic theories; these have a more direct link to the sorts of policies governments are likely to adopt and implement ideologically, so can be investigated as an evaluation of that ideology. Also understandably, it's complicated. To begin with, the connection between the theory/ideology and the policy isn't always clear—for example, as discussed in Chapter 8, there's still debate whether the economic theories of John Maynard Keynes had a direct influence on President Roosevelt's 'New Deal' policies in the 1930s—or indeed, in the end, whether it was these policies, or the money spent on the Second World War, that brought the US out of its Great Depression. Similar debates continue on the consequences of the monetarist policies adopted by the Thatcher and Reagan regimes in the 1980s, or on what intervention or policy is actually being assessed as an intervention if you consider the 2007 banking crisis as an outcome—was this a failure of capitalism, as some have claimed, or was it just some component of capitalism?[2]

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[1] And/or perhaps contemporaneous social scientists.

[2] It's worth pointing out, incidentally, that economics as a research field does have a more conventional experimental component—for example, investigating relevant behaviours and actions through gaming or simulations. However, although I'm sure they're informative, these types of study tend to be testing hypothetical, rather than actual, phenomena.

The more catastrophic large-scale interventions are easier to pin down, although perhaps are less informative as evaluations for the future. In the late 1920s, Trofim Lysenko (Figure 11.1), a Soviet biologist, started criticizing prevailing theories of evolution by natural selection and Mendelian genetics. Drawing on interests in agriculture, he proposed instead that he could transform wheat species by other means (for example, turning the wheat used for pasta into the wheat used for bread, despite the fact that these have different numbers of chromosomes), as well as increasing crop yields through exposure to humidity and low temperature (proposing that the effects would be inherited), and altering the genetic profile of trees by grafting. Lysenko's theories were expressed as an attack on 'bourgeois pseudoscience' (meaning the consensus of other scientists at the time) and he publicized them just after a severe Soviet famine in 1932–1933. His ideas were enthusiastically

Fig. 11.1 A rather sinister looking Trofim Lysenko, Soviet agronomist and biologist, addressing the Kremlin in 1935 as Stalin looks on approvingly (as far as you can tell anything from Stalin's expressions). Above and beyond the numerous scientists who were sacked, imprisoned, or executed for disagreeing with him, Lysenko's misguided (or at least misapplied) theories about agriculture may have been responsible for around 30 million deaths in the USSR and China. Whether the fault here lies with Lysenko personally, with the politicians who accepted his theories without question, or with wider top-down political systems that stifle debate, is up for argument. However, it reveals the dangers of theories that are not put under academic pressure, and probably the dangers of letting researchers anywhere near political power.

Trofim Lysenko speaking at the Kremlin (1935). From Soyfer, V. N. (2001). The consequences of political dictatorship for Russian science. *Nature Reviews Genetics* 2, 723–9, cited in Peskin, A. V. (2001). Science and political dictatorship. *Nature Reviews Genetics* 2: 731. Public domain, via Wikimedia Commons. https://upload.wikimedia.org/wikipedia/commons/5/5a/Lysenko_with_Stalin.gif

adopted by Stalin's government because they suited its revolutionary ideology, as well as helping to cover up the fact that the 1930s famine had probably been caused by ideological collectivization of farming in the first place. Soviet geneticists opposing Lysenko's theories were executed or imprisoned and crop yields declined as a result of the misguided advice, resulting in millions of deaths. Beyond the Soviet Union, 'Lysenkoism' was unfortunately also favoured in China by Mao Zedong's 'Great Leap Forward' in 1958—a policy disaster in many respects and without redeeming features, resulting in millions more fatalities. Lysenko's influence waned after Stalin's death, and he was denounced and formally relieved of his duties by the mid-1960s. He died in 1976 at the age of 78—considerably older than some of his unfortunate academic colleagues or the estimated 30 million who may have died of starvation as a result of his theories.

Research output has its darker sides, and this is clearly one of them. There's a reasonable suggestion that Lysenko was responsible for more deaths than any other single scientist in history. However, thinking more specifically about researching cause and effect, the intervention being evaluated here is open to question. In one respect, these disasters can all be seen as an evaluation of Lysenko's scientific theories.[3] However, on another level it's not just mistakes in theories that are being evaluated here—it's their wide application as a policy without adequate scrutiny or review. And on another level still, perhaps what's actually being evaluated is a political system where an individual academic can attain such a high level of unchallenged influence, where scientific debate is curtailed (brutally in this case) for ideological reasons, and where a policy error is allowed to continue because there is no capacity to reverse it, for fear of losing face. Therefore, although it was Lysenko's misguided theories that resulted in the disasters, it could be argued that Stalinist and Maoist political structures (and Stalin and Mao as unchallengeable dictators) might have supported any number of similarly catastrophic ideas—driven by ideology rather than evidence—and that the degree of top-down control and dissent suppression would have resulted in similarly magnified adverse consequences across the large populations involved. If Lysenko's ideas hadn't killed 30 million people, then something else would have been adopted, sooner or later, with the same results.

So, a sizeable number of past interventions might be seen as driven by a particular theory or ideology and might therefore be seen as a way of evaluating that theory or ideology—or at least evaluating its application in a particular

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[3] And his theories weren't completely without foundation—the field of epigenetics does allow for heritable traits to be passed on without DNA alterations; it's just that epigenetics acts in a much more minor way than Lysenko was proposing.

setting at a particular time. Clearly there are all sorts of challenges in drawing conclusions. To an outsider, the field does seem to weigh towards trying to learn from mistakes—the 'let's definitely not try that again' take-home message. However, evaluations are also made of more balanced policies, such as the Truth and Reconciliation Commission founded by Nelson Mandela in South Africa in 1996. This might be seen as essentially a test of restorative vs. retributive justice following regime change and a long history of violence and oppression. The debate continues as to whether it met its goal, although it at least might be classified in the 'could have been a lot worse' category. Finally, as mentioned in Chapter 8, anthropogenic climate change might also be considered as a rather large and profound experiment currently underway, from which we don't yet know the answer for certain, but which we might be wise not to continue for too much longer.

Natural experiments

Not all imposed interventions are quite so difficult to conceptualize and evaluate, however, and 'natural experiment' has been coined as a term to describe research studies that have capitalized on events to test particular theories where the event can be seen as a more specific intervention of sorts. A variety of questions have been addressed in this way. For example, plenty of research has evaluated what happens to health outcomes when a regional or local policy is introduced (for example, a smoking ban). Other studies have followed up offspring born to mothers during a famine, comparing them to people born before or after the famine, to investigate the effect or not of maternal nutrition on health conditions occurring much later on in their offspring. The Dutch famine of 1944–1945 towards the end of the Second World War has been commonly used for this, as food availability for mothers before and afterwards was relatively normal, so a very specific generation of offspring can be investigated. Similarly, comparisons have been made between people born before or after 1963, which was the year when atmospheric nuclear weapons testing was banned and low-level exposure to radioactive isotopes changed considerably. As a final example, a recent US study wanted to look at the long-term influence of military service on earnings and was able to take advantage of the fact that the Vietnam War draft lottery was randomly assigned, therefore creating a randomized controlled trial (RCT) of sorts, ensuring that the comparison groups (interviewed much later) were similar. This moves us on to thinking about the second limitation on experimentation which is the ethical one.

Chapter 10 discussed the use of the RCT to investigate whether or not treatments for illnesses work. However, clearly there are ethical considerations with these sorts of interventions. You can reasonably carry out a trial if you think that something *might* be beneficial, but where you're not sure. It's generally considered unethical to give treatments unless there's adequate evidence that they work, so you need the trial evidence to answer that question. This justifies the fact that not everyone will receive the intervention in your trial. However, you can't ethically carry out a trial of a treatment that has been shown to work, because you can't deny an effective treatment to a control group. Therefore, at some point, trials become unethical when the evidence has accumulated in favour of the treatment—not always an easy decision and sometimes a trial is terminated early because positive results from other studies change the ethical landscape. More importantly, you can't give an intervention that might harm someone, so trials can't be used to evaluate potential risk factors for diseases[4]—clearly a key area of research that has had to find other solutions. Alternative study designs have received most consideration and discussion in the field of epidemiology. However, they are not restricted to this, and you can find them used from time to time in other disciplines.

Cohort studies—health effects of smoking

There's really nothing particularly complicated in the alternative designs. If you think that something might be a risk factor for a disease, one approach is to take people without the disease and compare those who do or don't have the risk factor, following them up over time. If you're correct in your theory, people with the risk factor will be more likely to develop the disease than those without the risk factor—all very simple. The design is called a 'cohort study'; each comparison group is a cohort and is expected to share a particular characteristic (a bit like the Ancient Roman military units, from which they get their name). In many ways, a cohort study is the same as an RCT —it's just that the risk factor isn't an intervention and hasn't therefore been imposed on anyone; it's just a characteristic or a behaviour or some environmental exposure that people just happen to have, or not, at the time the study starts.

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[4] Well, you can if you're randomizing people to something that might mitigate the risk factor and prevent the disease outcome, but you're going to need strong evidence for something being a risk factor in the first place before anyone's going to fund an expensive prevention trial.

One of the earliest cohort studies in medicine was the British Doctors' Study. This was set up in 1951 by Austin Bradford Hill with his junior colleague Richard Doll and ran all the way through to 2001. The aim was to investigate health effects of smoking, which weren't at all clear around the time the study began. Lung cancer rates were known to have increased dramatically before the 1950s, as had smoking, but then so had pollution from burning domestic and industrial fuel. Doctors were chosen as study participants because in those days they were cooperative, easy to contact, generally agreed to take part, and (crucially) stayed involved in the study and didn't drop out in large numbers. I suspect it also helped that they were a single professional group and quite similar to each other, although smoking in the 1950s was less linked to social class than it is today. The study began with around 40,000 doctors and the main comparisons were made between smokers and non-smokers; however, participants naturally changed their behaviour over time and the team were therefore able to estimate the effects of stopping smoking at different ages. A range of health outcomes were carefully collected over the years and successive reports were published, underpinning what is now standard, accepted knowledge about smoking and its consequences—the overall life expectancy lost (but gained again in those who stop smoking; the earlier, the better), and the increased risk of heart diseases as well as cancers and lung diseases. The British Doctors Study wasn't the only investigation reporting these associations—a single study, however well designed, would never have been enough. Similar findings were coming out of other cohort studies, such as the Framingham Study, set up in Massachusetts in 1948 to investigate risk factors for heart attack and stroke in the general population, and later on from the Nurses' Health Study—a similar design to the British Doctors Study, but with wider health outcomes and involving over 120,000 participants in 11 US states.

The design of a cohort study doesn't need to be complicated—it's mostly a matter of obtaining accurate measurements of the risk factor(s) you're interested in, of the outcomes as they occur, and of any other characteristics you might want to consider and adjust for. The challenges mainly lie in the logistics—a traditional cohort study might well involve quite a large sample of people, because even the commonest disease outcomes are quite rare in absolute numbers and you're going to need enough cases occurring over time in order to make an adequate comparison. You may well also have to follow the groups up for long periods, and the biggest problem when it comes to drawing conclusions (i.e. the whole point of the study in the first place) is if you've needlessly lost a lot of people along the way (moved house, changed phone number, no alternative contact method, or lack of interest due to time

between examinations). This 'attrition' limits the number of people with outcomes that you can analyse, and it might also introduce error ('bias') into your findings if it's somehow more or less likely in people with the risk factor and/or the outcome. In the British Doctors Study, for example, if smokers were more likely than non-smokers to drop out of the study, and if health outcomes were worse in the group who dropped out, then you'd be preferentially losing people who smoked and had worse health outcomes and therefore would be underestimating the association between the cause and the effect in the people who remained in your study. Fortunately, follow-up rates were very high in that study and there were no major concerns about bias.

Cohort studies, therefore, do tend to be expensive because of all the staff required to keep them running efficiently, and the nightmare scenario for anyone in charge is that you end up with massive attrition and not much you can realistically say about outcomes. Like randomized trials, they're definitely not for the faint of heart. There are also questions that can't be feasibly answered by this design. For example, if an outcome is rare, it may simply not be possible to assemble a large enough cohort. Or you may be interested in a cause–effect relationship over a long time period (e.g. the association between someone's circumstances at birth and their health in middle age), but no one's going to fund a study that doesn't provide its answers till decades later. The alternative design in these circumstances is to find people with the disease outcome, find an appropriate comparison group and then look at the differences in the risk factor of interest. If it's a risk factor, then you expect it to be more common in the first group than the second. This is called a case control study.

Case control studies—early AIDS research

In theory, a case control study ought to be a lot easier than a cohort study, and certainly cheaper—after all, you only need to see your participants once and collect the necessary information about their past lives and experiences in order to compare risk factors. There's no follow-up involved and no worries about attrition. You might even be able to perform the study yourself and not have to employ a research team to help. However, although case control studies are indeed generally cheaper and logistically easier than cohort studies, a lot more thought goes into their design to guarantee that you can answer your research question and not end up wasting everybody's time. First of all, you need to consider your control group: you may have assembled a group of

'case' participants (i.e. people with the disease or outcome you're interested in) but you now have to think about who to compare them with (i.e. people without the disease or outcome but similar in other respects). This might be straightforward but it's often the factor limiting the design because the study depends on there being an adequate comparison. For example, there used to be a fashion for collecting healthy controls from university staff or students, because they were quite easy to find and approach. However, comparing ill people attending a hospital to staff or students is comparing two groups that are likely to differ in many respects and give rise to all sorts of false ('biased') findings.

The other challenge of a case control study is that you're generally having to look at the past in order to work out whether or not people have been exposed to the risk factor of interest. This is fine if it's something very obvious, like a serious illness or injury, but it's more difficult if it's something that depends on a participant remembering it or not. If someone is affected by an illness, they've usually put in quite a lot of time and effort into thinking about what might have caused it, so 'cases' tend to be much more likely to remember relatively minor things in the past that might be risk factors. For example, if you go down with food poisoning, you're bound to think through every-thing you've eaten over the last few days in order to identify a potential cause, whereas if you're an unaffected 'control', you've probably forgotten even what you ate the previous evening. It's still trickier if you've got something more severe and gradually developing, like cancer. So, it's quite easy to end up with observed differences between cases and controls due to this different level of recall, and to draw incorrect conclusions as a consequence. There's a huge advantage if you can find an old information source (e.g. historical medical records or some other administrative database) that tells you about the risk factor you're interested in, because you don't then need to worry about people's memories. For example, if you were interested in pregnancy- or birth-related risk factors for diseases occurring in adulthood (such as heart disease or schizophrenia), you'll probably need to use cases and controls who have old maternity or obstetric records, rather than having to ask people (or their parents) to remember. Furthermore, you must ensure that you or your interviewers don't treat the cases and controls differently when you're asking questions about the past (e.g. trying harder to find information in cases than controls); this will also give you false results.

Case control studies therefore do need a lot of thought and must be inter-preted with caution because of all of these challenges. However, they may still play an important role in health research, particularly when a quick result is needed. For example, when the disease we now call HIV/AIDS was first

recognized in the late 1970s/early 1980s,[5] there was a lot of initial uncertainty about its cause and if it required urgent investigation. At the time, it was known that there were growing numbers of gay men[6] with a type of cancer called Kaposi's sarcoma, affecting skin and lymph nodes, and a particular type of pneumonia caused by a yeast-like fungus called *Pneumocystis*, both of which were recognized to occur in people with a suppressed immune system. On 15 May 1982, *The Lancet* published one of the earliest investigations of this—a case control study carried out by a team from two New York universities.[7]

In this study, the authors recruited 20 gay men with Kaposi's sarcoma receiving treatment at the New York University Medical Center between 1979–1981. For each of these cases, they recruited two controls from a local physician's practice who were also gay men and were the same race and age as the case. Quite a lot of information was taken, which was understandable given the need to identify risk factors as quickly as possible; however, the researchers were particularly interested in three potential causes. Was it:

1. a side-effect of metronidazole, a medication used in the treatment of amoebiasis (a common sexually transmitted disease at the time in gay men)?
2. a side-effect of amyl or butyl nitrate—recreational drugs that were being commonly used as sexual stimulants by gay men at the time in New York?
3. a direct effect of one or more infections?

When they had analysed the results, the team were able to rule out the first of these theories, as there was no difference in metronidazole use between cases and controls. The second theory received some support because the cases were more likely to report using amyl nitrate, but there was no difference in butyl nitrate use. The third theory also had support because cases did report higher numbers of recent sexual partners. The authors were quite careful in their conclusions and felt they couldn't absolutely rule out a toxic effect of amyl nitrate as a cause, although the lack of association with butyl nitrate (which ought to have the same toxic effects) was felt to weigh against this, as was the lack of Kaposi's sarcoma in other people taking amyl nitrate therapeutically for heart disease. They therefore tended to weigh in favour of

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[5] This was when it started to be noticed in the US, at least. From retrospective evaluations and tests of stored samples, the first cases of HIV found so far were in 1959. It is believed to have evolved first in chimpanzees and may well have crossed the species barrier and begun affecting humans in Central Africa from the 1920s.

[6] Note, the nomenclature of the 1970s/1980s is being used here for consistency with the research report.

[7] Marmor, M., Laubenstein, L., William, D. C., Friedman-Kien, A. E., Byrum, R. D., D'Onofrio, S., and Dubin N. (1982). Risk factors for Kaposi's sarcoma in homosexual men. *The Lancet* 8281: 1083–7.

infection as a cause, although they primarily considered cumulative effects of a number of known sexually transmitted diseases (all of which tended to be higher in cases), rather than a single unknown disease. The actual cause was identified less than a year later, in January 1983, when Françoise Barré-Sinoussi at the Pasteur Institute in Paris grew the retrovirus (Figure 11.2) that is now called HIV[8] using a biopsy of a swollen lymph node from a patient at risk of AIDS.
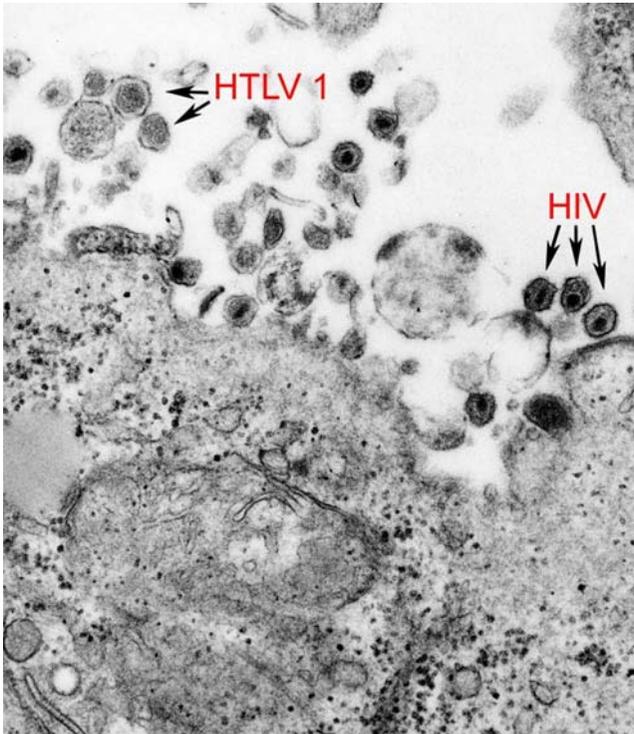
Fig. 11.2 The HIV-1 virus alongside the very similar HTVL-1 (human T lymphotrophic virus type-1; a retrovirus underlying a type of leukaemia). When the illness later called AIDS started emerging at scale in the late 1970s, it wasn't clear what was causing it and there were several competing plausible theories. Just as John Snow had investigated patterns of cholera occurrence over a century earlier to distinguish between air-borne or water-borne spread, early case control studies compared the lifestyles of men with and without AIDS illnesses to investigate whether these might be due to infections or toxins.

An image of the HTLV-1 and HIV-1 retroviruses. From the Centers for Disease Control and Prevention's Public Health Image Library, ID #8241. Public domain; https://commons.wikimedia.org/wiki/File:HTLV-1_and_HIV-1_EM_8241_lores.jpg

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[8] The virus went through quite a few names before being called Human Immunodeficiency Virus in 1986. Acquired Immune Deficiency Syndrome (AIDS) was first proposed as a name for the disease in mid-1982.

A single case control study is rarely definitive, and this early investigation of AIDS was clearly only suggestive, and imprecise in its conclusions—it narrowed down the field of interest but still couldn't distinguish between an infection and a chemical toxin as the cause. If the virus had taken longer to identify, perhaps there would have a lot more investigations of this sort, trying to reach a consensus. To my mind, the study was impressively rapid in its set-up and reporting—after all the US Center for Disease Control (CDC) only dates the AIDS pandemic from 1980. Also, given the unsatisfactory way in which the news media reacted to the emergence of the condition,[9] it is to *The Lancet's* credit that it rapidly published what was quite a small-scale study.

Of course, it would be easy to assume that the discovery of the HIV retrovirus was always going to solve the matter, although it's worth bearing in mind that the research remained primarily observational for some time. The isolation of the virus allowed the HIV test to be developed for diagnosis, and then there were further studies to investigate prognosis—tragically unmodifiable for a decade until the introduction of drug therapies in 1992 following successful clinical trials. I don't know whether anyone considered or cited Bradford Hill's causal criteria (Chapter 7) at the time, but clearly there was a self-evident strength and specificity to the observation (i.e. HIV-positive status was strongly and specifically linked to risk of developing AIDS), just as in Chapter 7, where we considered asbestos exposure as strongly and specifically linked to the occurrence of mesothelioma. We're in the 'real world' natural science domain here, so there are inevitably anomalies and inconsistencies. For example, one of Barré-Sinoussi's many further contributions to HIV research (other than identifying the virus in 1983) was to investigate the small proportion of HIV-positive people who continue to maintain low virus levels without any drug treatment. Nothing's ever completely clear cut.

Ethics and experimentation

With this in mind, it seems that the main limitation on the experimental approach is ethical. After all, there may always be some way around the feasibility limitation. Having made this point, it's worth acknowledging that not everything is as ethical as it should be. Jenner's experimental inoculation of an eight-year-old boy was hardly an ethical intervention, even if Jenner meant well—but that was the eighteenth century. More notorious, and certainly inexcusable, was the US Tuskegee Syphilis Study, in which 400 impoverished

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[9] And politicians—consider the rapid actions taken by the Hawke government in Australia from 1983 and compare them to the US, where President Reagan didn't even mention AIDS publicly until 1985.

Fig. 11.3 Blood being drawn from a participant in the Tuskegee Syphilis Study. This photograph is believed to have been taken around 1953, by which time penicillin had been a standard treatment for syphilis (but not administered to these participants) for six years. From this point, the study (and 'experimental' lack of treatment) would continue for a further 19 years, and the wait for a presidential apology a further 25 years after that. Immoral and unethical on so many levels, the study's impacts on public perceptions of health research are still being felt.

Subject blood draw, c. 1953. Photo taken of the Tuskegee Syphilis Study. From the holdings of the US National Archives and Records Administration (National Archives Identifier 956104). Uploaded by Taco325i. Public domain, via Wikimedia Commons. https://upload.wikimedia.org/wikipedia/commons/e/ef/Tuskegee_study.jpg

African-American residents of Alabama with the disease syphilis were followed up essentially to see what happened (Figure 11.3). Although they received some rudimentary treatments early on, they were never told about their disease (even when it was re-diagnosed for those who were drafted for military service in the Second World War), they were denied penicillin treatment when it became standard, and, by the time the study was terminated, more than 100 had died needlessly from a treatable condition. The profound immorality of the whole affair is chilling enough, but what makes the jaw drop even further is the level of support and sponsorship from US institutions (its Public Health Service and the CDC), by the fact that it went on for a full 40 years until 1972, by the fact that it was only stopped at that point because of a whistle-blower[10] who took the story to the press, and by the fact that it

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[10] Peter Buxton, who had already raised formal concerns in 1966 and 1968 that were rebuffed by the CDC. He wasn't the first either—the ethics had been questioned since at least 1955.

took until 1997 for President Clinton to apologize on behalf of the government. Understandably, trust in the medical research establishment from the African-American community was profoundly damaged, and repercussions persist to this day.

Beyond the clear-cut ethics around experimentation on humans are the less-clear (or at least less-agreed-on) frameworks for experimentation on animals. I'm not going to cover this in detail as opinions run strong on the matter and others have considered it at length and with much greater expertise. However, it's important to acknowledge the issue and that it is complicated— or at least it will be complicated for most readers. The simple position at one extreme is that no animal experiment is ethical, and this will be held by some people. The simple position at the other extreme is that all animal experiments are ethical, although I'm not sure that anyone really holds this (or at least I hope not), so I'll ignore it. The majority view (assuming 'I don't know' or 'I don't want to think about it' aren't allowed as options) will be somewhere in between. The two primary determinants are the species involved and the purpose of the experiment. Considering the species, I suspect that most people have no particular qualms about the large numbers of fruit flies that are bred for laboratory research,[11] but that they are likely to disagree with experiments involving chimpanzees. If there's a line to be drawn in between about acceptability or not (e.g. fish, frogs, mice), I suspect most people would need to know the purpose of the research. If it involves essential knowledge that could lead to a potentially life-saving treatment (for humans or animals), I suspect the line of acceptability would be different than for a much more trivial 'knowledge for its own sake' experiment. Likewise, a different view might be taken on testing the safety of a new medicine compared to a new cosmetic. What regulatory systems try to do is encapsulate this sort of balancing act, requiring the use of animals for research to be justified in terms of the species involved and the purpose of the study.[12] And I guess the systems try their best to be sensitive to majority opinion[13] and to reflect this (and any shifts in opinion over time) in their judgements—which is better than nothing. Of course, we could argue that ethics are absolute and not a matter of following majority opinion. This is a reasonable stance, but then there needs to be some process for deciding the absolute position in the context of differing opinions, and that's definitely beyond the scope of this book.

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[11] Or, if they have qualms about fruit flies, they might not have ethical concerns about experiments on laboratory-bred bacteria or viruses.

[12] As well as dictating standards for the welfare of animals that are sanctioned for experiments.

[13] Or at least the perceived majority opinion, I suppose; I'm not sure how much polling is involved.

12

Designing Research

R&D

Research and development (R&D) is a term so well-established that it's easily glossed over. However, at its heart it's expressing the truth that research has always depended on the technologies available at the time, and that research is likewise a key component in the development of these technologies. So, when you're embarking on a research project, you might feel that the focus is on your particular question or objective. Yet, although you might not be conscious of it, you're likely to be shaping your questions according to the feasibility of an investigation, for example, on the measurements, information resources, or facilities available. You might also be seeking (or your supervisor may be encouraging you) to incorporate technologies and resources that are 'cutting edge', enabling you to address new objectives for the first time. And you might even find yourself in the slightly odd situation of not knowing quite what you want to do, but instead trying to find new questions to answer that will make use of the cutting-edge technologies that have recently become available. This is the 'solution in search of a problem' scenario that has become rather common in recent times as a result of huge leaps forward in capability. How do we make sense of all of this?

Tools of the trade

The relationship between the design of research projects and the technologies or facilities available cuts across most disciplines. For example, you might be thinking about whether you can measure the disease risk factor you're interested in, or whether you have access to the right sort of laboratory environment for your study of protein structures, or to a newly discovered archived source required for your English thesis, or to the necessary statistical procedures and software for your economic modelling. Across the history of research, these technologies tend to begin as simple measurement issues, although there also seems to be something important about group support and complementary expertise—the 'academic environment' in early centres

of learning. More recently, resources have moved beyond simple measurement, developing and expanding into a range of domains determining the types of study possible and the ways in which research institutions are shaped and funded.

It's no accident that mathematics is one of the oldest academic disciplines. For pure mathematics, you don't need much more than your brain, although having the space and support to think, and having the opportunity to exchange and develop ideas collegiately, are also clearly important. Hence, the tendency for mathematics to advance during times when trade routes were functional (the exchange of ideas), as well as its periodic flowering at particular centres of excellence (the institutional support). For geometry, all you need is a ruler and a compass to make a start, and the mathematics displayed in astronomy was there for all to see in the centuries before light pollution. Other early fields of 'development' were driven by the needs of societies and states, and perhaps are better described as successive inventions (e.g. those underpinning advances in metallurgy (iron weapons), architecture (pyramids), or agriculture (crop rotation)). However, as discussed in Chapter 10, most of these inventions were likely preceded or accompanied by experimentation and were built on advances in background understanding (e.g. of farming techniques) and shared knowledge among experts. So, I think we can count this as research, albeit less 'academic' in its early days. Sometimes the knowledge was closely guarded, like the manufacture and delivery of 'Greek fire', an early form of flamethrower that could be taken to sea and directed at hostile shipping. This gave the Byzantine empire its naval advantage, kept the empire intact for nearly a millennium, and was the topic of extensive but ultimately fruitless research by its enemies.[1]

Inventors and researchers

Archimedes is a good example of someone very much at the forefront of early applied research and development. Living in Sicily in the third century BCE, he was the archetypal polymath, contributing significant advances in astronomy, physics, and mathematics, as well as being an engineer and inventor. Mathematically he was dealing with the irrational number pi, and its application in calculating the area of a circle and the surface area of a sphere, as well

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[1] The secret impressively died with the Byzantine empire, partly because a chain of components was required to make Greek fire, from its chemical composition to the equipment for its manufacture and projection from ships. This knowledge was kept compartmentalized, and no one was allowed to know the full chain.

as giving his name to a particular type of spiral followed by an object with constant speed and angular velocity. In physics, he is best known for using immersion in water, and the amount of water displaced, to calculate the volume of irregular solids. The story goes that he developed this to calculate the volume of a crown and therefore to work out (through also knowing its mass) that silver had been mixed into the supposedly pure gold. This is also the story of the 'Eureka' moment (Greek for 'I have found it')—where Archimedes, having his idea, leaped in excitement out of his bath and ran naked down the street, an image that has dogged scientists ever since.[2] Archimedes' inventions, or at least the ones written about, were mostly for military use to help defend his city of Syracuse against invaders. As well as various types of catapults (pulleys and levers were mathematical interests of his), he is said to have invented a giant claw for lifting hostile ships out the water, and parabolic mirrors to burn them with concentrated heat rays from sunlight—both of which have been tested in modern times with varying degrees of success. Unfortunately for Archimedes, his inventions weren't enough to save Syracuse from Roman invasion and he was killed during the siege (Figure 12.1).

Chapter 2 discussed a variety of social/political factors key to the advances made in Western European science after 1500 (movement of scholarship, competing states and universities, etc.). However, it's fair to say that inventions and technical advances played an important part, and not just the invention of printing[3] and the wide communication that this afforded. For example, lenses for spectacles had been steadily advancing in quality from the thirteenth century using glass grinding and polishing techniques developed initially in Venice and Florence. This ultimately led to the telescope—believed to have been first developed in 1608 on the basis of a patent submitted in the Netherlands that year, although improved on significantly by Galileo and others. The first microscope followed soon after, around 1620,[4] and was popularized by Robert Hooke in his *Micrographia* best-seller of 1665 (Chapter 4). Therefore, advances in glass-making technology, probably mostly driven economically by a growing market in reading glasses, allowed substantial steps forward in the understanding of previously unseen large and small worlds.

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[2] And an assumption that this sudden, out-of-the-blue inspiration is the way scientific discoveries are made. I can't say I've ever had a Eureka moment myself, or that I've met any other researcher who's had one either.

[3] Or perhaps 'adoption' might be a safer description for the European printing that helped accelerate its Renaissance. Metal movable type is believed to have been in use since the thirteenth century in Korea and China. It appears to have been independently invented in Mainz, Germany in 1439 by Johannes Gutenberg, although it's hard to know how much (if at all) this was influenced by the spread of the idea from Asia.

[4] There are a few competing candidates for the inventor of the microscope and it's a question that remains undecided, although most were around 1620. Galileo was definitely in there early on.

Fig. 12.1 A rather fanciful depiction of the death of Archimedes, the celebrated inventor, killed during the Roman invasion of Syracuse (Sicily) in around 212 BCE. The equally fanciful story goes that he was deep in study when the soldier arrived and that his last words were: 'Do not disturb my circles'. It seems fairly unlikely that a soldier would admit to killing him, let alone pass on his dying words, as there had been specific orders to keep him alive. The Roman military machine with Archimedes in its employment (as they'd hoped might be the case) would have been seriously formidable, although I guess they managed well enough without him in the end.

Death of Archimedes by Thomas Degeorge (1815). Public domain, via Wikimedia Commons. https://upload.wikimedia.org/wikipedia/commons/f/f3/Death_of_Archimedes_%281815%29_by_Thomas_Degeorge.png

Mathematics as a technology

Following the line of astronomy, it wasn't only the technology of the telescope that moved the field forward but also the development of mathematical techniques, particularly by Kepler, who moved around various central European academic centres in the early seventeenth-century trying to earn a living while repeatedly falling on the wrong side of the various religious shifts of the time—too Lutheran for the Catholics, not Lutheran enough for the Lutherans. Amongst all of this, he managed to lay the mathematical foundations used by Newton later that century. However, Kepler had help from John Napier, who invented logarithms in 1614. A Scottish aristocrat, Napier was as interested in alchemy, necromancy, and the *Book of Revelation* as he was in

mathematics.[5] Logarithms make the job of long multiplication and long division a great deal easier (hence the use of slide rules at schools in pre-calculator days), so opened up a range of calculation possibilities. Using these, Kepler was able to publish tables that predicted planetary movements considerably better than Copernicus had done over 80 years earlier. Whether mathematicians like to be thought of as equivalent to lens makers and engineers, advances in applied research have frequently had to depend on developments in that field. Much later on, when Einstein was putting together his 1915[6] theory of general relativity, which unified the concepts of acceleration and gravity and defined the relationship between spacetime and matter, he drew on Gauss and Riemann's work at the University of Göttingen in the previous century (Chapter 6), where they had worked out the principles of geometry on curved planes[7] and using multiple dimensions. In turn, these were further developed at Trinity College, Cambridge, and University College London by William Clifford, who had begun presenting theories about curvatures in space, foreshadowing general relativity.

The advances in mathematics made during the sixteenth and seventeenth centuries allowed better modelling of the movements of the planets, as observed from Earth, with successive advances from Copernicus to Kepler to Newton, and then to Pierre Simon Laplace later on in Paris who mathematically removed Newton's requirement for God to correct the planetary alignments every few hundred years (Chapter 5). Telescopes also improved, for example, thanks to William Herschel, a musician and amateur astronomer who built mirror lenses and, in 1781, was the first person to discover a new planet (Uranus) since Babylonian times. However, this still left the challenge of estimating interplanetary distances, because if you don't know how big something really is (e.g. Venus or the Sun), then you can't work out how far away it is—and if you don't know how far away it is, you can't estimate how big it is. These discoveries depended on developments in the much more mundane 'technology' of long-distance travel. To work out distances, you need to triangulate—that is, to observe the distant object at the same time from more than one place, plotting its relationship to 'fixed' points like the stars in order to work out the angles in your triangle. However, the base of this hypothetical triangle needs to be long enough, and you need to make sure you're taking

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[5] Napier was thought by his neighbours to be a sorcerer and to have a black rooster as his familiar, although in the reign of the witch-hunting King James VI, he doesn't seem to have had any trouble—it's not as if he was an unmarried woman living with a cat. Perhaps it helped that he dedicated his prediction of the Biblical Apocalypse to the king.

[6] Or 1916; opinions vary . . .

[7] Hence referred to as 'non-Euclidean' geometry because the principles developed by Euclid (in the third century BCE) applied to flat surfaces only.

your measurements at the same time. Triangulating the closest astronomical object, the Moon, doesn't require a huge base and the stages of eclipses provide a way of keeping time constant for multiple observations. Consequently, the Greek astronomer and mathematician Hipparchus was able to provide the first estimates of the distance to the Moon, calculating these from the solar eclipse in 190 BCE and differences in its recorded appearance between Nicaea (northern Turkey) and Alexandria (northern Egypt).[8]

Triangulation and its dependencies

For anything further away than the Moon, you need a much greater distance between the points of observation. This, in turn, depended on there being safe-enough navigation to take the same readings from points as far away from each other as possible. The first serious attempt at this was carried out in 1671, when Jean Richter travelled to French Guinea and observed the position of Mars at the same time as his colleague Giovanni Cassini in Paris. Once they'd estimated the Earth–Mars distance from this, they had the necessary mathematical projections of planetary alignments to calculate all the other distances.[9] Further refinements of these estimations were made using the 'transits of Venus'—the movement of Venus across the Sun's disc that occurs regularly on two occasions, eight years apart, every 243 years. Edmond Halley, the British astronomer (who predicted the next appearance of the comet that bears his name) had suggested that these transits could be used for distance estimation. However, they needed to be observed from different parts of the world and the 1761 transit sparked a rush of eighteenth-century voyages, one of the first examples of international scientific collaboration (or competition—the distinction is always a little blurred). Astronomers from France, Britain, and Austria travelled all over the world to take measurements that year and even more were taken at the next transit in 1769.[10] Transits of Venus have continued to be exploited as an opportunity for researchers, although the questions have moved on over the years. For example, the most recent transit in 2012 was used to investigate changes in the Sun's brightness and known properties of Venus' atmosphere, hoping that these findings

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[8] Hipparchus estimated this to be within a range of 59 to 72 Earth radii. The actual average distance from today's measurements is 60.5 radii, comfortably within this range.

[9] This was also quite accurate—for example, the distance between the Earth and the Sun from their calculations was only about 7% different from the current accepted value.

[10] One of the 1769 transit observations was taken in Tahiti by James Cook. After he'd finished there, he opened sealed orders from the British Admiralty telling him to explore the south Pacific for a fabled new continent. He landed in Botany Bay the following year and claimed Australia as a British territory.

might help to glean information from the much less visible transits of planets across other stars.

Estimating even greater distances requires an even longer base for triangulation, or else a different form of measurement. Chapter 3 discussed Ptolemy's second-century model of space, with its seven planetary spheres followed by the eighth sphere of the 'fixed stars'. It was an outmoded model by 1500, but only because the planetary relationships weren't modelled well enough (and the fact that Uranus and Neptune hadn't been discovered yet). For centuries afterwards, and indeed even for most purposes today, you might as well view the stars as sitting on the surface of a sphere somewhere out there because there's no relative movement that can be meaningfully observed in real time from Earth. The idea of the fixed sphere had begun to be discarded in the seventeenth century when Halley compared contemporary star charts with those drawn up by the Ancient Greeks (including one by Hipparchus) and realized that, while most of the observations were very consistent, some of the stars had clearly moved significantly in the intervening 2000 years. This indicated that stars were bodies in relative space, just as planets were, and then as soon as astronomers started to estimate interplanetary distances, they began realizing how vast everything was beyond that if it wasn't showing any observable relative movement. But how do you start estimating distances to the stars? To begin with, this still had to rely on triangulation and the only way to extend the base of the triangle was to observe the same stars from different ends of the Earth's orbit around the Sun—that is, observation points 300 million km apart (the diameter of the Earth's orbit) compared to a maximum on our planet of 12,700 km (the diameter of the Earth). However, this in turn required extremely accurate star charts and had to wait until photography had been invented, although the technique had been applied to single stars from the early nineteenth century.

Time and space

All of this progress in measuring distances in space was in turn reliant on measurement of time. If you're measuring relative positions of celestial bodies from different parts of the world, you need to know when the observations are taking place, as well as needing timekeeping devices for safe enough long-distance navigation in the first place. Accurate timekeeping starts with the pendulum, and Galileo is supposed to have been the first to grasp its potential while watching swinging lamps in Pisa Cathedral—realizing that the timing of the swing depends on the length of the pendulum rather than the
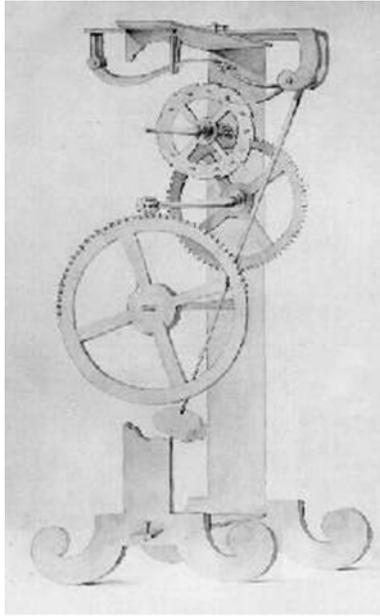
Fig. 12.2 The first known design of a pendulum clock by Galileo Galilei in 1641, although this drawing was probably by his student, as Galileo was blind by that time and within a year of his death. Inventions are mixed blessings—undoubtedly, the ability to measure time was an important prerequisite for many fields of research; however, how many of us would say that clocks and timetables make life any more pleasant, or haven't yearned for a simpler age? If Galileo hadn't figured out the pendulum, someone else would have done, which is another truth about research and human history—what we call 'progress' will happen regardless as a product of our curiosity, whether it leaves us better or worse off at the end of it all.

Drawing of a pendulum clock designed by Galileo Galilei (1659). Vincenzo Viviani. Public domain, via Wikimedia Commons. https://upload.wikimedia.org/wikipedia/commons/8/8c/Galileo_Pendulum_Clock.jpg

amplitude of the swing or the weight at the end. Of course, there was no standard for Galileo to compare it against, so he had to use his pulse. He designed a pendulum clock but never completed it (Figure 12.2), and the first one was built in 1656 by the great Dutch scientist and inventor Christian Huygens, just seven years after Galileo's death.[11] For around 150 years, timekeeping relied on pendulum clocks and their mechanical successors until the development of quartz clocks during the nineteenth century and atomic clocks in the twentieth.

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[11] The spirit of Archimedes was very much alive in people like Huygens and Galileo, and, of course, in Leonardo da Vinci.

After interstellar distances became clearer, the questions in astronomy started to be driven more visibly by findings and technologies from other fields, and astrophysics began to take over as a discipline. Knowing the distance to a given star allows you to estimate distances to others from relative brightness; then spectroscopy is developed and allows a calculation of the composition of stars. Doppler effect calculations applied to binary star systems allow you to tell how fast they are moving around each other, and the relationship is found between a star's colour and its brightness. The constancy of estimated core temperatures provides evidence for nuclear fusion once quantum mechanics is developed. Better telescopes allow galaxies to be delineated and the observation by Edwin Hubble of redshift in these galaxies, coupled with Einstein's general theory of relativity, indicates an expanding universe and therefore suggests the idea of a Big Bang at the beginning of it all. And so on . . .

Technologies and progress

The progression of astronomy and astrophysics is therefore not only the result of researchers generating and testing hypotheses but is just as much a product of the technologies available (lenses, navigation, clocks, photography, spectroscopy, etc.) as well as cross-application of knowledge from other fields, mathematics in particular, theoretical physics later on. Similar dependencies underlie the history and progression of other disciplines, of course; in particular, there are often periods of rapid progress when a technological development 'unlocks' previously closed-off areas of enquiry. Earlier chapters mentioned the invention and development of vacuum containers as a kick-start for chemistry, and Volta's 1799 invention of the battery clearly helped the study of electricity over the nineteenth century. Similarly, it could be said that the field of human biology was advanced at an early stage in the modern era by the legalization of dissection in Western Europe, improving on Greek models of 'gross anatomy' (i.e. what's visible to the naked eye). Shortly afterwards, the invention of the microscope opened up 'microanatomy'. For example, William Harvey, an English physician,[12] had carried out a series of experiments involving tourniquets in the early seventeenth century, enabling him to conclude that veins and arteries were somehow connected, and that blood circulated from one to the other, although he was unable to take

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[12] Appointed to King James I, so a contemporary of Napier, but more in Bacon's mould of scientific enquiry. He was sceptical about witchcraft allegations at the time and is believed to have saved the lives of at least four women by showing the charges against them to be nonsense.

the theory further. Then microscopes were invented and Marcello Malpighi working at Bologna later in the century was able to identify lung capillaries in frogs, completing the picture. This was followed soon afterwards by experiments at Oxford University that showed purple-coloured venous blood could be turned into red-coloured arterial blood by shaking it up with air, allowing conclusions to be drawn about what was going on in the lungs, centuries before oxygenation and haemoglobin were understood.

Technologies and the human condition

The implications of the discovery of blood circulation were considerably wider than human biology and are an example of how research can influence culture and ways of thinking about the world. For a start, it relegated the heart as an organ from its previously exalted position as the seat of the soul to being a rather ordinary, if essential, pump. Also, the image of blood itself shifted from it being a spiritual and mystical substance to having a more mechanical irrigation function. At the same time, the understanding of health and disease moved from fluids (the four 'humours' of blood, phlegm, black and yellow bile) to solids (the heart, lungs, liver, brain, etc.). This tied in nicely at the time with the Enlightenment period and a general distinction, promoted by the philosopher René Descartes in particular, between the purity of the brain and the limitations of the body—hence all those Palladian houses and immaculate geometrical gardens of the time, where wealthy men (mostly) could imagine themselves embracing Reason and sorting out the world's problems through rationalism. Chapter 6 described its legacy, which also probably includes the current, and often-unhelpful, splitting up of Western medicine and medical research into organ-specific specialties (cardiology, respiratory medicine, hepatology, neurology, etc.) as distinct from the more holistic approaches in other world regions where Enlightenment philosophy has been skipped over. And all of this because of the application of dissection and the microscope in seventeenth-century Europe.

Technologies in the humanities

Technological advances have played their part in humanities research— for example, the use of medical imaging on mummified remains, or the chemical analysis of paint to understand the provenance of fine art, or applications of digital textual analysis to literature. For research fields which deal near-exclusively with information from the past, accelerations in research

Fig. 12.3 A diagram by Jean-François Champollion, one of the key figures involved in deciphering Egyptian hieroglyphics using the Rosetta Stone. Champollion had considerable struggles in his life, caught up in the fluid politics of post-Napoleonic France and having to deal with local academic rivalries, not to mention tense relationships with English scholars engaged in the same race to decipherment. He was ground down by it in the end and died of a stroke at the early age of 41, although not before he had established a considerable reputation in the field.

Jean-François Champollion's table of hieroglyphic and demotic phonetic signs (1822). From Champollion, J-F., Lettre à Monsieur Dacier. British Museum. Public domain, via Wikimedia Commons.

activity are necessarily determined by new material becoming available (e.g. a new fossil for palaeontology, a new manuscript for history/literary research), or else a new way of thinking about currently available material. The discovery of the Rosetta Stone in 1799 might fall under both of these

categories.[13] This records a royal decree issued in 196 BCE by the priests of the Egyptian king Ptolemy V covering various matters of taxation, some recent military successes, and instructions about worship. Because it contains essentially identical texts in Ancient Greek, Egyptian hieroglyphics, and Egyptian Demotic (the late Egyptian language in script rather than hieroglyphics), its discovery was a major step forward in deciphering the hieroglyphics (Figure 12.3)—the first time this writing had been understood since the fall of the Roman Empire. In turn, this opened up a wealth of new knowledge from all the previously indecipherable scripts at the time, and the nineteenth and early twentieth centuries saw rapid progress in translations of other ancient languages.

Military and commercial influences

It's probably no accident that many of these discoveries and accelerations in research are tied in with military activity—the Rosetta Stone following French and British campaigns in Egypt, or the archaeological excavations and similar deciphering of Linear B (Ancient Mycenaean script) following the British capture of Crete from the Ottoman Empire in the 1890s. Even the second voyage of HMS *Beagle* in the 1830s, with a young Charles Darwin on board, about to formulate his ideas on natural selection, was planned primarily to ensure that the British Navy had an accurate strategic picture of coastlines and harbours in South America. Also, as mentioned in Chapter 3, the mapping of ocean floors for submarine navigation as the Cold War began was a great help in establishing deep-sea volcanic activity as a mechanism for continental drift. Later exploration of these vents also showed up a surprising abundance of life-forms living off the chemical soup, providing an idea of how life might have emerged at very early stages in Earth's geological history. Like it or not, warfare has been a key impetus for research and development over the centuries. Sometimes the research has been in direct service of military goals, from Archimedes and his catapults to twenty-first-century drones. Sometimes military development builds on research that had no original intentions in that area—for example, early twentieth-century progress in radiochemistry and particle physics feeding into the Manhattan Project and the first nuclear bomb. However, sometimes conflict-driven research

[13] The 'discovery' consisted of it being noticed by French soldiers who were strengthening defences during Napoleon's campaign in Egypt at the time. Luckily, they were accompanied by Egyptologists, or it might have been crushed into the fortifications. The stone was later appropriated by British forces but was translated by both French and British academics. It remains in London and hasn't yet been returned to Egypt.

and development has wider non-military benefits—advances in reconstructive surgery following the Vietnam War, for example, or figuring out how to mass-produce penicillin during the Second World War.

Military organizations are just one of many external agencies driving the 'development' side of R&D—although clearly the armed forces can exert a lot more power to drive research agendas, and even forcibly co-opt researchers, at times of their choosing. As the military examples illustrate, the relationships between 'research' and 'development' are complex and multidirectional. We've mostly considered in this chapter the way inventions or discoveries can open up new opportunities for research. However, previous chapters have also described instances where research has influenced invention, or at least development—the need in astronomy for better visualization, from better lenses right up to the twentieth-century space and radio telescopes; the need in chemistry for increasingly elaborate glassware allowing experiments in vacuums or purified atmospheres; the need for polar research facilities to study climate change. And then there are the primarily commercial inventions that draw on preceding research carried out with no application in mind. There are many examples of this, but the invention of the radio is as good as any.

The invention of the radio

The invention of the radio has its origins in electromagnetism. Back in the early 1820s, Hans Christian Ørsted[14] at the University of Copenhagen discovers that electrical current generates a magnetic field, a principle that Michael Faraday at the Royal Institution in London[15] uses to create an electric motor. Electrical current can now be turned on and off, making a needle move miles away at the other end of the cable, resulting in the invention of the telegraph in the 1830s. This then leads to the code systems developed by Samuel Morse and others, clearly with huge commercial potential as European settlers are spreading across the US and need to keep in touch with each other. In the meantime, the idea of further-reaching electromagnetic forces is mulled over and pulled together into a set of theories in the 1870s by James Clerk Maxwell, working at the time at the Cavendish Laboratory in Cambridge.[16] This is one of those big unification moments in physics, up there with Newton's work beforehand and Einstein's afterwards, but without any practical application

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[14] Coincidentally a friend of his near-namesake, Hans Christian Anderson, the author of all those bleak children's stories.

[15] Working for Sir Humphry Davy, the nitrous oxide enthusiast from Chapter 10.

[16] A distant legacy from the family fortune that supported Henry Cavendish from Chapter 10.

in mind. It's also on the complex side and not understood by many at the time. Luckily one of those who does follow it is Heinrich Rudolf Hertz in Berlin, who is looking for something to do for his doctorate and ends up building sets of apparatus in the late 1880s that can both transmit and receive electromagnetic waves—originally called Hertzian waves but renamed radio waves around 1912. In the meantime, Kentucky professor of music David Edward Hughes has invented what becomes the standard means of printing telegraph messages,[17] uses the profits to move to London, and invents the first microphone in 1878.[18] Detection of Hertz's waves is taken further in the 1890s by Édouard Branly in Paris and Oliver Lodge in Liverpool, with Lodge (crucially) also developing 'syntonic tuning' that allows for simultaneous transmission without interference.[19] Around this time, scientists start to demonstrate that signals can be transmitted at a distance, although apparently still without much thought of commercial potential, and Lodge is displaying transmission in London, although more as an experiment for the general public. Jagadish Chandra Bose (Figure 12.4), working at the University of Calcutta, is also transmitting and receiving radio waves and shows that these can pass through several walls, using a device like a more modern antenna. Meanwhile, in New York the great inventor and electrical engineer Nikola Tesla is interested in using radio waves as a means of remote control, and Alexander Stepanovich Popov in St. Petersburg is using a radio signal receiver to detect lightning strikes. Sooner or later, someone has to take the initiative to commercialize 'wireless telegraphy' and Gugliemo Marconi turns out to be the man of the moment, travelling from Bologna to London in order to file a British patent for this in 1896. He establishes the Marconi Company Ltd. and gets going on a series of demonstrations of transmission across ever increasing distances—over the English Channel in 1899, transatlantic from around 1901. Further developments come from Karl Ferdinand Braun at the University of Strasbourg, and he and Marconi share the 1909 Nobel Prize in Physics.[20]

If nothing else, this example demonstrates the internationalism involved in these sorts of developments, with accrued knowledge bouncing around

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[17] The very same paper tape system beloved of Westerns—where some poor operator in an isolated telegraph station gets the warning about outlaws on their way just after their train has drawn in and there's already a gun pointing at his head.

[18] Hughes might also have produced radio waves before Hertz, although seemed happy for Hertz to take the credit.

[19] Lodge viewed electromagnetic radiation as evidence of wider phenomena and was a firm believer in spiritualism, telepathy, and clairvoyance, which raised scientific eyebrows at the time.

[20] Marconi's invention was credited with saving the lives of those who survived the sinking of the RMS *Titanic* in 1912, as it was used to signal for rescue. Marconi was probably more thankful that he wasn't one of its passengers in the first place (he took an alternative crossing only three days earlier).
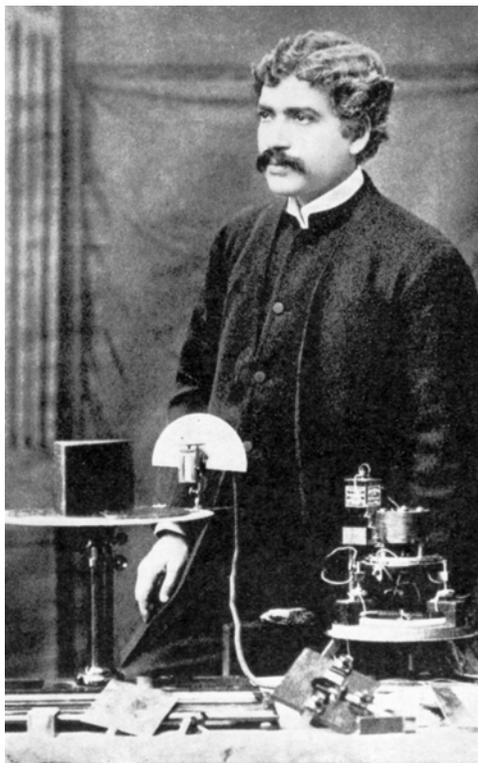
Fig. 12.4 Jagadish Chandra Bose, a scientist with broad interests across biology and physics, working at the University of Calcutta. Like many others involved in the development of the radio, he was uninterested in the commercial element—indeed, he met Marconi in 1896 and was aware of the potential here. Instead, he suggested that his research in the field should be used by others, an early advocate of open science, at least in his own domain (although a century later, university employers were taking a different view of intellectual property). Bose's work underpins much of the technology deployed in microwaves and Wi-Fi, and the Bose Institute in Kolkata is one of India's oldest research establishments. Having succeeded against the odds, his inaugural address there in 1917 emphatically declared: 'It is not for man to complain of circumstances, but bravely to accept, to confront and to dominate them; and we belong to that race which has accomplished great things with simple means.'

Jagadish Chandra Bose at the Royal Institution, London, 1897. The Birth Centenary Committee, printed by P. C. Ray. Public domain, via Wikimedia Commons. https://upload.wikimedia.org/wikipedia/commons/5/56/J.C.Bose.JPG

widely dispersed centres. The relationships between research and inventions, new discoveries, and new technologies, have been complicated over the centuries. It's therefore highly likely that your own research will be fitting into its particular zone of complexity—for example, in the facilities or

measurements or materials you need, or the scope for your research to generate intellectual property or even patentable products. As mentioned, for humanities research (but applicable to all fields really), if you want your study to be novel, you're either going to have to bring in a new technology or discipline or discovery, or else you're going to have to make do with what's already there and think up a novel way of using it.

Solutions in search of a problem

A final situation, touched on briefly at the beginning of this chapter—slightly strange but becoming increasingly common—is when a new discovery comes along with no immediate applicability but a general sense that it ought to be incorporated into research. We're familiar enough with commercial products that are heavily marketed but without a very obvious purpose: 'solutions in search of a problem'. Sometimes they do succeed in finding their niche after a period of uncertainty—for example, I remember a time when the iPad came out and no one was completely sure whether tablet computers would catch on. However, sometimes there's a more prolonged struggle for a purpose and you do wonder what the inventor was thinking of.[21] It's perhaps a little too recent a development to draw firm conclusions, but 'big data' is a possible example of this in research. I really shouldn't be knocking it because I've spent a sizeable proportion of my academic life in a corner of that field. However, there is sometimes an imbalance in the high-level presentations on the subject, which are top-heavy with exciting, dreamy possibilities and a little lacking in demonstrable practical applications. The traditional way to carry out research is to start with the question and then try to tackle it, developing new techniques or resources along the way if needed, but always with an eye on the original challenge. However, more often than you would expect, you'll find yourself in a situation where there's no question on the table. Instead, there's a perceived need to use a particular technique or resource (e.g. your university has just bought some shiny new lab kit), and an expectation that suitably relevant research questions will be constructed to demonstrate the usefulness of that technique or resource. This scenario does feel the wrong way around, although I feel less negative about it than I used to—I think there are ways to make it work, if only by trying to understand where the novelty and opportunity might lie. Perhaps it's the way of the inventor and perhaps we have to discover our inner Archimedes.

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[21] The Segway, for example?

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Communication 1

Getting Published

As we've established, 'research' didn't start in post-1500 Western Europe and it isn't even exclusive to our species. Animals draw conclusions about the world from their observations, carry out experiments of sorts and learn from them, and humans have been doing the same since prehistory. Communicating knowledge is where the differences start to emerge, although these are quantitative, rather than qualitative, differences. Animals clearly have the ability to communicate in complex ways and to learn from each other's experiences, but the development and ultimate dominance of *Homo sapiens*, for better or worse, has closely followed advances in communication that have outstripped those of other species by ever-extending margins: language complexity to bind hunter gatherer groups together, religion and culture to maintain larger cohesive agrarian societies, writing to disseminate knowledge between societies, and printing and the Internet as extra pressures on the accelerator. Behind it all, there exists a relentless curiosity and capacity for invention. And opposable thumbs.

The importance of communication

The sharing of knowledge from learned experience means that a species doesn't have to wait for slow natural selection processes in order to take advantage of its environment. Beyond this, written communication means that we no longer have to rely on our own individual experiments and experiences, or on what has been orally communicated to us from our immediate family or society. If we are prepared to take on trust the discoveries of others, our knowledge is widened to a near-limitless extent, depending only on what we can feasibly read, digest, and apply in a lifetime.

Communication has therefore always been at the heart of research. Chapter 2 shows that advances in early mathematics tended to happen when empires were at peace and trade routes flourished, allowing wider knowledge exchange than would ever be possible within one nation. During

more turbulent periods, advances have depended on what has flowed from one collapsing empire to another emerging one—for example, theories of mathematics and life science emerging in early Middle Eastern civilizations, developed in Ancient Greece and Rome, surviving in translation for further development in Middle Eastern Islamic empires (with or without influences from South Asia and China along the way), and finally, re-translated or rediscovered to feed the Western European Renaissance and Enlightenment.

Other examples of more direct transmission might include the East-to-West flows of knowledge in relation to printing and, possibly, gunpowder. In the ages before printing, knowledge was fragile and dependent on the survival of one or two unique hand-written manuscripts. The burning of the fabled Library of Alexandria (Figure 13.1) is likely to be a myth, covering up a much more mundane, slow decline through lack of support. However, there were probably any number of more minor losses, resulting in the rather stop-start pattern of progress early on. Once printing got going, there was a safety net in the much larger numbers of copies potentially available, assuming a decently sized print run to begin with and the idea taking on. This meant that if one country decided to embark on some book-burning and censorship, there was a reasonable chance that the knowledge had been passed on for adoption and storage elsewhere. And then, in turn, more modern library systems emerged to house obscure works until they were ready to be rediscovered by a PhD student looking for something to do. Whether the digital age has helped this process is perhaps still up for debate. The vaults of information are clearly larger by many magnitudes, although that also makes for a higher chance of work getting lost in the midst of everything. Hence, the importance of publication and research dissemination, hence this chapter, and the next . . .

If research isn't communicated, it's hard to see it as anything other than a waste of time, so writing and publication have always been central to academic life. However, published work has taken many and varied forms over the years. For example, the three volumes of Newton's ground-breaking *Philosophiae Naturalis Principia Mathematica* would be an anathema to a physicist or mathematician today (even if they could read the original Latin) and I don't suppose many biologists would be thinking about producing original artwork to the level and quality contained in Hooke's *Micrographia* or Maria Sibylla Merian's *Metamorphosis Insectorum Surinamensium*. The expected form of published work also differs widely between research specialties today—from the terse prose of a multi-author scientific report to the single-author book that might be used to communicate a particularly important piece of research in history or literary studies. The academic publishing
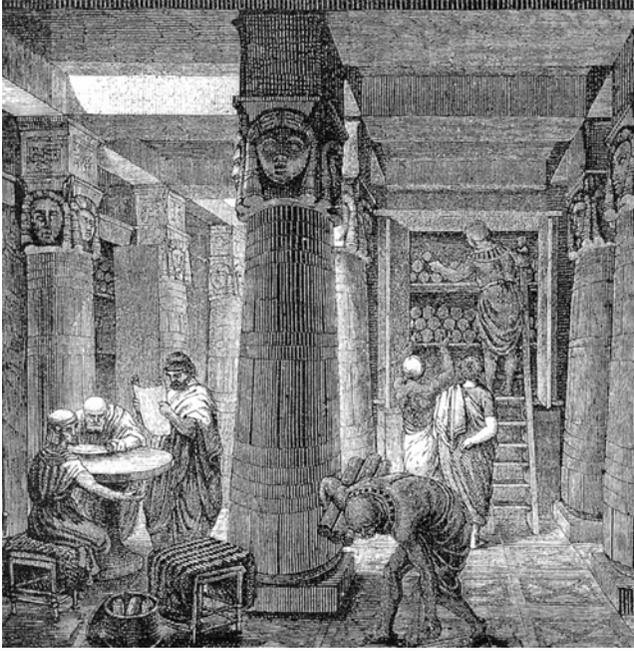
Fig. 13.1 A nineteenth-century illustration of what the Great Library of Alexandria might have looked like in its heyday. This library was created by some quite assertive purchasing of manuscripts (papyrus scrolls), particularly from markets in Greece, sponsored by the Egyptian king Ptolemy II and his successors. The scholars followed the manuscripts and hence a centre of academic excellence was formed—not unlike a government nowadays investing in cutting-edge laboratory facilities. However, in Alexandria the focus was on the ability to communicate and share findings, the limiting factor for research progression at the time (i.e. before this was democratized through printing and, more recently, digital media).

industry itself has changed beyond recognition over the years, particularly so in the more recent digital era, and it would require a braver author than I to predict where it is going, even in the next 10–20 years. The nature of academic communication, therefore, is (and always has been) variable, but I think it's worth taking some time to think about writing for publication as it's so much a part of life for all of us who are already carrying out research, and an unavoidable task ahead of you if you're just starting. Also, I do feel there are some fairly widely, if not universally, applicable challenges to overcome.

Getting started

Perhaps it's worth starting off with the writing process itself. Whatever your background, it does take some getting used to. Where they can get away with it (i.e. particularly in the sciences), education systems have tended to steer away from long-form writing over the last few decades, so increasing numbers of school-leavers are faced with undergraduate dissertations at university without much preparation. Alternatively, they may have attended a university course that has given up on this with undergraduates, leaving them to face the same thing at post-graduate level and/or definitely with a doctoral programme. I don't blame the schools and universities— lengthy prose is time consuming to mark and it's questionable how well it discriminates between good and mediocre candidates; it's probably also more difficult to mark objectively and defensibly if students are feeling litigious about their results. However, the fact remains that progressing in a research career involves writing relatively lengthy prose sooner or later. Therefore, it's worth accepting this and figuring out what to do if you think it might be a challenge.

Having read or listened to a few interviews with novelists over the years, I'm not convinced there's much of a difference between the processes of academic and creative writing. Writer's block is certainly a common enough obstacle in academia, and I think the solutions are much the same as those for a writer of fiction. In particular, the best (maybe only) way to start beating it is by simply by forcing yourself to write something, rather than making yourself yet another cup of coffee, attending to the shelf that needs dusting, answering your texts and emails, and all the other myriad distractions. It's fine to write a poor-quality first draft—you don't need to show it to anyone else at this stage and it's likely that your thoughts will be a little more in order by the end of it than they were at the beginning. You can then start hacking it into shape at the next draft. Unlike a novel, academic writing tends to split naturally into a series of points to be made, and it's helpful if paragraphs can be arranged and ordered so that the reader is taken on a logical journey through your argument or description. You might find it helpful to have a subheading (and sub-subheading, etc.) system for all the paragraphs to help with that organization, and you can try to get over any writer's block by dividing up the larger work into bite-sized chunks (as well as making life easier for your poor reader). If the final product isn't meant to have subheadings (e.g. if it isn't the house style for your target journal) then you can generally remove them later on without the paragraphs losing their flow.

What's the big idea?

Before you even start, however, you do need to have some idea what you're trying to communicate.[1] This might involve you going back to the beginning of this book and working out what your research is really about, and where it fits in the scheme of things. However, let's save time and assume that you've remembered enough about the principles of research and its flow through history to have an inkling of an idea. Perhaps start out like Socrates (Chapter 3) and think through what it is that you and the world don't know—what was the reason for the piece of research you want to describe, what question was it trying to answer, or what was the blank space on the map that needed filling in? Next (and see Chapter 6 if this is getting hazy), decide whether what you're doing is marshalling arguments (or equations, or evidence from historical sources, etc.) in order to take the reader on a new path you've drawn from A to B—the rationalist approach to knowledge generation,[2] or if you're describing a study that's taken observations or an experiment putting a theory to the test—the empiricist's way. Perhaps it's some mixture of the two (in which case, do try to disentangle them—what is the empirical evidence and what is the rationalist argument?). If it's empirical research, is it simply a piece of description, or is it testing a theory? Either is fine, but be careful to keep to what was decided in advance of the study; in particular, don't try to take unexpected findings from a piece of exploratory observation and pretend it was a theory that you'd always intended to put to the test—any reader worth having will be able to see through that, however hard you try. And it's fine to describe and explore. What you can conclude may be limited without the experiment to test your findings, but that can always come later.

Pitching to readers and publishers

Assuming you have an idea of what you need to communicate and that you have overcome your writer's block, you now have to get down to the communication itself. This is when matters start to become discipline-specific, but a good guiding principle is to think of your reader as objectively as possible, having pity on anyone who has to wade through academic texts, and making

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[1] And if you are getting writer's block, at least consider whether it's because you've got a mess of findings you don't know how to communicate because you're not sure what you were doing in the first place. That's fine, and by no means unheard of, but you might need to get some senior advice on sorting it out.

[2] Descartes and 'I think therefore I am' as a starting point in his argument for God—that sort of thing. Or the succession of mathematical principles that Newton used to build towards his laws of motion.

sure that you're writing with them in mind. There's no harm in imagining a general rather than expert reader—one of your colleagues, for example, or someone more junior—just to make sure that you're putting your ideas together clearly and compensating for the fact that your prose is unlikely to be particularly gripping. That's just basic politeness. However, you also need to have in mind the reader who's going to be judging your work for publication, in relation to everything else that's competing for space. So, do get to know what they're likely to expect—if you're submitting to an academic journal, read their instructions for authors and have a good look at what they publish. Try to find a few pieces of work similar to your own and use them as a template to work out the approximate length, style, use of subheadings, and other formatting issues. If there isn't anything remotely similar to your planned submission, are you really sure you're sending your work to the right place? Ultimately, reviewers and editors will make their judgement on the quality of the research you're describing and whether they believe it's right for the publication, but it does help if they feel they're in comfortable, familiar territory when they read what you've written—for example, the flow of the text, the relative length of any subsections, the layout of any tables or figures.

Different academic publishers have different processes for evaluating submissions, but most involve some sort of peer-review process—occasionally with the reviewers identified, much more often anonymous. However, most academic journals are seeing the volume of their submissions outpacing the pool of reviewers they can draw on, so they have to be quite choosy about what to send for review (particularly as most reviewing remains unpaid and essentially a favour). This means that a much more cursory editor's review is increasingly the first hurdle to overcome, the judgement here being whether the submission will definitely interest the journal if the reviewers like it. I suspect that editors vary widely in the depth to which they consider submissions, but they're likely to focus heavily on your abstract or other similar summary, and on any cover letter you've been invited to include as part of the package. With this in mind, you do need to get good at writing short, pithy prose that summarizes your longer piece of work and presents it in an easily digestible format. I suggest getting senior advice (and certainly a read-through, if you can) when pitching for publication, as the style may need tailoring. You do need to express clearly why the publisher should consider your approach. Keep in mind, though, that most academic editors and reviewers don't like a 'hard sell' and will be suspicious if you seem to be exaggerating—it just doesn't feel right for research. Also, if your submission's summary is over-hyped at the first stage, any reviewers reading the piece in depth later on

will pick this up quickly enough and it's unlikely to put them in the best of moods. Quiet, calm assertion is probably the safest style—being clear about your objectives, the findings you're communicating, and why you think they matter—and then leaving it for others to judge.

The core text

Coming on to the submission itself, scientific articles tend to have a standard four-section structure—background, methods, results, discussion—and I suspect the same principle applies to a great deal of research reporting outside the sciences, even if those headings aren't used. For example, any communication really needs to start with some sort of background material—setting the research in context, explaining what's known and what's not known, and giving the reader at least some idea of the reason for the research about to be reported and its objectives alongside (if appropriate) any hypotheses or theories being tested or evaluated. How else is anyone going to understand the research if it isn't introduced? However, the length of this may vary considerably. In some academic journals, it may be no more than one or two paragraphs; in others, several pages (or book chapters) may be expected—again, do make sure you're fitting in with house style and what your readers are likely to be familiar with.

   After the introduction, it's reasonable to make sure the reader knows what the research is that you've carried out, so the 'methods' section naturally follows the background in most scientific journals. For humanities research and the more rationalist approach, the nature of the work may already be self-evident, and the introduction will merge seamlessly into your own new material, although it does seem the decent thing to let your reader know in advance what you're going to cover, how you're going to cover it, and something about the approach you're taking. While it's more natural to be introducing first before explaining the approach, I guess it might also work if you start with summarizing the approach and then explain why you took that route (i.e. the methods before the rationale). Each to their own. In the empirical disciplines, the methods section is the most important part of the report because it's this material that tells the reader what you actually did, so that they can draw their own conclusions from what you found. Ideally the information should be sufficient to allow other groups to carry out the same, or similar, studies in order to see whether the findings still hold—building up consensus (Chapter 8) through repeated, (hopefully) consistent, independent findings.

Disciplines vary as to the balance between the other two sections. In the typical scientific report, the 'results' section lays out what was actually found, often quite briefly and relying a lot on tables and/or figures. Then, a 'discussion' section considers what can be concluded from this. In other fields, for example in social sciences, the two may be more conflated and the findings discussed as they are presented, with no need for anything more than a brief concluding round-up at the end. Similarly, a report of humanities research or a mathematical paper are unlikely to need to separate their findings from their discussion. However, there are still advantages in many fields in keeping them distinct, if only in your thinking. You need to clarify what you're communicating (your observations), and separate it from the deeper truth that you're inferring, using the overlay of your own reasoning or argument.

Inference

The idea of inference has received particular attention and discussion in my own field of epidemiology, but its elements are much more widely applicable in other disciplines, even if they're not thought about in the same way or described with the same terminology. Inference simply describes the process of drawing conclusions from observations and bridging the gap between what's been found and what it means (Figure 13.2). At its worst, it's a tedious list of niggles that epidemiologists think they can use to pull apart everyone else's research,[3] but at its best it's proper academic objectivity, as close to Socrates as any of us are likely to get. The reason I consider it in this chapter is that it's a key component of published communication—the point where the writer engages with the reader and they both consider objectively what can be concluded from what has just been described. And it should ideally involve that joint process—the invitation to be dispassionate and thoughtful, that only-too-rare moment of objectivity and acceptance of ignorance (Chapter 2). If you want, you can approach research communication as a novelist or journalist might—pushing for the best publishing deal you can manage, getting into the widest-read or highest-impact sources. Everyone falls into that trap sooner or later, and academic institutions don't help with their metrics obsessions. However, that's not what research is really about. Research is about the fascination of discovering something, wondering what it might mean, and designing the next experiment to try to get a little closer to what's out there. Your conclusions, and the 'message' of your research paper

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[3] Although sometimes a good way to infuriate biological scientists working in health research, or at least the ones who actually care what anyone else thinks.
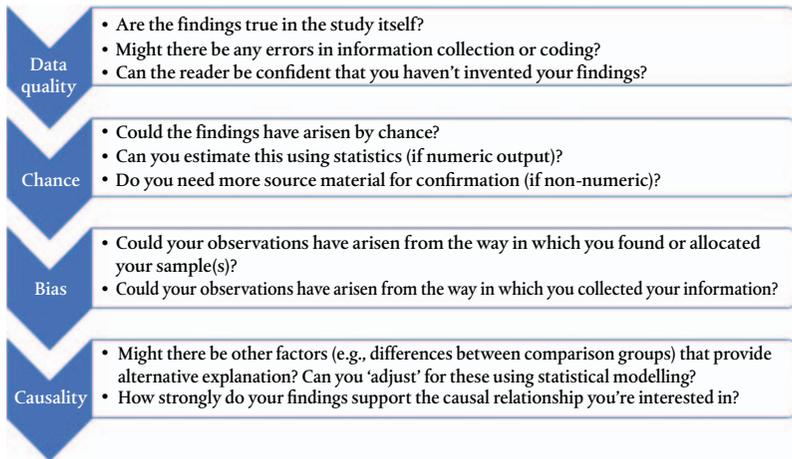
Fig. 13.2 The process of inference, which I think can be made to apply to most empirical research, even if the language comes from epidemiology—after all, it's common enough to have source material (whether human participants, cell samples, or a historical archive), to take measurements, and to consider whether your findings have just arisen by chance, and whether they represent a cause–effect relationship. The point with inference is that a good research report will be open and self-critical about it when you discuss your findings, sharing your objectivity with your reader. However, it's also likely to be key to the way you design your studies in the first place (i.e. to forestall any concerns that a reader might have further down the line).
Author's own work.

(or book), might be derived from empirical observations, or they might be the product of a progression of arguments or equations. Either way, it helps if they can be seen as objective and unblinkered, which means taking your reader along with you, putting the evidence before them, and allowing them to make up their own mind as you make up yours.

Inference receives particular attention in epidemiology because it's a natural science that deals with messy observations and uncontrolled environments and applies measurements that can never be perfect. We considered the difficulties drawing firm conclusions in Chapter 7—how, by definition, you can never prove that a theory is true but how it's probably also impossible to prove it's false in circumstances where you can't carry out a pure experiment in a vacuum. Instead, you have to build up evidence across a large number of studies, looking for consistency and achieving consensus; however, each one needs scrutiny and some consideration of what its findings are really indicating. There are various steps along the way, and these are as important in the reporting of findings as they are in the design of the study.

Step 1—are the results true?

As a starting point, the reader has to accept that the actual reported results are genuine—for example, that the average blood pressures in Groups 1 and 2 really were what the authors have recorded, or that the quotations underpinning a literary thesis are correctly provided. If you're thinking about your own findings then there's plenty you can do to make certain they're correct: double-check your calculations, make sure you've transcribed your measurements accurately, go back to your source material and re-read it—whatever applies to the way you've collected your information. This is nothing more than the principle of careful observation—the attention to detail we considered in Chapter 4 as a characteristic of any good research project.

If you're looking at someone else's findings, then you have to accept them because what else are you going to do? Of course, they could be incorrect because the other person or team has made an inadvertent mistake. Alternatively, the other person or team could have made their findings up—research fraud does happen and there are flare-ups of worry from time to time about what to do about it. I don't think there's an easy answer to either of these scenarios. An advantage of academic consensus (Chapter 8) is that it tends to be cautious about findings that come out of nowhere and sound too good to be true—or at least this ought to be the case if the research discipline is a healthy one. And the academic community does tend to notice if a group or individual is consistently putting out over-hyped findings that no one else can reproduce. The difficulty is that it's near-impossible to prove most cases of fraud or sloppy practice, so when they are suspected, the most likely response will be for findings to be quietly ignored, rather than called out as false.[4] Publishers naturally have more to lose, and they periodically anguish over this matter (nobody likes to be left holding responsibility for falsified information), although I have yet to hear of any wholly watertight fraud detection system. It may become a rarer issue as research communities grow in size and fewer important studies are carried out by individuals working in isolation. Fraud, like high-level conspiracy, is harder to get away with when there are a lot of people involved—sooner or later there'll be a whistle-blower, or at least rumours will get around. The commercial sector is more of a worry here because of the tighter controls that can be imposed on whistle-blowing by current and previous staff, as well as the more obvious incentives for skewed or even falsified information. However, any institution, public or private,

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[4] Unless you're lucky enough to witness a proper academic shouting match—insecure, irritable professors with limited social skills can really go for it when they want to.

should be viewed with caution if it has a top-heavy power structure and limited staff/trainee freedom.

Step 2—are the results generalizable?

That's enough about sloppy or nefarious practice. Having accepted in good faith that the findings of a study are genuine, there's then a series of questions to answer on what they actually mean—the real process of inference. For a lot of life sciences, there are errors that can creep into studies through the way they're designed, and which are collectively referred to as 'bias'. This isn't the place for details (whole books have been written on the subject), but it's simplest to think of most studies as having two elements: the material being chosen and collected for investigation, and the measurements being taken. Bias can arise from either one of these sources, and the 'methods' section in a scientific research paper is typically divided up into a description of the sample and a description of the measurements, as well as usually a third sub-section describing the statistical analysis, if appropriate. In the discussion at the end of the paper, the material and the measurements are then revisited to consider whether there might have been any source of error. Similarly, your key thinking points in designing the study in the first place are likely to concern the steps you need to take to minimize risk of this error.

In epidemiology, the 'material' of a study is the people taking part—for example, people with bowel cancer being compared to a 'control' group of people without bowel cancer. Both groups need to be identified somehow and need to agree to participate; any of these steps might create artificial and unwanted differences between the groups, and these might in turn be influenced by whatever it is that's going to be compared. For example, if someone is worried about a particular diet they've followed, they might be more likely to volunteer as a control in the cancer study order to have the health check being offered; this might therefore generate an artificial difference if diet is one of the things you want to compare between groups. Similarly, the accuracy of measurements might vary, in that people with cancer might be more likely to remember or report some past risk factor than unaffected people because they've had a lot of reason to think about what might have caused it.

We discussed some of these challenges for case control studies in Chapter 11, although they apply outside epidemiology if you think more generally about the material you select for your research (historical sources accessed, cell lines assembled, national economic systems evaluated) and the measurements you make (records of eighteenth-century rents and tenancies,

protein structure changes, GDP). Also, they don't only apply to observational studies. In a randomized controlled trial of a drug treatment, group differences might be observed if people know (or guess) whether they're receiving the new treatment rather than the placebo and if they're more likely to report improvement as a result. Or if you're comparing two cell lines under different experimental conditions, and if the results involve some level of subjective evaluation, you might unconsciously skew the findings one way or another if you know which cell line you're looking at.

The point about bias isn't that the observations themselves are false. Rather, it's that the results in the actual study might not be reflected 'out there' in the situation or population that the study is trying to represent. For example, a difference we've observed between people with and without bowel cancer in our sample might not really be present if we were to investigate everyone with and without that condition. Instead, the observations might have come about because of the way in which the comparison group was selected, or the way in which the measurements were applied in the study.

An additional, but simpler, issue is that even a perfectly designed study doesn't include everyone or everything in its comparison groups but takes what it hopes are representative samples. There will therefore always be some difference observed by chance between selected groups, even if there's no actual underlying difference. For studies using numeric (quantitative) outcomes, helpfully the expected random distributions of values follow mathematically predictable patterns (e.g. the bell-shaped 'normal distribution'). Statistical techniques can therefore be used to quantify this role of chance—not involving particularly complicated equations, although most often carried out by computer software nowadays, rather than hand calculation. If your observations aren't numeric, you may have to use other approaches. For example, in 'qualitative' research, where the output consists of narrative accounts (e.g. transcribed interviews), findings tend to start repeating themselves after a while and new themes (or at least important ones) become less and less likely to emerge, referred to as 'thematic saturation'. So, a decision is usually made to stop at this point, on the assumption that the findings now adequately reflect the population from which they've been drawn.

Step 3—causation?

After you've considered chance and bias—governing the relationship of the observations in a study to the true relationship 'out there'—you may still have

a little more thinking to do about your results if you're interested in cause and effect at the end of it all. As discussed in Chapter 7, if you can't isolate your material in a vacuum, then you can't be sure there aren't other influences accounting for your findings besides those you're interested in. To take a hypothetical example, you could carry out a perfectly designed study showing that people with grey hair are more likely to die, but this wouldn't mean that grey hair itself was contributing to this risk because you haven't taken age into account—older people are more likely to have grey hair, older people are more likely to die, so age 'explains' the association between grey hair and mortality (a genuine association, just not causal). Epidemiologists refer to these additional considerations as confounding factors, but it's a common enough consideration across many disciplines. We have seen how the consensus around anthropogenic climate change (Chapter 8) was complicated in its earlier stages by some uncertainty over different gases and working out which one might be responsible for past and current global temperatures—carbon dioxide, methane, water vapour, dust particles, aerosols, and others. And I imagine that history research is full of the same considerations, even if they're not quantified and referred to as confounding factors—Tolstoy's lengthy thoughts, for example, in *War and Peace*, about the Napoleonic-era upheavals in Europe and what really caused them (Chapter 6): were these the actions of one man or a combination of smaller social changes? And then, beyond rather simplistic thinking about confounding factors, you can move on to the trickier considerations of proving causation that we've spent most of the past chapters covering, from Socrates to Popper.

Strengths and limitations

The process of inference is at the heart of good research and so it's helpful if you can show evidence of it in your writing—either in the gradual description of your findings, considering their meaning as you go, or else in a dedicated discussion section kept separate from the results—whatever the tradition is for your discipline. However, do try to balance the discussion. One cheap and easy route is not to think about possible limitations at all and accepting all your findings at face value.[5] The other extreme is to list out everything that's wrong and leave the reader wondering what's left to take away. If you do feel that your study has a fundamental flaw that prevents you from drawing any conclusion, then it may be best not to attempt to publish—after all, there has

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[5] And, therefore, rather presumptuously expecting the reader to do the same.

to be some meaningful communication involved, or what's the point? However, most research isn't wholly useless and has at least something to add, so it's important to consider the strengths of your study as well as its limitations. Indicating where your study is strong is as much a part of inference as acknowledging where it's weak. At the end of it all, it's helpful if a research report can demonstrate self-awareness and at least some consideration of the methods adopted, accepting that nothing is likely to be perfect. If you can set it out clearly enough to take the reader along with you and help them to consider the issues alongside you, so much the better. No one likes to feel they're being coerced into an opinion.

A word about the woes of peer review and rise of rapid communication

Up to this point, we've considered the traditional route of research publication, which currently involves vetting by peer review. The research is presented to the reader with the assurance from the publisher that it has been read and approved by a suitable number of independent experts in the field, as well as any prior or subsequent editorial vetting. This is a system that has developed for most (if not all) academic specialties over the twentieth century and can be considered as a component of the consensus process (Chapter 8). However, as mentioned earlier in this chapter, the peer review process is coming under increasing pressure in many specialties and, at the time of writing, I think it's reasonable to have doubts about its long-term sustainability as a framework. Most reviewers aren't paid for their input, so are essentially providing a free service to the academic publishing industry. I think that academics provide peer review primarily because it seems the right thing to do, and because they're aware that they are benefitting from the same free service when they submit their own research. Some journals subtly remind reviewers of this fact. In my experience, it's beyond coincidence how often a request for a review comes from a journal when you already have a manuscript under consideration there, and it's beyond coincidence how often the feedback on your own manuscript arrives not very long after you've finally completed your reviewing duty for that journal. While this mutual understanding may still hold for traditional, high-impact, and otherwise favoured periodicals, it does seem to be less sustainable now that authors are increasingly having to pay significant costs for publication[6] and are increasingly wondering why

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[6] Rather than the old system where academic journals made their profits from subscribing readers and/or academic libraries.

they're providing a free service to a commercial agency. This is compounded by the rapid expansion in journals, particularly online-only titles, and a similar expansion in the volume of research output submitted each year that is rapidly outstripping the number of people who are qualified and willing to provide peer reviewer input. Quite what will emerge from the current crisis is anyone's guess. However, it does seem likely that there will be a profound change of some sort—it's hard to imagine that the model we have now will last much longer, and I know I'm not the only researcher who has sometimes faced very long publication delays because of difficulties finding reviewers.[7] The problem may be more acute in some academic fields than others, although I suspect that it will spread in time to become all-encompassing unless a specialty (and its publications) remains small enough to manage its own reviewing.

Even when it's working well, the process of peer review necessarily introduces a delay between findings emerging and findings being communicated, particularly when there are several rejections and resubmissions required before a manuscript finds its home. This generally feels appropriate for the sake of the additional assurance provided by the reviewing process, and for most reports it doesn't matter too much, given that the research itself may have taken a long time to develop. However, it clearly becomes a problem if the findings have important and urgent implications, or if the research field is very active and there's a race to publish or a danger of ideas being misappropriated, particularly if competitors might be providing the peer review input and might not declare a conflict of interest.[8] In response to this, a growing number of platforms offer near-immediate online visibility for research output, and it's increasingly common for research to be posted first on a pre-print site of this sort, while it awaits peer review. Sites will then generally allow the pre-print report to be linked to the final reviewed article.

Previously mostly of interest to the basic sciences, use of pre-print sites accelerated in health research during the early stages of the COVID-19 pandemic when there was a need for rapidly available output and when traditional journals were struggling under the chaos of lockdown policies. Thinking longer term, if the peer review system begins to struggle more seriously, these sites become an obvious alternative means of publication for authors frustrated by lengthening processing times. However, the inherent danger here is that unreviewed (and potentially flawed) material can appear in the public domain without any filter, resulting in misleading evidence and

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[7] And even the occasional rejection at the end of that long period with 'failure to find a reviewer' as the reason given.

[8] Which does, unfortunately, happen from time to time, although how these people live with themselves, I've no idea.

research fields swamped with misinformation. Post-publication review is a possible solution, although still doesn't address the question of who will have the time or incentive to provide this.

Returning to the writing process, I don't believe that there needs to be any difference in approach when writing for a pre-print site compared to a peer-reviewed publication, except to bear in mind the caveats mentioned, and to be clear in your mind why you are opting for this. If your intention is to post a report rapidly that you will then submit for peer review, the format may as well be the same. Bear in mind, however, that not all peer-reviewed publications will accept submissions that are already in the public domain, so it may limit your options. If, on the other hand, you simply wish to make your findings publicly available as quickly as possible and have no intention of submitting them elsewhere, then you might wish to publish in as full and unambiguous a way as possible—perhaps even uploading the source data so that readers can carry out their own checks and draw their own conclusions. Most peer review should focus on the author's interpretation of their findings, rather than the findings themselves (which, as discussed earlier, have to be accepted as true rather than invented) as well as ensuring that methods are described sufficiently clearly for this interpretation to be judged. Therefore, one way forward may be to remove the need for peer review by cutting out the element of interpretation in a research report, as well as expanding the methods description to as much detail as possible, although this is easiest done if what you are presenting is entirely descriptive.

Strategies for lengthier writing—the doctoral thesis

I suspect most researchers end up, out of habit, finding themselves most comfortable with the word length and structure of the standard short report for their field—the typical output of a single study submitted to an academic journal, for example. However, there are also longer formats used to publish research findings. Generally, the trick to 'writing long' is finding a workable superstructure to build around; for those of us used to stringing together paragraphs in short scientific reports of no more than 5000 words or so, assembling larger volumes of text can feel like embarking on a trackless open ocean in a small rocky boat. So, we need something to keep the descriptions or arguments coherent, as do our readers.

I discuss wider communication methods in Chapter 14; however, I think it's worth considering the doctoral thesis at this point, as it's a publication of sorts and it's where most researchers come up against proper long-form

writing for the first time. By the end of their thesis, many students will feel no inclination to revisit the experience, but it's probably the best training for lengthier reporting when this does come around. The tradition of requiring a book's worth of prose as a rite of passage in academia is a strange one, particularly as a thesis is often not read by anyone apart from the student, their supervisors (you would hope), and a couple of examiners. However, it is what it is. Doctorates and their examinations vary widely between countries. The British model, for example, is typically a weighty novel-length thesis, which the candidate defends very privately, usually in a small office with two or three examiners. European models (those I've seen at least) tend towards a shorter (novella-length) thesis and a public defence in a lecture theatre.[9] Quite a few European theses I've come across are also produced in a print run and posted around to other academics for their enjoyment and edification; British theses, when printed versions were still required,[10] rarely ran to more than three or four weighty final copies—for the university library, the student, and the supervisors, perhaps. However, I assume that at least some, perhaps particularly in humanities fields, end up turned around and reformatted for potential commercial publication.

I suspect that writing a doctoral thesis comes as most of a shock to science researchers who never went into academia with the intention of composing anything that lengthy. Agreeing a chapter plan is certainly important—the superstructure I mentioned—although you shouldn't feel that this has to be set in stone; also, try to make it fit your material rather than feel that it's some immutable external imposition. Because university regulations have to cover doctorates from all disciplines, they tend to be quite vague on the structure of the thesis and can be interpreted flexibly—it's your readers (i.e. examiners) and their expectations that count most of all, and your supervisor should have a good feel for what's acceptable.

Having assembled your chapter plan, do also bear in mind that you don't have to start writing at the beginning. Completing the middle sections—the methods and results chapters or equivalent—is often an easier starting point because they'll be familiar, and you may have even managed to publish some of the material already. Once you've drafted the middle sections, the introductory chapters tend to be easier to write because you've got something ahead of you to aim at. I don't know how widely applicable it is, but my own

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[9] The presence of friends and relatives being rather intimidating for examiners.

[10] The requirement for hard-copy theses may turn out to be a permanent casualty of the COVID-19 pandemic, now that everything has had to shift to electronic circulation. If so, I don't think they'll be very much missed—they look impressive on an office shelf, but the printing and binding processes do seem to create unnecessary stress for students at the already-tense hand-in point.

supervisor taught me to think about the thesis as a double-ended funnel (see Figure 13.3). The introductory material starts broad and gets successively narrower in scope as it centres gradually in on the particular questions or objectives that the thesis will address. Then there's a narrow section through the methods and results, followed by a gradual opening up of the discussion (beginning with the specific findings of the thesis, then broadening out to encompass their relationship with background literature, before finishing off with wider implications). I'm sure there are plenty of other models around, but I've found this one helpful for science students, as the long introductory sections (reviewing the background literature) can be quite challenging, and the tendency is to get side-tracked—it helps to have a focus. Beyond this, the task with long prose, even more than short prose, is to have pity on your reader and try to guide them through your descriptions and arguments. Summarize what you're going to say, say it, then summarize what you've just said—it sounds a little patronizing, but you'll tend to find that it's appreciated. You want to have your readers on your side from the start, whether they're academic colleagues or thesis examiners.
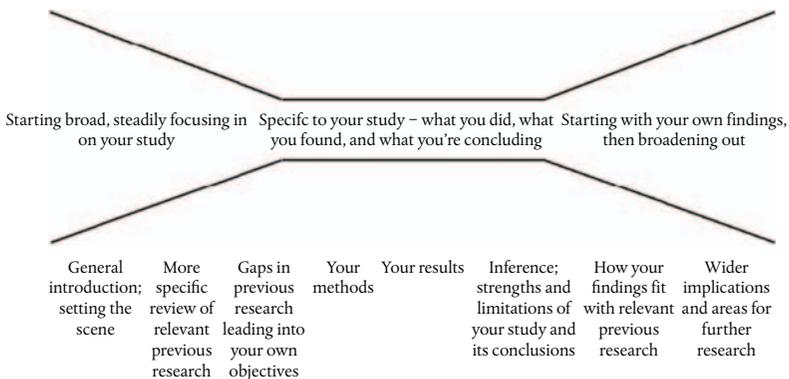
Fig. 13.3 A structure that you might find helpful for a PhD thesis. The challenge that a lot of students have with this sort of long document is not so much the part in the middle (describing what you did and what you found); hence, it's sometimes best to start writing the middle chapters first. The difficulty is in guiding a lengthy review of previous research so that it leads naturally towards the specific objectives for your own study. If each section of the introduction is not more relevant to your study than the one before it, then there might be some rearrangement needed—it will certainly be easier for an examiner to read if you structure it that way. The opposite is true (i.e. starting specific and becoming less so) when you discuss your findings in the final chapter(s), although this tends to be easier as you're on the home straight by then.

Author's own work.

Whether you write anything as lengthy as your thesis again depends on your research discipline and what's expected of you. In humanities, commercially published single-author books represent a marker of academic prowess to strive for; in my field, writing books tends to be discouraged (although not quite forbidden) by university employers—they don't count for much in the metrics and there are felt to be better uses of time. Which does raise the question of why on earth I decided to embark on this enterprise. Perhaps this is something for reflection later on; in the meantime, I'm three-quarters of the way through and did agree a submission deadline with my generous publisher. Deadlines are another feature of writing for publication, and academic life in general, but I think I'll leave that for another chapter.

14
Communication 2

Getting Known

Receiving an acceptance letter from a publisher is always a lovely thing. If nothing else, it's hard proof that at least someone out there thinks that your work is OK, even if it's no more than an editor and a couple of reviewers: these things matter. However, it won't surprise you to learn that it's not going to change the world on its own and that there's a whole lot more to do, particularly in these days of information overload. If you want to make a difference, you have to build up at least some sort of profile—not necessarily to push your own work, but more to highlight the questions you feel need answering, to get to know others with the same enthusiasms, and to be an ambassador for your field as a whole so that it gains (and maintains) visibility and reaches the people that it needs to reach. If you're working in research because you like your subject (and why else choose an often hard and underpaid career?) then all this is natural.

Accidental recognition

The history of research is littered with lucky escapes—people whose contributions might easily have been missed and whose work was picked up seemingly by chance. For example, Copernicus probably developed his theories about the Sun being at the centre of planetary orbits by around 1510 but didn't get around to publishing them until 1543. As discussed in Chapter 3, this may have been simply because he was busy with other things, or it might have been because he didn't feel his theories were complete enough. It was lucky that they were published at all, as he died in the same year as the publication, and even then, his influence was still left to chance and the efforts of those who followed him. It didn't help, of course, that his *De Revolutionibus* was condemned immediately by the Lutherans and would end up banned by the Catholics as well, once they'd given it some thought, so its longevity was always going to be in doubt. However, luckily the work found its way to the English astronomer Leonard Digges and his son Thomas, who published in

English rather than Latin, and so were better-read in their country.[1] Thomas Digges grew up in the household of John Dee, the philosopher and magician at the court of Elizabeth I, and he rose to political power and influence as a result, transmitting Copernican ideas through his own strangely titled book, *Prognostication Everlasting*, published in 1576. As well as this, the ideas found their way around academic sites in mainland Europe, enough to get to Kepler and Galileo, who took them on, developed them further, and attracted most of the hassle from the religious authorities for their pains. So, having the right readers did help matters back in the sixteenth century as much as it does in our own age of influencers.

Some researchers can gain a reputation for their work, despite being reluctant to publish, although it does rather stand in the way. Henry Cavendish (Chapter 10) was a very clever but painfully shy eighteenth-century scientist who also happened to be phenomenally wealthy, and so was able to build and staff his own laboratories and support his experiments without having to seek funding. He was known to his contemporaries for his chemistry, which he did publish, but he also carried out pioneering work in electromagnetism, which went unpromoted and unnoticed, ending up re-discovered and attributed to scientists from later generations. Cavendish was notoriously cautious about his findings and their dissemination, so was fortunate to gain the influence and recognition that he did, as there were plenty of other faster workers in the field. Darwin was another scientist rather prone to sitting nervously on his theories (evolution by natural selection famously). Caution, scepticism, and attention to detail are important qualities in researchers; however, an excess can paralyse productivity and impede output and influence.

The issue here is more about personal attribution than the science itself. The understanding of chemistry developed by Cavendish would have emerged sooner or later—it would just have others' names attached, as happened with the electromagnetic principles he discovered. And Alfred Wallace, or someone else, would have probably ended up producing a serviceable theory of natural selection if Darwin hadn't been spooked into action when he realized how close Wallace had come, and if he hadn't attracted acolytes like Thomas Henry Huxley ('Darwin's bulldog'), who assertively publicized and advocated his work after its publication.[2] As we've come across repeatedly, knowledge seems to get there in the end, and at least some fame

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[1] England at the time was under regimes where religious conflict tended to focus on burning people, rather than science books. Good for science: less good for people.

[2] Huxley's publicity machine certainly ensured that Darwin was remembered rather than Wallace—although to be fair, Wallace viewed Darwin's work as better than his own, and Darwin and Huxley did help to get Wallace a royal pension when he had fallen on hard times later in life. Wallace was also an ardent spiritualist which didn't help his scientific standing; neither did his campaign against the smallpox vaccine.

is accidental. Perhaps it doesn't matter in the scheme of things, although academic employers and funders do tend to feel otherwise and you're going to struggle in a research career if you generously donate all your discoveries to others.

One researcher lucky to be recognized at all, albeit posthumously, was Gregor Mendel, an Augustinian friar working in an abbey in what is now the Czech Republic. Mendel carried out ground-breaking experiments with pea plants in the 1850s and 1860s, demonstrating the dominant or recessive ways in which characteristics are inherited—a pattern that's still called 'Mendelian'. He died in 1884 and his work wasn't picked up until the twentieth century, although his 'unknown pioneer' story is sometimes a little over-mythologized. While he did fall back on the priesthood as a means of supporting himself financially, Mendel was otherwise a well-educated scientist who, as a young man, spent two years of intensive study in physics and mathematics at the University of Vienna. This gave him the know-how to apply experimental principles to plant breeding and biology—the research opportunity that was open to him at his abbey, and that he exploited while he was able. Also, crucially, he did publish and present his findings in 1865, and only stopped his research at that point because he was promoted to Abbot and was too busy with administrative matters to carry out experiments on the side. The actual publicity for Mendel's work came about because of its independent verification in 1900 nearly simultaneously by three researchers: Erich von Tschermak in Austria, Hugo de Vries in France, and Karl Correns in Germany. It's tempting to wonder whether Mendel would have been credited at all if only one successor had taken the field forward—it would have been very easy to fail to cite the earlier work and leave it to the historians to re-discover him long after the attribution had been claimed. In the event, for three people coming up with the same findings at the same time, there were clear advantages in all of them referring back to Gregor Mendel to avoid arguments over primacy; hence, he ends up being called the founder of modern genetics, although I don't get the impression that he would have minded too much either way.

From individuals to institutions

The environment for publication and wider dissemination of research has certainly changed markedly over the last 500 years. In a smaller community of people carrying out and/or reading about research, there was clearly the potential for output to be both ground-breaking and popular. For example, Hooke's *Micrographia*, published by the newly established Royal Society in

Fig. 14.1 Caroline Herschel in her late 90s, still active and engaged with the scientific community. As a young woman, she was desperate to escape a life of domestic drudgery in Hanover and was rescued by her brothers in England, assisting William with his music initially and then with his astronomy as this took off. Not long afterwards she was making her own discoveries and achieving recognition in her own right for these. She therefore represents one of the last women to chart a career in science without having to overcome hostile institutional prejudice—largely because eighteenth-century astronomy was still an amateur field, not requiring university education or society membership. The professionalization of science was probably inevitable, but it brought with it all the discriminatory trappings of wider society.

Caroline Herschel, unknown artist, after an engraving by Georg Heinrich Busse, Hanover, 1847. ETH Library. Public domain, via Wikimedia Commons. https://upload.wikimedia.org/wikipedia/commons/4/40/ETH-BIB-Herschel%2C_Caroline_%281750-1848%29-Portrait-Portr_11026-092-SF.jpg

1665, was a rapid best-seller, with its presentation of a new world as seen through the microscope, helped by the author's high-quality illustrations. Over 150 years later, Charles Lyell was able to achieve the same level of impact with his *Principles of Geology* (published in three volumes from 1830–1833) and, indeed, was able to live off the royalties (a rarity nowadays with published research output). These were times when a researcher from any background stood at least some chance of recognition, assuming they could publish, as there was a relatively small community to reach. Hence William Herschel, trained as a musician and working as an organist in Bath, could

read up on astronomy, design and build a telescope in his back garden, discover Uranus in 1781, and be elected as a Fellow of the Royal Society the same year. Similarly, his sister Caroline (Figure 14.1) could learn the same trade, publish independently, receive a scientist's salary, and hold a government position, remaining widely respected and consulted up to her death in 1848 at the age of 97.

As research in the sciences accelerated, the number of researchers expanded and started to coalesce around institutions. Historically, healthy research fields do seem to need communities of like-minded people for collaboration and exchange of ideas, and the development of lines of communication within and between groupings was an obvious driver of progress. Also, the cutting edge of science became increasingly dependent on laboratories, equipment, and funding—less easy to achieve at home, even for wealthy men like Cavendish. While there were clear advantages in the shift from individuals to institutions (self-evident if you assume that research success has been governed by its own natural selection processes), this sort of coalescence does also tend to be accompanied by conservatism and exclusion.

Dalton and Davy—breaking into the system

As an example, John Dalton (Figure 14.2a), a weaver's son born in Cumberland in 1766, faced quite an uphill task to establish himself in research. Aside from his humble origins, his dissenting Quaker faith meant that he was barred from attending a conventional university at that time. Instead, supporting himself in northwest England as a teacher and then private tutor, he managed to build a local (and then national) reputation through lectures in Manchester on the properties of gases. He also published prolifically as he did so, including an influential textbook on chemistry. As a result of his work, he was elected as a Fellow of the Royal Society in 1822 and was honoured with a lavish funeral following his death in 1844.[3] His most important achievement was to bring together an atomic theory for chemistry, although this again took a lot of patience and hard work. He formulated his ideas around 1808 but they were a little too innovative at that point, taking several decades to be accepted by the establishment,[4] although at least he witnessed this within his lifetime.

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[3] Something he probably wouldn't have welcomed, as a man devoted to simplicity and modesty.
[4] No one could quite get their head around the implication that apparently solid materials consisted of a lot of empty space between atoms.

Fig. 14.2 John Dalton (left) and Humphry Davy (right) both came from humble origins—Dalton a weaver's son from Cumberland, Davy a woodcarver's son from Cornwall. Both achieved prominence and reputation in physics and chemistry through prodigious hard work, although very different in style. Dalton's progress was steady evolution as a respected teacher in Manchester and careful but opportunistic accumulation of research, building towards his atomic theory. Davy clearly excelled at networking and his charm and charisma made him a good fit for public lectures and experimentation at the new Royal Institution. Here, he was influential in supporting Dalton and helping gain him recognition, as well as discovering the young Michael Faraday. However, like Newton, his later career was more political than scientific, whereas Dalton and Faraday were more continuously productive. It does bring to mind the fabled race between the hare and the tortoise.

a: Engraving of a painting of John Dalton (1895). b: *Portrait of Sir Humphry Davy, 1st Baronet, FRS (1778–1829), chemist and physicist*. Source a: Frontispiece of *John Dalton and the Rise of Modern Chemistry* by Henry Roscoe (William Henry Worthington, engraver, and Joseph Allen, painter). Public domain, via Wikimedia Commons. https://commons.wikimedia.org/w/index.php?curid=3248482; Source b: Wellcome Collection. Attribution 4.0 International (CC BY 4.0). https://wellcomecollection.org/works/f48jat3q

Dalton therefore did manage to build reputation and influence but had to overcome considerable disadvantages. Although the world of researchers (and the world population in general) was still small in the early nineteenth century, it was beginning to create environments that were more difficult to break into than they had been previously. As research moved from an amateur to professional activity, it increasingly orientated itself around universities and other institutions. And the institutions, as Dalton knew and took for granted, were already set up with deliberate exclusivity—on grounds

of religious affiliation in his case,[5] but of course continuing to exclude or discriminate in relation to class, gender, and ethnicity over a much longer period after religion ceased to matter. Like a lot of fellow Quakers around that time,[6] it doesn't sound as if Dalton took the discrimination to heart; he simply applied himself to the research opportunities that were accessible to him and sought to gain recognition for his work. In this respect, his early output was scattergun (to say the least), although it does suggest that he was a born researcher. He began writing on current knowledge and discoveries for popular magazines as a young man while helping his brother to run a local school, and achieved his first publication at the age of 27 on meteorology.[7] Four years later he published a book on grammar. Taking advantage of his holidays in the Lake District, he provided the first estimates of the heights of the mountains (using a barometer), and around the same time wrote about the condition of colour blindness that affected both him and his brother. In the end, he took the ideas he was developing about vapour particles as part of his meteorological interests and started to apply them more widely to gases, resulting in the atomic theories for which he is now best known.[8]

Dalton also had help in the person of Humphry Davy (Figure 14.2b), who recommended him to the Royal Society and who himself (perhaps not coincidentally) had risen to prominence from even more humble origins. Born in Penzance, Cornwall, Davy was apprenticed to a local apothecary at the age of 16 when his father, a wood carver, died leaving the family in poverty. Having shown little promise at school, Davy taught himself French and discovered chemistry from a book of Lavoisier's. Helped along by a succession of well-connected people who happened to take holidays in Penzance, he landed an apprenticeship at the Bristol Pneumatic Institution, where he discovered the properties of nitrous oxide by self-experimentation (Chapter 10). This led to publicity and further helpful connections,[9] culminating in his 1801

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[5] Or lack of religious affiliation, as the philosopher David Hume (Chapter 5) had found.

[6] For example, eighteenth- and nineteenth-century Quaker industrialists such as Edward Pease, the 'father of the railway', or those who gave their names to companies in confectionary (Cadbury, Terry, Fry, Rowntree), banking (Lloyds, Barclays), and manufacturing (Bryant & May, Clarks).

[7] A lifelong interest—he kept a diary of the weather from the age of 21 and continued it till the day before his death 57 years later.

[8] The Dalton is the standard unit of atomic mass to this day. Interestingly, 'Daltonism' is also sometimes used as a term for colour blindness, so his early work did bear fruit in the end. His data remained the standard estimates for the heights of mountains in the Lake District until Ordnance Survey maps started being produced in the 1860s.

[9] Not all his acquaintances were scientific. Samuel Taylor Coleridge was an enthusiast for nitrous oxide inhalation (of course!), and William Wordsworth asked Davy to proofread the *Lyrical Ballads* that he and Coleridge had composed—possibly an unwise choice, as that edition ended up notoriously full of errors.

appointment to the newly established Royal Institution in London as a lecturer and laboratory director. It was here that Davy truly made his name as an attractive and charismatic speaker, as well as an experimental researcher in electricity and chemistry, discovering sodium, potassium, and chlorine among his other achievements. He was an important ambassador for his subject (e.g. on its applications in agriculture) and was influential in helping others' careers, Dalton's included. However, Davy was not the most careful or methodical of scientists (as was fairly clear from his gas inhalations discussed in Chapter 10), and his later career was probably helped because he took as his assistant the much more rigorous Michael Faraday—unknown at the time but destined to succeed and eclipse him as a scientist (Chapter 16).

The careers of Dalton and Davy reflect a time when professionalism was taking over for researchers in the sciences and illustrate the different ways in which ideas can gain publicity and traction. Both wrote and lectured, although Dalton's progress seems to be more one of dogged, more extensively published progress, whereas Davy clearly had a talent for networking and public display, rather similar to the approach taken by the electrical engineer and inventor Nikola Tesla in the early twentieth-century US. Probably reflecting this, Dalton pursued research throughout his life and remained widely respected. Davy, on the other hand, definitely peaked; he was awarded a baronetcy, but his later years were marred by a declining reputation following a failed attempt to protect the hulls of Royal Navy ships electrochemically and a rather unsatisfactory tenure as president of the increasingly fractured Royal Society. Both men suffered strokes before they died. After his stroke, Dalton carried on experimenting and keeping up his meteorological recordings. Davy, on the other hand, wrote a rather odd (although well-received at the time) mixture of prose and poetical musings. After he died, Dalton lay in state in Manchester Town Hall with 40,000 people filing past his coffin, whereas Davy had a much quieter funeral in Geneva.

Other forms of writing—literature reviews

So, achieving recognition does involve publication, but public presentation and networking are also important. Taking the writing first—how do you build on your published research output? To begin with, there are generally opportunities to write more broadly—to communicate more than a single study's findings, to delve deeper into the topic and the ideas that need taking forward, or perhaps to provide a history or snapshot of a whole field of research. This needs to be considered carefully, because there's clearly a

risk of devoting a lot of time and effort to writing about research and never actually doing any. On the other hand, if well directed, writing of this sort can help promote a field of enquiry and improve the visibility of discoveries as they emerge, as well as help synthesize individual findings into the communication of more robust truths.

As with all writing, it helps to be clear at the outset what it is you're wanting to communicate and consider carefully what opportunities there are for publishing it. In my own field of health research, for example, the academic journals tend to distinguish between 'original research' (i.e. a research paper describing a single study and its findings; the sort we considered in Chapter 13), literature reviews, and opinion pieces. Literature reviews bring together multiple studies to draw conclusions from pooled findings—that is, a vehicle for the consensus process and Bradford Hill's 'verdict of causation' (Chapter 8). Those reviews that call themselves 'systematic' aim for as complete as possible a coverage of previous investigations and are better considered as original research, in that they have objectives, methods (the approach taken to identifying and collating relevant studies), results (the actual collation and synthesis of individual findings), and conclusions. However, some journals also accommodate what are sometimes called 'narrative' reviews, where the author does not make any claim to comprehensiveness or objectivity in their choice of material. These might, for example, be assembling literature to provide readers with a summary and digest of recent findings on a particular topic, or of methodological approaches in a field. They tend to be quite lengthy compared to original research papers and might have reference lists running to 100 citations or beyond.[10]

A narrative review might be published as an article in a journal, following peer review and editor approval, but the same sort of material might alternatively be contributed as a chapter in a commissioned multi-author book. The value placed on this format depends on the field of research. In the sciences, books risk being already out of date before they're published, so it's worth considering at the outset whether this the right vehicle for dissemination. However, even the faster-moving fields of research do tend to involve underlying truths that emerge more slowly and benefit from the level of consideration that can be devoted to them in a five- to ten-thousand-word chapter, particularly if the book as a whole is edited so that chapters are complementary and allow for broader themes and connections to be developed. Ideally, there will be some discussion between editor(s) and publishers about this in

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[10] I guess an analogy in humanities might be the editing of an anthology, or the curating of a gallery exhibition—both also opportunities to bring together source material in order to convey some broader principle or underlying thread.

the commissioning process, and some consideration given to potential readers and their interests. As well as conventionally published books, this sort of long-form writing might also be required for more focused work, including monograph series or published reports with a particular agency in mind—for example, commissioned reviews for governments or charities. As discussed in Chapter 13, clarity of writing and a coherent structure are the most important elements to think about. Again, it's about trying to put yourself in the position of your lucky readers in order to help them through the text, and to ensure that the points you want to make don't get lost along the way.

Other forms of writing—shorter pieces

In the other direction entirely, there are important (and probably growing) opportunities for communication through short-form writing. This is clearly an area that has been transformed by the Internet, although I don't think the principles have changed very much. In my own field, the pre-Internet publication opportunities lay particularly in 'editorials'—brief, focused pieces (usually considerably fewer than 1000 words with limits on references) providing an opportunity to say something that might be of interest to readers. In medical journals, where they're still widely available, these are probably the articles that stand most chance of being read in their entirety, whereas research papers tend to be scanned at the level of the abstract (summary) and only read in depth by those who are actively investigating in that field or considering application in clinical practice. So, composing editorials or equivalent opinion pieces is well worth bearing in mind in terms of communication and 'getting out there', even though they're currently given little value in most university metrics. Sometimes they're free form in remit, just saying something of interest; sometimes they're commissioned to accompany a research article that the editor wants to be highlighted, and so have to both consider the source article itself and think through its wider implications. Finally, at the most extreme end there are opportunities to submit letters for correspondence sections, which are shorter still (maybe 100–200 words if you're lucky).

If a structure is important in keeping a 100,000-word thesis or book together, it's just as important at the small scale. In general, there simply isn't the space to make more than a few points, or perhaps just a single one, so sentences and paragraphs have to be tightly organized in order to construct the argument and take the reader along with you. I'm sure there are different approaches, but personally I would go for successive drafting with proper rest

breaks in between—overnight, if possible. It tends to be easier to write long to begin with (e.g. the word count limit plus 50–100%) and then cut back in later drafts. If you've given it the overnight break, it's easier to see the redundant material, such as the sentence that isn't really relevant enough or just repeats something you've said already in the previous paragraph. The shorter the piece, the more ruthless the editing.

Discovering your inner journalist

Social media and other online platforms have clearly expanded the scope substantially for wider, more informal communications about research without the constraints of the publication process (submissions, rejections, revisions, formatting, proofreading, etc.) and clearly with the advantages of immediacy. Writing blogs to accompany research output has become routine nowadays, as has apologizing to university communications colleagues if you don't happen to subscribe to this or that social media platform.[11] I don't think that there's sufficient perspective yet on the digital age to judge whether particular communication strategies are helpful in achieving impact for research, or whether it's all just a waste of time.[12] However, the principles for writing haven't really changed very much. If you're pitching an opinion piece to a journal or if you're composing the same thing for a blog, you still need to have some sense of your readership, some purpose in writing, and some structure to make sure that your prose is as readable as possible and gets its point across. Practice is probably the most important factor—the more you try writing beyond pure research output, the better you are likely to become at it. In this respect, it probably helps if your writing isn't confined to self-published blogs and if you're actually having to interest and enthuse an editor. It's a challenge I would recommend, but each to their own . . .

There's an overlap here, of course, between research and journalism, and probably a rather ill-defined distinction between the two. At some point, publishing articles about research for researchers drifts into publishing articles about research for a wider readership, and there are of course junior and senior academics from both the sciences and humanities who have crossed over entirely into mainstream media. It has probably always been the case. Dalton began writing for the general public as a journalist of sorts before he

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[11] A time management issue really; how on earth does anyone get any work done if they're trying to keep track of all this stuff flying around?

[12] Just as it doesn't seem clear whether the platforms are really any good for advertising more generally, despite the money that's poured in that direction.

managed to get a foot in the academic door. Davy, once he got his break at the Royal Institution, combined celebrity and showmanship with scientific enquiry of sorts—not the most rigorous technique, perhaps, but enough to discover a few new elements, which is more than most of us might dream of. And the journalistic side is as important as the academic: those who can communicate their field sensibly to a wider public may have more impact than any number of pure researchers. However, it's an uneasy relationship. On the one hand, researchers ought to make good journalists because of the practice they have in writing for a less-than-captive audience. On the other hand, researchers do tend to get used to writing for other researchers. A good journalist really needs to be able to write fast to order and the process of communicating complexities has to involve some simplification, which the researcher might view as corner cutting. Hence there's a reputation (often, but not always, deserved) of academic writing as obsessive and precise to the point of being desiccated and unreadable.[13] I suspect a lot of academics would like to have higher media profiles (we're all human and everyone likes an audience) and some of the carping about journalistic over-simplification probably does arise from jealousy. However, it's also reasonable to say that some erstwhile academics do drift to journalism because they're not particularly good researchers. As I said, it's not always an easy relationship.

Visualizing truth

Communicating complexity has been a challenge throughout modern research and probably increasingly so as the more straight-forward questions are answered, and as the cutting edge moves ever further beyond what an educated member of the public might reasonably be able to understand. Visualization can therefore be an important part of getting the message across. Sometimes this is literal—John Snow's map of the 1854 cholera outbreak in London, for example, or the pioneering X-ray crystallography work by Dorothy Hodgkin (Figure 14.3), mapping the three-dimensional structure of penicillin, insulin, and other molecules. Sometimes the visualization is more conceptual—the lines of electromagnetic forces conveyed by James Clerk Maxwell, the now-coexistent wave and particle models for light, or the theory of one-dimensional vibrating strings in quantum mechanics.[14] Whether

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[13] For example, see Edward Casaubon in George Eliot's *Middlemarch* (Chapter 3).
[14] And, once again, there's nothing new in this—Plato's famous mind-picture of projected shadows on a cave wall viewed by chained prisoners was just a way to visualize the idea of higher levels of reality.

Fig. 14.3 Dorothy Hodgkin, a leader in X-ray crystallography, looking happy about her Nobel Prize win in 1964. It's tempting to link her flair for visualization, underpinning her pioneering research into molecular structures, to the copies she made of mosaics as a child assisting her parents during their archaeological work in the Middle East. She progressed towards her research career by being an academic high-flyer at school, helped by a supportive, encouraging family and what was, by the 1920s, a mature Oxbridge college system for women. Despite her politics, which were considerably left of centre, her portrait hung in the Downing Street office of Margaret Thatcher, whom she had taught at Oxford in the 1940s.

Dorothy Mary Hodgkin. Unattributed photograph from Les Prix Nobel 1964. http://www.nobelprize.org/nobel_prizes/chemistry/laureates/1964/

you're communicating your own findings in a research report or summarizing a wider field of knowledge in a narrative review, it's helpful to consider the scope for visuals and, if used, what design (a graph, an image, etc.) will most clearly reach your readers. It can make a sizeable difference in impact. However, do bear in mind that less is generally more—it's better to focus your efforts on a single image related to the main message than to attempt to visualize all your findings and end up obscuring what you want to say.

Conferences and wider academic networking

As mentioned, although research communication does tend to begin with writing, it's important not to neglect the opportunities offered by public

communication and networking. One of the things I definitely didn't anticipate when I began a research career was the amount of travelling that would be involved. Some of this was a product of the type of research I carried out for a while, helping colleagues get projects underway in countries without much of a pre-existing research infrastructure. However, a lot of the travel has been related to academic conferences. Around the time I was writing this book, the COVID-19 pandemic had resulted in a near-complete pause in this activity across all research fields and it's one of those aspects of professional life where no one's entirely sure what we'll return to. Personally, I think at least a modification of the old-style conference will end up being regained. Virtual, online-only conferences do provide novel and exciting opportunities to democratize research—to involve and inform much wider audiences who would not normally be able to afford the travel and accommodation costs of attendance at a face-to-face event. And that's definitely a good thing. However, the interactions have been lost—the chance to meet like-minded colleagues, to exchange ideas, to set up collaborations. This is probably manageable for senior academics who have built up enough connections that can be maintained virtually (at least for the moment); however, it's a major problem for researchers in early stages of their careers who need to get established. I'm going to continue the chapter, assuming that face-to-face networking will continue in future, but with the caveat that we really don't know.

Of course, it's worth acknowledging at this point that a whole lot of researchers travelling around the world to conferences necessarily involves a whole lot of air miles and a whole lot of carbon. So, as with many areas of life, these pandemic times may be a good opportunity to re-think lifestyles and switch to practices that are more environmentally friendly. My personal view is that the benefits to be accrued from a cohesive and interacting research community outweigh the air miles, but I accept that others may disagree. Either way, there's certainly an opportunity for significant change, without committing to a complete cessation of travel. For example, having established networks and collaborations through face-to-face events, there's a lot more scope nowadays to maintain these virtually. Also, at the more commercial end of research, I do hope that a moratorium can be called on the old practice of flying in for an industry-hosted board meeting at some soulless airport hotel, then flying home the same day; in my limited experience, these were invariably exhausting and depressing affairs and hopefully redundant now that videoconferencing facilities have advanced.

Academic networking events are many and varied but generally focus on accommodating presentations of new research and the wider opportunities for discussion and development of relationships. Although it's difficult to

conclude what's cause or effect, acceleration in knowledge generation has tended to accompany increased communication, and a key development of twentieth-century (compared to nineteenth-century) research was the continuation of an evolution from individuals working alone, to research groups and institutions, to international networks and between-institution competition and/or collaboration. One example of the beginnings of this transition (see also Chapter 6) was David Hilbert proposing his influential 23 unsolved problems facing mathematics at the 1900 International Congress of Mathematicians, held in Paris. This particular conference series was created as a result of ideas and conversations in the 1890s and had started off with its first meeting in Zürich only three years earlier. It later settled down to a four-year cycle up to its last physical meeting in Rio de Janeiro in 2018, changing to a completely virtual event with the 2022 congress.

Research conferences come in a range of sizes, and it's worth trying to experience a selection before you settle down to those you find most valuable. At one extreme there are the huge science congresses held in dedicated locations the size of aircraft hangars with attendances in the thousands. They're strange places—rather intimidating in their scale and in the sense of complete anonymity and otherworldliness. So, they take a bit of getting used to, although do tend to host the sessions where cutting-edge findings receive their first airing. On the other hand, and if your field accommodates them, there are sometimes more satisfying events with, say, a few hundred delegates, often focusing on a relatively circumscribed discipline or topic, where you have a much higher chance of your presentation being adequately attended and of meeting and making connections with junior and senior international colleagues.[15] And you may come across even more focused smaller meetings, although possibly with less comprehensive coverage—it does depend on the size of the research field in which you find yourself working.

Oral and poster presentations

Although you might be just attending a conference to get a sense of current research activity and to familiarize yourself with the discipline (and you might be lucky enough to have available funding for this), more usually you'll be there to share your own findings. Most of the time you'll be pitching for an opportunity to share via an oral presentation, so public speaking is an

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[15] Or, indeed, colleagues from your own institution. A lot of people whose offices are in neighbouring buildings or different floors acknowledge that they only properly get to talk to each other at a conference halfway around the world or waiting in an airport departure lounge.

unavoidable part of academic life along with its other accompanying challenges (writing for publication, applying for funding, coping with rejections, all the rest of it) and you'll need to find a way to deal with difficulties if you encounter them.

Everyone's unique and will differ in their weak spots. Personally, I've never found informal small-group sessions easy,[16] but I've never had any particular problem with large lecture theatres (which surprised me the first time this was required, many years ago now—I had anticipated a whole lot of stage fright, which just never happened).[17] You'll discover your comfort and discomfort zones soon enough, and will no doubt orientate your activity in the comfort direction. However, there are limits and you're likely to need to deal with scenarios you find more difficult. For example, I avoid invitations to run workshops if I can get away with it, and I've given up any expectation that I'll get over whatever psychological hurdle is causing me the problem, but sometimes these sessions are just inevitable and have to be endured. Likewise, if you get nervous speaking to large audiences, you're likely to need to face up to it somehow.

At a basic level, the solution tends to lie with plenty of practice and repetition. You also need to accept that you're going to make mistakes along the way and try to view these in a positive light, as learning opportunities, rather than getting discouraged. And don't ever assume that anyone else has had an easy ride—even the really slick operators will have crashed and burned at some point. There are many resources available to help improve lecturing skills, with peer feedback often emphasized as particularly important. This doesn't have to be formal; it might just involve asking a supportive colleague to give you some pointers when they've heard you speak. It's helpful generally to have a self-critical mindset (i.e. be prepared to accept that improvement is always possible) although do keep the self-criticism within limits (i.e. be willing to accept that you might, on occasions, have done something well). Also, you can sometimes get direct feedback from the attention or boredom of the audience, depending on your venue.[18] Finally, when you find yourself watching one of these sessions, try to switch to a position of objectivity from time to time, reflect on the speakers you admire or not, and work out why it is

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[16] Particularly sessions referred to as 'workshops', whatever that means.

[17] Incidentally, I'm not implying here that I'm any good as a lecturer (that's for others to judge)—it's just that I don't get nerves, which is something entirely different. I've seen nervous lecturers who are very good, and confident lecturers who are not.

[18] However, do be careful about over-interpreting or personalizing simple drowsiness if you happen to be presenting directly after a lunch break or towards the end of the day. And don't get disheartened if you're allocated a 'graveyard slot'—we've all been there.

that their talks are engaging you or boring you or annoying you (and you can probably learn as much from the bad ones as the good).

The format and style of research presentations varies enormously between disciplines, as you'll find out if you ever attend events that are designed to be cross-disciplinary. So, you definitely need exposure to talks from senior colleagues and develop a sense of what works and what doesn't.[19] Science presentations tend to orientate around slide shows on the standard platforms available, whereas humanities presentations often involve pre-prepared prose read out to the audience without visuals. Within slide-based presentations, the expectation might be for bells-and-whistles visualization (animations, embedded videos, etc.), or it might be for the simplicity of a few key messages on a plain background. My colleagues in ophthalmology routinely present on independent double screens (one for each eye), whereas I've never had to do this myself and wouldn't want to risk it. For my specialty, the submission for a conference requires nothing more than an abstract (and research that contains findings but is not necessarily finalized). However, my colleagues in computer science will expect to submit a fully prepared research paper for publication in the conference proceedings, which may rate as equivalent or superior to an academic journal publication.

Beyond paying attention to the preferred format, so that you come across as 'belonging' to your field, preparing a presentation involves similar principles to those we've discussed for academic writing. Knowing your audience is obviously key to the pitching element, the language that you'll use (on slides as well as verbally), the images that you'll show, and the way in which you'll convey your message. For example, if members of your audience are likely to come from different research disciplines, you may need to incorporate some orientation to your own field;[20] otherwise, they'll disengage at the first technicality—and there's nothing more irritating than listening to a speaker who clearly doesn't care who they're talking to. Structure is just as important, if not more so, in an oral presentation as in writing, because your audience needs to be taken through the arguments you're presenting. And instead of word count limitations, you need to consider timing very carefully—overrunning is a discourtesy (and generally felt that way) to whoever's chairing your session, as well as to fellow speakers (whose allotted time you are rudely encroaching on), and to the audience (who will start worrying about the coffee break). If you're using relatively simple slides without animations or

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[19] Although it's probably best to pay closest attention to presentations from colleagues a few years ahead of you in experience, rather than those from senior professors—who are often following a format that was fashionable in their youth, but which may now be a little tired.

[20] Without coming across as patronizing—that's the challenge.

videos, a good rule of thumb is to allocate one per minute and aim to finish a few minutes short of your total allowance. Personally, I find slide presentations more engaging when the speaker isn't reading directly off the slide, so favour brevity and limited visuals, but I'm aware that preferences and expectations vary. As a science researcher, I rely quite heavily on getting a narrative in place in advance through the slideshow; I assume that speakers from other disciplines who read out research papers verbatim or who talk off the cuff will place a similar reliance on structure in their prose or lecture notes. With speaking, as with writing, developing an argument is best done by taking your audience gently through a coherent series of steps; otherwise, their attention is liable to drift.

A poster is often an alternative to an oral presentation. Many conferences accommodate both and for some, the poster is actually preferable—for example, in the aircraft hangar events, a well-positioned poster may attract more attention and generate more impact and valuable conversations than an oral presentation in a poorly attended parallel session at some remote corner of the venue. Poster formats and expectations also vary between disciplines, so again you should take the opportunity to survey the field and work out what you think looks best. Obvious considerations for poster presentations include making absolutely certain in advance whether the display boards are going to be portrait or landscape in orientation (there's always someone who doesn't get the message) and thinking about the visual appeal of your poster from about a metre's distance. You're looking for a strong visual to attract the passer-by, a title that's legible at maybe two or three metres, and a font size for the rest of your text that doesn't require someone to bring along a magnifying glass. Finally, do make sure to turn up to talk about your poster at the time allotted, and think about taking along some handouts and business cards, as these are often the times when you get to meet like-minded colleagues—the networking function that, if anything does, will ultimately resurrect the physical conference after its recent pause.

Public engagement and more journalism

Just as writing about research can drift from academia into journalism, presentations to researchers merge into oral communications to wider audiences. Pitching for interviews with newspapers, radio, and sometimes television is increasingly part of the process of maximizing impact for research output, and most institutions have a communications officer or department to facilitate this. It's therefore well worth looking for training opportunities

on media engagement, or at least informal advice, if you think that your findings might be taking you into that territory. The main challenge for researchers here is the different audience (the general public, rather than other researchers) and therefore the need for focused, digestible messages. 'Lay' communication of this sort tends not to be easy for academics, who are predisposed to thinking in complexities and technicalities, but practice and feedback can help a lot along the way. Unless you're particularly confident in your ability to speak off the cuff and keep to the point, it's wise to have a few short statements prepared in advance. You can call them soundbites if you want to feel uneasy or disparaging about the experience, but if you can keep your integrity about you and don't start overplaying your findings, media (and wider public) engagement is actually nothing more than a different way of conveying the truth.

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Money

Research doesn't come for free; it never has. To begin with, researchers need to earn a living somehow. Then there are the additional costs of research projects themselves. If the acceleration in knowledge generation over the past 500 years can partly be attributed to advances in communication, it has similarly been accompanied by an expansion in the number of people engaged in research (above and beyond the expansion in their nations' populations). This in turn has been accompanied by an expansion in the professionalization of research disciplines and in the funds and structures available to support them. I think it's fair to say that there hasn't been a great deal of planning in this process. Although particular funding streams and structures have been set up with strategic objectives in mind, for the most part these have been short term and subject to variations in leadership and priorities. The job of the researcher, as well as actually generating and communicating new knowledge, has therefore always involved a certain amount of scrabbling around trying to make ends meet—tiresome, but the way life is nowadays, particularly in the sciences.

Early costs—dedicated time

In the early days, 'pure' research (i.e. knowledge generation for its own sake) didn't come with particularly high costs—or, looked at another way, was constrained to questions and topics that didn't require high costs. So, you could feasibly live the academic life if you had at least some independent income that freed you from working every waking hour to keep food on the table and enemies away from the door. Pure research (Chapter 2), of course, built on a background of applied research and discoveries since prehistory (fire, metallurgy, agriculture, etc.). These, we have to assume, came from small societies working full-time but incorporating what would now be called R&D into their activities—that is, experimenting with different metal alloys or different agricultural practices, looking for ways to improve, and communicating any knowledge gained to the family or community, if not more widely. At some point there would have been individual members of

primitive communities who were given time off from normal activities to increase knowledge in this way, and who were recognized and supported by the rest of their society as prototype academics or inventors. We can't be sure of this until written records begin, although there's an interesting theory about advances in 'civilized behaviour' coinciding with evidence of a decline in circulating testosterone levels in our ancestors from around 65,000 years ago. So, it's possible that reduced aggression and competition allowed more cooperative societies to emerge and advance in knowledge, presumably involving some level of specialization in tasks. Whatever the process, it's difficult to imagine a structure like Stonehenge being assembled without someone being put in charge of logistics and absolved from the job of actually hauling the stones from their origin,[1] even if this took place over several centuries.

Early research was therefore made possible (or at least accelerated) by the division of labour that accompanied the transition from hunter gatherer to settled agrarian societies. It's therefore hard to escape the fact that researchers are only able to make a living when there are other people doing the harder and less-rewarding jobs, so what we do is probably more reliant on inbuilt societal inequality than many left-leaning academics might like to acknowledge. Certainly, philosophy and the beginnings of the pure end of research in ancient Athens might have coincided with the emergence of what we now call democracy, but definitely relied on a whole lot of disenfranchised slave labour (and disenfranchised women) to support the few privileged men who had the leisure time to sit around, think deeply, and discuss their ideas.

Early costs—academic centres

As mentioned in Chapter 2, research activity does tend to rely on communities of like-minded thinkers to move a field forward—perhaps because of the accelerating nature of an academically stimulating environment, or perhaps because there's a higher chance for discoveries to reach their target audience and survive in written form for posterity. Therefore, beyond researchers having something to live on, the main other early resource requirement was a meeting place. No one is sure how the Athenian Academy came about, but the myth is that an old Greek king, Academus, averted a Spartan attack by

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[1] 150 miles or so for the two-tonne stones, 15 miles or so for the 25-tonne stones—both formidable enough challenges.

revealing where their Queen Helen was hidden.[2] The land that he owned, a little north of Athens, was named after him and became a revered centre for intellectual and sporting activity. Several centuries later, it seems that the philosopher Plato inherited property here and founded the Platonic Academy in the fourth century BCE, presumably influenced by Socrates and wanting to develop his line of thinking. The school operated for around 300 years until it was destroyed by the Romans, but its influence continued through a revived 'Neoplatonic Academy' until the sixth century CE and in the more dispersed centres of learning across the Byzantine and other Middle Eastern empires.

Although the Platonic Academy was set up with its founder's private income, there wasn't a stable enough aristocracy in those days to sustain that funding model, so the successor institutions were primarily dependent on rulers and their interests. The Library of Alexandria, for example, was founded and developed under the Ptolemies, the dynasty ruling Egypt in the last few centuries BCE, who were keen to show off their prestige through an assertive and well-funded programme of manuscript purchasing.[3] The manuscripts and the library attracted scholars, and Alexandria became an academic hub. However, the obvious problem with this model was the dependence on rulers to continue the funding stream. This was complicated further by power structures and the never-helpful blurring of lines between academia and politics (see Chapter 16). The inevitable happened at Alexandria in 145 BCE when Aristarchus, the sixth head librarian, supported the wrong side of a dynastic struggle, which resulted in the aggrieved successor sending him and other scholars into exile. Once the Roman Empire took over and their interest in Egypt (and dependence on Egyptian grain) declined, so did Alexandria, and scholarship moved on elsewhere.[4]

In what became a long-standing tradition, or at least necessity, research activity followed the money and appeared again wherever support and sufficient intellectual freedom was available, regardless of the regime supporting it. For example, an academic community grew up in Baghdad in the ninth century,[5] supported and funded by the Caliphs of the Abbasid Empire until its destruction by a Mongol invasion in 1258. Or there's the example

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[2] An abduction this time by King Theseus (of minotaur fame) before she was later abducted by Paris, starting the Trojan War, and prefigured by Zeus's rape of her mother while disguised as a swan. Rather grim, really, and does rather make you wonder why Ancient Greek culture has so often been held up as a touchstone for Western European civilization.

[3] Particularly trawling Greek marketplaces, resulting in a sizeable export of written work.

[4] It's worth noting that academic traditions did take a long time to fade. Hypatia, the pioneering mathematician mentioned in Chapter 2 was leading a school at Alexandria and following in its tradition of pre-eminence until her murder in 415 CE.

[5] Commonly called the House of Wisdom, although it's unclear whether a specific academy existed of that nature, and little survived after Baghdad fell.

of research communities and their manuscripts drifting away from Constantinople as it became increasingly obvious that the Byzantine Empire's days were numbered, settling in Italian city states, and kick-starting the Western European Renaissance. Or, more recently, there's the way in which British and American research communities were boosted by academics of Jewish origin, Einstein included, moving away from persecution in Nazi Germany. Or, in the opposite direction, Ernest Everett Just (Figure 15.1), a highly gifted Dartmouth graduate and leading biologist, affiliating himself with the German Kaiser Wilhelm Institute in 1930 in order to advance his career because of the disadvantages he faced in the US as an African-American researcher.

Fig. 15.1 Sometimes researchers have to follow opportunities wherever they present themselves. Ernest Everett Just was a marine biologist and embryologist with a growing reputation, and one of the few non-White researchers to receive a doctoral degree from a major US university (University of Chicago, 1916). However, his skin colour still prevented him from getting an adequate post-doctoral appointment. Therefore, at the same time of the mass exodus of scientists in the other direction, Just began working in Berlin and Milan, moving to Paris when the Nazis began taking control in Germany. His work flourished in pre-War Europe, where he felt more encouraged, and one of his particular contributions was to study cells in their natural setting, anticipating the live cell imaging now possible today. He was evacuated back to the US in 1940 but died from cancer not long afterwards.

Photograph of Ernest Everett Just by unknown artist (unknown date). Public domain image. https:// commons.wikimedia.org/w/index.php?curid=191183

Later costs—employment

Once printing got underway and it was possible to communicate more widely, opportunities opened up for individual researchers to move their field forward while supporting themselves through other employment or via specific patronage. Early universities at this stage in Western Europe were emerging as predominantly religious institutions focusing on learning, rather than research, and so were just one of many employment options. Astronomy required little more than observation facilities and telescopes (when they'd been invented), so Copernicus was able to develop his heliocentric theories while working as an administrator and physician in Poland–Lithuania. Tycho Brahe developed detailed astronomical charts with his sister Sophia under the patronage of the king of Denmark and an island given to him to work on,[6] while Johannes Kepler, Tycho's pupil who provided the mathematical theory behind many of the observations, had to eke out a living as a mathematics teacher and astrologer on the side, picking up court patronage intermittently from the unstable European kingdoms of the time. By the time Newton developed Kepler's theories into his gravitational models 80 years later, university positions were opening up, through which to earn a living—at Cambridge in Newton's case. He was fortunate in acquiring his position, as he came from a farming family of quite modest means—a background he was desperate to escape. The eighteenth century then saw universities start to play more assertive roles—for example, when Adam Smith, the eminent economist and professor at Glasgow at the time, provided a workshop for James Watt, a jobbing engineer with no university background, enabling him to revolutionize the steam engine and provide a major impetus for the British Industrial Revolution.

The gentleman scientist (male and female)

The combination of both a secure personal fortune and the right talent and mindset were clearly an advantage in those early days, as they freed researchers from the vagaries of funding and patronage. Boyle (Chapter 10) and Cavendish (Chapters 10 and 14) were both blessed with wealth and

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[6] Amongst other discoveries, Tycho plotted the course of a comet, showing that it passed through the planetary orbits, disproving the old ideas about crystal spheres surrounding the earth. However, he still proposed an Earth-centred model of the Universe, albeit one that allowed the outer planets to orbit around the Sun (which itself orbited the Earth).

intellectual ability, allowing them to carry out their work in chemistry and physics in the way that they pleased. These and other examples (Darwin in Britain, Lavoisier in France, Benjamin Franklin in the US, Alessandro Volta in Italy, Hans Christian Ørsted in Denmark) gave rise to the idea of the 'gentleman scientist', although it's worth bearing in mind that the opportunities were also open to those women who had independent incomes or support. Maria Sibylla Merian (Chapter 4) had sufficient freedom within and after her marriage to produce her seventeenth-century botanical and entomological descriptions, and to seek the training she needed in the graphical and academic skills (natural history, Latin) for effective communication. She published under her own name and was widely read and respected, even if Linnaeus failed to cite her. In France, Émilie du Châtelet (Chapter 5) was fortunate to be the daughter of a court administrator to King Louis XIV who was already well connected with contemporary writers and scientists. As well as access to this network, she received an extensive education and reached an understanding with her husband after she had given birth to two surviving children, allowing her to live separately,[7] resume studies, and develop her wide intellectual interests, including the experimental tests she carried out on Newton's theories of motion.

The beginnings of institutions

As research activity expanded and institutions developed, opportunities became less equal in situations where these institutions were gateways to recognition and communication. Margaret Cavendish, married to an English aristocrat, published early work on scientific theory and did manage to become the first female member of the Royal Society in 1667. However, she struggled with the male-dominated environment there, her wider writings (e.g. biographies) were considered eccentric, and she was consigned to obscurity until her rediscovery in the twentieth century. Requirements for, or expectations of, a university education developed as a key barrier for potential female researchers who weren't allowed to attend universities, as well as for academics like John Dalton from excluded religious groups (Chapter 14).[8] However, there were opportunities remaining in fields less affiliated with universities; these included astronomy, where innovative and acknowledged

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[7] And cohabit with Voltaire.
[8] Newton had a narrow escape as a follower of the Arian Christian sect, outside the Church of England; however, King Charles II, a science enthusiast, granted a dispensation for Newton's Professorship at Cambridge that excused the normal requirement for professors to take holy orders.

work could still be carried out by researchers such as Maria Margaretha Kirch in early eighteenth-century Germany and Caroline Herschel in early nineteenth-century Britain. Representation in most fields by the nineteenth century, however, depended on the long, hard slog to achieve equality of access to universities and other institutions—for example, by Marie-Sophie Germain and Sofja Wassiljewna Kowalewskaja in mathematics (Chapter 6), or Marie Curie in physics and chemistry.

University salaries—the options

Over the eighteenth to twentieth centuries, universities steadily grew in prominence as places to organize research activity, to provide a career structure for those carrying it out, and (importantly for this chapter) to provide focal points for its funding. For better or worse (and inequalities in university entry and appointments have clearly been a long-standing down-side), this is the system we've inherited and are currently working within, although it's worth bearing in mind that it wasn't always the case and needn't necessarily continue that way. The role of universities in supporting academic salaries has evolved differently between countries and academic traditions. At one extreme is the model of minimal structural support, where academics may be hosted and employed by a university but have to attract funding for all their salary and costs, essentially living from grant to grant. At the other extreme is the tradition of life-long, paid-up institutional positions. The first model does favour entrepreneurial activity and output, but the lack of longer-term security risks losing talent because of inevitable troughs in funding cycles; it is also likely to be shorter-term in its strategic goals. The second model is clearly more secure, allowing for longer-term development and strategy without needing to be constantly thinking about the next year's pay cheques. However, everyone has heard stories of unsackable professors who go to seed as soon as they're appointed, soaking up institutional resources that could be better directed. These sorts of jobs-for-life also tend to be surrounded by stories of favouritism and dubious internal fixes when they come to be handed on. My impression has been of a general shift towards a middle road, where universities are prepared to provide some salary underwriting and security, but equally keep an eye on productivity and have the ability to hire and fire according to strategic priorities and individuals' performance. How employers make these judgements is not always clear or predictable (however much they claim otherwise), so staying in employment does remain a preoccupation for most researchers.

New fields for research

The gradual professionalization and institutionalization of research over the last couple of centuries has also been important in determining the disciplines in which it is represented, particularly as the role of universities has diversified beyond simply providing higher education. Taking English as an example subject (and I believe the same process would apply to literary studies in any other language, albeit with different timelines), the emergence of a 'literature' in the eighteenth century was accompanied by an enthusiasm for studying it (both the emerging field and earlier prefiguring work—Milton, Shakespeare, Spenser, Chaucer, and the rest of them). There was nothing particularly new about this; literary criticism can be traced back to Plato writing about political rhetoric, Aristotle coining terms such as 'catharsis', the Natya Shastra Sanskrit texts on performance arts, or mediaeval Biblical scholarship. I'm not sure that all literary criticism would count as research, and I suspect the line is rather blurred. However, there are clearly elements that involve the generation of new knowledge. With English, an academic field started emerging in the nineteenth century—outside the universities to begin with, but then finally getting a toehold as a studied (and therefore researched) subject with Arthur Quiller-Couch's professorship at Cambridge in 1912[9] and George Stuart Gordon's at Oxford in 1922. The somewhat formidable husband and wife team of F. R. (Frank Raymond) and Q. D. (Queenie Dorothy) Leavis took the field forward after the First World War and popularized it intellectually into the 1930s with what I guess might be called a rationalist approach, if our research terminology can be applied—certainly strongly based around building up arguments and evidence, although fairly opinionated as a result of these arguments, and deeply unfashionable for later generations. More recent academic work in this discipline then diversified into 'isms'—that is, applications of particular philosophical (or other) frameworks to interpreting literature, taking it towards empiricism (testing these theories against evidence from source materials), as well as drawing on other disciplines such as history (the source materials) and even computer science (text analytics). So, at some point in the past, it would be hard to identify 'research' as a component of literary studies, but then, along the way, English emerged as an academic specialty with research posts, conferences, funding opportunities, and all the other trappings. It's hard to imagine this transition without universities as a sector to enable it, providing communities and a

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[9] He wasn't the first, but his predecessor had only lasted a year and was primarily a classics scholar, whereas Quiller-Couch was a productive novelist and literary critic; he might also have been the model for the character of Ratty in Kenneth Grahame's *The Wind in the Willows*.

career structure, and a means of attracting and administering funding. Again, perhaps this way of doing things won't last for ever, but it's the way life is now.

Other employers

It's worth noting at this point that of course there are other entities besides universities that accommodate researchers and administer funds. We discussed R&D in Chapter 12 and there are clearly any number of industries that host and fund research, although generally with applied commercial goals in mind, rather than knowledge generation for its own sake. Also, the career structure on offer in the commercial sector is likely to be corporate, rather than one oriented around research skills development, and there is also little ability or inclination to provide anything equivalent to doctoral training. So, industries do rely on the university system to some extent for researcher training, although that's not to say that some of them won't decide to become more autonomous in future. Beyond this, there are public sector bodies employing and supporting research—government departments and healthcare providers, for example—as well as larger charities and sometimes the research funding bodies themselves (e.g. the intramural programmes at the US National Institutes of Health) or independent endowed foundations like the Cavendish Laboratory (Figure 15.2). Finally, the university system itself is an ever-changing landscape with a mixture of private and public provision between and within countries and a vagueness about what constitutes a 'university' in the first place. A dividing line might be drawn between institutes able or not able to award a doctoral degree, but how central this will remain as a feature of a research career is anyone's guess.

Looking for funds

So, like it or not, the life of a researcher tends to be oriented around funding. At a basic level, a salary and contract are needed, and then there is the additional requirement of having somewhere to work (usually but not always accompanying the employment) as well as, of course, the costs of the research itself. Unless you're very lucky to land a secure salary and to be working in a field where you don't need to buy any equipment or fund any travel, the likelihood is that you'll be looking out for money for most of your research career—whether leading bids yourself or supporting the general effort by your colleagues and employers to attract resource. I don't personally see this
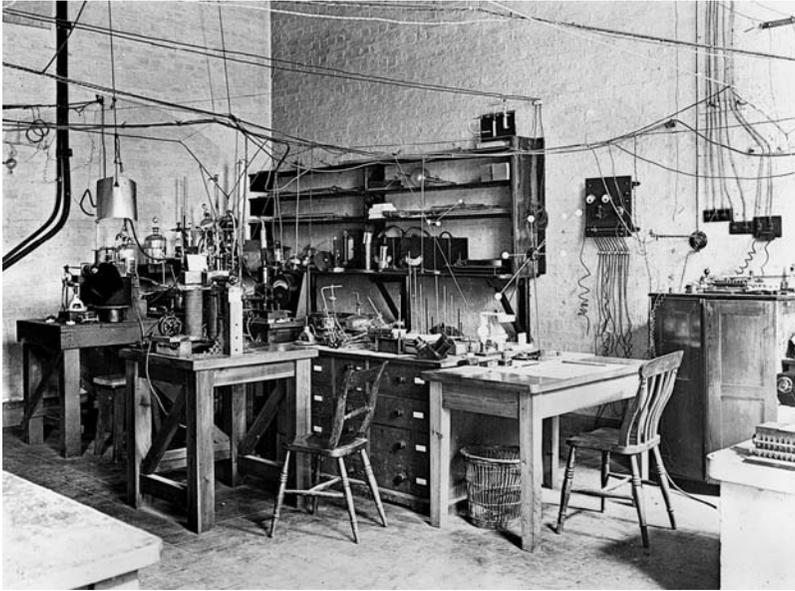
Fig. 15.2 Ernest Rutherford's set up at the Cavendish Laboratory founded in Cambridge in 1874. This was named after Henry Cavendish (Chapters 10, 14) and supported by a bequest from a distant and similarly wealthy relative (his first cousin's grandson, the 7th Duke of Devonshire). James Clerk Maxwell (Chapters 8, 12) was the founder, and the Laboratory was particularly famous for hosting the atomic physics research group under Rutherford, who took over as Director in 1919. Thirty Cavendish researchers have won Nobel Prizes.

Sir Ernest Rutherford's laboratory (1926). Science Museum London / Science and Society Picture Library, CC BY-SA 2.0 <https://creativecommons.org/licenses/by-sa/2.0>, via Wikimedia Commons. https://upload.wikimedia.org/wikipedia/commons/9/97/Sir_Ernest_Rutherfords_laboratory%2C_early_20th_century._%289660575343%29.jpg

as a problem—many jobs in other fields involve touting for business, so why should research be any different? It's just that it isn't always uppermost in people's minds (mine, for instance, back in the day) when they're embarking on a possible academic career, naively thinking that it's all about the research questions and that everything else will somehow sort itself out.

Funding sources are many and varied—within and between disciplines, within and between countries—so, it's not possible to provide detailed coverage for all circumstances. A key task at an early stage in your career is to get an idea of what's out there by watching colleagues in more senior positions and, if the opportunity arises, getting involved in application processes. As is probably the case in any business, those who are most successful in securing funding tend to have a mixed economy in their approaches—on the one

hand, developing expertise in mining particular productive recurring sources of funding, on the other hand, keeping an eye out for new and sometimes left-field opportunities. There's definitely a risk associated with long-running funding schemes, however successful you are with them, because funding agencies do have a habit of changing strategy (or simply running out of money) at short notice, so you need to make sure you have other options when a particular tap runs dry. There are often ample opportunities for junior researchers to develop entrepreneurial skills because, while applying for the longer-standing resources tends to be the province of more senior staff, finding small pots of support for this or that research activity (useful for the CV if you're successful) is better suited to younger team members who can keep an eye out for opportunities outside of, as well as within, established schemes.

Applying for a project grant—preparatory work

Although research funding structures vary considerably, the awards do tend to fall into broad categories. To begin with, there are project grants—that is, funds to support a particular, defined piece of work over a particular, defined period of time. When proposals are adjudicated, the funding panel tends to be focusing on certain features:

1. The overall idea (what it is that the project will deliver, why this is important).
2. The plan for delivery (the methodology proposed, any logistical challenges and how these will be addressed).
3. The proposing team (whether they have sufficient track record and expertise).
4. The hosting institution(s) (whether the facilities and wider environment are appropriate for the project).
5. The budget (whether this is realistic to support the study, i.e. not too low, and demonstrating value for money, i.e. not too high).

The application form will therefore cover these aspects and proposals will be judged and ranked accordingly. Particular research fields may have additional requirements in their funding applications. For example, in health research there are usually sections to complete on ethical issues (the external approval to be sought, whether the project presents any particularly challenging features), public involvement (how patients and the wider public have been and will be involved in directing the study), communication plans (how the

findings, if relevant for health care, will get to the people/agencies who need to know), and data management plans (goodness only knows—I've never met anyone who feels confident about this section, but there are generally two or three pages to be filled somehow).

So, how to write an application for a project grant? Well, clearly there are no magic answers, or we'd all be putting our feet up. To begin with, do read any blurb on the funding available and the application process, and if the funder offers any workshop or symposium about their call, do sign up and pay attention. Some funding is open to anyone with a good project, but a lot of calls have particular strategic objectives in mind, wanting to support research in particular fields, or for particular questions. Also, some funders will favour or restrict themselves to certain sorts of research and specifically exclude others. Although you might get lucky with submitting something slightly off target, your chances are going to be substantially diminished. And grant applications do take a lot of time and effort to prepare, so it's worth being quite focused on opportunities with some chance of success (I don't think any applications can be said to have a high chance in terms of strict probability). At this point, it's also important to get an idea of the level of funding available. Many funders will give an indication of the total pot allocated to a given call and how many applications they're interested in supporting. If they don't, and if the funding has been available before, try to find out about the size of studies that have been successful. Again, something relatively expensive might stand a chance if it's a particularly important study being proposed (and one that clearly can't be delivered more cheaply), but you'll have a much higher chance of success if you can fit it into a more expected budget. On the other hand, don't imagine that a study with costs that are much lower than expected will be at a significant advantage—the chances are that the project will look either under-resourced or not important enough if it's seen as unusually cheap, unless there are good reasons for this (for example, if you're applying for overseas funds and exchange rates are in your favour).

Applying for a project grant—coming up with the big idea

Having established the broad type of study the funder is interested in supporting, probably the trickiest element of the whole process is developing the central idea for your proposal. I think this difficulty is partly because it feels the wrong way around. All the core principles of research, as discussed in previous chapters, focus on a question to be answered—the acceptance

of ignorance, the initial observations, the theories derived from these, and the experiments to test these theories. Therefore, the natural process is for a researcher to have a question in mind that's been drawn from their experience and reading, and then to look around for resources to support a study that will answer that question. However, many funding calls already have in mind their broad questions or topics of interest and the job of someone looking for research funding is to think of a study that will fit in with the funding objective—which may not always be the study they would ideally like to set up. We discussed the same challenge with fitting ideas to data resources and 'solutions in search of a problem' towards the end of Chapter 12 and I think that split mindset is often required for financially supporting what we do—focused like good academics on the questions that need answering, while also keeping an eye out for opportunities like good inventors or entrepreneurs. Either way, I think this is why the central idea in funding proposals can be a tough one, particularly for more junior researchers who have had less time to immerse themselves in their field of interest, but it's just another one of those challenges to overcome. As well as experience and developing the right mindset, it's also worth creating and maintaining some flexibility in your ideas as you progress as a researcher, assembling a few questions and interests to draw on. My first supervisor talked about it in terms of pots on a stove, keeping a few on the back burner simmering away so that they're ready to be brought forward at an opportune moment.

The central idea is particularly important for the panels that decide on (or at least formally recommend) funding allocation. Panel members will generally be senior academics but not necessarily experts in every specific field; they will be faced with a shortlist of proposals that have all been judged as excellent by peer reviewers, but only a fraction of which can be supported with the money available. So, all the projects are methodologically sound and rated well by specialists, but panel members have to make a further choice somehow. This final ranking tends to end up hinging on what the studies will deliver and how important this knowledge generation is perceived to be by senior non-specialists, as well as possible considerations of 'strategic fit' with the funding call (and there will be funder representatives on the panel advising on this). Projects that are 'good quality but a bit dull' will inevitably be weeded out at this point, so your proposal does need to convey a strong sense of what's known, what's unknown, what the study will deliver, and why it matters. It also helps if this is expressed as clearly as possible, because a great idea buried in impenetrable text stands a sizeable chance of being overlooked.

Beyond formulating this central idea, the rest of the application doesn't need to be complex, and the preparation process is more of an endurance

test in writing and negotiation than anything intellectually taxing. You've got to have a study design that's watertight and reassure the assessors that there aren't going to be methodological flaws, and then you've got to convince them that the logistics have been thought about and covered, so that the project is seen as deliverable. This reassurance is provided by the description of the proposed study, by the team of applicants (their previous achievements and areas of expertise), by the participating institutions and facilities available there, and by the funds requested and the resources that they will provide.

A way of life

Quite a large part of academic life is devoted to trying to apply these principles in making arguments for funding support. There's a rather broad (and unsatisfactorily gendered) term, 'grantsmanship', applied to this process, which describes the expertise of those involved in putting together applications. Some of this simply reflects experience and an instinct as to what might be worth pitching, and some of it reflects the quality of writing applied to the proposal—which does help if it's clear, readable, precise, and well structured, just like research communication (Chapters 13, 14). Peer-reviewers and panel members are only human, and no one is going to be well-disposed towards dense, turgid prose or a proposal full of typos. However, 'grantsmanship' also describes skills in the process of submission, which is clearly going to vary according to the discipline and setting where you work. The following list provides a few principles that I think are likely to be common to most circumstances.

1. Allow plenty of time. There are always occasions for rapid responses with punishing deadlines, and you might get a personal kick out of living on the edge in that way. However, you've generally got to bring a co-applicant team along with you on a grant application, most of whom are likely to prefer a calmer and more considered planning process. Cajoling colleagues to help with a tight deadline once or twice might be OK (particularly if it's clear that you don't have a choice and the funder hasn't left you very long), but it does start looking discourteous if you're doing this repeatedly.

2. Decide on and negotiate the co-applicant team early. The remit of the funding call or study may determine this (e.g. expectations for multiple institutions, coverage of multiple disciplines). However, you need

to be careful about costs, and it's easy to swamp a proposal with senior salary contributions (if you're expected and allowed to claim these), which may not leave enough for you to employ the researchers you need to run the study. And it's obviously much trickier to explain to an invited co-applicant that you can no longer afford them than not approaching them in the first place.

3. I'm told that there are sometimes dark arts involved in the politics of applications and the co-applicants you need to have on board, so there may be some cautious early negotiations to be conducted. I have absolutely nothing to say on this—best have an informal chat with a senior colleague if you're in doubt.

4. If you're in a small research field, you might not want to involve all your friendly colleagues from other sites in an application or there'll be no one to peer review it (apart from the unfriendly ones).

5. Ask for cost calculations as early as possible because these will often need to be carried out, and ultimately signed off, by finance staff at your institution. Most of these staff, if you approach them with a last-minute request, will know full well that you could have given them a lot more notice if you'd been more personally organized. And sooner or later they might say no, in which case, you're not going to be able to submit your application. As with point 1, it's just another basic courtesy issue.

6. You may need to prepare for several rounds of costing and re-costing, as you try to get your budget to reach the desired amount—another reason to get started early on this.

7. Unless you're in a research field with massive equipment needs, the majority of your costs are likely to come from salaries. Fitting into a budget limit is therefore most likely to be achieved by altering pay grade levels or durations of employment, rather than fiddling around with more trivial categories like travel costs.

8. Do consider the feasibility of any salaries you're including. Personally, I try to avoid proposing any project with a less than 18-month researcher salary, because who's going to apply for a shorter-term post and how are they going to get up to speed in time? If you're confident that you'll have a good field of applicants when you advertise a shorter-duration position (or a pool of current salaried staff who can be rapidly deployed), then by all means ignore this. It's just that failing to get a funded study up and running because you can't recruit to it is sometimes worse than not getting the funding in the first place.

9. You generally only need a short summary of the project when you're approaching co-applicants and requesting costings. The longer and more detailed writing can be carried out while you're waiting for these things to come together.

10. Do try to submit at least a day before the deadline. Most funders now operate online application systems which cut out irreversibly at the published day/time and with no scope for a deadline extension (as there used to be for paper-based applications). You're not going to be popular with your finance colleagues or co-applicant team if, after all that work, you fail to submit because your computer crashed or your ISP went down at the last minute.

Other funding structures

There are of course other research funding opportunities beyond project grants. At the more expensive end, there are calls for programmes—that is, what amount to multiple projects within a broader theme (and usually of longer overall duration). Many of these larger-scale applications will apply a 'workstream' or 'work package' structure with individual leads for each of their components, feeding into the work as a whole, and coordinated by a core team. When these are multi-site, there tends to be a flurry of calls and emails following each preliminary announcement as the teams assemble themselves and work out who wants to lead.[10] In addition, there may be funding calls (again, usually sizeable) to support general or specific (e.g. equipment) infrastructure, sometimes involving the setting up of centres the size of departments, or larger. The applications for these tend to be led by senior staff but might well involve junior researchers in their assembly and are likely to draw on previous successes and the articulation of much broader strategies than individual research questions. On the smaller side, there are also sometimes opportunities to fund collaborations or networks (primarily non-salary costs to support meetings and coordination), and then individual awards to cover travel costs, or funds to support limited very early 'pilot studies'—that is, for gathering preliminary information to support a larger application.

The other common category of funding opportunities for early career researchers are fellowships and other personal awards. These tend to cover the full salary of the researcher for a given length of time, plus support for

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[10] A process that feels very similar at times to the informal assembling of teams in the school playground, so don't be surprised if it conjures up less-than-happy memories.

training, travel, and other incidental expenses (e.g. publication fees), but often do not include any salary for assistants or substantial research costs. Many of the principles of grant writing apply, as described in the last few paragraphs, but there are important differences. Unlike project grants, the adjudication process for fellowships tends to focus on three elements: the candidate, the research proposed, and the hosting environment. Costs are generally requested but don't tend to be a deciding factor as they're more or less the same for everyone. Instead, the 'package' being pitched is a promising candidate who will gain clear benefits from the training and experience on offer, and who will be supported by one or more institutions with appropriate resources for this purpose. The project and its general usefulness are important, but not quite as central to the proposal as in a grant application. What the funder ultimately hopes to get at the end of the process is a new generation of well-trained researchers who will be leaders in the future. Applications for these schemes go through the usual peer-review process, but those shortlisted nearly always end up with an interview, whereas panel interviews are only occasionally used for project grant adjudication.[11] Sometimes externally funded fellowships or packages of personal support (e.g. doctoral training studentships) are awarded to institutions as a block, and obtaining one is therefore more like a job application.

Other funding sources

Beyond the more organized, formal funding processes administered by agencies set up for this specific purpose, there are other options for disciplines with the right connections and common interests. For the sciences in particular, there is the commercial sector, which may house its own R&D but might also fund research elsewhere if there are resources it can draw on and common interests. I imagine that negotiations here are the same as any other proposed business relationship—some informal negotiation, a formal proposal, and then a lot of back and forth between legal/contracts departments on the small print if everyone agrees. In addition to funding, there are important parameters upon which to agree—for example, how intellectual property is allocated and who gets to share in any commercial gain as a result of the research. There are also complexities for researchers themselves around future conflicts of interest: well-recognized, for example, in healthcare research fields with the pharmaceutical industry or more widely in groups or institutions who have received funding from tobacco, alcohol, and food industry sources. Explicit

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[11] Although this might become more common, now that we're all used to virtual meetings, which make interview panels a lot more logistically feasible than the old practice of travelling across the country to sit in a boardroom for hours at a time and drink far too much coffee.

declarations of interest are a basic expectation, although it's actually better to steer well clear of future research that might have commercial implications for the company that has just funded you (or at least to be rigorous in ensuring oversight to avoid perceived risk of influence). Beyond the commercial sector, other independently negotiated sources include legacies and bequests, usually dealt with at an institutional level, but potentially involving support from researchers.

Wider politics

So, there are a variety of potential mechanisms for funding research, but not all countries and not all disciplines adopt all mechanisms, and all you can do is keep an eye out for what's available (and perhaps a particular eye out for any novel sources that others might not have noticed). Funding agencies themselves have an unenviable task of deciding on mechanisms to distribute and administer their awards, as well as being constrained by higher-level influences on the funds they have available,[12] and redirected by turnover of senior staff. As a result, and as mentioned earlier, strategies can often change uncomfortably for the research community—anyone who has been in any field for ten years or more will probably have witnessed or heard of the nasty consequences of a funding shortfall. For example, centres of excellence are sometimes set up and provided with infrastructure support, often brokered by a productive and politically influential group lead. When that leader retires (or moves on, or becomes politically less influential for whatever reason), the highly productive, long-standing research group might possibly remain intact, but there will be a strong temptation for the funding agency to curtail the support in order to increase resources to other groups, or at least to generate some flexibility in their portfolio—and new opportunities always look more attractive than maintaining something that's been around for a while. The results at a personal level can be unpleasant and there's rarely much thought given to legacy, so it's probably wise never to assume that anything will last. If we can be objective and sceptical about research questions, then we can apply the same to the motivations and pressures of our employers or funders.

Strategically, there are a few underlying contradictions that are not often acknowledged by funding agencies, but that are worth bearing in mind at times when you're trying to negotiate the system. For example, it's quite

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[12] For example, because of changes in government spending priorities when this is the means of support, or because of investment income fluctuations for independent foundations.

common to encounter enthusiastic calls for 'blue sky' ideas—meaning left-field, highly speculative, but potentially high-yield projects. I've personally never heard of much coming from these initiatives. It's a nice idea in principle, but the adjudication structure depends on the usual process of peer review and academic panels, all of which tend to be as conservative as ever, and likely to favour what seems like a safe deliverable. I do believe that highly speculative research is possible and does need to be a component of what we do—I'm just unsure of the role of specific funding schemes in promoting this and have seen it more often achieved using spare time and internal funds followed by an effective communication strategy if the new idea looks like it has legs. But mine is just one opinion, and no doubt calls for speculative projects will continue—just bear in mind that the project you submit ought to look safe as well as speculative, if you think that circle can be squared.

Slightly related to this is the tension between delivering funding through a few large infrastructure awards or many smaller grants for individual projects. You're unlikely to get an unbiased opinion from any jobbing academic on this because the response will depend on where they work. Large, high-ranking universities will argue strongly for the infrastructure because they'll be in the best position to bid for these awards. Research groups in smaller universities will argue that this creates structural inequalities—for example, there's often a substantial social and regional bias if high-ranking universities also happen to be based in relatively wealthy parts of a country (as they often are). However, infrastructure awards are likely to create more secure environments for the speculative, innovative studies discussed in the previous paragraph, whereas successful project grants will tend towards more conservative (but not necessarily less useful) output as a result of the layers of academic adjudication involved.

One way around this is to insist on infrastructure funding being more distributed across institutions. This may improve representativeness; however, as well as the challenges of making anything work across multiple sites with all their local bureaucracies, this throws up another tension in funding strategy. On the one hand, funding agencies would like to see multi-institution networks and collaborations develop.[13] On the other hand, competition is healthy and expected—no funder is going to be keen on everyone always clubbing together on applications and monopolizing the awards. Therefore, the same sites that collaborate as a network for one award will be expected to compete for another. I'm not sure whether this funding philosophy has been

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[13] Not least because I suspect you can keep more institutions happy and engaged at a lower cost through a single funding award spread thinly than through multiple individual awards.

around for long enough to evaluate, but it seems likely that the success or not of a collaborating-while-competing network is going to depend strongly on the personalities of its constituent leads.

The final point is one that's perhaps not the wisest to make while I'm still in employment, although I know I'm not the only one to feel this way. I think that there *is* such a thing as too much funding. Large awards do sound attractive, and any academic allowed a word in the ear of someone with money will always argue for as much as possible—why wouldn't they? However, there are large volumes of work entailed in their administration (all the posts to appoint and line manage, the extra space and facilities they'll require, the time taken in coordinating component work packages, etc.) and the situation can arise where managing a huge award is more problematic than if the funding application had never been successful in the first place. In addition, the success of very large awards can be next to impossible to scrutinize by the funder, particularly when there's too cosy a relationship between the awarder and awardees (academic institutions and their representatives, for example, involved in both the allocation and receipt of funding). Unless there are tight monitoring processes, failures may only be picked up when it's too late to do anything, by which stage no one wants to know. However, tight monitoring processes require administrative resources on both sides; otherwise the groups meant to be delivering research output will be incapacitated with paperwork and progress reports. I suspect that it's not dissimilar to government procurement and the occasions where money disappears into chaos and non-delivery. Even the largest research funding allocations pale into insignificance next to the worst government procurement failures, so perhaps it isn't something to get unduly worried about. And some large awards are well administered, have clear vision, and do genuinely deliver value for money—tight and effective project management is usually involved, ideally closely integrated with those delivering the research so that everyone is clear on the objectives and sharing the vision. Perhaps the only solution is to have large awards administered by wise and disinterested experts—which is a little like the impossible ideal of benign dictatorship for political leadership.

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Power and politics

Perhaps these new digitalized and distributed times will witness the renaissance of the researcher who works alone and independently, connected virtually rather than physically with their academic community. Or perhaps there are already lots of people working in this sort of way and I'm behind the times. However, I think it's still reasonable to assume for the moment that most researchers, or at least those in early stages of their career, are based in an institution of some sort, most likely a university. And if you're employed (or a student) at an institution, then you have a supervisor/line-manager and a wider group of senior colleagues with influences on your working life, not to mention all the fellow researchers at your own level, or more junior, with whom you may have rather complex part-collaborating, part-competing relationships. Some of these will be colleagues at the same institution, while others will be researchers in the same field across other sites nationally and internationally. Within this tangled web of connections are any number of relationships, and within these relationships there are likely to be power structures—some formal, others implied; some benign, others less so. Unless you're very lucky, negotiating these relationships is going to occupy a significant part of your life, although, as with research funding (Chapter 15), there's probably nothing unusual in this—it would be much the same in any other line of business.

Actually, it could be a lot worse

In many respects, a research career is quite straightforward and attractive for those who find social interactions a little challenging. For example, at least nowadays, there are generally quite clear and concrete metrics indicating success and ranking—number of publications, journal impact factors, H-indices, grant income, qualifications, etc. So, you know roughly where you stand in the scheme of things, based on what you've achieved on paper, rather than having to try to work out complex interpersonal hierarchies. Of course,

there are challenges in achieving all of these metrics,[1] hard work ahead, and a certain amount of luck required, but there's usually plenty of scope for aptitude and application to receive the rewards they deserve and simple lines on a CV to reflect this. In a well-functioning research institution, there also tends to be the professional space for everyone to work away quietly on their area of interest without causing trouble for anyone else. Funding is theoretically a point of competition, but it needn't cause difficulties if perceived adjudications involve fair and independent processes. When conflict does brew up in relation to awards, it tends to be around wider perceptions of unfair influence, which ought to be avoidable.

Interestingly, office accommodation does tend to be an enduringly predictable battleground and the focus for any number of long-running feuds— or at least that was the case in the pre-pandemic days of physical environments when space was at a premium and you could attract any level of funding and still have trouble housing your staff due to limited desks. I guess it's sensitive because of being a hard-wired territory protection issue, tapping into primitive and even pre-human instincts, and I don't think academics as a whole are well-endowed with the sort of self-awareness to spot this. Also, it's quite a visible marker of leadership ranking. If you're in charge of a research group and someone else successfully claims some of your desk space, all your junior staff are going to know this, whether you accept it with good grace or not—not easy if leadership doesn't come naturally and you're already feeling insecure. However, this sort of thing does tend to blow over quickly and is usually an internal matter that no one's going to want to go public about. The academic conflicts that are potentially more damaging and do often end up publicized, or at least widely known in the community, tend to be rooted in quite nebulous issues of reputation or self-worth, particularly as reflected by recognition and perceived influence.

The importance of being recognized

Arguments over recognition and influence became a regular feature of research once it started to become personalized—that is, it didn't particularly matter when most researchers had other jobs to do and were just trying to publish anything that stood a chance of being read. It does sound as if there was a rather tense relationship between Tycho Brahe, who had assembled

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[1] And don't ever fall into the trap of assuming that the metric of importance at the moment will be the same in a year's time; it's best to cover your bases—for example, number of publications generally, number of lead-author publications, impact of publications, research funding, other measures of esteem.

uniquely detailed astronomical observations in the late sixteenth century, and his assistant Johannes Kepler, who was desperate to have access to the observations in order to apply his mathematical skills (see also Chapters 8, 15). Brahe was worried that Kepler would support the Copernican theory of a Sun-centred planetary system, but in the end, he recognized that Kepler had the right abilities to take the field forward, and so handed his work on to him on his deathbed in 1601. However, all of this tension was around research and its legacy, rather than personal posterity. Neither probably had much interest in whether their name was attached to a theory—they both had quite enough work on their hand, trying to support themselves and their families through patronage and selling horoscopes.

The infamous Hooke–Newton feud occurred in the late seventeenth century, by which time research was becoming more of a full-time occupation and personal reputations and influence were becoming more important. Hooke came from a fairly humble and unpromising background, but managed to work his way up the ladder,[2] beginning his career as an assistant to the generally philanthropic Robert Boyle (Chapter 10) who secured him employment and a fellowship at the newly established Royal Society as well as supplementing his income during straightened times. However, Hooke was understandably insecure as he mixed with aristocratic scientists, having only recently been a servant, so when the more junior (but only seven years younger) Newton published work on optics with only a vague mention of Hooke's prior research (despite having been obviously influenced by this), the gloves came off and a running battle was commenced between them.

After four years, this bickering became tedious for everyone else at the Royal Society and a reconciliation was brokered in 1676, signified by an exchange of letters between the two. Newton's letter included the famous acknowledgement that he was able to do his work because he had been *standing on the shoulders of Giants* which might possibly have been a genuine compliment to those who came before him (unlikely for Newton), or else *Giants* with a capital G might have been a less-than-veiled reference to Hooke's short stature and twisted back. Either way, it's beyond coincidence that when Newton took charge of the Royal Society after Hooke's death in 1703, Hooke's portrait was the only one that was mysteriously lost in a move of premises, so that we have no reliable image of him to this day. Although Newton ended up the winner and Hooke was nearly completely airbrushed

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[2] For example, obtaining an Oxford University chorister's position during a period when choirs weren't allowed to perform under Cromwell's puritan regime, allowing him time to hang out with, and earn some extra money from, the scientists there.

out of history for centuries to come,[3] it's hard not to conclude that the two of them were as bad (or damaged) as each other. Both came from humble backgrounds, both were very much self-made men and extremely intellectually gifted, but neither seems to have had much of an ability to see another person's perspective. In Hooke this came out in fairly tactless, unfiltered, prickly irritability, although he was sociable and had a number of close friends. Newton, on the other hand, seems to have been solitary, sullen, and (given the 34 years between his 'resolution' with Hooke and the 'disappearance' of Hooke's portrait) capable of bearing a grudge over a very long period indeed. And Hooke wasn't the only recipient. Newton had another long-running dispute with the German mathematician Leibniz over who invented calculus, although they probably both devised it at the same time, just as Hooke was coming close to proposing the inverse square law of gravity and had corresponded with Newton on the subject four years before Newton proposed it publicly in 1684.

The myth of the individual—inevitable discoveries

The problem with 'reputation' in research is that advances are to some extent inevitable—as mentioned previously, it seems that sooner or later knowledge will accrue, regardless of who carries it forward. Newton was revolutionary but his theories would have come to light through someone else if he had never existed (through Hooke, for instance, who had very nearly got there, or perhaps through Halley, who understood such things). So, researchers are not unique originators in the same sense as Shakespeare or Van Gogh, whose works definitely wouldn't exist without them. Instead, we're really just riding a wave of human discovery that's taking us wherever it is we're going. However, most researchers don't see themselves that way, and the way in which human discovery is written about (including here, I guess) does tend to focus on the discoverers, the individuals, as if they really were Shakespeares or Van Goghs. Everyone has to get their self-esteem from somewhere, so reputation and influence do make a difference.

If it's common for discoveries to occur simultaneously (because sufficient knowledge has 'matured' to make this possible), there can therefore be an unedifying race to first publication, and unedifying acrimony if it's not clear

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[3] Although one of the things that didn't help Hooke's posterity was the fact that his notebooks were considered unpublishable throughout the nineteenth century because of all the information on liaisons with maidservants in his employment; also, his mistresses included his teenage niece. Newton, on the other hand, was helpfully celibate—or at least heterosexually celibate and very discrete otherwise.

who got there first. We've encountered this in the other particularly famous pairing of Darwin and Wallace (Chapter 14)—by all accounts a lot friendlier than Newton and Hooke, but still with a clear winner and loser. Other examples include Priestley and Scheele discovering oxygen (Chapter 10), Gauss and Bolyai coming up with non-Euclidean geometry (Chapter 7), or the race to decipher hieroglyphic script after the French discovery and British seizure of the Egyptian Rosetta Stone (Chapter 12). In theory, arguments about the applications of research ought to be a less emotional affair because there's some means to weigh up the outcome. However, there wasn't much evidence of this in the late nineteenth-century (acrimonious and often dirty) battle between Thomas Edison and George Westinghouse over whether DC or AC electricity provision should be adopted in the US. Although a lot of money was at stake, reputations and personalities were dominant factors and the conflict only began to subside once the intransigent Edison had lost his majority shareholding in the company that would become General Electric, and thus had been eased out of the picture.[4]

Early theories and discoveries in thermodynamics were another case in point. William Thomson (later Lord Kelvin) in Scotland and Hermann von Helmholtz in Germany fought it out, with their rival supporters, in the mid-nineteenth century as the first (conservation of energy) and second (heat flowing from hot to cold) laws of thermodynamics were conceptualized and applied to tasks such as working out the possible ages of the Earth and Sun. The German physician Julius Robert von Mayer had actually got there before both Thomson and Helmholtz, although his publications in the 1840s on the subject went unnoticed by physicists at the time. Earlier still, the French military engineer Nicolas Léonard Sadi Carnot had also laid important groundwork, but his death in the 1832 cholera epidemic (Chapter 1) meant that his belongings, including paperwork, had to be destroyed to avoid contagion, so it's not possible to know how far he progressed. At least Carnot was beyond caring about his reputation as the field took off and names started to be made. Mayer, on the other hand, became profoundly depressed at the lack of recognition, attempted suicide in 1850, and was confined in a mental health institution. Luckily, in the end his work was acknowledged and appreciated with several prestigious prizes, and he recovered his health in his later years. John Waterston was less fortunate. Taking early retirement in Edinburgh after teaching civil engineering in India, Waterston had devised and self-published

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[4] One of the more underhand strategies in that dispute was when the Edison company (supporting DC current) successfully lobbied for the first electric chair execution to be carried out using AC current, thus trying to imply that it was more dangerous. Westinghouse in turn had to hire lawyers to try to defend the murderer sentenced to be executed in that way. He lost that battle (the murderer was executed, albeit very messily) but won the war.

theories about the kinetic energy of gases that received little recognition when circulated in the 1840s. He was left embittered and reclusive by the experience, which didn't help matters. His theories were independently developed by James Clerk Maxwell (Chapter 12) among others, and it wasn't until 1892 that someone noticed and publicized Waterston's original work. However, this was too late for Waterston himself—nine years earlier he had walked out of his house and was discovered drowned in a local canal.

The early history of thermodynamics doesn't appear to have been a particularly happy one. As well as the problem of simultaneous discovery, recognition clearly depended to some extent on where you worked and who you knew, as well as on the state of the scientific community at the time. Mayer and Waterston, the losers, were simply not understood sufficiently by their contemporaries, and the community (conservative, like any community) needed to have advanced a little in order to accommodate the new ideas. There were more mundane issues as well. For example, it didn't help that Waterston's key theories were contained in a book called *Thoughts on the Mental Functions*—hardly an informative title. I suppose it also didn't help matters that the field of thermodynamics was becoming so important[5] at a time when research was still in the process of shifting from people working alone to those in universities or other institutions—that is, in a structurally rather vague period between the gentleman scientists of the eighteenth century (Chapter 15) and the employed research groups of the twentieth. Feeling unclear where you stand in the scheme of things, or having a sense of being ignored and misunderstood, are not conducive to contentment. This was particularly the case as progress remained very personalized with prizes and society memberships, as success fostered more success through influence over funding and other resources, and as research became increasingly expressed in the stories of 'great men' of the past. And the big portraits in the institution entrance halls were always male. And white.

When the odds are stacked against you

If it's hard enough gaining recognition and influence as an educated white male researcher because you don't know the right people or belong to the right institutions, it's clearly harder still if you're non-white, or female, or both. Examples discussed previously include Maria Sibylla Merian's work being used, but not cited, by Linnaeus in his species classification system (Chapter 4), or Mary Anning's palaeontology wholly attributed to all the

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[5] Along with electromagnetism, a bridge of sorts in physics between Newton and Einstein.

men she helped over the years because of her exclusion from the scientific community of the time (as a non-Anglican as well as a woman; Chapter 4), or the biologist Ernest Everett Just having to pursue his research in 1930s fascist Europe because his ancestry excluded him from appointments at suitable American universities (Chapter 15). There's also the disturbing example of Alice Ball (Figure 16.1), a highly gifted African-American chemistry researcher at the University of Hawaii who, in 1915 devised and developed, at the extraordinarily young age of 23, what would become the standard treatment for leprosy. Tragically, she died a year later,[6] and her work was appropriated wholesale by a senior male (white) colleague and taken forward but passed off as his own with no credit or citation. It took a considerable

Fig. 16.1 Alice Ball, a chemistry graduate, discovered what would become the standard treatment for leprosy before she died at the age of 24. I guess more than one story can be told. On the one hand, there's the injustice of the senior colleague who took her work after she died and essentially passed it off as his own. On the other hand, at least her contribution was belatedly acknowledged by another senior colleague and sufficient records were kept, allowing her achievements to be unearthed by later historians. And there's now an 'Alice Ball Day' every four years in Hawaii, where she made her discoveries.

Photograph of Alice Augusta Ball, photographer unknown (1915). Public domain, via Wikimedia Commons. https://commons.wikimedia.org/wiki/File:Alice_Augusta_Ball.jpg

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[6] Tuberculosis was listed as the cause of death, although there are suspicions about chemical exposure related to her occupation and an altered death certificate.

amount of work by later historians from the 1970s onwards to re-establish her reputation and achievements.

The race to discover the structure of DNA is also hardly a glorious one in the way it turned out. To begin with, it was the same old example of knowledge accumulated from many sources that should never have had 'winners' in the first place. This is compounded by the considerable controversy over the contribution made by Rosalind Franklin (Figure 16.2) at King's College London to the ultimate discovery and publication by James Watson and Francis Crick at Cambridge University. A lot of this hinges on a particular X-ray image taken by Franklin's team in 1951 that came into the hands of Watson
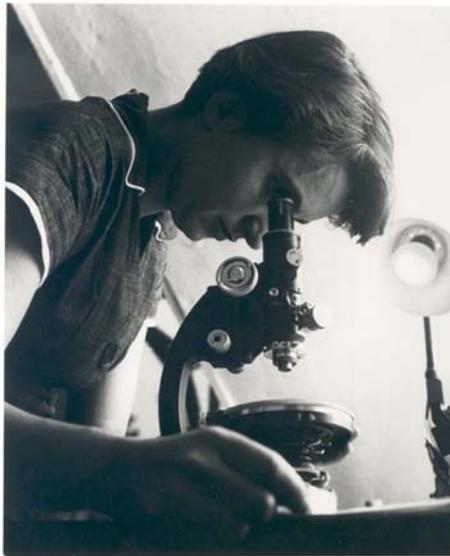
Fig. 16.2 Rosalind Franklin at work. Like Alice Ball, Franklin's story can be told as one of injustice, missing out on credit and recognition in the race to discover DNA because she didn't get on with Maurice Wilkins, her colleague at King's College London, who may or may not have passed on her work without her knowledge. On the other hand, why on earth does the whole thing have to be seen as a race anyway and isn't that just perpetuating an alpha male cliché about research and the myth of 'discovery'? Besides, it sounds as if she gave as good as she got. I quite like the description of Franklin unnerving colleagues with her intense eye contact and concise, impatient, and direct manner, whereas Wilkins was shy and avoided eye contact—it's easy to imagine the dynamic in the laboratory.

and Crick, possibly without her knowledge via her senior colleague Maurice Wilkins, although there is some dispute about this. Whatever went on with the X-ray, Franklin's work was undoubtedly fundamental to the double helix theory put forward in 1953, but it was Watson, Crick, and Wilkins who shared the Nobel Prize in 1962. There are Nobel rules to justify this—Franklin had died in 1958,[7] the prizes aren't awarded posthumously, and aren't shared between more than three people; however, it does leave a bad taste. It doesn't help matters that the laureates were the three men, although perhaps the fault is more in the idea of awarding prizes in the first place. There was some justice when King's College London opened its 'Franklin Wilkins Building' in 2000 (i.e. with the names that way around); it's just a shame the acknowledgement couldn't have been associated with something architecturally more attractive.

Not victims

Of course, it's important not to perpetuate victim narratives. Rosalind Franklin's career can be viewed more helpfully as enormously productive, setting aside any Nobel Prize injustices as an irrelevant side-show, only of interest to those who foolishly believe in such things. And by all accounts she was as abrasive at times as Hooke or Newton, giving Watson an earful on one occasion when he questioned her data (or at least that's what she thought he was doing). Marie Curie and Dorothy Hodgkin ploughed similarly productive twentieth-century furrows, studying hard, excelling intellectually, and pressing doggedly ahead with their research regardless of any obstacles or disadvantages faced, as did Marie's daughter Irène Joliot-Curie.[8] Similarly inspiring is Katherine Johnson (Figure 16.3), an African-American mathematician who gained employment as a NASA scientist and simply didn't take no for an answer when it came to obtaining named credits on reports and getting included on editorial meetings. She worked on most of the major space missions in the 1960s, then spent her later years encouraging and supporting future students. Among other awards, she was honoured by President Obama with the Medal of Freedom in 2015[9] and died in 2020 aged 101.

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[7] Of ovarian cancer, very likely to be related to her work with X-rays.

[8] Having said we shouldn't care about such things, it's worth mentioning that both Marie and Irène won Nobel Prizes, as did both their husbands. Both also died of diseases probably related to their work with radioactive material.

[9] An award that at least reflected years of service rather than being on the 'winning' side of a scientific discovery.

Fig. 16.3 Katherine Johnson, mathematician and NASA scientist, pictured at work in 1966—possibly about to put an early computer through its paces, making sure that it could keep up with her calculations. Her work on trajectory modelling was key to the Apollo 11 Moon landing and the safe return of the Apollo 13 crew when they had to abort. Widely and appropriately lauded, Johnson was well aware of the potential challenges she faced in her career, but just stared them down and trusted that her abilities would be recognized. Which they were. And are.

Photograph of Katherine Johnson, NASA (1966). Public domain, via Wikimedia Commons. http://www.nasa.gov/sites/default/files/thumbnails/image/1966-l-06717.jpeg

What to do about stolen ideas

While on the subject of simultaneous discoveries and conflicts over credit between institutions, 'stolen ideas' remain a potent source of acrimony within institutions. This is particularly the case among junior and middle-grade researchers in highly competitive, 'pressure cooker' environments. It's not unheard of at a more senior level, although territories of influence have tended to be carved out more distinctly by then and/or professors have learned to keep their cards close to their chest. It's harder if you're on your way up the ladder: on one hand, you're expected to be part of a cohesive research group, but on the other hand, you understand that your colleagues are potentially direct competitors. It's quite easy to develop a little bit of paranoia, and sometimes that's entirely justified—there may genuinely be people working

against you. However, it's also possible to go a little over the top on this one, and paranoia to the point of delusion is not unheard of if stress levels are high enough. A few things are therefore worth bearing in mind, if you can consider them at a moment when you're feeling objective and rational:

1. The same ideas do sometimes occur simultaneously by chance (as just discussed).
2. Sometimes ideas are reproduced inadvertently—it's common to hear something in a meeting that slips in and starts germinating, but by the time it matures and you're acting on it, you've forgotten where it came from in the first place.
3. Sometimes people (particularly supervisors) are just careless and forgetful in attributing credit—not good practice, but not necessarily malicious in intent.
4. If your idea really does get stolen, then it's better to put it down to experience and learn from it (e.g. be more careful about when to share in future) rather than letting resentment brew—which isn't going to do you any good.
5. Try to make sure that you have plenty of ideas on the go so that you can immerse yourself in an alternative one—useful for failed funding applications as well.

As a final point, it's worth bearing in mind that bad ideas get stolen as well as good ones. I can think of at least two occasions when, in retrospect, I was very glad that someone had stolen (and taken full ownership of) an idea that I'd originally proposed—each time, the idea was flawed, the results weren't good, but it wasn't me who had to carry the can. So, there is an upside.

Decency in academia—there's more of it than you might think

Despite its periodic unedifying conflicts and injustices, it would also be wrong to portray research too negatively. In comparison with other competitive environments, such as big business and government departments, it's remarkable how tolerable relationships are in academia, particularly if funding structures are fair, if authorship and accreditation reflect contributions accurately, and if desk space skirmishes can be foreseen and averted. Also, most personal stories in research are of mentorship and the calm passing on of responsibility, even when the student outstrips the mentor. Brahe

bequeathed his astronomical observations on to Kepler, recognizing that the mathematical genius would make best use of them. Boyle gave Hooke his start in life, was generous in his financial support, and remained a friend to his prickly former assistant. Sometimes the mentoring relationships are a little unusual—Michael Faraday began his career as an assistant to Humphry Davy (Chapters 10, 14) at the Royal Institution and within six months was following Davy and his wife around on a tour of France as both a junior scientist and personal valet. The experience seems to have been rather wearying (the valet role was taken seriously by Lady Davy), but the apprentice did benefit from all the meetings with fellow researchers and was in a position to take on Royal Institution leadership as Davy shifted towards a more political career. Faraday went on to transform electromagnetism (among other achievements), was revered by his successors, and ended up as one of the three portraits Einstein had on his wall.[10] Like Dalton (Chapter 14), Davy's other protégé, Faraday was deeply religious and self-effacing, turning down a knighthood and the offer of a burial in Westminster Abbey, quietly continuing his research while his mental abilities allowed. He did in the end accept from Queen Victoria a retirement property where he spent his final nine years. Shortly before his retirement, he had been asked by the British government to give advice on chemical weapons production for their Crimean War, but creditably declined on ethical grounds.

It's therefore entirely possible to be a decent person and a researcher at the same time—it's a career that attracts all sorts of personalities and there's probably something about the process that enforces humility. After all, you do have to have Socrates' position of ignorance underpinning the work you do, and if you spend your time trying to prove you're right, you're not really a true researcher and the world will see through you sooner or later. Many great researchers are remarkably selfless. The seventeenth-century naturalist John Ray, for example, spent 14 years assembling and publishing the work (on fishes) of his deceased friend Francis Willughby, was probably responsible for a large part of the final product, but took no authorship credit. In the nineteenth century, Wallace expressed himself as entirely satisfied that his paper on natural selection as a mechanism for evolution had stimulated Darwin to write his *Origin of Species* and take the credit. Mendel (Chapter 14) quietly shelved his research on heritability to take up his senior clerical duties. John Michell, an eighteenth-century science polymath, friend of Henry Cavendish, and the first person to propose what would later be called black holes, similarly worked quietly away as a rector of a small country

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[10] The others were of James Clerk Maxwell and the philosopher Arthur Schopenhauer.

church. Even Newton, in one of his better moments, described himself as being like a boy picking up small stones on a beach *whilst the great ocean of truth lay all undiscovered before me*.

Big 'P' Politics

As if the mixed-up, muddled-up, shook-up world of academic institutions and research communities isn't enough, there's a wider universe out there of 'everyone else'. National politics has a complicated relationship with researchers, as researchers have with politicians. On the one hand, a lot of funding for academic institutions has traditionally come from governments (Chapter 15). In part, this has been a prestige competition, particularly at times when military rivalry has settled down to a cautious stalemate for a while, and as an alternative to massive building projects. However, this is also no doubt quietly encouraged by academics with vested interests in the funding. Academic foundations set up as vanity projects by monarchs or governments are exemplified in Alexandria, in the mediaeval Middle Eastern empires, in the competing Italian Renaissance city states providing protection for their academics (e.g. Venice for Galileo) against interference from religious power blocks, as well as in various seventeenth- and eighteenth-century courts of 'enlightened despots' in the rest of Europe. It also underpinned bodies such as the London Royal Society and Paris Académie des Sciences (founded by Charles II and Louis XIV, respectively[11]), and the American Philosophical Society (founded by Benjamin Franklin). However, the internal contradiction here is that, while research requires intellectual freedom in order to flourish, the same intellectual freedom does tend to foster perceived or actual sedition. So, there's a mutual suspicion on both sides from the start and it's not uncommon for researchers to fall on the wrong side of politics.

Problems tend to occur when researchers are too close to power. For example, Bacon in seventeenth-century England (Chapter 4) and Shen Kuo in eleventh-century China (Chapter 4) were politicians themselves, as well as researchers, and ended up forced out of power, as tends to happen in the end to those who follow a political career. On the other hand, sometimes researchers simply end up tied to the fortunes of one faction or another and suffer the consequences. As discussed in Chapter 15, the Library of Alexandria went into decline because its head librarian supported the wrong party

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[11] Although supported in quite different ways—encouraged but not funded in London, funded but with obligations to the Crown in Paris—resulting in differences in ethos which may or may not have a bearing on the earlier Industrial revolution in Britain than France. One for the historians.

in a disputed succession. Later on, the mathematician and philosopher Hypatia (Chapter 2) developed a highly respected reputation in Alexandria which came with political influence, but, in a feud between the ruling Roman prefect and the bishop in charge of the city, she was caught on the wrong side and was murdered by a mob. Archimedes (Chapter 12) likewise served the city of Syracuse on the losing side of a siege, and was killed in the fighting. Sometimes researchers just don't read the signs well enough—Lavoisier (Chapter 10) presumably thought his scientific achievements would give him sufficient credit during the French Revolution, despite his association with a hated tax collection system, but he was guillotined nonetheless, whereas his circumspect colleague Laplace (Chapter 5) was more adroit at keeping out of the way. Priestley (Chapter 10) on the other side of the Channel was an ardent republican and vocal supporter of the French Revolution at the wrong time in England, so had to emigrate to the US. Even Galileo, the 'first scientist' of the European Renaissance, who had spent most of his life dodging the complicated politics of Italian city states and a suspicious but still-ambivalent church, eventually ended up with a published book that was too much in favour of Copernican theories, with a hostile Pope and cardinals now in charge, and without his old protectors. He was forced to recant his theories under threat of torture, as an old man with crippling arthritis, although the fact that he survived to die peacefully in 1642 at the age of 77 is quite an achievement in itself, given the times he lived in.

Best kept separated

In the modern era, the rise of institutions around both researchers and politicians has tended to act as a cushion, keeping the two apart, so there's a lower likelihood of a researcher exerting direct political influence. I suspect this is generally a good thing—I'm not sure that research and politics make for a very good mix. For a start, there are horror stories like that of Lysenko (Chapter 11), where a misguided researcher is given free rein with government policy and 30 million people die in the ensuing experiment. Researchers may have strong views about the implications of their work, but they're not really trained to implement and own those implications—and they ought to be viewing them as experimental and subject to falsification anyway, if they have any integrity. Politicians, on the other hand, are there to make and accept responsibility for decisions, most of which will be based on incomplete evidence at the time. This accounts for the slightly tense atmosphere that tends to be present on the occasions when politicians and academics are in

the same room together. The politician is likely to be looking for an answer, the academic is likely to hedge their opinion with uncertainty and therefore won't answer the question in the way that the politician wants. Alternatively, the academic will answer the question more directly but have to bury their own uncertainty and feel uncomfortable as a result because this isn't really what research is about. An academic in regular contact with politicians may become an expert in this split thinking and as a result may be able to bury uncertainty without even noticing it or feeling guilty, which does sound a little like losing your soul.

Economics is a field where research and politics are particularly likely to meet, for obvious reasons. Indeed, political parties or factions are frequently associated with, or almost defined by, a particular school of thought in economics—for example, from left-wing Marxism to less-left-wing Keynesianism (Chapter 8), to right-wing monetarism. The political adoption of economic theories can be viewed as real-world experiments of sorts (Chapter 11), although the actual relationships between the theory and the policy are often quite hard to delineate if you're evaluating the outcomes of these experiments. I may be wrong, but I imagine that economists in government are quite practised in asserting the theory they espouse and don't tend to adopt Socratic positions of ignorance very often. If so, this may be better classed as propaganda than research and better considered in another book. Public health is another point of contact between research and politics, and one that is rather prominent in everyone's minds at the time of writing. This will be considered in more detail in Chapter 17.

Of course, it's all very well to say that research and politics are best kept at arm's length from each other, but from time to time there is bound to be a need to speak truth to power, whether power wants to hear it or not. As mentioned, academic institutions traditionally thrive when kept free of undue influence, but funding this is a risk taken by any ruler or government, and governments have a tendency to view their nation's universities with distrust. In the UK, although universities have a reputation of being hotbeds of left-wing thinking, I haven't noticed much difference in levels of wariness from left-wing compared to right-wing governments—it might therefore be more a concern about the principle of clever people (and/or truculent students) gathering in groups, rather than what they're actually talking about. However, most countries accept that universities serve a purpose of some sort and can generate prestige, so tend to tolerate the grumbling from that quarter. Whether governments expect (or want) significant research output from their universities is another matter, and the internationalism of research nowadays means that the talent in that respect will gravitate towards settings that are pleasant to work in, well-funded, and free from interference.

Ground-breaking research tends to be impeded by oppressive, controlled settings, although not all research has political relevance, and some activity might be left alone. As discussed (Chapter 6), pure mathematics thrived in several centres under what were fairly harsh totalitarian monarchies in the eighteenth century.

And then there's the general public . . .

While at times there may be deliberate suppression of academic voices,[12] most politicians have an eye on popular opinion, so most disagreements with researchers are because politicians (rightly or wrongly) feel that they are speaking for a wider public. The complex and sometimes tense relationships between research and public attitudes are familiar nowadays, although perhaps overplayed as a general issue. Most research features very little in public (and therefore political) imagination. If you're a historian, for example, you might find that people persist in characterizing King Richard III of England as being an evil tyrant while you're trying to rehabilitate him by saying that he was misunderstood, never murdered the little princes in the tower, and was the victim of a Tudor smear-campaign popularized by Shakespeare's play.[13] However, you're unlikely to get more than a bit huffy about the uphill struggle; indeed, it might give you a bit of extra media coverage if you're speaking against the grain—no one's that interested in stories being proved that we think we know already. Therefore, if you're embarking on a research career, however illustrious, the chances are that your findings for the most part will generate little public controversy and have minimal interest from politicians. This is probably reassuring. And if you enjoy a public argument, you probably know full well what field of research to choose for yourself—although do try to keep objective and dispassionate about your findings. As we discuss shortly, thriving from being seen as contrary can be as much of a conflict of interest as any amount of commercial funding.

The 'whose consensus' problem

Public controversies in research have tended to arise in circumstances when academic consensus has been (or is still) evolving and when the findings have

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[12] Or worse, such as the fate of those disagreeing with Lysenko's agricultural science theories in Soviet Russia.
[13] These arguments used to include suggestions that Shakespeare fabricated Richard's hunched back to make him more villainous, but then Richard's body was found in 2012 under a car park in Leicester and the skeleton did indeed show severe spinal deformity.

wide relevance for behaviours and lifestyles. Chapter 8 considered the process of consensus and how this has been important for consolidating knowledge in fields where demonstrative experimental evidence can't be obtained—that is, where a theory can't be proved and is difficult to disprove as well. Anthropogenic climate change is an example of this process and clearly there are elements of that debate that continue to a varying extent,[14] much to the annoyance of the scientific consensus. However, the problem is that it was always a gradual process, as any consensus should be that involves so many different strands of evidence. At some point climate change didn't exist at all because there were insufficient greenhouse gas emissions; at some point later, it was detectable but very few scientists were taking it seriously, or at least very few were accepting the anthropogenic element. But then at some point the consensus began to emerge. And, at some point in that consensus process, the debate was (or could have been) a healthy, friendly one where scepticism was just good scientific rigour. But then at some point, the scepticism was interpreted as damaging to the consensus, felt to be more misplaced or vindictive, and/or coming with trappings of dodgy funding and conflicts of interest. And then the scepticism is called 'denial', and the sceptics are placed in a category with other people labelled as 'deniers'—of course, they get angry about that, and the time for calm discussion has long since passed. And of course, it doesn't help that the outcome matters so much and has profound implications—not only on the longer-term future of the planet and the potentially catastrophic consequences for the next generations (who are now quite reasonably questioning why they've been left with this legacy), but also on the shorter-term costs and loss of livelihoods that are likely to occur as a consequence of the measures put in place to avert the catastrophe. If there weren't the shorter-term costs, there wouldn't be any controversy in the first place, or at least it would be a lot less public. I remember living through CFCs being banned from refrigerators and aerosols, and lead being removed from petrol—both of which were accompanied by grumbling from the industries affected. However, they happened, nonetheless. Interventions to limit global warming have much wider public consequences, so it's not surprising that the battle is difficult, even without social media echo chambers and commercial interests lurking in the background.

Everyone ends up compromised

This illustrates why research doesn't mix well with power and politics. Knowledge tends to accumulate gradually, but no one outside the research

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[14] That is, more around what's to be done about it than whether temperature levels are genuinely rising.

community wants to hear about stimulating academic debate if the questions matter and are urgent, or if the decisions are needed now and can't wait any longer. As a result, the pressure grows on the community to jump the gun and to come out with a consensus statement for example when the evidence is going in a particular direction but isn't quite as strong as you'd like. In turn, this may require some assertive corralling of opinion and stifling of debate, which then looks like an institution closing ranks. And if you've grown up with stories about rebels fighting against the system, prophets crying out in the wilderness, and all the rest of it, then it's only natural to view the consensus vs. the sceptics as the many against the few, perhaps even more so if you associate academia with power and governments and if you and your community haven't been treated well by governments in the past (or are in current conflict). The problem in all of this is that the members of the research community have stopped being quiet, curious people who discover things, and they've started developing propaganda, however genuine and well-meaning. And anything that smells of propaganda is going to attract opponents who like the image of being the few against the many.

An example of this process can be seen in the MMR vaccine controversy as reported in the British satirical magazine *Private Eye*. After the initial publication in 1998 of a research paper, now retracted, proposing links of the vaccine with autism and inflammatory bowel disease, the magazine was initially dismissive. However, as the lead researcher started to be portrayed in some circles as a lone crusader against the medical establishment, *Private Eye* took the side of the 'small man', as it has done creditably on many occasions before and since. In 2002 it published a 32-page report strongly questioning what was becoming a growing scientific consensus, despite study after study emerging that failed to support the link originally proposed. However, by 2010, the academic debate was over, the original paper was fully retracted, and the lead author was found guilty by the British General Medical Council of serious professional misconduct. That year, *Private Eye*'s medical correspondent carried out a review of their coverage and concluded as follows:

> The *Eye* was right to keep asking questions on behalf of parents. There have been plenty of medical scandals exposed by investigative journalism, and plenty more to expose. This could have been one, but it wasn't. By the time of the second Cochrane review,[15] the *Eye* should have conceded the argument.[16]

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[15] A systematic review of evidence collated; the one referred to in this piece was published in 2005.
[16] http://www.drphilhammond.com/blog/2010/02/18/private-eye-dr-phil%E2%80%99s-private-eye-column-issue-1256-february-17-2010/

So, the process of expert consensus, while it's occurring, can be hard to distinguish from a conservative establishment rounding on a helpless individual trying to fight their corner. And it doesn't help matters that research communities have failed to recognize innovation time and time again in the past, and that medical research has had its darker moments (e.g. obviously the US Tuskegee Syphilis Study discussed in Chapter 11, but plenty of others); also, it isn't generally hard to find or imagine commercial vested interests on the side of the establishment. On the other hand, there are equally vested interests in being anti-establishment: careers made, books sold, support from like-minded sceptics. These are not the sort of things people typically include in declarations when they publish[17] so tend not to get much further than a vague reputation among colleagues in the field. As highlighted in Chapter 3, always be cautious of an academic who's never changed their mind. Perhaps it's the sort of thing of which investigative journalism needs to be more aware, and one more element of academic 'power' that does tend to complicate research. The MMR controversy resulted in a noticeable drop in vaccination rates and therefore, undoubtedly, gave rise to otherwise-avoidable illnesses and deaths as a direct result. What's more, social media was getting well underway during the course of the arguments, adding fuel to an anti-vaccination movement that hasn't looked back.

In conclusion

This feels like a chapter in search of an answer and I'm not sure I'm able or qualified to provide one. Academic power games have occurred ever since research output came with a desirable reputation (rather than just hassle from the Church) and ever since the myth of discoveries by individuals began to be perpetuated. Infighting and toxic rivalries are tedious but do still grumble on, flaring up from time to time. The shift in many fields towards institutions and research groups, rather than individuals, may help matters, and the encouragement by funders of multi-site and/or multidisciplinary networks might improve things still further. The increased visibility that research groups provide, and the increased co-dependency of practitioners, may also reduce the risk of problematic power dynamics and abusive or exploitative relationships. The lurid stories that any senior researcher will have heard from a generation or two ago are hopefully fading into rarity, now that problematic behaviour is more widely witnessed when it occurs, and now that employers are more

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[17] That is, not just 'I've received research funding from X and Y pharmaceutical companies', but also 'I've been going on about this for years and might be disinclined to change my viewpoint as a result'.

willing (or at least legally obliged) to take disciplinary action. So, I suspect that academic environments may shift towards becoming quieter and more supportive—possibly more boring than they used to be, but who really needs obnoxious professors to gossip about anyway? And have the obnoxious ones ever been particularly productive?

I certainly don't claim to have any answer to the challenge of communicating consensus opinion or of defining who it is that should be involved in the consensus in the first place. As someone whose research has at least some roots in public health, I find it difficult to suggest a complete withdrawal from political and public engagement. John Snow put forward his view that cholera might be prevented if water supplies were cleaned up, and we shouldn't be afraid of passing on equivalent messages today. However, the spirit of Socrates is still buzzing around somewhere, and it does feel compromising to be moving away from that point of objective scepticism we started with in Chapter 3. I think for most researchers, this is sufficient. Discovery brings ample rewards, and power, influence, and recognition are not really what makes a research career worthwhile, although you may have to face up to them at times.[18] Power ultimately tends to skew priorities and breed conflicts of interest, as well as making otherwise intelligent and talented individuals behave rather badly towards each other. Which is never a good thing.

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[18] The 'imposters' (triumph and disaster) in Rudyard Kipling's *If* poem come to mind.

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How to be a Researcher

Some Conclusions

As discussed throughout this book, research isn't really anything more than a by-product of our species' curiosity. Our inquisitiveness about the world, and willingness to learn by experiment, was there at the beginning and we have to assume is hard-wired into what we are. There have been clear points of acceleration, such as when we shifted to agriculture-based communities, allowing us (if we weren't doing this already) to specialize and allocate individuals to research or invention amongst other duties. Also, advances in written communication were important, allowing us to store knowledge, rather than have to rediscover it every time a community failed to pass it on. Printing was a key factor underlying the last 500 years of advances, not only because of the more rapid and wider communication on offer, but also because it resulted in a higher probability of effective storage and archiving of knowledge accrued—important for research because, as discussed, knowledge is sometimes accrued ahead of its time and needs to be rediscovered when the conditions are right. Whether the digital revolution will have a similar influence remains to be seen—it might accelerate the process still further, or it might have the opposite effect by swamping the world with volumes of information that it can't handle, prioritize, or sift from disinformation. In the course of the expansion in knowledge over the last few centuries, there have of course been technologies developed that have enabled further progress (whether developed for that purpose or not), the research community itself has expanded beyond recognition, and institutions and career structures have evolved to hold that community together. The number of academic specialties carrying out what can be called 'research' has also grown to near-comprehensive coverage across the humanities and sciences.

Unchanging fundamentals

Behind all of this, the principles underpinning research have remained remarkably consistent—indeed, it's questionable whether they've

fundamentally changed at all since they were first written down. We still begin with an assumption that the world around us operates according to certain laws that can be discovered and by an acceptance that there are things we don't know (Chapter 3), we still derive new knowledge from careful, systematic observation (Chapter 4) or from logical deductive argument (Chapter 6), but then we still need to retain our sceptical objectivity and put these conclusions under pressure through experiments or other checks (Chapter 5). All of these principles can be found in the earliest writings on 'natural philosophy'. However, contexts and research questions have changed over the years, so we have had to learn how to build accepted knowledge in circumstances where experimental evidence isn't available (Chapters 7–8), to refine the way that studies are designed (Chapters 9–11), and to grow or adapt to technologies as they develop (Chapter 12).

As discussed in Chapter 16, the more you look at the ways in which knowledge has developed over the years, the more you feel that it's better portrayed as an inexorable and rather uncontrollable process, predestined almost—a product of being a curious species that discovered a way to bypass evolution by natural selection and thus outstripped everything else on the planet by ever increasing margins, albeit without a very clear end in sight. It certainly looks much less reliant on individual researchers than history books would sometimes have you believe—that is, if the famous names hadn't existed, the knowledge would have been obtained at some point by someone else. On the other hand, the research community over the years has contained its fair share of influential individuals who have played a major part in helping the process along, however inevitable. So perhaps it can be seen as both a product of individuals and underlying forces—the same complexity that Tolstoy was musing on with historical events in the early nineteenth century (Chapter 6), and just as light can be described as both a wave and a particle (Chapter 5). And it does seem worth considering the individuals in the story if we're to put some context around what it is to be a researcher today.

The typical researcher?

To begin with, it's important to recognise that leading researchers have come in all shades of personality—humble and devout (Faraday, Dalton), paranoid and vindictive (Hooke, Newton), profoundly introverted (Cavendish, Darwin), social and flamboyant (Davy, Leclerc), prickly and combative (Rosalind Franklin, with justification), arrogant (Einstein, with justification), and all the rest. As discussed in Chapter 16, power dynamics have sometimes

strained relationships, although it's remarkable actually how little fighting there's been, considering the reputations in play. There are also a beyond-coincidental number of family pairings in the field. For example, the English William Henry Bragg and his Australian-born son Lawrence Bragg shared a Nobel Prize for their work in X-ray crystallography, and George Paget Thomson won this for his work on electron properties, following the award to his father Joseph John Thomson 31 years earlier for the same line of enquiry. Wife–husband pairings have included Marie and Pierre Curie, Irène (their daughter) and Frédéric Joliot-Curie, Marie-Anne and Antoine Lavoisier, Maria and Gottfried Kirch, and sister–brother pairs include Caroline and William Herschel, and Sophia and Tycho Brahe.

So, if the field is a broad one, what makes for a good researcher? Well, I think the career itself shapes and filters its members to some extent, and those who stay are the folk who find it fulfilling enough to make up for the challenges, although the culture at a given department or institution is likely to have a role in this self-selection. I guess that's the same for any line of work. There are plenty of positives about research as an activity, although I suspect a lot of the job satisfaction (again, like any career) relates to line managers, colleagues, and the day-to-day working environment, in any order of priority. Beyond that, I suspect most people would say that moments of discovery are a key fulfilment—actually finding out something new that wasn't known before, however small and specific. And then there are other positives, like the opportunity to work with and visit colleagues overseas, and the absorbing and self-generating, longer-term nature of the discovery process itself (finding out one thing invariably leads to a cluster of further questions to be addressed). These can often be lost among all the short-term hassles and daily tasks, but there are generally moments (e.g. after a paper is accepted, following a presentation that went well, having delivered some successful and appreciated teaching on your subject) when you remember how enjoyable and fulfilling your job really is. That's my story anyway—if you're interested and still exploring the career, take the opportunity to talk to as many people as possible, or at least to those whose opinion you respect.

Facing facts—what lies ahead in a research career

So, what about the challenges? To begin with (and as discussed in Chapters 13–14), it's probably essential to be happy with the idea of academic writing because there's a lot of it. I've known people who aren't keen on writing but manage to scrape through to a level of seniority where they

can find junior researchers to do it for them. However, this situation is rare, and it seems a strange career to be choosing on that basis. It doesn't mean that you have to be naturally gifted or a good writer at the start, but it is something that you'll have to work on because practice (with feedback) is really the only way to achieve this. An ability to give clear verbal and/or audio-visual presentations is also helpful but probably less important—or at least there are certainly plenty of successful professors who don't seem to have picked up presentation skills along the way. And the need to attract funding rather depends on what discipline and setting you're working in— some institutions and research fields expect this more than others. On the subject of funding, do be careful that obtaining grants doesn't become too all-consuming an activity—it's quite possible to become very successful at this but to lose all sense of advancing knowledge, moving from grant to grant without really achieving much by the end of it all. As discussed in Chapter 16, there's something about attracting funding that's opportunistic and that can distract from central research questions that need answering, so a balance is needed somewhere along the line.

Dealing with rejection is certainly something you'll have to get used to in a research career, as it never really goes away. It's often said that if you aren't having your research papers rejected very often then you aren't aiming high enough—whether that's true across all disciplines, I'm not sure, but it sounds as if it ought to be. It's never easy because the rejection either comes with little more than a brief note from the editorial office saying something bland about just not having enough space, or else it's accompanied by reviewer comments picking holes in your meticulous submission.[1] I don't think there's a universal way to deal with this sort of disappointment and probably everyone in the business has their own personal strategy. The sadness, annoyance, and dented self-esteem usually fade quite quickly, so one approach is just to switch off work and do something enjoyable that evening to distract yourself. Definitely try not to get angry and paranoid, and don't slip into thinking that you can guess the names of your anonymous reviewers (people have investigated this and found that the guesses are nearly always wrong). With research papers it's best to start thinking as soon as possible about the next place to try, so that you can refocus on something more positive and regain the affection that you originally had for your submission. Also, any decent-quality research should get accepted somewhere in the end, so it's just a matter of picking publishers who might be interested and sending it around. The rejections that

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[1] Or, worst of all, reviewer comments that say it's excellent, but the editor rejects it all the same—if they were always going to turn it down, why waste everyone's time by bothering reviewers about it in the first place?

are generally harder to deal with are those for funding applications, where there is sometimes no obvious alternative option and where you may have to go back to the drawing board.[2] In my field, these arrive more often than not late on a Friday afternoon, so you just have to accept that you'll have a gloomy weekend and try your best not to take it out on your friends and relatives. By Monday, things are usually a bit brighter.

   With persistence, a thick skin will develop, although it takes a while and, in the end, no one's completely invulnerable. It's also never pleasant having to do your duty and inform your co-authors or co-applicants about the lack of success. However, you'll get sympathy from anyone who's been in the field for a while, as they'll have gone through the same experience countless times. Finally, if you do find yourself feeling persistently damaged by rejections, you might need to have a think about the career choice, as it very much goes with the territory—although, again, it's no different really to many other jobs that involve touting around and pitching for business.

The a-word—neurodiversity in research

Having described the wide variety of personalities in research fields historically, there are some traits that do tend to crop up commonly. A lot of core research activity involves repetitive work with painstaking attention to detail, whether you're pipetting in a laboratory, working your way through an archive for your history thesis, carrying out community interviews for a social science project, or running statistical analyses on a dataset. Research careers are therefore well-suited to people who can tolerate and even enjoy that level of repetition and sustained concentration—abilities that sometimes come at the expense of other characteristics, such as proficiency and/or ease in social situations. I think it's helpful that adult autism is more openly discussed nowadays, although it does still suffer from not having an entirely satisfactory name. I've personally never been happy with 'autism' as a label because there seems to be a danger of appropriating this term from people with a co-existing learning disability who face much harder challenges. But then 'autism spectrum' doesn't feel particularly satisfactory either and no one really wants to name themselves after Hans Asperger nowadays because of his associations with Nazism. However, it is what it is, and at least there's a

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[2] Although if the funder is interested enough in your rejected proposal to offer a follow-up conversation, do take them up on it. There are plenty of stories of people being allowed to resubmit and being successful on a second attempt following one of these discussions.

bit more advice and support on the matter than there was when I began my career, regardless of what you enter as your search term.

The relatively recent terms 'neurodiversity' and 'differently abled' are helpful, I think, and particularly apt for this cluster of characteristics or experiences that might be called autism or autism spectrum. What I mean by this is that the cluster includes skills that can be very valuable for research work as well as disabilities that present challenges for other elements of the career. An eighteenth-century scientist like Cavendish could be extremely shy without too much impact on what he wanted to achieve, because he was wealthy enough to work alone at home with his employed assistants (with whom he preferred to communicate via written notes)—this helped him build working relationships with fellow researchers at the Royal Society until his formidable intelligence became more widely appreciated and he could be accepted for who he was. However, he clearly found the collegiate researcher lifestyle a strain, as he was sometimes observed hovering outside the door of scientific meetings, summoning up the courage to go in.[3] A lot of research nowadays is straightforwardly meritocratic and dependent on carrying out studies and communicating output, neither of which present any particular challenge. On the contrary, they may be well suited to those who are able to focus on work that might frustrate others. Building up networks needn't be problematic either because they tend to be based around concrete common interests like research questions and resources, rather than social skills. However, there's still a certain element of socializing inherent in the institutions and communities that accommodate research nowadays, and this can be more challenging. If you find that uncomfortable, as many do, it's worth anticipating the issue and figuring out a way through, as there should be no need for it to affect your career—and it would be a shame if it did, bearing in mind the other advantages you may have.

Having lived with this cluster, whatever you want to call it, since before it was properly recognized or before there were support resources, I wish I had received useful advice. However, the problem of growing up with an unnamed condition is that you become an accretion of all the coping mechanisms you've built up over the years without being aware of them, not all of which are necessarily helpful. And then you come to a rather late realization and feel that it would have been nice to have known earlier and to have had that chance to anticipate, rather than react to, difficulties. Personally, I would suggest viewing social events in much the same way as rejection letters— just a necessary component of the career that's challenging (for some) but

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[3] That, and running away to avoid encounters with women he didn't know.

that has to be faced, nonetheless. Others coming to the field more recently and with more self-knowledge are likely to be better placed to advise, and (thankfully) there does seem to be more practical support than there used to be, so hopefully you'll have an easier time of things. I similarly have nothing much to say about how on earth the cluster fits in with delivering supervision and leadership, when these become part of your role, except that I think that workable day-to-day leadership might be less about the clichés of charisma and social ease, and more about providing consistency and support/advice that's as altruistic as possible. That, and trying not to take things too seriously.

Leadership and networks

Leadership is another element of a research career that's nearly inevitable, even if it goes no further than project supervision. However, I don't think there's very much here that's unique to research. People generally start careers because they like the work and never imagine themselves becoming supervisors or senior members of staff some way down the line. When this happens, I still think that most people with leadership roles somehow muddle through, finding a style that suits their personality and attracting a team around them who can accommodate this—and probably most of the time it works, more or less. If you can find it, formal leadership training is a useful opportunity to anticipate some of the more specific challenges ahead, or at least to anticipate the situations that you're likely to find challenging. And it's probably fair to say that your periods of greatest stress are likely to come from leadership issues—for example, a supervision that isn't going well, dealing with poor performance, or protecting your team's interests when these come under external threat. Senior academic life tends to become like the circus trick of plate-spinning—you can have any number of plates spinning on their poles happily, but there'll always be one or two that are beginning to wobble and those are the ones that cause the stress. However, again, for leadership as a whole I think you can go a long way by just trying to be consistent and altruistic ('trying' being the operative word).

Beyond your research team and immediate day-to-day contacts, another fact of life is the wider network of colleagues, collaborators, competitors (sometimes people who are all three at once), and general connections that you'll accrue when you've been in the field for a little while. Mostly this is an entirely positive experience, or at least you are hopefully in an environment

where you can ignore the people you don't get on with.[4] However, your networks will tend to generate informal expectations and extra work, so it's worth being prepared. I remember hearing a professional author talking once about a 'favour bank' in his work, which I think often underpins research communities. The idea is that if you do help someone out with something, it's reasonable to expect some help from them in the future, so you might well go the extra mile in order to have that favour 'banked'. Similarly, if you ask a favour of someone and they kindly help, you've placed yourself under an informal future obligation to them that they can draw on at a future date. It's nothing more complicated than that, although perhaps a bit more transactional than an informal friendship. You'll find, of course, along the way that there are people who don't play fair—that is, who are known for not helping out much and yet still expect people to collaborate with them. The main thing is just to know this and what to expect. Thankfully there aren't too many non-players, and most colleagues are pleasant and helpful when they can be (everyone's busy). I think this helpfulness is likely to become more common with time, as collaborative networks are an increasingly important feature of research, and the natural selection process is likely to favour those who are known to play fair.

Another common issue that can cause friction in academic communities is attitudes to deadlines. Someone older and wiser once shared their theory with me that attitude to timekeeping was the only absolute compatibility factor in romantic relationships—that the parties either had to be both inclined to lateness, or both punctual; any other personality trait could differ and still be manageable, but not that particular one. I suspect that's an exaggeration, but there's some truth in it, and it applies to academic life as well—although you have less choice over who you work with. Research careers do tend to involve collaboration and collaborations do tend to involve deadlines, whether these are for funding applications, submission of material for publication, preparation of slides in advance for presentations, or simply turning up for meetings. The issue of last-minute, tight-deadline funding applications was discussed in Chapter 16 as a courtesy issue, but some people do seem to operate that way and if you're more of a plan-ahead type of person you might find this something of a strain. Alternatively, you might be someone who works better to quite a short deadline and find yourself frustrated with gradual, longer-term planning activities. Ultimately, however, you're unlikely to change a colleague's attitudes or behaviour on this—either you

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[4] If not, have a think about whether you're in the right place; it needn't be this way.

work around the difference, modifying your expectations, or else you avoid each other—I'm not sure there's a middle way.

How to start a research career

Before moving on to consider wider issues, a few words about getting started—that is, for readers who are interested in getting involved in research but haven't really had much experience yet. Finding the right supervisor is probably the most important first step in your process, because no one starts research on their own nowadays—you need someone to help you get going. To some extent, this will depend on what jobs or studentships are advertised, although if you have the opportunity to make informal contacts (e.g. after a lecture or through a supervised student dissertation), then do so. Failing that, many senior researchers will be happy to discuss their work if they can find time to give you an appointment—it's not very often that they get a wholly appreciative audience and most of them will have their eye on recruiting the next generation. Beyond coinciding in interests, it's important to make sure that you're with a supervisor who will support you and has a track record in enabling career development, rather than just someone who'll expect you to do the job and nothing more (or, worse, someone who is known for exploiting their junior staff). So, if you can, do talk to researchers working with that person to make sure that they're right for you. As with any occupation, there are occasionally good reasons to work for a boss who is known to be difficult, so it's up to you, but I think it's still best avoided.

After finding the right supervisor, it's mostly then a matter of following their advice and seeing if there's a way to get on to the first step of the ladder. Remember at this stage, that anything relevant on your CV is a positive—that is, if you don't have any publications yet, it doesn't particularly matter what or where you publish. You should just try to do something to show that you can get through that process. There tend to be two extremes of behaviour that any sensible academic employer is looking out for as warning signs in a current or future employee. The less complicated scenario is someone who is unlikely to have the rigour or patience to stick with a research project. This is usually evident quite soon. If it's missed at interview, it can generally be managed for the period of a fixed-term post, following which both parties generally agree that research as a career choice isn't the right one and can part on good terms. The much trickier situation is when someone is bright and highly meticulous but to the point of obsession and perfectionism. This isn't generally a problem for the research project itself, but it becomes apparent when the papers

have to be written but nothing gets circulated because the researcher never feels it's quite good enough, or there's always some more checking to do. This can be paralysing for a career and very difficult for all concerned, as well as being something that may not show up for a while. There's sometimes a misconception that obsession is a desirable trait in a researcher. As described earlier, it's more the ability to tolerate and enjoy repetitive, detailed tasks that's advantageous—not a dependence on routine or perfectionism. The reason all of this is relevant here is that any publication on your CV shows an ability not only to write, but also to let go of and submit, a manuscript (which may not be a perfect but is 'good enough'), as well as seeing it through the process, responding to reviewers' comments, resubmitting if needed. Therefore, having one or two first-author publications is quite a good sign to an employer that you not only have an aptitude for carrying out a research project, but also you show potential for progression in the career that follows. Importantly, if you do find yourself struggling with a perfectionism or prevarication that hinders your productivity, then you need to seek help and advice.

Beyond identifying a supervisor and trying to find work in your area of interest, I think the remaining steps are likely to be particular to your chosen research field. For some fields, for example, it may be helpful to study for a Master's degree or other qualification to gain additional discipline-specific training. For some, it might be advisable to gain experience as an employed researcher prior to seeking a doctoral studentship; for others, early doctoral training may be the norm. Given the time and effort (and funding) required for a doctorate, it's probably a step to take when you're fairly confident that you want to pursue a research career, although the degree needn't constrain you if you change your mind by the end of it, as many do—there are generally plenty of other job opportunities. For example, some researchers enjoy working in their field but prefer to move into more management-oriented positions, rather than the traditional academic posts with all the attendant pressures of university expectations and metrics. And research projects nowadays, particularly the large ones, often need effective management considerably more than they need senior academics. Alternatively, some people find themselves preferring teaching to research and may modify their post-doctoral career accordingly. I suspect that the opportunities will broaden further as time goes on and it's possible that university lecturers and professors may become antiquated posts in the future as research careers become focused on skills and experience rather than qualifications and titles. Time will tell . . .

On the danger of conclusion chapters

Conclusions from books that have attempted very broad historical sweeps are varied, but interesting as contexts for the way research has evolved. Yuval Noah Harari, in *Sapiens: A Brief History of Humankind* (see Chapter 3), outlines a series of thoughts about the history of our species; these include a suggestion that what we like to call our 'progress' has not necessarily been accompanied by greater species well-being, and has certainly come at considerable cost to other species, and to the world in general. Bill Bryson, in *A Short History of Nearly Everything*, draws similar judgements about our impact as a species, illustrating these with the near simultaneity of the start of the Enlightenment in London (the ideas being put together for Newton's *Principia*) and the extinction of the dodo in Mauritius. Andrew Marr in his *History of the World*, on the other hand, contrasts the enormous advances and transformations arising from research with the fact that we still haven't figured out a political system that works. John Gribbin, in *Science: a History*, discusses research shaping our view of ourselves, starting with the time 500 years ago when the Earth was believed to be the centre of the known Universe, and Man a divinely-chosen species, and ending today with the Sun as an ordinary star in a suburb of an ordinary galaxy, and *Homo sapiens* as a species containing the same elements that are most common across the Universe as a whole (with no major differences chemically from any other life form). And the excellent 20-minute *history of the entire world, I guess*,[5] having taken us up to the present, with the Earth in its troubled state of pollution and inequality, decides to finish off with a comment on runaway technology that seems to have lost any sense of purpose: *"'Let's invent a thing inventor", said the thing inventor inventor, after being invented by a thing inventor; that's pretty cool; by the way, where the hell are we?'*

   In preparing examples to illustrate research principles and practice for this book, I deliberately chose to look back quite far in history. There were various reasons for this:

1. The principles themselves are so long-standing and have been with us for centuries/millennia, even if some of them have been formulated and refined more recently.
2. We belong to a long tradition of essentially similar people trying to advance knowledge in their many and varied settings.
3. I don't feel it's my place to write about researchers who are still living.

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[5] https://www.youtube.com/watch?v=xuCn8ux2gbs

4. As any good historian will tell you, it's risky to write about anything too recent because of the lack of perspective.

Having said that, I think a concluding chapter does allow some licence for reflection on more recent events and how they reflect or change some of the principles we've been discussing.

Where we are now—lessons for/from the COVID-19 pandemic

To give some context, at the time this book is being finished it's the middle of 2022 and London, my city, has emerged from a sizeable wave of omicron-variant COVID-19 infections. Hospital admissions with COVID-19 have been high but not as severe as the delta-variant 'second wave' that we saw in early 2021 when few people were vaccinated, and large numbers were very ill or dying. The problem recently has been more the level of staff absence because of infection or contact with infection, in healthcare as well as other sectors, so it's still not a good time to need medical attention and many people are continuing to lie low and self-isolate. We've been living now with the different waves of the COVID-19 pandemic for over two years, although there's a cautious hope that we might have passed the worst of it. This is exactly why it's not a good idea to write about recent history[6] because it could all look very foolish and naïve by the time this is published, let alone by the time you're reading some battered old copy in a second-hand bookshop.[7] All the same, from this particular perspective, the experiences of the COVID-19 pandemic illustrate a few of the principles we have discussed.

On the scientific side, the boundaries of our knowledge are clear and, with all the world's expertise at our disposal, there are still important challenges that may, for all we know, be unsurmountable. At a basic level, I think we still don't fully understand the route of transmission. When the pandemic began, there were concerns about both airborne and contact transmission, so we were advised both to wear masks and wash hands. Two years on, airborne transmission is more of a focus, although I haven't heard of anyone disproving the contact route and I know people who still wash supermarket vegetables and wipe down other items. Not that there's any problem with this; it's good, if perhaps excessive, general hygiene and it might reduce the risk of all the other infections knocking around. Also, it's quite difficult to imagine how actual

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[6] And I'm only going to write about the COVID-19 pandemic as an issue here; I'm not going to attempt to cover current conflicts and the world economy, which are very much still in flux.
[7] Or whatever the digital equivalent is.

transmission might ever be directly measured, or at least to an extent that would refute one or other of the possible mechanisms. Instead, the inferences are derived from what information is available on known transmission events and we're left resorting to the sort of reasoning that John Snow used back in the days of cholera (Chapter 1).

So, a lot of the boundaries of what's known are still determined by what we can adequately measure, and advances in this may in turn determine significant steps forward in knowledge. Similarly, there's considerable interest in T-cell immunity but much less known about this because it can't yet be measured directly at scale in the same way as antibody levels. However, we know that antibody levels are limited as a measure for viral immunity systems because they aren't raised for very long after a vaccine and yet immunity lasts for much longer. Or at least there seems to be longer-term immunity to severe disease associated with COVID-19, but we still don't understand as much as we ought to about the severe disease that caused all the deaths early on in the pandemic. We know how it manifested, and we know about certain vulnerability factors such as older age and cardiovascular disease, but it remains unclear why people experienced different severities of illness (e.g. was it just determined by the dose of virus they were exposed to, or were there other factors?), and why vaccines seem to have reduced the risk substantially, even if people are still becoming infected (and re-infected) with the virus. Finally, I don't think anyone's close to figuring out 'long COVID'. So, all in all, there's plenty of scope for keeping Socrates in mind and acknowledging our ignorance. And there's no shortage of blank spaces on the map (Chapter 3).

Chapter 12 discussed how crises like wars have frequently acted to stimulate research capability and there are obvious examples of this in the COVID-19 pandemic, not least the unprecedented speed at which vaccines and some disease treatments have been developed and mass-produced, despite a previous general disinterest by the pharmaceutical industry in vaccines as products. I suspect the period will also be characterized by leaps forward in virology and immunology (see the previous paragraph), although these may take longer to become apparent. Although a lot of the technical developments stimulated by lockdown policies have been in the entertainment sector, there are probably potential crossovers here of relevance to research careers—for example, advances in videoconferencing are likely to have a profound effect on the environments in which researchers find themselves working. On the other hand, as well as the high-quality research that has been stimulated by the pandemic, it would be fair to say that a substantial volume of much lower-quality work has also absorbed time, funding, and journal space. A lot of

this was well meaning: many researchers found themselves unable to carry on their usual work in the initial lockdown periods in the spring/summer of 2020 (e.g. because of laboratory closures) and yet felt that they ought to contribute. Unfortunately, a lot of people suddenly rediscovered themselves as epidemiologists and public health experts[8] and there was consequently a proliferation of rather badly designed online surveys doing the rounds in 2020, most of which I suspect will remain unpublished. Journals were also swamped with COVID-19 papers of sub-optimal quality, which made life hard for those trying to submit more robust data. However, as mentioned, they were (at least) well-meaning attempts, and it does seem to be dying down now as everyone gets back to their day job.

'So-Called Experts'

We discussed the interface between research, politicians, and the public in Chapter 16 and this tension was obviously exemplified during the COVID-19 pandemic, particularly around vaccination programmes and other measures taken to reduce spread. Although important, I think this can be overplayed for research as a whole. Clearly, there are challenges around the interactions between research communities and the public. And clearly, it's concerning when senior researchers become labelled as 'so-called experts' in order to undermine them implicitly without presenting evidence for this. However, I'm not sure whether this can be portrayed as an attack on research in general. The people referred to as 'so-called experts' tend to be those who are involved in telling the general public what to do or who are up against a particular pressure group. I doubt that particle physicists or Tudor-era historians or Molière scholars find themselves commonly referred to as 'so-called experts' on social media. Instead, I suspect their expertise is assumed on the basis of the length of time they've been immersed in their discipline, just as most people assume that a garage mechanic is best qualified to repair a car. So, the controversy does depend on whether your work is likely to affect a wider public and require them to behave or think differently—and, if so, whether there are vested interests and pressure groups who might feel strongly otherwise.

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[8] Much to the bemusement of those of us who had worked in a discipline that had always felt a little out of the limelight. Although, incidentally, if epidemiologists are feeling aggrieved that other self-appointed experts have been taking the limelight, since when did it become appropriate to use social media as a vehicle for *ad hoc* scientific peer review? And how is it in any way supportive for the next generation of researchers to have their papers savaged publicly on social media platforms by epidemiologists in senior and respected positions? Some decency please! You know who you are . . .

The psychology of 'denialism' has been written about extensively[9] and is too complex a subject to cover adequately in a conclusions chapter. From the perspective of those researchers affected, it clearly does represent a challenge because the opposition feels irrational and yet is often highly organized and effective; also, the vested interests are harder to pinpoint than they were when the battles were against the tobacco or sugar industry. As well as my feeling (see Chapter 16) that researchers should 'hand on' their evidence and stay out of these debates, it's important to remember that a lot of the public health (and wider scientific/humanities) traditions we're following are rooted in much more paternalistic and subservient societies that were hardly free of problems to do with discrimination and exploitation of one sort or another. Allying research with old-style authority risks bringing it into conflict with those who, for very good reasons, view old-style authority with suspicion at best.

There also does seem to be at least some paradox when a research community that prides itself on not taking facts for granted but keeping an objective scepticism finds itself in disagreement with sections of the non-research community who apply their own scepticism to the facts presented to them and decide that they don't agree. Of course, the research community will then say that they've built up evidence, and the sceptical group will ask why they should believe that evidence. And then it doesn't take too many steps before the research community is cornered into saying 'well, because we know best', or implying that the sceptics should be better educated, or adopting some other paternalistic, authoritarian position. But equally I can see that it does seem a shame to have had 500+ years of pioneering work by so many amazing people only for someone to come along and say that they don't believe it and would rather return to alchemy, a flat Earth, and the nine planetary spheres. And some of the controversies, such as holocaust denialism, clearly have distinctly sinister undercurrents.[10] The individualism of our age would suggest that those who disagree with academic consensus should be allowed to make their own life choices. However, vaccination programmes and anthropogenic climate change are tricky because the life choices made have wider effects than those on the individual. As I warned, I don't have any answers here.

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[9] For example, Kahn-Harris, K. (2018). *Denial: the unspeakable truth*. Notting Hill Editions Ltd., Kendal.
[10] Hence the term 'denialism' itself has become pejorative and a sensitive issue with those, for example, who call themselves 'climate change sceptics' and would get very angry at being called deniers.

Looking (cautiously) ahead

Returning to more concrete matters, the pandemic may well have important consequences for the way research is carried out and therefore for the life of the researcher. In March 2020, the group I work with relocated overnight from university offices to everyone's homes. Luckily, because we work with data that can be remotely accessed, the research could continue, unlike a lot of colleagues' studies that had to be paused. We therefore transitioned to remote working and were back in that position again in early 2022, having only had about six months since the previous summer of part-time office-based activity. This is obviously similar to the experiences of many people in other lines of work, and questions at the moment revolve around whether we'll ever return to the way things were in 2019. If new opportunities are embraced, then it's possible that the nature of a research group could change fundamentally—no longer confined to a set of university buildings but a wider virtual network. And once wider virtual networks become normalized then the nature of research-hosting universities (or any institutions) may need to be re-thought. For example, the idea of a highly distributed national or international entity hosting research and employing multi-national researchers is a lot more feasible now than it was in the days when face-to-face meetings were the norm and when any virtual meeting was felt to be decidedly suboptimal (and nearly always subject to computer crashes, audio-visual problems, and such-like). As discussed in Chapter 14, the age of the 'virtual conference' has also definitely begun and there will need to be some thought put into what benefits were conferred by physical conferences as these re-emerge, possibly in hybrid virtual-physical formats.

As a broader conclusion, perhaps the most important lesson to be drawn from the COVID-19 pandemic is life's unpredictability. In 2019 it would have been inconceivable that so much research would have to be paused, that so many other research communities would have been enabled to work virtually so rapidly, or that epidemiology, public health, virology, and immunology would have suddenly become such important topics. However, an ever-sceptical research community shouldn't really be surprised by these things because, in our clearer moments, we should be remembering that all assumptions are best kept at arm's length and open to question. The context in which research has been supported and carried out has changed immeasurably, so there's no reason to suppose that anything we take for granted now will be static—universities as hosting institutions, current funding mechanisms, peer-review as a means of vetting publications, the doctoral degree as a rite of passage for an academic career, or the very idea of research groups being

geographically co-located. Most research fields and disciplines haven't always existed, so there's no particular reason why they have to continue indefinitely. While it feels unlikely that a discipline like mathematics will cease, it's theoretically possible that every known particle will be discovered, leaving nothing more for the CERN Large Hadron Collider to do, or that the causes of Alzheimer's disease will finally be understood, or that there'll be nothing more to write about Shakespeare than has already been written. So, the topics may come and go. On the other hand, the principles underlying research don't seem to have changed appreciably for the entirety of human history, and it therefore does seem reasonable to assume that they are genuinely fundamental—or at least it would be quite surprising if someone came up with a better way of doing things after all this time. Perhaps you should try.

Bibliography

Although this book is about research, I hope I have made it clear that it is not an academic text itself, and that it has never had any aspirations to be so. For the historical elements, I have not done what every good historian should do and sought out original source material. Instead, I have taken what I believe and hope to be reasonably uncontested (and often well-known) historical facts and biographical material and have assembled them to suit my purposes—that is, to illustrate the particular points I am making. It is also clearly not intended to be a comprehensive history of science or any other type of research, as you will find plenty of examples of important figures who are mentioned only in passing, or not at all.

Where I happened to pick up controversy or uncertainty around a particular historical or biographical fact, I have tried my best to imply this in the language used. If there are any errors or over-simplifications, I do apologize, although I don't believe that any of my arguments rely heavily on any one particular fact, so I hope that the conclusions still stand. However, do let me know if you spot anything demonstrably incorrect in what I claim—I'm very happy to be instructed by those with more expertise. Likewise, I have tried to indicate as clearly as possible where I am stating a personal opinion or impression, and again, while I hope you can follow the line of thinking, you're very welcome to disagree.

In conceiving this book, I was strongly influenced conceptually by a number of other authors who have taken relatively broad, light-touch sweeps across history or other fields and who have therefore drawn their conclusions (as I have) from overviews rather than detail. Of particular help when I was preparing this book was *Science: A History* by John Gribbin (Penguin Books, London, 2002), *Philosophy of Science: A Very Short Introduction* by Samir Okasha (Oxford University Press, Oxford, 2002), and *A Concise History of Mathematics, 4th edn.* by Dirk J. Struk (Dover Publications Inc., New York, 1987). I believe the idea for the book germinated a long time ago when reading *A Short History of Nearly Everything* by Bill Bryson (Transworld Publishers, London, 2003) and cross-disciplinary commonalities were informed by *In Search of England* by Michael Wood (Penguin Books, London, 2000), *A History of the World* by Andrew Marr (Macmillan, London, 2012), *The Story of Art* by E. H. Gombrich (Phaidon Press, London, 1950), and *Europe: A History* by Norman Davies (Oxford University Press, Oxford, 1996), as well as by many *In Our Time* podcasts (BBC Radio 4). Contextual thinking on the development of our species in relation to research principles owes rather eccentric debts to *Sapiens: A Brief History of Humankind* by Yuval Noah Harari (Vintage, London, 2011), *Psychology and Alchemy* by C. G. Jung (Routledge, London, 1953), and *The Golden Bough: A Study in Magic and Religion* by J. G. Frazer (Macmillan Press Ltd., London, 1922), among others I've forgotten.

I have been fortunate to have had the opportunity to think about broad research theory through teaching duties over the years and through contributing as an author and co-editor to *Practical Psychiatric Epidemiology* (Oxford University Press, Oxford, 2003, 2020). It was a long time ago, but I believe my first teaching materials on research theory drew strongly on the overview in *Modern Epidemiology, 2nd edn.* by Kenneth J. Rothman and Sander Greenland (Lippincott Williams & Wilkins, Philadelphia, 1998).

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