A Guide for Independent-Minded Scientists

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Contents

Foreword

By Director General, Forest Research Institute Malaysia (FRIM)

I would like to thank Dr Francis Ng for this book. Dr Ng joined FRIM in 1964 when the last of the colonial British scientists were preparing to retire. He himself retired in 1990 when he was Deputy Director General of the Institute, and joined the Food and Agriculture Organization (FAO) of the United Nations. After that he was Director of one of the divisions of the new Center for International Forestry Research (CIFOR) in Indonesia before settling down as an independent consultant. Throughout his career Dr Ng has maintained a close working relationship with FRIM in various capacities, as advisor, trainer and consultant.

Modern science began as an intellectual discipline 400 years ago in Europe. It was brought to the tropics by colonial scientists as part of the colonial drive to understand, control and manage global natural resources. With the end of colonialism, the control and management of tropical natural resources was passed on to the newly independent developing countries of the tropics. However, the contributions of developing countries to the growth of scientific knowledge have been feeble in the face of global climate change, environmental degradation and threats to food and economic security and public health. This book reviews the challenges that scientists in developing countries face and lays out possible remedies.

From history, we know that modern science was initiated by independent-minded individuals. Science is now supported by buildings, laboratories, equipment, budgets and growing ranks of employed scientists, but these are not enough. Individual scientists should, on their own initiative, develop the independent-minded spirit needed to drive the search for knowledge.

Dr Ismail Bin Hj. Parlan

Chapter 1

Introduction

Science in its modern form came into existence four hundred years ago as the result of independent-minded individuals pioneering a new way of making knowledge. These pioneers and their followers were eventually recognized as a body of knowledge-workers different from all others, and given the distinguishing title of *scientists*.

The first scientists were self-motivated and self-financing. The practice of paying salaries for scientific research only began after governments realised the power of science as a source of new technologies that could increase national wealth and influence. There are now over eight million salaried scientists in the world.

The world is magical, and the role of the scientist is to unravel the magic. As each layer of magic is unravelled, more magic is uncovered and this process never ends. I have written this book to help readers understand better what scientific research is all about: how it began, the nature of the independent-minded spirit without which the scientific effort cannot flourish, and how to enjoy scientific research.

As a schoolboy, I was fascinated by science, but it was only after obtaining an Australian scholarship to the University of Tasmania that I met scientists for the first time. At university, I began a lifelong inquiry on how scientists work, and how the best become the best.

After graduation in 1964, I was appointed *Forest Botanist* at the Forest Research Institute Malaysia (FRIM). My predecessor, John Wyatt-

Smith, had just left under Malaysia's post-independence programme of replacement of British by Malaysian officers. Dozens of new graduates like me were assigned to technical positions of high responsibility because of the departure of our British predecessors. My job was to manage the plant identification services of the Institute to support the expanding timber industry. This was an immense task because we had almost three thousand different species of trees in our forests—more than the total in all of India and Burma combined.

I felt that, in addition to distinguishing and naming every species, it was important to understand trees as living things, and forests as living communities. However, newly independent countries were advised not to waste resources on 'pure science'. I had no support and quickly discovered that although self-help books were available on almost every subject imaginable—cooking, golfing, making money and even on the art of war, there were no self-help books for scientists in my position. I had read *The Art of Scientific Investigation* by W.I.B. Beveridge (1950) as a student. This was entertaining and inspirational but provided no practical guidance. Later, I read *Advice to A Young Scientist* by Nobel Laureate Peter Medawar (1979), but was not impressed. Medawar presented science in the context of western philosophy, which I found irrelevant because science is independent of western and other systems of philosophy. This is not just my opinion. There is an account by John Casti (1989) recounting how, when Princeton University organised a celebration to honour the 100th anniversary of the birth of Einstein, they searched for eminent scientists, historians of science and philosophers of science, to grace the occasion. That was when they discovered that the scientists at Princeton did not know the philosophers of science and had no interest in their philosophical arguments.

Back to my story: it so happened that my predecessor, Wyatt-Smith, had advised the British Government to sponsor a project to update the documentation of all the species of trees of Malaya. A British botanist, T.C. Whitmore, was sent to FRIM to initiate and manage the project. I was assigned to understudy Whitmore.

I explored forests all over the country, spending about one week per month in forests, scanning the canopy with binoculars, collecting flowering and fruiting specimens during the day, and writing notes by candlelight at night. In deep forests, it gets dark early and the air is so still that the candle gives a steady light for reading and writing. During those nights, quiet except for the frogs, crickets, cicadas and night birds going about their business, I pondered about tropical trees as living things and forests as ecosystems of high complexity.

Whitmore arranged for me to get further training in England under a British scholarship. He contacted his own professor, the world-renowned tropical botanist E.J.H. Corner of Cambridge University. Corner already had his hands full with PhD candidates and recommended me to Frank White of Oxford University. Frank White agreed and arranged for me to be enrolled at the newly-established Wolfson College, Oxford, of which he was a Fellow. Frank White was well known for his work in East Africa and he was the perfect supervisor for me. My wife and I spent three happy years at Wolfson College in Oxford in 1968-1971. On completion of my PhD (known as D.Phil. in Oxford), I returned and took over leadership of the Tree Flora of Malaya project from Whitmore (Whitmore & Ng 1972-1989). Corner had been my external examiner and he continued to mentor me from afar. With a small team of botanists, including volunteers located overseas, I completed the Tree Flora project in 1989. The project took twenty-four years, and turned out to be the largest feat of species discovery in the country. Our team documented a total of 2830 species of trees, of which 315 species were new or probably new to science.

I carried out independent research of my own choice by working extra hours every day and by 1976, I had published 26 papers, mostly in the journal *The Malayan Forester.* Getting published was not a problem because I was the journal's honorary editor, having been appointed upon the recommendation of its departing British editor, Brian Mitchell, in my first year of service. But I had no idea if my papers were making any impact anywhere. Then in 1976, I received an invitation from Prof. P.B. Tomlinson to speak at a symposium at Harvard University, with all expenses paid by the Cabot Foundation. At Harvard, I found myself in the company of 27 leading botanists from around the world. I already knew many of them by their reputations. Each of us had been assigned one topic to speak on. Our papers were published in a book *Tropical Trees as Living Systems* (Tomlinson & Zimmerman 1978) by Cambridge University Press.

In 1978, I was sponsored by the French Government to speak on seedlings at a botanical symposium at the University of Toulouse. In 1982, I was sponsored by the British Ecological Society to speak on tropical forest ecology at an international symposium at the University of Leeds. Then in 1985, I was sponsored again by the French, to speak at an international conference on the subject of trees, at the University of Montpellier, organised by Prof. Francis Hallé.

At FRIM, I was successively head of botany, then head of biological research including entomology, mycology and plant physiology, then head of plantations research, which included soils, watersheds and silviculture, and finally I became Deputy Director General of the Institute. Nevertheless, I made it a point to spend one or two hours each day on research of my own choice.

At the end of 1990, I accepted a post in Rome at the headquarters of the Food and Agriculture Organization (FAO) of the United Nations. Before leaving, I finalised my two-volume *Manual of Tropical Forest Fruits, Seeds and Seedlings* (Ng 1990, 1991) describing the morphology, anatomy and development of fruits, seeds and seedlings of 600 species of trees, representing 300 genera in 86 families. This was the biggest work on tropical fruits, seeds and seedlings in the history of botany. I had started working on this in 1965 as a fringe activity in parallel with the *Tree Flora of Malaya*. In the course of this 25-year research, I produced thousands of seedlings that I made available to property managers to plant in urban spaces all over the country. My work resulted in hundreds of indigenous species being introduced for the first time to urban planting in Malaysia and Singapore.

At the FAO I was in charge of its new Forestry Research Education and Training Service, established to help developing countries upgrade their research, education and training in forestry. This turned out to be a hopeless mission. Research projects started with foreign funding would collapse when the funding ended, thereby generating more demand for foreign funding. There was a lack of independent-minded scientists to sustain scientific efforts without foreign funding.

In my fourth year, I left FAO to head one of the divisions in the new Center for International Forestry Research (CIFOR) located in Bogor, Indonesia. There, I established its Research Support Division. After completing this assignment, I resigned and returned to Malaysia, where I took part in a series of research, plant-exploration and horticultural activities, and completed a 360-page book *Tropical Horticulture and Gardening* (Ng 2006). In 2009 I received an international award—the David Fairchild medal for plant exploration—at Miami, USA. This, briefly, was my journey as a scientist.

This book is based on my personal experiences and readings in science. It is offered as a guide, not as a manual. Readers should evaluate the ideas in this book according to their own circumstances, and what makes sense in one situation may not make sense in another.

In the training of scientists, the preparation of the mind for discovery should be given highest priority, but there are no training manuals available except on narrowly-focused topics such as plant taxonomy, molecular biology, and statistical methodology. Such narrow training produces technicians without a comprehensive and inspirational understanding of science. In countries with a well-established scientific culture and environment, new scientists are guided by the general environment in which they work. Where such an environment does not exist, narrowly trained scientists tend to remain narrow in their understanding of science and this limits their performance on the global stage.

It is commonly assumed that there is a well-defined *scientific metho*d that scientists learn and apply, but there is no such method. The scientific method has even been called a myth (Bauer 1992).

Modern science began with the inquiries of Galileo Galilei (1564-1642) and William Harvey (1578-1657). Galileo and Harvey emphasized observation and experimentation as sources of evidence for the making of knowledge. This method was new and revolutionary 400 years ago, when the prevalent method of inquiry was the method of disputation, in which knowledge was advanced through arguments between learned people. Hence the method of science may be called the method of inquiry by observation and experiment as opposed to the method of inquiry by disputation.

Some have described the scientific method as the making and testing of hypotheses, but Isaac Newton, in an essay entitled *General Scholium* in his monumental book *The Principia* (2nd Edition, 1713; Motte 1995) vehemently declared *hypothesis non fingo*, (I do not pursue hypothesis), to emphasize that in his research, he took care to avoid being biased by any preconceived hypotheses. However, Einstein, who eventually overshadowed Newton, was a master of 'thought experiments' which are hypothetical experiments. From these, he made incredible predictions that he himself did not have the resources to test, but which generations of scientists have tested and found to be true.

Some claim that the scientific method is the introduction of mathematical precision to science, but the degree of precision applied in science is flexible, depending on the nature of the investigation. Science deals with reality, which has real costs and limits. Scientists work to an appropriate level of precision and do not waste time and money on unnecessary precision.

Many important discoveries have been break-through discoveries, so called because they break through the intellectual walls that prevent thinking 'out of the box'. In such cases, luck and instinct, difficult to explain, and impossible to predict, play important roles.

When the Royal Society of London was established in 1660, it took for its motto: *Nullius in Verba*, which is Latin for "Take nobody's word for it". Following this motto, the method of science may also be described as the sceptical method. All claims made through scientific inquiry are required to be verifiable independently by any competent person.

Whereas there is no simple way to define what scientists do, the core interest of all scientists is the same —to get as close to the truth as possible.

In this book, I have to drag my readers through botany and tropical rain forests, which happen to be my fields of research, but other scientists should be able to relate my experiences with parallel experiences of their own. Readers will find the opinions here simplistic, because my purpose is to help scientists formulate their own research strategies without being bogged down in endless disputes. Most importantly, I hope to make scientific research attractive and intellectually satisfying for those who are interested.

Chapter 2

Theory

This chapter discusses the role of theory in science. The general public has the misconception that science is a body of facts, and the mass media promotes this misconception with headlines like "*Scientists say that* ……" as if scientists are all lined up to support whatever the statement says. Actually, scientific knowledge consists of a core of tried and tested theories that are unlikely to change, around which is a mass of competing and changeable theories, which is why the textbooks of 20 years ago are obsolete today, and the textbooks of today will be obsolete in 20 years' time. Science is an arena of competing theories, and it is the making and improvement of theories that drives the growth of scientific knowledge.

Before modern science, many technologies already existed around the world, including technologies in agriculture and animal husbandry, construction of buildings and monuments, and the manufacture of earthenware, porcelain, metalware, fabrics and gunpowder. These technologies were developed anonymously. Associated with these technologies were magical theories to explain their origin and power. By magical, I do not mean trickery or deception. Theories were magical because everything about the world was magical and could only be explained by magic. Such pre-scientific theories had no power to generate new technologies.

Scientific theories have the power to generate new technologies because they are based on observations and experiments that can be consistently repeated and confirmed, and for this reason, countries and organizations are driven to invest in scientific research. Such investments are visible in the form of organizations, buildings and equipment, but the most important ingredient is the spirit of science. Every advance in science is traceable to an individual scientist identifiable by name, date and place. The spirit of science is kept alive by recognition of such individual efforts.

The rise and fall of the flat earth theory

As an Asian, I wondered why Europe was the birthplace of science and not Asia. The most likely trigger seems to be the discovery of America by Christopher Columbus, in 1492, that involved the rise and fall of the flat earth theory. Europe was, at that period of history, unable to trade with India and China because the overland trading route was under the control of Islamic powers then at war with Christian Europe. The Portuguese prince, Henry the Navigator, attempted to establish a sea route to India and China. It was a long, terrifying and expensive venture, requiring dozens of expeditions over a period of 70 years before an expedition under Bartholomew Dias managed to reach the southern tip of Africa, in 1488. Progress was slow because of the theory that the earth was flat; if one sailed too far away from land, one would be swept over the edge of the ocean. For safety, the expeditions kept close to the coastline of Africa. Each expedition would sail a little further until the mounting fear exceeded the bravado of the sailors. Each voyage would then turn back in relief, to report that they had not yet reached the edge of the world. The specimens of plants and animals that they brought back and their stories of the people they met showed that the new lands they had reached were still able to support normal life. It would then take several years to organise another expedition of brave men willing to take the risk of going further.

Christopher Columbus believed in the theory that the world was round, without any edge, and that he could get to India by sailing west into the ocean. He persuaded the Spanish court that he could reach India before the Portuguese. The Spanish court, eager to beat the Portuguese, agreed to sponsor Columbus, but after sailing for 20 days in the endless ocean without sight of land, Columbus himself began to lose confidence and his terrified sailors were about to mutiny, when flocks of birds were sighted, indicating that land was near. Following the path of the birds, they reached land, which Columbus thought was India, on 12 October 1492. On their return to Spain, the news had a stunning effect on Europe. The flat earth theory was demolished and the discovery of new lands with peoples, plants and animals previously unknown, demonstrated forcefully that there was new knowledge to be made outside of what was being taught by existing books of wisdom.

The flat earth theory had been reasonable in an age when phenomena in nature could only be explained in magical terms. A flat earth appeared to be more reasonable than a round earth. If the earth is flat, water would flow over the edge and the oceans would be drained dry, but that was not a problem if the water is continuously replenished by magic. This was reasonable at that time because the world's water could be seen to be replenished magically as rain falling from the sky.

The replacement of the flat earth theory by the round earth theory banished the fear of being swept over the edge of the earth. This released an incredible burst of energy that drove the Portuguese to India in 1498, on to Malacca, which the Portuguese conquered in 1511, Moluccas in 1512 and Canton in 1515. Following this, Ferdinand Magellan, who had seen action with the Portuguese in Malacca, led a Spanish expedition out of Seville on 10 August 1519 with 237 men in five ships, to make a complete journey around the world (Bergreen 2003). They crossed the Atlantic, navigated past the tip of South America into the Pacific Ocean and arrived in the Philippines in 1521. There, Magellan was killed in a battle, but one of his ships managed to make its way home to Seville on 10 September 1522 with just 18 men of the original 237. The time elapsed between the discovery of America and Magellan's circumnavigation of the world was merely 30 years.

It so happened that a mighty Chinese fleet had already reached East Africa 100 years before the Portuguese began to cautiously feel their way down the coast of West Africa. The Chinese maritime expeditions, under Admiral Cheng He, were diplomatic missions to establish contact with overseas countries. However, China's maritime expeditions did not have the intellectual impact on China that the European maritime expeditions had on Europe.

The Europeans were held back by the flat earth theory and broke free explosively when that theory was demolished. The Chinese and other people had no such theory. This carries a very important lesson—theories have the power to energise and drive the human mind. Furthermore, theories do not have to be right in order to have intellectual impact.

However, for nearly 120 years after Columbus, the making of new knowledge was dominated by physical efforts in geographical exploration. The centres of knowledge in academia were unaffected. Learned people, known collectively as philosophers (lovers of knowledge), continued in their old ways of developing and imparting knowledge by the method of disputation, involving skills in reasoning and argumentation.

The emergence of modern science

It was only after a long incubation period of 120 years that the making and unmaking of theory began to emerge as an intellectual driving force in knowledge creation, in the 1600s, through a series of dramatic struggles that engulfed European intellectual society. This resulted in the emergence of modern science as a new discipline separate from other intellectual disciplines. The process was initiated by two independent-minded individuals—Galileo Galilei (1564-1642) of Italy and William Harvey (1578-1657) of England. Coincidentally, Galileo and Harvey were connected by their association with the University of Padua—Galileo as its professor of mathematics from 1592 to 1610 and Harvey as a student in medicine from 1599 to 1602, but there is no evidence that they ever met each other.

The theory that the earth is the centre of the universe was then being quietly challenged by a new theory, that the sun is the centre of the universe. The new theory was published by the monk Nikolaus Copernicus in 1543 (English translation by Wallis 1995) in highly mathematical language that few could understand, so it made little impact until Galileo Galilei provided new evidence based on observations using a telescope of his own making.

Galileo was already famous for his discovery of the principle of the pendulum. The story goes that Galileo, then 19 years old, was in the Cathedral of Pisa, when he took an interest in the movement of the chandeliers hanging from the ceiling. The chandeliers were swinging slowly back and forth. Galileo wondered whether the swings kept constant time. Nobody had ever thought about this before. The only timing device that Galileo had was his own heartbeat or pulse. He timed the swinging of the chandeliers and found that each swing took a constant number of heartbeats, and theorised that a pendulum (any heavy object suspended at the end of a string) would keep constant time.

Galileo had become the professor of mathematics at the University of Padua when he learnt of the design of an optical instrument in Holland for viewing distant objects. Galileo was a skilled maker of survey instruments, which he manufactured to supplement his inadequate income as a professor. Galileo immediately experimented with lenses that he shaped himself and fixed to two ends of a tube. His telescopes turned out to be much more powerful than those made in Holland. He saw that there were many more stars than could be seen with the naked eye. The objects in the sky had been given magical properties and the moon, as a heavenly object, was supposed to be perfectly smooth. Galileo found the surface of the moon to be landmarked, like the earth, with elevated features and depressions, and the elevated features cast shadows according to the angle of the sun shining on them.

Galileo also trained his telescope on the planets. One of the planets, Jupiter, was associated with four points of light that looked like stars but these points of light were always associated with Jupiter. Surprisingly, every time he looked for them, they would be in different positions with respect to Jupiter. Sometimes, one or two lights would disappear from view. He plotted their positions on paper night after night, and concluded that they were moons orbiting Jupiter, that would disappear from view when they happened to be behind or in front of Jupiter at the time of observation (Bolles 1997). This contradicted the prevailing belief that earth was the centre of the universe, around which all other objects in the sky—moon, sun, planets and stars—were orbiting.

Galileo announced his astronomical discoveries in 1610 in a publication called *Sidereus Nuncius* (The Starry Messenger) and was immediately criticised for questioning established truths. His critics claimed that what Galileo saw were artefacts created by his telescope. Galileo's response was to invite his critics to see for themselves with his telescope. How could the telescope create artifacts around the planet Jupiter but not around other planets? Galileo's critics were scholars skilled in philosophical disputation and unwilling to accept observation and experiment as sources of evidence.

Galileo's findings provided evidence for Copernicus. Galileo was advised to keep his ideas private but he chose to publish. For publicly challenging established beliefs, Galileo was tried for heresy and sentenced to house imprisonment in 1633. Galileo was by then a well-known intellectual giant and a member of the prestigious Academy of the Lincei, founded in 1603, devoted to the study of natural phenomena (Drake 1978). His trial and punishment had the effect of accelerating acceptance of the sun-centric theory.

Meanwhile, in England, William Harvey had become personal physician to King James I and King Charles I of England. Harvey was troubled by the prevailing theory of blood. Blood was known to be pumped from the heart into the arteries that branched into finer and finer tubes until they disappeared into the flesh. In the flesh, blood was magically regenerated and returned to the heart through the system of veins. Harvey measured the volume and rate of flow of blood in animals and theorised that blood must be circulating within a closed system, with fine vessels, too small to be seen, making the connections between the arteries and the veins. This explanation excluded *magical* regeneration. Harvey's theory was rejected by his peers—senior colleagues in the medical establishment. Western medical thinking was at that time dominated by Galen (AD 129-199) a Greek physician. Generations of doctors had made their reputations by their knowledge of Galen's teachings. Harvey's theory would fundamentally change all that by substituting the magical model with his mechanistic model. To bypass his senior colleagues in the medical profession, Harvey decided to appeal directly to all learned members of society by publishing a book in 1628, in which he described his new theory, with supporting evidence or data consisting of observations, measurements and analyses. His book was in Latin. In English its title would have been *An Anatomical Exercise on the Motion of the Heart and Blood in Animals*.

Harvey feared for his safety, but felt it was his duty to publish 'for love of truth'. His theory was in Chapter 8, which he introduced with the statement: "But what remains to be said upon the quantity or source of the blood which thus passes is of a character so novel and unheard-of that I not only fear injury to myself from the envy of a few, but I tremble lest I have mankind at large for my enemies, so much doth want and custom become a second nature." Harvey was afraid that his opponents might harm him physically and that mankind at large would reject him because it is 'second nature' for people to resist change. Harvey's publication was circulated among learned people throughout Europe. The language of learning throughout Europe at that time was Latin. Harvey's theory gained acceptance although it was only after Harvey's death and after the invention of the microscope that the existence of microscopic blood vessels—capillaries—was confirmed by direct observation, by Marcello Malpighi (1628-1694). Harvey had no idea what capillaries looked like and had referred to them as 'porosities of the flesh'. The human body is now estimated to contain 80 km of capillaries.

Galileo and Harvey were responsible for four revolutionary features that became the distinguishing features of science.

1. To make new knowledge by challenging and replacing existing theories.
2. To establish the superiority of observation and experiment over the method of scholastic disputation for the advancement of knowledge of the natural world.
3. To replace magical explanations with mechanistic explanations that could be confirmed by repeatable observations and experiments.
4. To replace judgement by an elite few with evaluation by the general body of learned people, through publication.

These innovations were carried forward by other great personalities in science such as Isaac Newton (1642-1727) and Antoine Lavoisier (1743-1794). To distinguish the growing numbers of such individuals from traditional philosophers, the word ‘scientist’ was coined in 1840 by William Whewell of Cambridge University, about 200 years after Galileo and Harvey had started the new trend. Whewell derived the word from ‘science’ which was itself derived from the Latin word *scientia*, meaning ‘knowledge’.

The practice of paying salaries to employ full-time personnel for scientific research began when governments realised the power of science as the source of new inventions and technologies. Science has this power because scientific theories are mechanistic, based on evidence that can be confirmed by any competent person. The magical theories of the past had no such power.

The idea of state involvement in the promotion of knowledge and invention was first formulated by the English statesman-philosopher Sir Francis Bacon, who in 1605, addressed to King James I of England, a book entitled *Proficience and Advancement of Learning*, in which he promoted state support for learning, invention and discovery. Bacon’s ideas prepared the way for the formation of the Royal Society of London in 1660 and the French Academy in 1666.

In 1675, King Charles II established the Royal Greenwich Observatory for research in support of astronomy and navigation. This was the first government scientific research laboratory in the world, and it contributed to the rise of Britain as a naval and colonial power.

UNESCO estimates that there are now over eight million scientists employed worldwide, mostly in national research institutions and government-supported universities.

Because scientific theories are based on evidence, and evidence can be interpreted in different ways, theories can come in competing versions. One scientist may say that a particular food is good for your health but another may say that it is bad for your health. These are competing theories, both supported by data but interpreted in different ways. All over the world, thousands of PhD candidates train to be scientists by examining a chosen topic each. These candidates evaluate the theories pertaining to their topics. A thesis that confirms existing theory is not rated as highly as one that finds fault with existing theory and offers a better one. This critical attitude differentiates modern science from all other fields of knowledge. It is expressed in the motto of the Royal Society of London as *Nullius in Verba*, which is Latin for 'Take nobody's word for it'.

The importance of scientific theory comes out clearly in the case of gunpowder, which started as a Chinese discovery. When it reached the West, its magical properties caused a lot of excitement and curiosity. Through the process of making and testing mechanistic theories about gunpowder, the nature of explosive forces was understood, opening the way to the development of more powerful explosives, and to the use of controlled explosions to drive rockets and jet propulsion engines.

Magnetism was known in ancient China and ancient Europe and its mysterious ability to attract and repel could only be explained by magic. The Chinese invented the magnetic compass, and had magical theories about it but it was in the West that mechanistic theories and experiments led to the connection between magnetism and electricity, resulting in the invention of dynamos, transformers and methods for generating and distributing electricity.

The replacement of magical explanations with mechanistic ones proceeded at different rates in different areas of knowledge. Isaac Newton, a pioneer in physics, also immersed himself in experiments with chemicals but was unable to escape from the magic of alchemy. He had one foot in the emergence of physics as a science and the other foot in the magic of alchemy. In his biography of Newton, Michael White (1998) labelled Newton 'the last sorcerer' before the magic of alchemy was replaced by the science of chemistry through the work of Antoine Lavoisier and his colleagues. In their 300-page *Methode de nomenclature chimique* in 1787, Lavoisier's team grouped chemicals by their affinities as determined by experiments, and standardised their names following the example set by Carolus Linnaeus for plants in *Species Plantarum* in 1753. 'Oil of vitriol' became sulphuric acid and was grouped together with nitric acid, hydrochloric acid and other acids. 'Flower of zinc' became zinc oxide and took its place among other oxides such as calcium oxide, magnesium oxide, and so on, in a binomial system of naming based on properties established by scientific inquiry.

Even as recently as 1726, an Englishwoman, Mary Toft, had been able to claim that she had given birth to rabbits, and it took considerable effort to debunk her claim because in the 1700s most of biology was magic. The best way to debunk fakery was to demand proof in the form of repetition, and of course, there was no way Mary Toft's claims could be repeated.

In 1720 the Royal Society of London decided to examine the issue of whether reproduction in plants involved sexual interaction. The biography of Thomas Fairchild, a nurseryman and plant breeder (Leapman 2000), describes how Fairchild was invited to a meeting of the Royal Society in February 1720 to discuss the new theory that plants had male and female organs and reproduced through sexual means. The whole idea of sex in plants was revolutionary and even repugnant. Fairchild was able to provide proof in the form of plants that had resulted from cross-mating of the garden Sweet William (*Dianthus barbatus*) with the Carnation (*Dianthus caryophyllus*) in Fairchild's garden. These hybrid plants had mixed features demonstrating, without doubt, their mixed parentage. The new mechanistic theory of sexual reproduction in plants opened the way for experiments by Gregor Mendel to discover the mechanisms by which heritable traits were passed on from generation to generation. This resulted in Mendel's theories of inheritance that provided the basis for modern genetics.

In the Middle East, where the date palm *Phoenix dactylifera*, has been cultivated since about 4000 BC, the palm has been known from ancient times to have two forms, one producing pollen and the other producing fruits. The growers harvested pollen-producing inflorescences and tied them to the fruit-producing inflorescences to ensure a good production of fruits. The growers had great practical knowledge but nothing that resembled a scientific theory and failed to develop any knowledge of genetics.

The principal characteristics of theories in science

The most important characteristic of scientific theories is that they are mechanistic, i.e. based on observations, measurements and descriptions that can be confirmed by any competent person.

Also, scientific theories are universal, because nature is universal. Galileo's theory of the pendulum applies to all pendulums everywhere. Harvey's theory of blood circulation applies to all animals with circulating body fluids. When the capillaries predicted by Harvey were discovered, the discovery was first made in the lungs of a frog, and extrapolated to all other animals with a similar blood circulation system including humans. Theories are made in one place and applied to all other places, or on one animal and extrapolated to other animals with similar body plans. This stretching of a theory is called extrapolation. Through extrapolation, there is a scientific explanation for everything although it is impossible to study everything. The extrapolation of theories in science only applies naturally to the 'natural sciences', not to the 'social sciences' dealing with man-made phenomena like the behaviour of economic and social systems.

Scientific theories are shared by the whole world through publication. Secret knowledge is not part of the scientific body of knowledge.

The principal characteristics of scientific theories may be summarised as follows:

1. Scientific theories are supported by evidence in the form of documented observations and measurements, collectively known as data, such evidence being open to confirmation by any competent person.
2. Scientific theories are universal explanations.
3. Scientific theories become part of the global pool (public domain) of knowledge through publication.
4. The global pool of scientific knowledge is not subject to control by any authority. Scientists may challenge any theory without any time limit.

5. Theories are tested by seeing if predictions based on them turn out as predicted.

The division of labour between theory-makers and data-collectors

In many research organizations, theory is made by scientists and data is collected by technicians under the supervision of scientists. In colonial times, research in colonised countries was done by expatriate colonial officers acting as scientists, with local people employed as technicians.

The colonial division of labour became a neo-colonial division of labour after decolonization when the newly independent countries received support from international development agencies such as the Food and Agriculture Organization of the United Nations (FAO) and the World Bank. In my institute, experts in various fields would come to work on specific developmental projects funded by foreign aid agencies, examine the data we had obtained, and publish papers based on our data. This caused much unhappiness until resolved by including the names of local scientists as co-authors. However, the underlying problem was that local co-authors were unable to make theory. Co-authorship merely provided a cover for a serious and undiagnosed deficiency.

The making of a scientist

Albert Einstein's theories of relativity and Charles Darwin's *Theory of Evolution* are rated among the most outstanding of all theories, influencing the way we think in physics and biology respectively. How did Einstein and Darwin come up with theories that eluded others? Their biographers have searched and found no early hints of outstanding intellectual ability. In fact, Darwin and Einstein were only average-quality students. The fact that they were not outstanding provides an important clue—that they did not consider it important to memorise and repeat taught knowledge with the aim of getting good examination grades. Their independent and sceptical attitudes made them different from the top students in school. In the case of Einstein, his professors in university thought so poorly of him that they blocked him from getting a university teaching appointment. He had to support himself and his wife as a patent examiner at the Patents Office in Bern. They had no income at one time and were so poor that they had to give away their first child, a daughter, for adoption (Brian 1996).

In all other fields of learning, students are taught to respect what they are taught, and examinations are designed to test how well students have learnt their lessons. The scientist's approach to knowledge is to master what is taught but at the same time to look for weaknesses to be exposed and rectified. This is challenging, and the challenge is made more daunting by the fact that any new theory in science can be expected to be strongly opposed by other scientists. Every new and original scientific theory begins as one scientist's idea against other scientists. Any scientist hoping to make impact has to learn how to deal with opposition from within the scientific community itself, and survive the challenge.

In the 1980s when China first began to send students overseas for training after its Cultural Revolution, an Australian professor told me of a Chinese PhD student who came to him, very troubled. The student's field of research was dominated by two eminent scientists whose theories were opposed to each other. The student was confused and did not know what to do. The professor told him he had to consider all arguments and develop his own opinion. In research, scientists, including beginners, are required to take it upon themselves to evaluate the experts in their fields of study, no matter who those experts may be. Otherwise, they themselves cannot become experts.

In science, we respect our teachers and mentors but we are also expected to rise above them. Each generation is expected to make the previous generation obsolete. Scientists have to be independent-minded in order to reach the top in science.

Challenging the experts

The first time I had to challenge a living expert was in my fourth year as a scientist. It was a tense experience. I had completed a study of the genus *Trigoniastrum* and had found that the internal structure of the ovary was different from what was described in all previous accounts of the genus and its family. Specifically, the number of ovules in each ovarian cell was two, not one. This mistake also appeared in the latest account published by Professor C.G.G.J. van Steenis of Leiden. The structure of the ovary provides critically important features for distinguishing plant families from each other, so mistakes are very rare at this level of taxonomy. I was puzzled that my observation contradicted the description by van Steenis and so I dissected a very large number of flowers, but there was no mistake. I was the best anatomist in my class in university but van Steenis was the most formidable expert in plant taxonomy. My own mentor in taxonomy at the University of Tasmania, Dr Winifred Curtis had recommended van Steenis to me with the following words of advice:

> "You could introduce yourself as a student from Hobart and mention my name. But I should explain to you the Professors (and Dr van Steenis also has this title) are, on the continent of Europe, very important people. They are not used to the informality of Australia or even that of England, so make an especially carefully worded approach."

Forewarned by Dr Curtis, I wrote a very careful letter to van Steenis for his advice, and provided him with flowers to check. He did not reply for several months, while I waited with growing anxiety. Finally, he wrote to say I was right, and he published a correction in 1969 (Blumea 17: 270). I suspect that he had earlier copied his description from previous authors without making dissections himself and that he had given my flowers to one of his staff to check. It may have taken his staff several months to build up enough courage to tell him the truth.

PhD thesis are training exercises in which the candidate takes up an existing theory and evaluates it with new evidence. The candidate is expected to find weaknesses in the theory and propose changes to it—perhaps even to replace it with new theory. My own PhD thesis was on the genus *Diospyros*, known for its fruits, which include the oriental persimmon *Diospyros kaki*, and its timbers, which include the ebony woods of commerce. In the Malay Archipelago there are over 190 species of *Diospyros* and they had been the subject of a detailed study by a Dutch botanist, Bakhuizen van den Brink, published in 1936-1941. In his monograph, Bakhuizen divided the genus into five subgenera and many small sections. My task was to evaluate his classification scheme.

I examined all the specimens of *Diospyros* that had been collected from the Malay Archipelago and in my first year, found that four of the species in *Diospyros* had been wrongly placed in *Diospyros*, three by Bakhuizen himself. I managed to place these four species in four other families and published the corrections in a one-page paper (Ng 1969). It was unprecedented for a first-year student to detect and correct four major errors in the works of his predecessors and it immediately established me as the new authority on the subject.

Through my thesis I also identified the progenitor of the cultivated oriental persimmon *Diospyros kaki* as a wild species of Indo China, Thailand and East India where it had been independently described three times, as *D. roxburghii, D. glandulosa* and *D. kika* (Ng 1978).

A beginner's example in theory-making: age at first flowering in dipterocarps

My first opportunity to make theory arose in 1966, two years after I started work in FRIM. It had been my intention, right from the beginning, to discover how trees behave as living things and how they contribute to the social organization of forests. Every piece of information that I could find was a clue that could help me to understand trees and forests.

Among the files and records in the Botany Section there were records of the trees planted in the arboreta in FRIM. An arboretum is a garden of trees planted for special display. One of the arboreta was devoted to trees of the family Dipterocarpaceae. The Malaysian timber industry is based principally on species of this family, referred to as dipterocarps. Dipterocarp trees in the forest are felled in logging operations and the forest has to regenerate from whatever dipterocarp seedlings survive on the ground after logging. These seedlings would take at least 30 years to grow to trees of harvestable size. These trees would, in turn, have to populate the forest floor with their own seedlings, but we did not know at what age dipterocarp trees would start to fruit and produce seeds.

Our arboretum record contained the dates of planting of each tree, and the trees were inspected periodically by staff, who would record observations like the death of a tree or when a tree was flowering or fruiting. This was an informal record, with many gaps. Nevertheless, we had records for fruiting for trees of known age representing about 50 species. I extracted this data (Table 1), which showed a pattern—that at 30 years of age, trees would be mature enough to produce seedlings for the next rotation. My contribution to knowledge was the theory that dipterocarps would reproduce before 30 years of age. I published this theory under the title "Age at first flowering in dipterocarps" (Ng 1966).

Table 1. Age at first flowering of dipterocarps
(a representative selection of 7 species of 50 in the original publication)

Species	Year of seed	Year of first recorded flowering	Age at first flowering (in years)
Neobalanocarpus heimii	1931	1963	32
Dipterocarpus baudii	1933	1957	24
Dryobalanops aromatica	1928	1957	29
Hopea apiculata	1935	1958	23
Parashorea malaanonan	1940	1962	22
Shorea acuminata	1930	1955	25
Vatica nitens	1930	1961	31

By itself, each item of data has little impact. I had data on 50 species, which was a big enough sample to show a pattern that could be applicable to the whole family Dipterocarpaceae. This pattern provided the basis for my theory that dipterocarps would reproduce before 30 years of age. For a few species, the recorded age was actually a little over 30 years but I decided to take a risk in favour of the round number 30.

A theory is much more powerful than the data it is based on because a theory consolidates voluminous data into a statement that is easy to teach and remember. Its power is further magnified by global extrapolation. My theory applies automatically to dipterocarps throughout the world *until proven otherwise*. If I had not made this theory, my paper would have been just a data paper, containing 50 bits of data specific to the FRIM arboretum and having no global meaning or value.

In summary, the process of making theory in this example involved the following activities:

1. Recognising the potential global value of the data.
2. Compiling the data and identifying a pattern.
3. Linking the pattern to a possible application.
4. Publishing to announce and share the information globally.

Theories are not made by accident. A theory is the result of a deliberate decision to make theory. Other staff in my institute had access to the same arboretum records that I had access to, but did not have the same motivation that I had.

This example illustrates why it is of vital importance for scientists to foster the habit of looking at data as materials for making theory. As soon as we have a body of data, we have the opportunity to search for pattern and make theory. This habit should be adopted early and maintained throughout one's scientific career. The cost of research is mostly the cost of collecting data, but if the data is not used to make theory, the effort is wasted. In this case, the data took over 30 years to obtain. The theory was made in a very short time and required no further expenditure.

Theory-making, example 2: length of time from flowering to fruiting

In the previous example, I used data that I discovered in departmental records that were available to me. Example 2 shows how I generated data to make theory.

In most parts of the world, the climate is seasonal, with cold seasons alternating with warm seasons, or dry seasons alternating with wet. Trees rest during the cold or dry season and resume growth when the weather becomes favourable. In particular, trees flower at the start of favourable weather and have six to nine months for the fruits and seeds to mature before the onset of unfavourable weather. This is repeated year after year, so people know, for example, which month to visit Japan for cherry blossoms and which month to go to Tasmania to pick apples without having to make any special study first, to predict the month.

In the humid tropics, it is always warm and humid and most trees do not have fixed times for flowering and fruiting. As a result, if we see a tree in flower and want to collect the seeds, we have no idea when the seeds will be ready for collection unless somebody has already made a special effort to obtain the data and publish it. Tropical rain forest is challenging not only because of the large number of species it contains, but also because the lack of seasons means that almost everything about tree reproductive behaviour has to be discovered independently for each and every species by a dedicated study. I decided to discover the length of time from flowering to fruit-ripening for as many different species as possible. With a Research Assistant, H.S. Loh, we searched for trees in flower on the grounds of FRIM and in accessible forests nearby. I selected the trees and Loh kept the trees under observation until he could bring me the ripe fruits.

I had to define the data carefully to avoid ambiguity in data recording. The flowering of a tree may stretch over several weeks and similarly the ripening of its fruits. To be exact, I decided to start timing at the start of flowering and stop timing when the first ripe fruits became available. The start of flowering was when the first opened flowers could be picked up from the ground below the tree. Fruits were considered to be ripe when they contained functional seeds. A functional seed is one that will germinate and produce a seedling. Some trees, like *Dillenia suffructicosa*, flower continuously, so it was necessary to select and label selected flowers, note the day a particular flower opened and monitor its fruit development until the fruit ripened, which in the case of *Dillenia* was when the fruit split open on the tree to reveal its seeds. Data had to be defined exactly otherwise it would be misleading.

Table 2. Period from flowering to fruiting, in months, for Malaysian trees (from Ng & Loh 1974)

	Species	Time in months
1	*Alangium ebenaceum*	3
2	*Alstonia angustiloba*	1¾
3	*Anisoptera costata*	3
4	*Anisoptera laevis*	5
5	*Aquilaria malaccensis*	3
6	*Artocarpus lanceifolius*	7
7	*Artocarpus lowii*	4
8	*Artocarpus rigidus*	6
9	*Bhesa paniculata*	3½
10	*Bhesa robusta*	3
11	*Crudia curtisii*	5¼
12	*Cyathocalyx pruniferus*	4
13	*Dacryodes costata*	4
14	*Dacryodes kingii*	3
15	*Dialium patens*	8

	Species	Time in months
16	*Dillenia suffructicosa*	1¼
17	*Diospyros maingayi*	9
18	*Diospyros pendula*	5
19	*Dipterocarpus grandiflorus*	3
20	*Dipterocarpus oblongifolius*	3
21	*Dracontomelum mangiferum*	5½
22	*Dryobalanops aromatica*	4
23	*Dryobalanops oblongifolia*	3½
24	*Durio zibethinus*	3
25	*Dyera costulata*	2
26	*Elateriospermum tapos*	7
27	*Erythroxylum cuneatum*	3
28	*Eugenia grandis*	2
29	*Eugenia polyantha*	2
30	*Euodia glabra*	2¼
31	*Fagraea fragrans*	4
32	*Ficus variegata*	1½
33	*Firmiana malayana*	1
34	*Garcinia mangostana*	4
35	*Grewia laurifolia*	2¼
36	*Gymnacranthera eugeniifolia*	5
37	*Hopea dyeri*	2½
38	*Hopea helferi*	2
39	*Hopea nervosa*	4
40	*Hopea nutans*	3¾
41	*Hopea odorata*	2½
42	*Hydnocarpus woodii*	7½
43	*Koompassia malaccensis*	3

	Species	Time in months
44	*Lagerstroemia speciosa*	2
45	*Licania splendens*	7
46	*Litsea rostrata*	5
47	*Melia excelsa*	3
48	*Milletia atropurpurea*	4
49	*Myristica malaccensis*	7
50	*Nephelium lappaceum*	4
51	*Palaquium hispidum*	10
52	*Palaquium regina-montium*	7
53	*Parkia javanica*	4
54	*Parkia speciosa*	2¾
55	*Payena lucida*	3½
56	*Peltophorum pterocarpum*	3
57	*Pentace strychnoidea*	5
58	*Planchonella glabra*	7
59	*Pterocarpus indicus*	4
60	*Pterocymbium javanicum*	¾
61	*Pterospermum javanicum*	5
62	*Santiria laevigata*	4
63	*Scaphium affine*	3
64	*Scorodocarpus borneensis*	4
65	*Shorea bracteolata*	2¼
66	*Shorea curtisii*	4
67	*Shorea faguetiana*	5
68	*Shorea gibbosa*	2¼
69	*Shorea leprosula*	2½
70	*Shorea macrophylla*	5
71	*Shorea macroptera*	2½

	Species	Time in months
72	*Shorea martiniana*	3½
73	*Shorea maxima*	4
74	*Shorea ovalis*	2½
75	*Shorea pauciflora*	2½
76	*Shorea platyclados*	2½
77	*Shorea resina-nigra*	6
78	*Shorea resinosa*	5
79	*Shorea singkawang*	4
80	*Shorea sumatrana*	4
81	*Sterculia parviflora*	6
82	*Swintonia schwenkii*	5
83	*Teijsmanniodendron pteropodum*	3½
84	*Terminalia subspathulata*	4
85	*Vatica ridleyana*	11
86	*Vatica wallichii*	6
87	*Xanthophyllum griffithii*	5

The 87 statements in Table 2 are based on individual trees but automatically, they serve as universal statements. For example, the observation that *Aquilaria malaccensis* takes three months from flowers to ripe fruits can be taken as a general statement for the species.

We know that the time from flowering to fruiting can vary a little from place to place and from time to time depending on weather, age of tree, state of nutrition, genetic differences and so on. Because of such variation, it is impossible to define an exact time. Once a tree of *Aquilaria malaccensis* is observed in flower, we can prepare for seed collection in about three months. I could have planned for such research to cover 20 trees and calculated means and standard deviations but such data would not enable one to predict exactly what the next tree would do or what the same tree would do next year. The work would never get done because in a tropical rain forest containing mixtures of hundreds of species of trees, it is difficult to locate even two trees of the same species close to each other. The only practical way to make progress is to learn from one tree and to extrapolate to its species. In this way, we covered 87 species.

I was happy with this, but many years later, I found that by grouping the data as in Table 3 an interesting pattern appears, which is displayed in the chart in Fig. 1. This pattern shows that for most species, flowering to fruiting is completed in 3-5 months, and the peak is 4 months. Extrapolating from this sample of 87 species to a multispecies tropical rain forest, I could theorise that in tropical rain forests, the time from flowering to fruit ripening is generally 3-5 months, peaking at 4 months. This is a theory about the reproductive behaviour of tropical rain forests that nobody had been able to make before.

Table 3. Distribution of species according to length of time required from flowering to fruit-ripening

Months from flowering to fruit ripening	No. of species	% of total
0-1	0	0
1-2	6	6.9
2-<3	17	19.5
3-<4	19	21.8
4-<5	18	20.7
5-<6	13	14.9
6-<7	4	4.6
7-<8	7	8.0
8-<9	1	1.1
9-<10	1	1.1
10<11	1	1.1
11-<12	0	0
Total	87	99.7

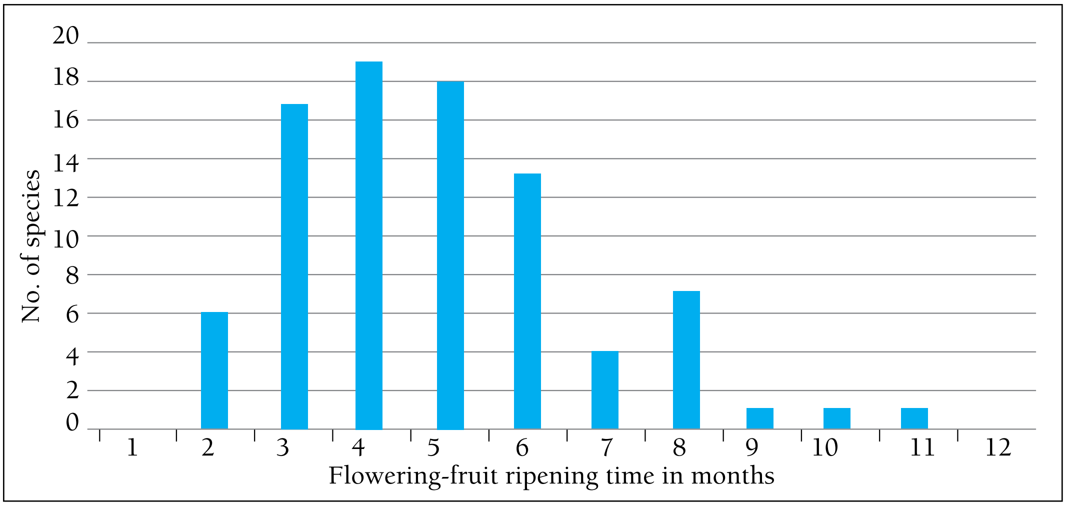

Figure 1. Distribution of species according to length of time required from flowering to fruit-ripening, displayed in a histogram

This analysis explains what happens during a mass flowering event in which the whole forest appears to be flowering at the same time, usually in March-April and ripens its fruits in sequence with a peak number of species in June-July and tapering off in October-November.

This two-year study improved considerably our knowledge of the reproductive behaviour of the forests of Malaysia. I did not need a budget. I did not have to make a proposal for approval, nor make any promises about the outcome. It filled a gap in knowledge that nobody else had done anything about.

In summary the activities involved were as follows:

1. Deciding what data to obtain and setting up the process for obtaining the data.
2. Fine-tuning the data-collecting process according to feedback.
3. Looking for pattern in the data, and formulating a theory to bring out its global significance.
4. Publish.

Selecting a topic for research

In selecting and shaping a study, the following factors should be taken into consideration: (i) whether data can be obtained readily with the resources available (ii) whether the inquiry is original (iii) whether it is possible to modify the inquiry in response to feedback.

Ability to obtain or generate data

When I was a student in Tasmania, another student's research involved an experiment that would be carried out in outer space in a rocket to be launched by the American space agency NASA. He had one year to design the experiment, but at the last moment, the launch was aborted and he had lost a whole year.

In selecting a topic, the most important consideration is whether the data can be obtained readily. The materials, the equipment, the personnel, all should be within the control of the investigator.

Originality

It might appear to be a good strategy to select projects with high impact, but impact is rarely predictable. It is a better strategy to make an investigation that is original.

An early example is provided by Galileo Galilei's study on pendulums, carried out in about 1583 when he was 19 years old. This was a completely original investigation, producing a completely unexpected result. After discovering that a pendulum keeps constant time, Galileo tried to apply the principle of the pendulum to the invention of a clock for keeping time, but failed. The first successful pendulum clock was invented by Christiaan Huygens 73 years later, in 1656. After that, the pendulum clock became the most important of all technological inventions for 300 years, occupying pride of place in every household that could afford one, and decorating clock towers that were the central landmarks of towns all over the world. Clockmaking contributed to the rise of precision engineering, beginning with the invention of clocks that could keep accurate time even when carried on sailing ships in stormy weather. Such clocks enabled navigators to determine their longitudes by time difference from Greenwich.

Freedom to adapt

It is important to have the freedom to modify a study according to feedback. In Oxford University, I got to know many other research students. We all started with bright ambitions but for some, the flame died out within two years and they eventually gave up. At the end of each day, we must feel that we have made some progress, and that becomes the incentive for the next day. If we are unable to make progress, our morale gets progressively worn down.

Research should not follow a rigid data-collection scheme according to a logical design made at the start, when one is a beginner. In the end, if the data turns out to be inadequate, one cannot turn back the clock and revise the plan. To give up would be a humiliating admission of failure. This is a terrible experience for a young person entering university with high hopes. Ideally, PhD candidates should have the freedom to explore and to be evaluated by what they manage to discover, not by their original plan. It is the responsibility of the thesis supervisor to guide the student away from rigid schemes and to explore creatively.

Linking data with theory

Search for patterns. The most important use of data is in the search for patterns. For a pattern to be detectable, there has to be a sufficient number of data points. The pattern may be a cluster around a central point, a straight line, a curve, etc. For my study on the time from flowering to fruit-ripening, I had 87 data points that could be arranged to reveal a bell curve describing what would happen after a mass flowering event.

Galileo's data on the time taken by a pendulum to make a swing could not have been exact because his timer (his pulse) would not have been exact and his reaction time would also not be exact. However, his timings would have clustered around some central value and this central value could be interpreted as a constant value of time while the individual measured values, more or less deviating from the central value, could be interpreted as the results of errors in measurement.

The most famous example of pattern recognition in science is in chemistry, when the periodic table of chemical elements was discovered and published, in 1869. by the Russian chemist Dmitri Mendeleev (1834-1907). What Mendeleev did was to discover the pattern by arranging and rearranging the known chemical elements until a rough pattern of repetition of chemical properties appeared. The periodic table of elements initially had many holes. It turned out that these holes were elements that had not yet been discovered. As these holes were filled one by one by the discovery of previously unknown elements, the effect was sensational. The proof of a theory is when predictions made with the theory turn out to be exactly as predicted, and Mendeleev's periodic table was the best example of this.

Recently, I was involved in the discovery of an amazing pattern, that in South East Asia, the Malay Peninsula has the highest concentration of species for nearly all families of plants and classes of vertebrates. Concentration or intensity is expressed by the number of species in a region divided by the land area of the region. Nobody else ever thought of doing this simple mathematical exercise before. Previously the emphasis had been on the number of species in each country, which enabled countries to be ranked according to the number of species they contained. Our discovery came as a surprise and it suggests that the Malay Peninsula has been an area of intensive biological speciation.

Dissections and drawings. A drawing is a pictorial pattern of relationships. In anatomy, theories about how the different parts of the body relate to each other are based on anatomical studies, recorded in drawings. Drawing sharpens and guides observation because anything that is not clear enough to be drawn would have to be clarified first by dissection. Details that are difficult to see would have to be carefully exposed by dissections and drawn to show clearly the boundaries between different tissues and organs.

My books on fruits, seeds and seedlings (Ng 1990, 1991), which cover over 600 species in 300 genera and 86 families of tropical trees represent one of the biggest studies on plant anatomy ever carried out. It took about 25 years to complete. When I was doing this work, I was asked why I did not employ an artist to do the drawings. This was impossible, because to make a scientific drawing one has to identify and distinguish clearly the different tissues from each other. Every line is a precise boundary between two tissues that has to be clarified by careful dissection. Every drawing is a theory about how the tissues are organised. As a result of my anatomical explorations on 86 botanical families, I developed such an understanding of plant anatomy that as soon as I dissected a flower of *Rafflesia* I sensed that the classification of *Rafflesia* as an angiosperm was a big mistake (Ng 2019).

Photographs cannot show the fine details that one can depict in a drawing, but photographs complement drawings because the camera captures other kinds of visual information that an artist might not think of including in a drawing.

Tables. A table is an arrangement of data to bring out patterns. For example, Tables 1 and 2 display the same set of data in two different ways. In Table 2, the species are arranged in alphabetical order; this is useful for searching species by their names but does not reflect reveal any natural patterns. Table 3 shows how the species are distributed by the period from flowering to fruit-ripening, revealing the behaviour of a multispecies community.

Maps. A map is useful for displaying data that are spatially related to each other. In comparing geographical maps made at different times in 1400-1700, we can see how maps became better and better as more information became available. Every map tried to depict the world and what was not clear was filled in by extrapolation and imagination. In other words, maps were like scientific theories. The travellers and explorers who used these maps passed from known to unknown regions and the new information they obtained helped to improve the next generation of maps. By the time Harvey published his work on blood circulation in 1628, the Europeans already had good maps of the world, because in 1488 the Portuguese had reached the southern tip of Africa, Christopher Columbus had landed in America in 1495, the Portuguese had reached India in 1498 and captured Malacca in 1511. Magellan's expedition then made a complete round trip of the world in 1519-1522.

Physical model-building. The most famous example of model building was the physical three-dimensional model of DNA that was built by Watson and Crick using components of various shapes and sizes to represent the various components of DNA. By trying to fit them together they arrived finally at the only possible solution, which was a double helix.

Graphs. The use of graphs to display data is an application of Cartesian geometry, a branch of mathematics developed by René Descartes (1596-1650) who was a contemporary of William Harvey (1578-1657) and Galileo Galilei (1564-1642). The story goes that Descartes was lying on his bed gazing at the ceiling when a fly landed on it. Descartes wondered how he could describe exactly the position of the fly. His solution was to regard the ceiling as a rectangle with adjacent sides at right angles to each other; the fly would occupy a position that can be defined by its distance along each of the two sides, which we refer to as the x and y axes.

I saw an interesting application of Cartesian geometry when my wife completed a jigsaw puzzle and one piece was missing. We went to the shop to complain and were told to identify the missing piece by counting the pieces from the top and the pieces from the left. The missing piece was in 10th place from the top and 28th from the left. We sent this information to the manufacturer and, after a couple of weeks, we received the missing piece. They would have retrieved the missing piece from their digital files using Cartesian geometry.

Figure 2. Missing piece of jigsaw puzzle identified by Cartesian geometry.

Correlation, cause and effect

Cartesian geometry has contributed greatly to the study of correlations and cause-effect relationships. Each class of data is called a parameter. When a change in one parameter is accompanied by a change in another parameter, the two parameters are said to be correlated with each other.

Figure 3, from a thesis that I supervised, show how the rate of leaf production and the leaf life span are correlated with the light environment for the sapling shoots of five species of trees: *Acacia mangium* (Am), *Cinnamomum iners* (Ci), *Dyera costulata* (Dc), *Shorea roxburghii* (Sr) and *Eusideroxylon zwageri* (Ez) (Tong & Ng 2008). Five light environments were arranged using shade nets above the plants: full sun, rated at 100%, followed by 50%, 25%, 7% and 4%. In Figure 3, the scale for the light environment is a logarithmic scale in which open sky with 100% Relative Light Intensity

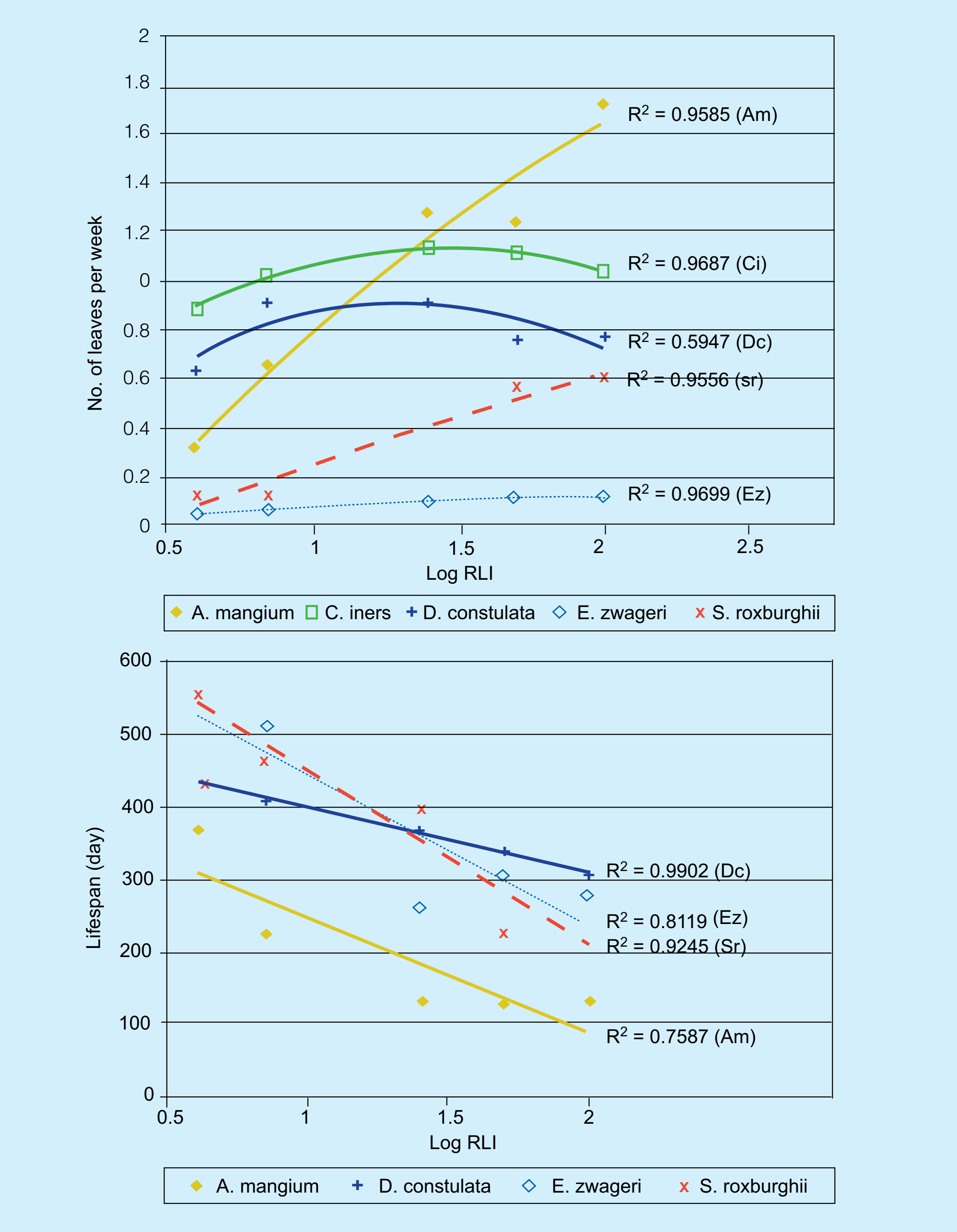

Figure 3. Rate of leaf production per week (upper graph) and life span of leaves in days (lower graph) corresponding with Relative Light Intensities between 4% (deep shade) and 100% (full sun).

(RLI) has a value of 2. The other values are 50%=1.7, 25%=1.4, 7%=0.8, 4%=0.6.

The results show how the light regime affects the rate of production and the life span of leaves. This was an interesting study because, in temperate regions, leaf production and leaf life span are controlled by the changing of seasons and therefore by temperature and day length, but in the humid tropics we found that leaf production and leaf life span are controlled by light intensity. For all the species studied, the life span of leaves is reduced as the light intensity increases.

In leaf production, the rate increases with light intensity up to a maximum for *Cinnamomum iners* and *Dyera costulata* at 25% RLI and then declines, but for *Acacia mangium and Shorea roxburghii*, the rate of leaf production increases with increasing light intensity all the way to 100% RLI. Since the rate of leaf production is a measure of growth rate, the graphs show that under full sun, *Acacia mangium* would be the fastest-growing species, followed by *Shorea roxburghii*. *Eusideroxylon zwageri* was unusual in that its rate of leaf production was slow under all light intensities.

The data are denoted by points and the points are used to produce lines of best fit in which the points are evenly balanced above and below the line, to display a continuous relationship. Before we had desktop computers, a line of best fit was made manually by eye and ruler. If the scatter of points is great, we might decide that any line would be misleading and it would be better to redesign the experiment to get a clearer result. Nowadays such lines are done by computer following a mathematical procedure which also provides an algebraic equation specifying how one parameter is mathematically related to the other. This procedure is called a 'regression analysis' but in reality, the scientist has been replaced by a computer programme making automated calculations. It still requires a brain to figure out its meaning. The line of best fit is considered to represent the relationship between the x and y parameters and the points, if not exactly on the line, are interpreted as chance deviations.

Theory in taxonomy

It has always been obvious that living things exist as distinct species. Primitive man had to know how to distinguish one species from another in order to survive. The process of cataloguing all the species on earth is an ongoing one, following the model established by the Swedish botanist Carolus Linnaeus (1707-1778). Every species is an interpretation of nature, in other words, a theory. The data used to describe a species are the observable attributes of the specimens under study. Each species is recognized by its pattern of attributes. The task of the taxonomist is to decide whether a pattern is unique enough to recognize as a distinct species.

Following Linnaeus, each new species in biology has had to be properly published to be scientifically valid, and the name of the author and the date of publication provides the basis for deciding priority of authorship. A system of rules has been put into place through an international convention, whereby a species may be subsumed (reduced) into another, rescued from obscurity, or transferred to another genus. The principle of priority in publication is most strongly applied in taxonomy—if a species is described twice, the name that is published first takes priority. Taxonomy takes a practical approach in that a species can be based on a single specimen. As more specimens accumulate, each species description is revised to provide a fuller description of the species. This process continues indefinitely.

Beyond the recognition and description of species, taxonomy has resulted in the creation of a map or pattern of relationships showing how all living things are related to each other. This enormous and comprehensive pattern of life provides support for all other research in biology.

Hypothesis, theory and law

If a theory is formulated at the start of research, such a theory is usually referred to as a hypothesis. The explanation presented at the end is referred to as a theory. At one time, theories that made a big impression were given the title of scientific 'laws', e.g. Newton's laws in physics and Mendel's laws of inheritance in genetics. Now, Newton's laws are overshadowed by Einstein's theories and Mendel's laws are overshadowed by advances in molecular biology. The grand sun-centric universe of Copernicus and Galileo is now shrunk into a small speck in an enormous galaxy of many solar systems. The concept of scientific laws has become obsolete.

Paradigm shifts

A theory that gains acceptance acts as a framework for thinking and is called a paradigm in a book by the philosopher Thomas Kuhn called *The Structure of Scientific Revolutions*, published in 1962. Kuhn visualised scientific progress as intellectual revolutions involving 'paradigm shifts' such as the shift from a flat earth to a round earth, and the shift to Harvey's model from Galen's model of blood behaviour. The flat earth theory was dominant in Europe before it was destroyed by the voyages of Christopher Columbus and Ferdinand Magellan between 1492 and 1521.

One of the greatest paradigm shifts in biology involved the theory of the origin of species. Before Charles Darwin and Alfred Wallace, the prevailing theory was that species were fixed entities that had remained unchanged since they first came into existence. *The Theory of Evolution* resulted from Charles Darwin's participation in a voyage of discovery in the British naval vessel HMS Beagle. In the course of collecting and studying animals, Darwin was enthralled by the immensity of biological diversity, and especially the many different species of finches in the different islands of the Galapagos Archipelago. He came up with a theory that the different species had originated from a common ancestor by evolving in different directions on different islands. Darwin's compatriot Alfred Wallace, who was independently making collections of insects and other animals in the Malay Archipelago, arrived at the same theory of the origin of species by evolution, which he and Darwin agreed to publish jointly in 1858.

Another great paradigm shift in biology was Pasteur's demolition of the theory of spontaneous generation of life in 1862. The theory of spontaneous generation was that organic decay is caused by microbial life forms that appear spontaneously. Pasteur designed an experiment in which nutritious beef broth was sealed, while still hot, within glass containers. He demonstrated that this broth could be kept indefinitely in sealed containers without going bad. Decay was explained as the result of microbes in the air and non-sterile surfaces getting into the broth and proliferating in it. The theory that microbial growth was due to contamination became the basis for the sterilization or pasteurisation of milk, the rise of the canned food industry, and the development of sterilization procedures for surgery.

Every theory in science is a paradigm as far as its followers are concerned, and the shifting of paradigms is the normal business of scientific inquiry. The world of science is a world of many paradigms and every attempt to reshape a theory is an attempt to shift an existing paradigm. Kuhn's contribution was to philosophy, not to science, because he was merely providing a philosophical explanation for what scientists had long been practising.

The rule of simplicity (Ockham's Razor)

The aim of theory in science is to explain natural phenomena as simply as possible. The rule of simplicity has a name, which was given to it long before science, when philosophers engaged in disputes, with no way to settle them. A proposal was made by the English philosopher William of Ockham (c. 1285-1349) that between competing theories, the simplest one should be preferred. This rule is known as Ockham's razor because it requires shaving down theories to reduce complications to the absolute minimum. This rule is useful in science because although science can settle disputes with experiments, it is expensive if not impossible to test complicated theories by experiment. Theories have to be reduced to their simplest forms before they can be tested.

Induction and Deduction

Induction and deduction are terms applied to two contrasted forms of reasoning that have figured prominently in western philosophical discourse.

In induction, one is supposed to begin with data collected in an unbiased manner, and then, like an impartial judge, make the best possible judgement. This method of arriving at knowledge is also known as the empirical method, after the 3rd Century Greek philosopher Sextus Empiricus who declared that knowledge should be based on experience. It is also referred to as the Baconian method because it was promoted by the English philosopher-statesman Francis Bacon in his book *Novum Organum* published in 1620. Because of Bacon's influence in England, this method was adopted early in the history of science by English scientists. Harvey presented his theory on blood circulation as the culmination of careful evaluation of unbiased data. Isaac Newton, in his famous book *Principia,* declared vehemently "*hypotheses non fingo*" or "I do not frame hypotheses" to emphasize that he arrived at his theories after examining the evidence and was not biased by preconceived hypotheses. Charles Darwin was also careful to present his *Theory of Evolution* as a theory arrived at by induction. In scientific papers, the format promoted by editors is the so-called IMRAD method of Introduction, Methods, Results And Discussion, in which the conclusion is presented at the end, through supposedly unbiased collection and evaluation of data.

The philosopher Karl Popper in his book *The Logic of Scientific Discovery*, has pointed out that no matter how much data we have, it only takes one exception to 'falsify' our judgement. He used the example of a theory that *swans are white*, based on observations on all swans available, but if a single black swan turns up, the theory is destroyed or 'falsified'. Popper argued that this apparent weakness is actually a strength because theories in science should be open to the possibility of falsification, i.e. of being proven wrong. A theory that does not allow for falsification by experiment is not testable and therefore not a proper scientific theory. Popper's contribution to science was to describe in philosophical terms what scientists had been practising.

Deduction is the form of reasoning applied most famously by Euclid in geometry, and by Aristotle in his general approach to knowledge, in which one begins with 'axioms' that are accepted to be true; then through logical arguments, one can derive other truths, all linked together by logic. The main philosophical criticism of deduction as a method of making knowledge is that nothing truly new can be discovered. All discoveries made by logic are already embedded within the foundation axioms. In science, if we begin with a hypothesis and then look for evidence to support the hypothesis, what is discovered may be constrained by the starting assumptions.

Actually, the differences between induction and deduction have no relevance in science because scientists move back and forth between deduction and induction in the course of an investigation without thinking about it. The claims of discovery from any scientific inquiry have to stand on their own merits. It does not matter what method is used.

The limits of theory

Scientists only publish what they can explain. Textbooks simplify science and scientific journals only publish what is positive. Difficulties and irregularities that interfere with the beauty of the narrative tend to be excluded from the narratives of science. Hence while the development of theory is synonymous with the development of knowledge, practical application requires a sceptical approach.

The distance between theory and practice is particularly striking in efforts to improve plants by breeding. Modern genetics began with the discovery of how genetic traits are inherited in garden peas, published by Gregor Mendel in 1866. Mendel's theories on inheritance made no impact until 1900 when attention was drawn to his paper by three other scientists in three different countries within a short period of two months.

Mendel had tracked the behaviour of the genes controlling easily observable traits, and ignored all the other genes. For example, Mendel tracked the gene controlling flower colour, which came in two heritable forms or 'alleles', one for red and another for white. When they came together in an individual (one allele from each parent), the red would be expressed and the white would be suppressed, but in the next generation there would be a re-sorting of alleles, and white-flowered individuals would reappear in those individuals that inherited two white alleles. Mendel discovered the mechanistic rules that governed how alleles were passed on from generation to generation.

If we want to combine a desirable heritable trait of one plant with another desirable heritable trait from another plant of the same species, we apply the pollen from one plant to the stigma of the other plant and produce hybrid plants. First-generation (F1) hybrid plants can be pollinated to produce second-generation (F2) hybrids and so on. The hybrid plants are raised and from them, one may find plants with the desired combination of traits. This sounds very straightforward, but a species has thousands of genes, with each gene existing in different allelic forms. As a result, although all individuals of a species have the same genes, it is unlikely for two individuals to have the same set of alleles. When two plants are crossed, there is a re-sorting of all the alleles and it would take a monumental effort to raise enough plants in the hope of finding a desired combination of alleles. The targetted set of alleles has to be supported by non-detrimental sets of all other alleles in the plant.

For most trees, e.g. rubber, mangoes, avocados, dipterocarps, the flowers are small, and only a tiny proportion develop into fruits. For rubber, the percentage of fruits set after hand-pollination at Malaysia's Rubber Research Institute was only 3-8% (H.Y. Yeang, personal communication, 2021). In one study on a tree of *Shorea leprosula* (FRIM Annual Report for 1981) the tree produced 750,000 flowers of which only 0.78% resulted in fruits. After flowering, most flowers are shed without developing into fruits. Of those that form fruits, many are aborted before they reach maturity. We have no idea why trees produce so many flowers in relation to the number of fruits. This makes hand-pollination hopelessly unproductive for most trees.

Also, unlike garden peas, trees take three to ten years to become reproductive so there is a long waiting time between generations. All these factors make plant breeding impractical except for those plants that are small, have short generation times of a few months, and that respond well to hand-pollination. It took a lifetime of dedicated work by rice-breeder Yuan Longping (1930-2021) to hunt for superior genetic traits all over China and recombine them to raise the yield of rice by just 30%. This was worthwhile for something as important as rice but would be hard to justify for other crops.

Most improvements in fruit trees are the result of selection of mutations, not of breeding. Most plants have multiple growing points or buds. Each growing point is a potential site for mutation. Alert growers spot mutant shoots that have desirable features and clone them.

After his research on garden peas, Mendel turned his attention to the hawkweed, a species of *Hieracium* and completely failed to make sense of its hereditary pattern (Henig 2000). The behaviour of hawkweed turned out to be totally different from garden peas.

The importance of theory

Scientific knowledge is a body of theories based on factual data, but whereas data is static, theories are open to challenge and improvement. Theory is what makes science the most demanding and exciting of all intellectual pursuits.

Most of the expense of scientific research is due to the cost of equipment, manpower and time required to obtain data, but the effort is wasted if data is not used to make or improve theories. However, theories do not appear by accident. They have to be made deliberately, by scientists who take it upon themselves to explore and fill in gaps or deficiencies in theory.

Data in the public domain

In making theory, one is not limited to one's own data. All data in the public domain are available for public use and some of the greatest scientific advances have been made with data available in the public domain. Albert Einstein, who was the greatest of the theoretical physicists, used the published data of others to make theories that nobody else had ever thought of. In biology, the large volume of data from studies on the known species in the world provides support for all other kinds of studies in biology.

Chronology of the early development of science

1492	Columbus' discovery of America and destruction of the flat earth theory
1522	Magellan's circumnavigation of the world.
1543	Copernicus' theory of the sun as the centre of the universe.
1583	Galileo's theory of the pendulum as a precise timekeeper.
1610	Galileo's publication of *Sidereus Nuncius* and the establishment of astronomy as a modern science. Destruction of the earth-centred theory of the universe.
1628	Harvey's publication *Exercitatio Anatomica de Motu Cordis et Sanguinis in Animabilis (An Anatomical Exercise in the Motion of the Heart and Blood in Animals)* and the establishment of medicine and zoology as modern sciences.
1660	Establishment of the Royal Society of London.
1675	Establishment of the Royal Greenwich Observatory.
1687	Newton's publication of *Principia* and the establishment of physics as a modern science.
1753	Linnaeus' publication of *Species Plantarum* and the establishment of botany as a modern science.
1787	Lavoisier's publication of *Methode de Nomenclature Chimique* and the establishment of chemistry as a modern science.
1840	Whewell coins the word scientist for those engaged in modern science.

Chapter 3

Publication

Through publication, scientists are united in a global system for sharing knowledge. The rate of publication has become the main measure of productivity of scientists while the total number of publications has been used as a measure of the scientific power of nations.

The number of scientific journals is estimated to be 28,000 and the number of scientists is estimated by UNESCO (2021) to be 8.8 million. If each journal publishes four issues a year, and each issue contains 10 papers, the total number of papers published annually would be 1,120,000. This works out to an average production of 0.13 papers per scientist per annum. However, productive scientists publish over 100 papers in a professional lifetime, or 2-3 papers a year. By this measure, the output of a productive scientist is equal to the combined output of 20 average scientists.

This chapter discusses how scientific publication began, the role of language in scientific communication, the activities associated with publication, such as authoring, editing, and peer review, and the way scientists make global impact.

The beginning of scientific publication

Effectively, scientific publication began in 1610 with Galileo's publication of *Sidereus Nuncius* (Starry Messenger), in which he described his astronomical observations and theories in simple readable style. This was followed in 1628 by Harvey's book on the motion of the heart and blood in animals. Both publications were written for public information, with the intention of bypassing those who would oppose change. A new standard for knowledge-creation was thereby established, in which claims are creditable only if published for public evaluation.

Priority in publication

Before the publication of *Sidereus Nuncius* in 1610, Galileo was already famous for his discovery of the principle of the pendulum. Galileo's pendulum discovery was not published in the form of a printed document, but through word of mouth. In the early period of science, to publish meant to publicise. Galileo's discovery was widely publicised and attributed to him.

Publication in print gained importance because of a bitter dispute between Isaac Newton and Gottfried Leibniz over who was first with the theory of calculus. Leibniz was first in print, in 1684. Newton claimed priority, but Newton had only communicated his theory in secret to a small circle of friends. The dispute ended in stalemate because Newton, as President of the Royal Society, had the support of British scientists while Leibniz, a German, had the support of the scientists of continental Europe.

The idea of publication by oral communication was still practised in 1858 when the theory of evolution by natural selection was presented orally on behalf of Charles Darwin and Alfred Russel Wallace, at a meeting of the Linnean Society in London. Wallace was first to prepare a manuscript for publication, but he had sent it to Darwin to get Darwin's advice. Darwin was shocked that Wallace's ideas were similar to his own but Darwin had been slow to write and now he was in danger of losing priority to Wallace. Darwin sought the advice of his friends Charles Lyell and Joseph Dalton Hooker and they arrived at a saving solution for Darwin, which was for Darwin and Wallace to present the theory of evolution as co-authors at a meeting of the Linnean Society. Both authors were absent at that meeting and the paper was presented by the secretary of the society.

It is now generally agreed that the date of publication in print should be accepted as the effective date of publication. This principle is most firmly established in taxonomy, and no other claims are entertained.

The impact of publication

Through publication, scientists make impact globally because multiple copies can be made, distributed, and preserved in libraries. In this way, publication has brought into existence a universal 'public domain' of knowledge. If one tries hard enough, one can locate almost any kind of information that has been published. The effects of publication may be summarised as follows:

- Publication has provided security for information because multiple copies exist in multiple places.
- It has contributed to the credibility of science by making it open to global public scrutiny.
- It has enabled contributions in science to be traced to source: who published what, when and where, thereby bringing into existence a global system for recognition of scientific achievement.
- It has reduced repetition due to ignorance of past work because authors cannot merely repeat what has already been published.
- The requirement for originality or novelty in publication has driven the growth of scientific knowledge.
- The 'first to publish' principle has provided powerful motivation for scientists to compete.
- The ability to track individual outputs has promoted the growth of scientific research as a salaried profession.
- Publication has enabled scientists to reach out globally to form professional networks.

The beginning of the scientific journal

For short communications of just a few pages, a book is not suitable. The solution was to combine short communications and publish them in what we now call a journal. The first such journal was the *Philosophical Transactions of the Royal Society*, launched in 1665. Later, journals were published by scientific societies separately from society matters, so authors who were not society members could publish in them. Later still, government research organizations and universities began to publish journals. Towards the end of the 20th Century, commercial publishing companies, realising that journal publication could be highly profitable because all major universities had to subscribe to journals in order to keep up-to-date with the latest research, went into the business of publishing scientific journals.

The language of scientific communication

To improve personal scientific productivity, scientists need to master the language of scientific publication early in their careers. This language is English because over 80% of scientific papers are published in English.

Scientists have no choice but to master English if they want to keep up-to-date in reading and reach a global audience in writing. There is a huge vocabulary of scientific words in English, and new words and concepts are being continuously added. For example, in morphology, there exists an extensive vocabulary to describe the position and orientation of plants and animals and their organs. Simple terms like *front* versus *back*, and *above* versus *below* are not enough. Morphologists need to specify *anterior* versus *posterior, dorsal* versus *ventral, proximal* versus *distal, adaxial* versus *abaxial, lateral* versus *axial, superior* versus *inferior, terminal* versus *basal*, and so on. In most languages there are no equivalent terms to the English scientific terms.

In rapidly developing areas of science, new words are coined at a rapid rate. A translator has to be a competent scientist to understand what is published in scientific English and, in addition, undertake to coin new words to parallel the technical English words. This is not worth the effort if the new words are not kept alive by active usage.

I once needed to get a translation of a paper published 100 years ago in botanical German when botany was dominated by German botanists. I got a German botanist to help but he could not do it confidently because botanical German is now a dead language even for German botanists.

Literature access

At one time, access to published works was limited by physical access to big libraries, so scientific institutions tended to be clustered around such libraries. Nowadays, with the Internet, it has become possible to access most published works from one's own workstation anywhere in the world.

Notwithstanding the importance of access to literature, it is better to begin a scientific investigation after reading just enough to get started. There are two good reasons:

1. It is difficult to understand what one reads until one is already actively working on the subject. It is better to start work and read as the work develops.
2. The literature provides biased guidance. Positive findings get published. Negative findings are not published and stay hidden as booby traps for the unwary.

The format of a scientific paper

The scientific paper is not a true record of how the research is actually progressed, as Peter Medawar (1996a) has explained in his paper *Is the scientific paper a fraud?* Editors encourage authors to follow a reporting format known as the IMRAD format which stands for Introduction, Method, Results And Discussion. Under this format, the author acts as an unbiased judge, weighing the evidence available and coming up with the best interpretation at the end. This is the method of induction. The alternative method is deduction, in which the author starts with a hypothesis and searches for evidence, guided by the hypothesis.

In practice, scientists do not need to know the difference between induction and deduction and may move freely between induction and deduction as the investigation proceeds. At the end, the conclusion has to stand on its own merits regardless of the method used.

Title of a paper

The title of a publication should be carefully worded to attract the attention of its intended audience. A good example was set by William Harvey in his publication *An Anatomical Exercise on the Motion of the Heart and Blood in Animals*. For a poor example, there is a paper entitled *Plantation Experiments in Kepong*, published in 1935 in *The Malayan Forester*. I was searching for information on the use of bamboo tubes as containers for raising seedlings, and found what I wanted in this paper, purely by accident. The title of the paper gave no indication of what it contained.

Abstract

The abstract should summarise the paper and should announce its most important messages. We have to assume that most readers nowadays have no time to read long complicated papers.

Introduction

A scientific paper is usually an additional contribution to a body of knowledge that already exists. The new paper has to be placed in its historical and intellectual context to help readers understand the gap or weakness that the new paper intends to address. This is where previous authors who have made significant contributions should be cited to provide the context. There is no need to provide a long literature review.

Method

The method of investigation should be described to enable the reader to visualise how the study was carried out, unless the method is a well-known method. If the method is relatively unknown and borrowed from another author, the originator of the method should be cited and a brief outline of the method provided so that the reader does not have to stop and to look up the cited reference. Each paper should be complete without requiring the reader to refer to some other paper.

Results

The results of an experiment are data in the form of observations and measurements, together with the results of analysis done on the data. The presentation of results as tables, graphs and graphics is a vital skill for authors to master, with the aim of bringing out patterns as clearly as possible. Mathematical manipulations of data can provide different ways of looking at data, but the results still have to be presented so that the reader can see the pattern without necessarily understanding the mathematics.

Discussion

The discussion is where the author interprets the results and presents new theory or changes to theory. This is the part of the paper that will engage the reader intellectually and contribute most to its impact.

Citations

Plagiarism, which is the act of passing off somebody else's work as one's own is considered a form of cheating and a serious offence in science. If data, paragraphs, or illustrations are copied from other authors, the original authors have to be acknowledged.

Some authors produce long citation lists, perhaps under the impression that the number of papers cited increases the value of the paper. This can betray a lack of ability to separate important from unimportant contributions to the topic of the paper.

Authorship

Until about the 1960s most scientific papers had single authors. Partnerships between two authors who contributed equally, like James Watson and Francis Crick in the discovery of the structure of DNA, were uncommon. Authorship was restricted to those who contributed intellectually to the research. Others, especially research assistants or technicians, were acknowledged at the end of the papers. Other scientists who contributed information were acknowledged in the list of references or named in the text with the note 'personal communication' or *pers. comm.*

Now we see papers with lists of co-authors taking up half or even a whole page. It is impossible for so many people to contribute equally to a paper, so the first author in a long list of authors is assumed to be the intellectual leader and main author.

The role of editors

When I first became an editor, each paper would require only correspondence between the editor and the author. Now journals are expected to arrange for papers to be reviewed by two other scientists. This requires correspondence with potential reviewers to identify two willing reviewers, after which the editor has to correspond with the author and the two reviewers. If a paper is rejected, all this expensive effort is wasted. A journal containing ten papers per issue published four times a year would involve a mountain of correspondence. This is one reason why journals are passing into the hands of publishing firms, which charge heavily for subscriptions. Major universities have no choice but to pay. Authors are also being made to pay heavily to get published in 'high impact' journals.

Costs can be kept down by in-house vetting of papers to reduce the load for review. Most papers can be eliminated by vetting and the authors of such papers are informed quickly and are free to try elsewhere. The need for review is drastically reduced and reviewers only get to review papers that are worth the effort of reviewing. Papers that are rejected by the vetting process would include those that do not fit the scope of the journal, contain too little information of scientific value, or are written in language too complicated for the readership of the journal.

Many papers are too long in relation to the information they contain because most scientists are poor writers. I once recommended a 26-page paper be reduced to six pages, and that was already quite generous. It could have been done in three pages. It is a waste of editorial and review effort to pick out weaknesses page by page when the real problem is the author's lack of discipline in deciding what is scientifically significant and what is not.

Editors can shape the development of science by putting an end to obsolete topics. I once reviewed and rejected a paper on the grounds that the topic was out of date. The author was indignant and demanded to know who were the reviewers because he knew all the scientists on this topic. He was probably right, but he and his fellow scientists formed a small bubble that was overdue for collapse. The bubble was created around the idea of international provenance trials. From about 1950 to 1990, tropical forestry went into a phase of group-think in which every tropical country neglected its own indigenous species and concentrated on a handful of internationalised species like *Pinus caribaea, Gmelina arborea* and *Acacia mangium*. It was soon discovered that there were wide differences in growth rate depending on where the seeds came from and where they were being planted. It was felt necessary to find the best match between the location of planting and the source of seeds. 'International provenance trials' were set up in which seeds of the targeted species were obtained from different geographical sources or provenances. Trials were then conducted in different countries to discover which provenance would be best for which location. It took 10-20 years to evaluate a trial and by that time the original seed sources or provenances had disappeared and the chosen species were no longer of interest. Someone from outside the bubble had to inform the scientists involved that their bubble was no longer relevant.

Editors are also in a position to stop the practice of hiding data. For example, in a study of seed germination, the primary data is the number of seeds germinated in a specified sample of seeds. This can be converted into some derived value such as ‘germinative energy’ or whatever measure the author wants to promote. By hiding the primary data, it becomes impossible for others to use the data to develop other theories. Larger pools of data would allow for larger patterns to emerge. Hiding data goes against the spirit of global knowledge development.

The replacement of real data by percentages can be problematic. If in a study of an animal population, it is reported that 30% are females instead of 50%, such a report would be impossible to evaluate without the primary data. If the figure of 30% is based on 3 out of 10 specimens, the result could be a fluke. But if it is 30 out of 100, this cannot be a fluke and it would merit closer study. Some editors do not allow the conversion of data to percentages unless the sample size is above 100.

The overall standard and reputation of a journal is the responsibility of the editor. Reviewers advise the editor on the individual papers that they review. Reviewers are sometimes wrongly referred to as referees but it is the editor who makes the final decision to publish or reject.

The role of reviewers

Reviewers serve on a voluntary basis and are often referred to as peer reviewers. The word ‘peer’ refers to the British aristocrats who forced King John in 1215 to give up the power to pass judgement on the peers of the country. The peers demanded to be judged by their equals. Peer reviewers are scientists of equal status with the author of the paper under review. In practice they are scientists selected for their experience in the topic under review and are expected to point out mistakes and omissions in a paper.

Occasionally, there are conflicts of interest when a reviewer does not agree with the author. I experienced this with a paper (Ng 2019) in which I used evidence from morphology and anatomy to argue that *Rafflesia* (the largest flower in the world) has been misclassified as a flowering plant (angiosperm). Molecular biologists had placed *Rafflesia* close to the angiosperm family Euphorbiaceae. The peer reviewers were supporters of molecular biology. They were unable to find fault with my evidence but insisted that morphology and anatomy had been rendered irrelevant by molecular biology.

In the past 20 years, molecular biology evidence, previously used to resolve taxonomic issues that could not be resolved by morphology and anatomy, has been pushed higher and higher in status, and now attempts are made to downgrade the validity of morphology and anatomy. The reviewers decided to act as censors to remove what they regarded as a threat to the supremacy of molecular biology. My paper drew attention to a serious contradiction between molecular biology and morphology/anatomy. Such contradictions between two bodies of evidence have to be resolved by honest inquiry not by clumsy and unethical censorship. I got my paper published in another journal.

Scientific publication began when Galileo and Harvey published their works to bypass censorship by their opponents. The aim of the modern peer review system should be to improve the quality of publications, not to reinstate censorship.

Reacting to review

Peer review can be a shocking experience for scientists new to the process. Authors should, before submitting a paper to a journal, get friends to review their papers first. Those friends do not have to be experts. If they do not understand a paper, it means the author has a problem explaining the work and should rewrite.

If a paper is rejected, the author should not be embarrassed about seeking advice. In the case of a paper that the author is asked to amend, the author should amend the paper accordingly. If the reviewer has misunderstood the author, the author has to clear up the misunderstanding. At its best, peer review challenges an author to improve a paper. This is good for the author, the readers, and the journal.

Getting published

Know and respect the readership

Each journal has its own type of readership so it is important to select the appropriate journal for the paper.

All research is done locally, but a paper should explain how the local findings fit into the larger global context, for the benefit of the local as well as the global audience.

All essential information should be provided so that readers can make their own conclusions. The aim of a paper is to persuade, not to force one's views on others.

Making and measuring impact

Impact after publication is usually measured by the number of times a paper is cited by other scientists within a few years of publication. This measure is most often used in the rating of scientists even though it does not measure quality of research. The real quality of research may not be apparent until many years later.

The fate of failed or inconclusive research

In science, positive findings are published and negative findings are not. In the search for answers through scientific inquiry, it is inevitable for most attempts to be inconclusive. Positive findings add to knowledge and it is easy to write a narrative around something that works. Papers about experiments that failed will normally not be accepted for publication because negative findings may be due to any number of reasons including reasons that the experimenter may not have thought of. Normally, we can explain failures only after we have achieved success and can then look back to see what went wrong previously.

An exception may be made if there is a clear pattern of failures e.g. the failure of innumerable attempts to grow eucalypts in the lowlands of Malaysia (Ng 1996). In Malaysia, trial plots of eucalypts have been planted since the 1930s. If we consider only those plots with 20 or more seedlings grown in the lowlands (below 1000 ft elevation), FRIM's experiments covered 27 species in 77 plots. Every one of them failed within a few years, with 100% mortality, and the reasons were unknown. Nothing was ever published about these failures. The most massive failure was the complete obliteration of thousands of acres of *Eucalyptus camaldulensis* in 1984 in Kemasul in the state of Pahang. This plantation had been preceded by a trial of one acre, which had produced astonishing growth results, with trees growing as much as 10 ft per annum in the first two years. In May 1981 I examined the one-acre plot of *E. camaldulensis* and noticed that although the trees were tall, they were very slender and the ground below was densely occupied by lalang (*Imperata cylindrica*), a weed that competes aggressively for soil nutrients and space. The eucalypt was unable to shade out the lalang. This was a bad sign. Eucalypts have the habit of continuous growth, in which the shoot apices produce leaves one after another without rest. The accumulating foliage should shade out the lalang. However, the number of leaves on each plant in this plot was too few to create effective shade. They were few because the rate of production of new leaves was barely keeping ahead of the shedding of old leaves. The leaves were ageing and getting shed rapidly because of a leaf fungus. Then in 1983-1984 there was a long period of wet weather and the life span of the leaves became even shorter. The rate of loss of leaves began to exceed the rate of new leaf production. In a few months all the trees died. This provided the explanation for all the previous failures that had occurred and it explains why in the humid tropical lowlands, eucalypt plantations have always failed.

Publication helps to reduce unnecessary repetition of past work but this only applies to work that is published. Failed research does not get published. As a result, scientists keep repeating such experiments. Eucalypts continue to be promoted in the lowlands of Malaysia and Indonesia because their initial performance produces great euphoria for a few years. After that we hear no more and whole plantations disappear without explanation.

Literature reviews

Students writing their thesis are required to write reviews of the literature in their chosen fields of study. Such reviews are not suitable for publication because the literature that is reviewed is biased in favour of positive findings. The reviews that are useful are those by scientists with extensive practical knowledge of the topic being reviewed, not just knowledge based on reading.

Intellectual property, copyright and patents

The author of a paper is automatically the copyright owner of the paper unless copyright is transferred by the author to another party. An author may be required by the journal to transfer copyright to the journal. The journals that do this are those owned by big publishing conglomerates that want the exclusive right to sell what they publish.

Even if the copyright is assigned to a publisher, the author retains moral ownership of the work. Moral ownership means that the author is always acknowledged as the author. It is like a piece of art. The art may be sold but the name of the artist is permanently associated with the work.

A scientist with knowledge that has possible commercial applications might apply for a patent. The rights of the patent owner are protected by law for a fixed period, usually about 20 years. There are rules about whether something is patentable or not. Scientific ideas and theories are not patentable. For example, the discovery that a pendulum keeps time could not be patented. What could be patented were the inventive mechanisms for a clock. The details of inventions have to be clearly defined because legal protection is possible only for what is clearly defined. It normally takes one year or more for a patent to be processed. It is costly to obtain a patent because on top of a patent fee, the process is complicated and usually requires the help of a professional patent agent. Further costs are involved in detecting and taking legal action against infringement. It is also difficult to recoup one's investment before the patent expires unless backed by a powerful organization that knows how to commercialise the product.

Patent rights may be sold but the organization that buys the patent may keep it in storage as part of its business strategy, e.g. to protect a product that it is already marketing.

The patent system that is supposed to encourage innovation by giving legal protection to innovators actually slows down innovation because it has been weaponised by powerful corporations and countries, and the independent inventor has little or no chance of benefitting from it.

There is an alternative scenario, in which people do not bother about patents. Innovators market their products on the Internet through a reputable marketing network. Costs are minimised because the cost of marketing and promoting a product through the Internet is small. There are no patent fees, no waiting period, no retail outlets to maintain, no legal and enforcement costs, and the innovators have the satisfaction of seeing their innovations marketed. From the feedback they can immediately improve their products and come up with better versions. Others can jump in and make their own versions. Instead of trying to stop competition, the innovators keep innovating to stay in competition. The innovation cycle is reduced to months rather than 20 years. This scenario is already evident, in China.

For a scientist, being recognized for a theory is a lot more important than being recognized for an invention. Sometimes scientists hold back from publication until they can apply for a patent, but then if someone else publishes first, the scientist will lose priority. A better strategy is to publish the theory and be credited for it immediately while keeping invention details separate and confidential for the purpose of patent application.

Chapter 4

Luck, Pattern, and The Prepared Mind

The most famous statement on the relationship between luck and scientific discovery is that attributed to Louis Pasteur who, when asked about the role of luck in his discoveries, said, “Luck favours the prepared mind”. In spite of Pasteur’s remark, the role of luck has been absent from serious discussions about scientific discovery.

Lucky events that are recurrent can be predicted according to mathematical rules of probability. However, the kind of luck that is most important to scientific discovery are one-off events that have no relationship with mathematics and cannot be predicted. This chapter is about such one-off lucky events. Most importantly, it is about the nature of the prepared mind that can recognize and benefit from such luck.

The prepared mind is also prepared for pattern recognition. This ability seems to be instinctive and related to an aesthetic sense of balance and symmetry rather than to logic.

Louis Pasteur (1822-1895) and chicken cholera

In 1878, Pasteur began to investigate a fatal disease of chickens, known as chicken cholera, caused by a species of bacteria. The bacteria were present in the body fluids of sick chickens, and healthy chickens could be infected by injecting them with body fluids extracted from sick chickens. In the summer of 1879, Pasteur went on vacation for a month. He left a vial of virulent fluid in his lab, stoppered with a plug of cotton wool. On his return, Pasteur resumed his experiments, using the month-old fluid. He found that the fluid had no effect on the chickens. It had lost its virulence!

Pasteur made fresh samples of virulent body fluid and continued his experiments. He injected the fresh fluid into healthy chickens and to his great surprise, some died as expected but some remained healthy. Pasteur recognized the ones that survived. They were the same ones that he had previously injected with the expired month-old body fluids. This was Pasteur's great moment of discovery. The expired fluids had given the chickens immunity to the disease!

To make more of this expired fluid Pasteur experimented and found that exposure to oxygen would inactivate the virus. His original vial of virulent fluid had been stoppered with a plug of cotton wool that permitted the entry of air. He found that virulent fluid stored in air-tight containers retained its virulence. Pasteur then produced deactivated cultures of chicken cholera by deliberate exposure to oxygen and used these cultures to inject healthy chickens. This vaccine, as Pasteur called it, made them immune to the disease and eventually eliminated the disease. It was luck that opened the way to the discovery of a vaccine for chicken cholera. Three strokes of luck had occurred one after the other—the month-long vacation, the exposure of the virulent fluid to oxygen of the air, and the reuse of the same chickens for two rounds of experiment. However, it needed a prepared mind to recognize the meaning of what had happened.

It so happened that in Europe at that time, it had become the practice for people to get themselves deliberately infected with cowpox to gain immunity to smallpox. Cowpox caused mild temporary discomfort whereas smallpox was disfiguring and often fatal. The effectiveness of cowpox to confer immunity to smallpox had been proven by Edward Jenner (1749-1823). In his 1798 paper describing the effectiveness of an injection of cowpox fluid to provide protection against smallpox, he referred to cowpox as *Variolae vaccinae.*

Pasteur saw in his mind the connection between the use of cowpox to provide immunity to smallpox and the use of weakened chicken cholera to provide immunity to chicken cholera. He theorised that cowpox was a weakened form of smallpox, as his deactivated chicken cholera cultures was a weakened form of chicken cholera.

Pasteur then made an incredible mental leap, that other diseases caused by germs could be similarly defeated by using deactivated cultures of those germs. He applied the term ‘vaccine’ derived from Jenner’s *Variolae vaccinae* to such deactivated cultures and the term ‘vaccination’ to the process of applying the vaccine. Based on his theory Pasteur went on to develop successful vaccines for the dreaded diseases of anthrax and rabies. Pasteur’s theory of vaccines was a giant step forward in medical science and there are now vaccines developed to confer immunity against many diseases, most recently against Covid 19.

Alexander Fleming (1881-1955) and penicillin

Another often-cited example of the role of luck in discovery is the story of the discovery of penicillin by Alexander Fleming. In 1928, Fleming was working on *Staphyloccocus* bacteria in petri-dish cultures. Petri dish cultures often got contaminated with the spores of microorganisms floating in the air. Fleming was cleaning up contaminated petri dishes when one dish caught his attention. On this was a fungal colony forming a clear ring around itself. It looked as if the fungus was producing some kind of secretion that was able to kill the bacteria around it. He isolated the secretion and found that it had powerful antibacterial properties. The fungus was *Penicillium notatum*, and Fleming named its antibacterial secretion penicillin.

Fleming was lucky that a spore of the penicillin mould had infected his petri dish. However, contamination was very common and did not excite the attention of other microbiologists. Fleming was mentally prepared for penicillin because in 1921-1922, he had discovered that human tears and saliva had antibacterial properties due to the presence of what he called lysozymes. Lysozymes are present in the tears that protect our eyes from infection. Fleming had been a pioneer in the discovery and understanding of lysozymes, and was prepared for the concept of organisms producing antibacterial secretions. His discovery that a fungus could produce secretions to kill bacteria opened the way for the discovery of a range of other antibiotics—actinomycin in 1940 from the soil fungus *Streptomyces parvullus,* streptomycin in 1943 from another soil fungus *Streptomyces* *griseus,* and erythromycin in 1952 from *Saccharopolyspora erythraea* bacteria. Hundreds of other antibiotics have been found although most of them turned out to be toxic to humans and so could not be used in medicine.

William Beaumont (1785-1853) and how the stomach digests food

Our understanding of the process of digestion was first developed in experiments carried out by a US Army doctor, William Beaumont. Beaumont treated a man who had been shot in the abdomen in 1822. The patient survived, but the abdominal and stomach walls healed in such a way as to leave a permanent passage from the outside into the stomach, covered by a movable flap of tissue. Beaumont realized that here was a unique opportunity to make a study of digestion. With his patient's cooperation in 1825, he tied pieces of food with a string and inserted the food through the gap into the stomach. At various intervals of time, he would pull out the food to see what had happened to it. Beaumont continued his experiments for many years and discovered the role of gastric acid, rates of digestion of different kinds of food, and the effect of exercise and emotions on digestion. He published his work in 1833. Beaumont was lucky to have a patient who survived a gunshot wound in such a way that he could make a study of digestion without harm to his patient. Our knowledge of digestion took a big leap forward because Beaumont recognized his luck in having such a patient to work on, but it took a prepared mind to make use of this luck. Beaumont was not employed as a scientist but he was prepared to go out of his way to use the lucky opportunity presented to him. If Beaumont had hesitated, the opportunity would have passed.

These three examples show that luck takes many different forms. Since luck cannot be defined beforehand, how can we make luck and prepare our minds to recognize and benefit from luck?

Being hands-on

Pasteur noticed immediately when something unusual happened to his chickens. Fleming noticed immediately the unusual nature of the clear circle around the penicillium mould on his petri dish. To notice something unusual, one has to know what is usual. To know what is usual, one has to be willing to do usual routine tasks and not leave them entirely to assistants. For Pasteur to have recognized his chickens, he must have personally handled his chickens and could differentiate new ones from those that had survived his previous experiment. Fleming washed his own laboratory petri dishes. If we are not closely involved with all aspects of our work, we would not be on the spot to observe and learn from accidents.

Recognition of qualitative phenomena

One of my textbooks in science was a high-school textbook in physics. This emphasised the importance of measurement. The most important sentence in the book was its motto: 'Science is Measurement'. In training, scientists are taught how to use instruments to measure whatever is being studied and to use measurements to provide the data upon which theories are made. There is a possible negative side effect, that a mind focussed on planning, measurement and logic may be blind to the role of luck and instinct, and miss qualitative phenomena.

One of my most satisfying discoveries was the discovery of crown shyness in trees in Malaysia (Ng, 1977). It happened instantaneously. When I first 'saw' the pattern of tree crowns separated from each other by gaps against the sky, I was absolutely stunned.

Looking down from an aircraft, the canopy of a mature forest is made up of closely packed crowns of trees that appear to be in contact with each other. It is only from below that one can see that the crowns are separated by distinct gaps. However, it is very rare to get an unobstructed view of the canopy from below. In a normal forest, the space below the canopy is occupied by understorey trees, which obscure the view of the canopy. The forest in which I first saw crown shyness was a forest in which the vegetation below the canopy had been kept clear by periodic cutting of the undergrowth for experimental purposes. However, many others had gone through this forest before me without noticing the crown pattern. As soon as I saw the pattern, I had a name for it—crown shyness. This term was in my subconscious mind as a result of a study I carried out on *Eucalyptus* at the University of Tasmania. One of the books I referred to was M.R. Jacobs' (1955) *Growth Habits of Eucalypts* in which he mentioned crown shyness in eucalypt forests and attributed it to insect predation on the shoot tips. According to Jacobs, intense cropping of shoot tips at the edges of the crowns keeps the crowns separate from each other. By describing crown shyness and immediately explaining it as the result of continuous and intensive cropping of shoot tips by insects, Jacobs effectively killed further inquiry into crown shyness. However, the term 'crown shyness' stuck in my mind.

Jacobs must have overrated the intensity of shoot predation in eucalypts. Leaves have finite life spans and the successful production of new leaves at the shoot tips is necessary to compensate for the inevitable shedding of old leaves. If new shoots are so efficiently cropped by insects as Jacobs stated, the eucalypt trees would end up leafless and die because the insects would not merely crop the crown at the boundaries between adjacent trees—they would crop the entire tree crown and the trees would die from defoliation.

It has also been proposed that shoot tips are destroyed by abrasion when adjacent crowns brush against each other when they sway in the wind (Putz *et al.* 1984). This is better than Jacobs' insect cropping theory but I have found no evidence of shoot tip abrasion in Malaysia. The more likely effect of branches brushing against each other may be to cause whole branchlets to be shed, as evidenced by the abundance of shed branchlets on the ground under crown-shy canopies.

Now that we know what crown shyness looks like from below and above, it has become evident that all the trees that form the canopy of the forest tend to be crown-shy regardless of their species.

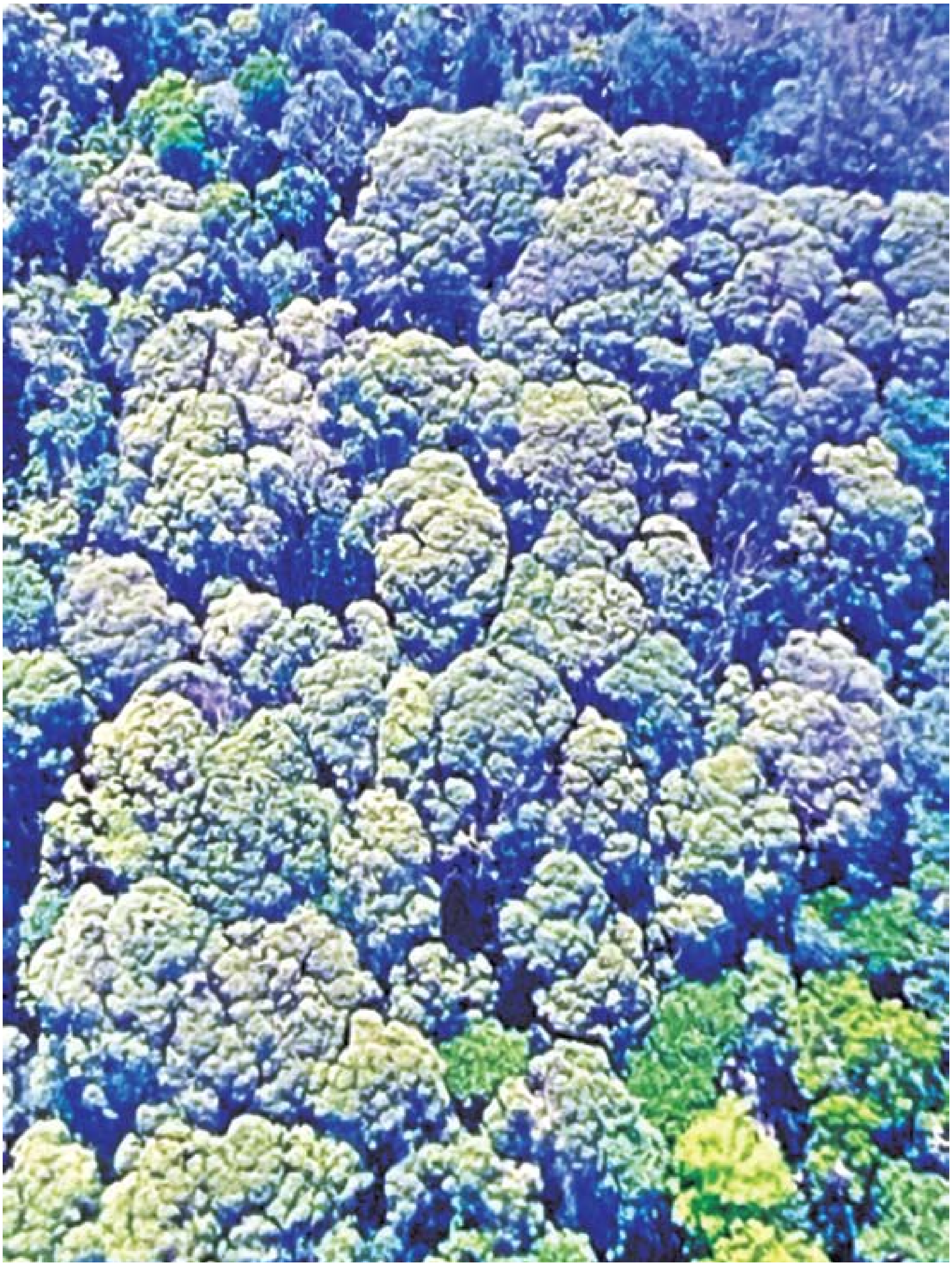

Figure 4. A forest canopy seen from above.

Figure 5. Crown shyness seen from below the canopy.

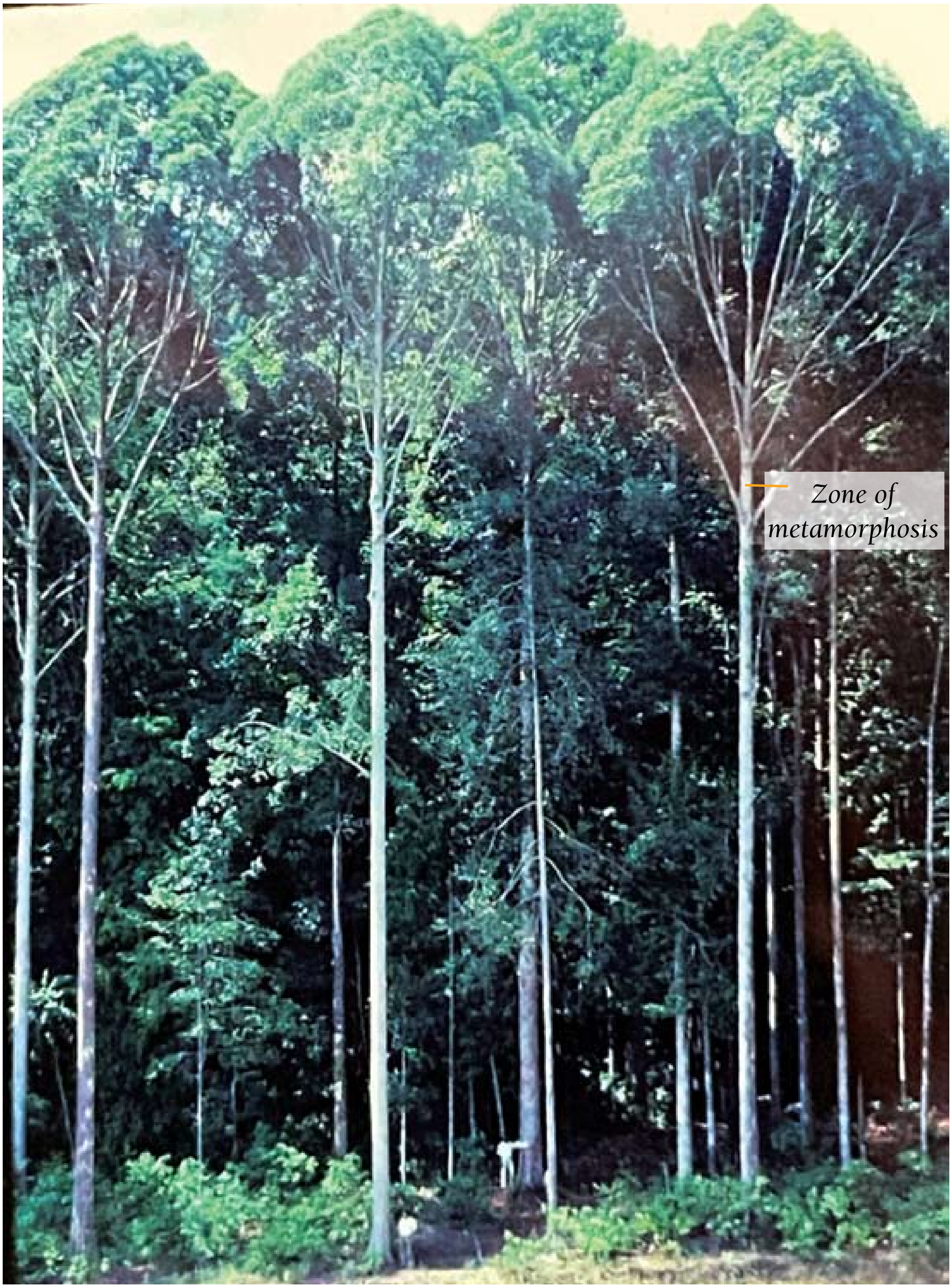

Figure 6. Zone of metamorphosis at the junction of the juvenile crown and the mature crown.

In 1981, the French botanist Francis Hallé and I used the term *metamorphosis* to describe how trees of the family Dipterocarpaceae grow through a prolonged juvenile stage in the forest understorey until they approach canopy level when a dramatic change or metamorphosis occurs. At ground level the light level is only 4% of the light at canopy level. To grow through the understorey, the juvenile plant develops a dominant central leader shoot that grows vertically into a long slender pole. Along its sides, the leader shoot produces branches that grow sideways. Such branches are produced one above another, providing the juvenile crown with multiple layers of shade-tolerant leaves. These branches have a limited life span and are eventually shed, to leave a clean trunk that gradually thickens and becomes the cylindrical logs of the timber industry. Metamorphosis begins with a change in the behaviour of the upper branches as the tree approaches canopy level. Such branches become part of the mature crown by becoming co-dominant with the leader shoot so that in effect, the mature crown is a collection of co-equal shoots that may be referred to as limbs rather than as branches. The leaves on the limbs are shade-intolerant and located at the extremities of the limbs. The overlapping of leaves at the canopy is minimal and crown shyness is an expression of the intolerance of the crowns to mutual shading.

Instinctive pattern recognition permeates the whole of science. Harvey's theory on blood circulation was based on dissections and measurements on animals and applied to humans because it was recognized that mammals have the same pattern of anatomy as humans. This pattern was already recognized by Aristotle who made dissections on animals to better understand human anatomy. The discovery of the capillaries that were predicted by Harvey was made by the Italian biologist Marcello Malpighi on the lung tissues of frogs, and extrapolated to humans and other vertebrates. The scientific theory to explain the pattern of similarities that unites all vertebrates only came about two thousand years later, in the form of the Darwin-Wallace *Theory of Evolution* that explains how species arise by evolution, with similarities in anatomy being the result of shared ancestry.

Galileo's discovery that the pendulum keeps constant time led to the realisation that all natural vibrations keep constant time and this led to the development of extremely accurate quartz clocks based on vibrations in quartz crystals, and to atomic clocks based on vibrations in the caesium atom.

Facial recognition technology is a well-developed feature of artificial intelligence but the human mind works differently from artificial intelligence. A baby recognizes its mother without the kind of training that recognition software requires. The Polynesians discovered the islands of the vast Pacific Ocean without the aid of modern instruments. A migratory bird finds its way across continents. Patterns in nature can be recognized even when we cannot explain them in mechanistic, repeatable and teachable steps.

Accidents

To a scientist who understands the role of luck, no experiment is ever a total failure. Experiments provide opportunities for accidents to happen. It is good if an experiment goes according to plan, but if an experiment does not go according to plan, we might still learn from it. A prepared mind is one that is prepared to learn from accidents.

Although lucky breaks cannot be planned, they can be generated. One of the ways in which I generate accidents is through my staff. When I give instructions to them, they will almost always do things a little differently from what I expect because different people receiving the same set of instructions will interpret them differently. The outcomes may then be different from what is expected.

A good example is what happened in a garden that I manage on the roof of a large shopping mall in Kuala Lumpur. I grew a Canary Island Palm, *Phoenix canariensis,* in the garden and this grew into a magnificent specimen. It is normally impossible to grow this palm in Malaysia but this specimen thrived in the Secret Garden. It developed a massively thick

Figure 7. Canary Island Palm six years after being slimmed down by chain-saw.

trunk in ten years and I began to worry that the palm would damage the roof by its weight. The palm had grown too tall to be taken down by the lift. I discussed the problem with my chief gardener, Supandi, an Indonesian former rice farmer, and told him to cut up the palm for disposal. When I went to the garden a few days later, Supandi had used a chain saw and sliced off the sides of the palm trunk, slimming it down to two-thirds of its original thickness. He might have intended to cut up the palm for disposal but after making an initial slice he continued slicing and ended up with a slimmed-down trunk. A normal tree would die if its bark is completely removed because the tissues of the bark perform the vital function of transporting nutrients from the leaves to the roots. However, in palms, the nutrient-conducting tissues are distributed throughout the trunk hence removing the outer layers of the trunk will not cut off the translocation of nutrients. The palm remained healthy and the exposed surface healed itself. As a botanist I could explain this, but no botanist would ever think of slimming down a tree by shaving down its sides. I rewarded Supandi with a new mobile phone. The slimming-down solution was helped by the fact that it is very difficult to fell a palm by cross-cutting because its fibres would jam up the chain saw.

Luck and the design of experiments

In the training of scientists, the emphasis on design of experiments has promoted the idea that with proper design, an experiment will always succeed. This has been a costly mistake because failure is more likely than success. The aim of experimental design should not be to design a perfect experiment but to use experiments as cheap and disposable probes for making discoveries—easy to abort in midstream and easy to reorientate according to feedback. The chances of success in experiment are improved by multiplication and diversification of experiments, not by gambling on big expensive experiments.

Empowering the subconscious mind

The subconscious mind is an important component of the prepared mind. People who are interested in many things have a larger body of information for the subconscious mind to work on than those who deliberately limit their interests.

In my first four years as a forest botanist, I spent one week every month exploring forests all over the country. My companions were members of an aboriginal community that we hired to climb trees. I usually had two or three of them in my forest expeditions. As we walked through the forest, I would scan the canopy with my binoculars and if I saw flowers or fruits, one of the men would climb the tree and cut down a branch. It would take about an hour to climb one tree and to prepare the specimens that I wanted to preserve. While waiting, I would examine other plants in the forest, small and big, and make notes. My men worked silently, for it was not their custom to engage in continuous chatter. When I was deep in my own thoughts my men would find things to do, for the jungle was their home and they felt very comfortable in it. Information about plants and forests went into my mind and remains there subconsciously. I was lucky to have had this experience at the start of my career.

Enlarging our mental boundaries

I once interviewed candidates for the post of forest products chemist. One of the candidates had a PhD in chemistry and seemed to have the right qualifications. At the start of the interview, I asked what I thought was a very simple question, to put him at ease. Between us was a wooden desk and on top was a glass plate. What is the chemical nature of wood? I asked. He did not know! What is the chemical nature of the glass plate? He did not know. I found this incredible. How could a chemist not automatically be curious about the chemical nature of everything?

In botany, close-mindedness is quite common. Those who deal with plants think of themselves as foresters, agriculturists or horticulturists and draw up boundaries limiting what they want to know. In this way they reduce their chances for making connections across arbitrary boundaries. How we use a plant is arbitrary. For example, the oil palm is treated as an agricultural crop in Malaysia, a forest crop in Bangladesh, and an ornamental plant in horticulture.

Features of the prepared mind

1. The prepared mind recognizes the importance of luck.
2. The prepared mind recognizes that accidents may have lucky consequences.
3. The prepared mind learns how to generate and recognize luck by keeping close personal contact with all aspects of the work, maintaining a high rate of experimentation, and maintaining a diversity of interests.
4. The prepared mind is sensitive to patterns and departures from patterns.
5. The prepared mind responds immediately to luck before the opportunity is lost.

Chapter 5

Observation and Experiment

Scientific theories are based on information obtained by observations made in nature or under controlled experimental conditions. The units of information obtained are called data. The data obtained are recorded in the form of measurements and descriptions. Such observations have to be open to repetition and confirmation by any reasonably competent person. This makes the act of scientific observation very different from casual observation.

This chapter discusses the different ways in which scientists make observations and experiments. The cost of research is mainly the cost of obtaining data, involving salaries, equipment, travel, and infrastructure.

The first beneficiaries of research are the scientists doing the research. It is important for scientists to master the skills of observation and experiment because they are the tools for self-instruction by which a scientist becomes expert enough to teach others and contribute to world knowledge.

Dissection. One of the earliest methods used to facilitate scientific observation is the method of dissection, to expose what is otherwise hidden from view. Dissection has provided the data needed for understanding the anatomy of the human body and of all other living things. Harvey explored the anatomy of the mammalian body by dissection to expose the blood vessels, and measured the total volume of blood and the rate at which blood was pumped out of the heart. Dissection is the basic tool of taxonomy and one of the most commonly applied methods used in scientific discovery, particularly in the tropics that is home to half or more of the world's biodiversity.

Magnification and enhancement of vision. The invention of the telescope enabled Galileo Galilei to explore outer space and see what had never been seen before. He found that the moon was not a perfectly smooth heavenly object but had landmarks like the earth, with elevated features casting shadows that moved according to the angle of the sun shining on them. He next discovered that the planet Jupiter had moons orbiting around it, contradicting the belief that the earth was the centre of the universe around which the moon, sun, planets and stars were orbiting. The invention of more and more powerful telescopes has enabled scientists to discover that the universe is rapidly expanding, without limits.

The invention of the microscope enabled Marcello Malpighi to confirm the existence of capillaries in 1661, as predicted by Harvey. The microscope opened a window for the study of cells and subcellular structures and revealed a whole new world of microscopic living things.

Objects under observation have been enhanced by various means to make them more easily observable under a microscope. These include the development of selective stains to make tissues stand out better from each other, the application of acetolysis for the cleaning of pollen grains to reveal their surface patterns, microtomes to cut very thin sections, coating of surfaces with gold to make them visible under an electron scanning microscope and so on.

Assisted sensing. Human sensing capabilities have been vastly increased by the development of scientific instruments. Thermometers for measuring temperature, clocks for measuring time, photometers for measuring light, and so on. Every new instrument for sensing, from thermometers to clocks and photometers has opened up new areas for discovery.

Experiment. An experiment is an arrangement designed to study how an action produces a reaction. All other factors are neutralised. For example, to study the effect of an experimental treatment on lettuce, a sample of lettuce plants would be given the treatment and a similar sample would be left untreated for comparison. The parameters to be measured would be the strength of the treatment applied and the reaction may be recorded as the dry weight of the plants after a certain period of growth. Other parameters are neutralised by subjecting both samples to identical conditions—same weather, same soil, same watering regime etc. In this way, the 'signal' from the experimental treatment would be free of 'noise' from uncontrolled factors of the environment.

Observations and experiments are tools of discovery but most importantly they are a scientist's tools for self-instruction. To be knowledgeable, a scientist has to develop personal knowledge and confidence by testing ideas and eliminating weak and unsustainable ones in their fields of research interest. As a biologist, I have a microscope close at hand, dissecting instruments, binoculars, camera, and a range of tools and facilities that I can use at any time. Other scientists should also try to have equipment available to check things out for themselves. Unless we are confident in our knowledge, we cannot teach others.

The myth of the perfect experiment

In the training of scientists, a lot of emphasis is placed on the statistical design of experiments. The idea that experiments can be designed to obtain mathematically precise answers was promoted by R.A. Fisher (1935) in his book entitled *The Design of Experiments*. To the scientific world in the 1930s, Fisher's ideas offered a way to perform experiments in a planned, systematic and logical manner to obtain results that would be indisputable. Editors of scientific journals began to insist on the use of statistics to support scientific claims, and training courses in scientific methodology became courses in the statistical design of experiments. Dissenting views made little or no headway.

In 2012, a story emerged in *Nature* (Begley & Ellis) that a large proportion of claims in 'landmark papers' published in leading peer-reviewed medical journals could not be confirmed by independent repetition of the experiments. This revelation shocked the scientific community because it had been assumed that peer-reviewed statistically-designed scientific experiments would produce indisputable results. This assumption had never been critically tested because there is no incentive for any scientist to systematically test the findings of other scientists except in the pharmaceutical industry. The pharmaceutical industry employs scientists to confirm and follow up interesting published research claims for possible commercial development. However, they keep their findings confidential. In practice, if a claim made by one scientist is tested by another scientist and fails the test, the other scientist will keep quiet and move on. It is too much trouble to refute another scientist's claims.

Hence errors in published research papers are almost never exposed. If a high proportion of claims made in medical journals fail the test, we can expect similar failures in all areas of science.

The shock from the revelation in *Nature* lasted only a few months. *Nature* then dismissed the controversy in an editorial entitled *Error Prone* (2012). It said, "….time and again, biologists fail to design experiments properly and so submit underpowered studies that have insufficient sample size and trumpet chance observations as biological effects." The editorial continues: "Biologists must seek relevant training and collaborate with good statisticians."

This advice is of no use because the readers of scientific journals have no idea if a paper has been certified by a 'good statistician'. Scientists have no idea who is a 'good statistician' to get advice from. Statisticians themselves are well aware of the dangers of statistics. My favourite statistician, M.J. Moroney had this to say in the introduction to his book *Facts from Figures* (Moroney 1956), "For the most part, statistics is a method of investigation that is used when other methods are of no avail; it is often the last resort and a forlorn hope".

I carried out a study as a student in Tasmania to test the theory that the presence of anthocyanins improves the rooting ability of cuttings. This involved comparing green (anthocyanin-free) clones with red-purple (anthocyanin-rich) clones of a range of different species. I found no appreciable difference between green and red-purple clones. This was a tedious study using thousands of cuttings and many hours spent counting roots. It took one year of my time to eliminate this theory. If it takes one year to test and eliminate one theory, a scientist would be doomed to a lifetime of failure because unproductive theories can be expected to greatly outnumber productive ones. In this case it was obvious after one month that any difference between plants with or without anthocyanin would be too small to be of practical use. There was no need to go through the entire statistical process. However, the influence of Fisher was so great that I felt it necessary to continue to the end in order to learn what I thought was an important component of the art of scientific experiment. It was many years before I became brave enough to question the usefulness of Fisher's approach to experiments.

To make comparisons statistically valid, four important concepts are applied. These are (1) the null hypothesis, (2) sample size (3) randomisation and (4) control.

1. ***The null hypothesis.*** In any comparison between two treatments, there would always be a difference. In biology, no two individuals are exactly alike. In all comparisons no two samples can be exactly the same. How can we distinguish a difference due to treatment from a difference due to unknown or 'chance' factors?

   Fisher argued that we have to begin by assuming that the observed difference is due to chance. This assumption is called the null hypothesis, i.e. that the treatment has had no real effect. Statistics is applied to calculate the probability of the observed magnitude of difference occurring by chance. If the probability is at most five times in 100 trials ($p=0.05$) the result is said to be significant and the null hypothesis may be rejected with a moderate degree of confidence. If the probability is at most once in 100 trials ($p=0.01$) the result is said to be highly significant and the null hypothesis may be rejected with a high degree of confidence.

Those who are attracted to science because of the idea of making discoveries are quickly disillusioned to find that Fisher has reduced discovery to the rejection of a negative, in a twisted process contrary to common sense.

2. **Sample size.** To enable statistical comparisons, the size of the samples used in experiment has to be big enough to enable means and standard deviations to be calculated with reasonable reliability. To keep costs down, the sample size is often limited to about 20. However, even a small sample size of 20 means that statistical experiments are impossible in situations where specimens are rare, expensive, difficult to handle or ethically impossible.

3. **Randomization.** To represent its population, a sample should be picked from its population in such a way that every individual in the population has the same chance of being taken into the sample. Despite its name, a random sample in statistics has to be carefully planned and organised. It is not random in the sense of being unplanned. A random sample can be obtained if the population is a bag of seeds, which can be shaken up thoroughly before drawing out the required sample. With a population of trees in a forest, we would have to assign a number to each individual and then pick our sample by shaking up the numbers in a bag before drawing out the numbers. In practice it is very difficult to assemble a random sample because that would involve tracking down every member of the targeted population to make sure that every individual has an equal chance of being picked. As a result, practically all experiments in natural communities involving so-called random samples are samples of convenience, not random at all. A convenient sample may consist of the most accessible individuals. The next sample may be from a more remote location and would not be equivalent to the first. In such cases, different samples may behave differently. The so-called random samples in many experiments are fake random samples that are not equivalent to each other.

4. **The control.** One of the most compelling ideas in the design of experiments is the idea of comparing an experimental treatment against a non-treatment or ‘control’. The sample used as a control should match the treated sample completely and be subjected to exactly the same environmental conditions apart from the treatment applied. This idea became so deeply embedded that it, together with the requirement for large samples, resulted in the most horrific abuses ever committed in the name of science. This happened in medical experiments in which two sets of patients would be used; one set given medical treatment and the other set given a fake (placebo) treatment. Patients who thought they were being treated were left to suffer in total absence of treatment. The full horror of this practice came to light as the world became aware of experiments in which prisoners and defeated populations in wartime were deliberately exposed to diseases in medical experiments to provide the statistics for scientific analysis.

   Quite apart from ethical issues, the concept of a control as a zero state can also be misleading. Early experiments on mycorrhizae involving the sterilization of soil to kill all living organisms produced amazing results. The plants grown on sterile soil to which mycorrhizae was added were much bigger than those grown on sterile soil without mycorrhizae. Mycorrhizae were then promoted as miracle treatments to improve the productivity of plants. However, in nature, mycorrhizae are universally present, so the comparison with sterile soil was completely misleading because sterile soil does not exist on agricultural lands. Compared with natural soils, the addition of mycorrhizae seldom produced real and sustained improvements. The same problem is prevalent with field tests of fertilizers. All agricultural fields already contain nutrients. The addition of nutrients may or may not help.

Louis Pasteur’s most famous achievement was to save a boy from rabies by administering a vaccine that he had developed but had never had the opportunity to test. Rabies was a dreaded disease for which there was no cure and all those affected would inevitably die. Pasteur had developed a vaccine based on his theory that a vaccine against a disease caused by microbes could be made by weakening a culture of that microbe so that it loses its virulence. Pasteur administered the treatment to the boy who had been bitten by a rabid dog, and the boy survived. It so happens that rabies has a long incubation period and during this incubation period, the vaccine was able to stimulate the immune system of the victim. The survival of the victim made Pasteur world famous. However, his treatment has been criticised on the grounds that it had not been tested against a control. This criticism is wrong because an experimental control is not needed if the result is already known. In this case, an infected person would certainly die.

The misunderstanding of controls was apparent even in experiments by a Nobel-prize-winning scientist. During World War II, a team led by Howard Florey took up the challenge to mass-produce penicillin but first they had to convince themselves that penicillin would really work. As described by Medawar (1996b), Florey decided to test the small amount of penicillin that he had managed to obtain, on white mice. Florey only had enough penicillin to treat four white mice with 10 milligrams each. He obtained eight white mice and injected all of them with huge doses of streptococcus—eight times the lethal dose—guaranteed to kill all of them. Four of the mice were then given injections of 10 milligrams of penicillin each. The other four were left untreated to act as controls. In 17 hours, the four untreated mice had all died whereas the four mice treated with penicillin remained alive although one died two days later. On the basis of this experiment, Florey and his team became confident that penicillin would work, and proceeded to raise money for what became a massive and successful effort to mass-produce penicillin.

In retrospect, a single mouse infected with eight times the lethal dose and surviving because of penicillin treatment would have been enough to demonstrate the effect of penicillin. There was no need to sacrifice four mice as controls.

Demonstration versus experiment

All the experiments we are taught to perform in science classes are demonstrations, not real experiments because they demonstrate what is already known. The art of experimentation, to discover what is unknown, to risk failure, and to emerge with a better understanding of the topic under investigation is something we need to discover for ourselves.

After developing a vaccine against anthrax, Pasteur was challenged to prove its efficacy in a public demonstration (Dubos 1950). He took up this challenge and arranged for a public demonstration. The demonstration took place at a farm in Pouilly le Fort. Twenty-four sheep were vaccinated on May 5 to protect them from anthrax and again on May 17, 1881 together with one goat and six cows. On May 31, all these vaccinated animals were injected with virulent cultures of anthrax, together with 29 unvaccinated animals consisting of 24 sheep, one goat and four cows. A crowd of observers assembled to witness this event, including farmers, veterinarians, doctors and media reporters, including a reporter for *The Times* of London. The demonstration lasted two days. The unvaccinated animals collapsed and died one by one in front of the audience and by the end of the second day, all the unvaccinated animals had died except the cows, which got sick but did not die. All the vaccinated animals stayed alive and well.

Pasteur's demonstration had been carried out at the high cost of the 24 sheep and one goat that died in the demonstration. The result was anticipated and this demonstration was considered worthwhile to convince a sceptical public of the effectiveness of the anthrax vaccine. This was not an experiment. In a demonstration, the results are known beforehand. In an experiment, the results are not known beforehand. If this exercise had been carried out as an experiment to find out if the vaccine would work, it would have been extremely foolish.

It is impossible to design a perfect experiment but quite easy to fake a perfect one if one knows beforehand what the result will be.

In summary, the testing of a null hypothesis is a poor method of discovery for the following reasons:

1. It is expensive and time-consuming because it requires large samples for statistical analysis.
2. Costly experiments cannot be modified or aborted easily—one becomes a slave to the experiment.
3. What we can discover is limited to comparison between defined treatments hence what we discover is limited by the logical framework of the design.
4. Discovery through the testing of a null hypothesis is counter to common sense; the meaning of statistical significance is lost on many scientists, who confuse it with real significance.
5. In the end, any claimed discovery has to be confirmed by actual application regardless of the statistics, so the concept of a statistically planned experiment being proven by design has no basis in reality.

Use of experiments to study correlations and cause-effect relationships

If a change in one parameter is accompanied by a corresponding change in another, the two parameters are said to be correlated. Correlations are easy to observe but cause-effect relationships usually require more work. I once rejected a paper sent to me for review in which the author described the environment in which a particular plant of special interest was growing in the forest. The author measured the light level, carbon dioxide level, and other parameters in the environment and declared that these were the factors that had to be replicated in any effort to cultivate this plant. These were correlations not supported by any cause-effect evidence.

Strategies in experiment

Keep experiments small and cheap

A creative scientist has many ideas that need to be tested. Our effectiveness depends on how fast we can eliminate wrong ideas by testing them in cheap and simple experiments. The faster and cheaper the learning process, the faster a beginner will rise to become an expert.

A big expensive experiment is just as likely to fail as a small cheap one but whereas we can easily write off a small failure, a big experiment is difficult to write off. Instead, it may suck in more resources before it is finally terminated.

Flexibility

An experiment designed at the beginning of a study can never be perfect because one cannot foresee every eventuality. As the experiment proceeds, the experimenter should be able to modify the experiment and guide it to a useful ending, which may be quite different from what was expected at the beginning.

One should have the option to stop. If an experiment is cheap to initiate, it is less stressful to abort. A big expensive experiment is a trap that has serious detrimental consequences. I know of PhD candidates who became totally demotivated as a result of being trapped in experiments that had already absorbed months of effort and which were obviously not going to produce the hoped-for results. Yet they felt they had to continue to the bitter end otherwise their investment in time would have been wasted. For a young person with high hopes, this is a disastrous experience.

Immediacy of response

The most obvious indication that a reaction is due to a treatment is when the reaction is immediate. For example, if we touch the leaves of the sensitive plant *Mimosa pudica*, the leaves close and droop immediately. After some time, the leaves return to their normal pre-disturbance state.

The immediacy of response, followed by the return to the original state after disturbance, shows that the response is a reaction to the touch. In Florey's experiment on penicillin, the results were obtained within one day. This was immediate enough to link the treatment to the cure.

Close monitoring

In a desktop experiment kept under close watch, the experimenter has full control of the experiment, and nothing can happen without the knowledge of the experimenter. In general, closely-watched desk-top experiments that can be completed in a short time are more reliable than experiments in the field that cannot be closely watched.

Serial experiments

Where the object of study is a single individual, this single individual can be experimented upon again and again in series. This was what Beaumont did in his studies on digestion.

In the development of surgical procedures, surgeons can operate only on one patient at a time. Improvements in surgery are made based on one surgery after another.

The greatest sustained scientific endeavour in the biological sciences has been to discover, name and document all the species of the world. New species are described as soon as a taxonomist decides that the specimens of that species are distinctive enough to distinguish it from other species. This process continues indefinitely because new species continue to be discovered.

Comparing treatments against a baseline

To compare the effect of a treatment against non-treatment, one can first establish a baseline by studying the characteristics of the object prior to treatment and then apply the treatment and observe what happens. This is what happens when a patient is hospitalised for a complicated ailment that cannot be immediately diagnosed. The doctor observes the patient, prescribes a treatment, and keeps the patient under observation to see the effect.

In the study of the effects of different systems of logging on water quality, the object of a study is usually a watershed, defined as an area defined by a stream and all its tributaries. No two watersheds are the same, so it is usual to first study the selected watershed for a few years to establish its baseline behaviour, and then subject it to logging to study the change in its behaviour.

In experimental studies on the behaviour of trees, we can work on the same trees repeatedly to establish baselines before making experimental changes. I once made a study on the growth cycle of a pair of trees of *Peltophorum pterocarpum* in the office quadrangle of FRIM (Ng 1980). My office on the third floor gave me a good view of the two trees from my window, so for seven years I kept the two trees under observation and worked out that they go through a cycle of change, starting with the appearance of new shoots and young leaves on a bare crown, followed by flowering and fruiting. As the fruits develop, the leaves age and are finally shed. New shoots immediately appear and a new cycle begins. One tree took six months to complete its cycle while the other tree took nine months. Having established their baseline behaviour, I experimentally defoliated the trees to study their response. I defoliated a major limb of one tree by pruning off all its leafy shoots, two months into its cycle. The defoliated limb reacted by producing new shoots and leaves, so the tree had a two-part crown, but at the end of its original cycle, all the leaves were shed, including the new leaves that had been induced experimentally, and the whole crown was unified at the start of the next cycle. The same happened with the other tree. Their basic cycles remained unchanged, at six and nine months. This explains why trees of *Peltophorum* planted in avenues in Malaysia do not flower in unison except where the weather is strongly seasonal in the north-western end of the Peninsula.

Experimenting with single specimens

Experiments conducted on single specimens account for the vast majority of experiments. A researcher on rhinos may only have one rhino to work on. A palaeontologist may only have one fossil bone, a taxonomist may only have one specimen, with no idea of when the next specimen will be found. All scientists need to learn how to make progress with a single specimen or even a fragment of a specimen. When I was a PhD student, I needed to examine the flowers on preserved specimens in the Kew Herbarium, and would get permission to dissect one flower only. I learnt how to get maximum information from a single specimen.

Whether research is done on one specimen or many, the findings can be extrapolated to its whole population or species and adjustments can be made as more information is obtained later.

Experimenting with two or more specimens

With two specimens or a small number, we can run two or more experiments in parallel to compare different treatments at the same time. We may regard one as "treatment" and the other as "control". Giving the two specimens exactly the same treatment would be a waste of opportunity.

The most I have ever paid for an experimental specimen was RM1000 which was about USD250 per seedling. I paid for two seedlings of *Amorphophallus titanum*. This plant had never been successfully grown in Malaysia. I had tried to grow one plant before, and failed. That plant died before I could figure out what corrective action to take when it started to fail. When I was offered two plants, I decided to pay the price and try again. This time, I placed one plant in full sun and the other in a partially shaded environment. As it turned out, the plant in full sun grew vertically while the plant under partial shade began to lean towards the light, indicating that it wanted more light. I moved the leaning plant into full sun. In response, it straightened out. When experimenting with living things, the experimenter must learn to read distress signals and have the freedom to alter the experimental conditions accordingly.

I manage a garden on the roof of the largest shopping mall in Malaysia, known as the Secret Garden of 1 Utama. Although I have been a gardener all my life and have personally grown over 1000 kinds of ornamental plants, as featured in my book *Tropical Horticulture and Gardening* (Ng 2006), I had no experience in growing a garden on a concrete rooftop. It took five years of experiment to get the garden into shape for public opening, with over 500 species of trees, shrubs, herbs and climbers. Whenever an attractive new plant came to my notice, I would buy it for the garden. I have successfully grown many temperate plants in this tropical rooftop garden. Apples, pears, peaches, plum, litchis and persimmons grow, but they only flower very sparingly so I stopped, to concentrate on species that are more responsive. Without a high rate of experimentation, it would not have been possible to create a botanical garden in such a short time. I would normally buy two or three plants and grow them under two or three conditions. This expands the number of learning opportunities at minimum cost.

Neutralising genetic and environmental variation

In biology, no two individuals are genetically exactly the same unless they are identical twins or clones. To eliminate genetic variation, one could use identical twins in the case of animals and vegetatively-propagated clones in the case of plants. However even genetically identical individuals can be different due to epigenetic differences. For example, if we clone a timber tree, a bud taken from a young tree will have the ability to produce a vertical tall straight trunk while a bud from the crown of a mature tree may have shut down the genes that enable the plant to build a tall vertical trunk. Such variations make research in biology very much more complicated than research in physics and chemistry.

Where the environment cannot be controlled, as in outdoor experiments, environmental variation can be cancelled by arranging for the whole experiment to be run within a compact space so that all parts of the experiment experience the same environment. The soil can be homogenized by digging up and mixing it thoroughly so that it is uniform throughout the research plot or if in potted experiments, each pot could be filled with the same kind of soil.

Record-keeping

We cannot depend on memory, so it is important to keep a written record for every experiment. I keep track of experiments in simple record books. Each experiment begins on a new page. Most experiments start and end on the same page; for example, I opened a new page for the germination of seeds of *Wisteria* but all three batches of seeds that I obtained failed to germinate, so its record was limited to two entries: the starting and the ending statements. This was a failed experiment but in spite of failures I now have records of germination of over a thousand species of plants.

I have been working on avocados for 40 years. There are no seasons in Malaysia so avocados fruit according to their own individual responses to local weather changes. Some trees never fruit and some fruit infrequently. Through my records I have discovered trees that fruit on average once a year but the most exciting find is a tree that fruits every six months.

The importance of being hands-on

In an experiment, the experimenter should be alert to anything that may affect the experiment. and not leave the running of the experiment entirely to subordinates. The recording of data should be done consistently, e.g. by the same person all the time, to avoid variations in interpretation between observers.

A late friend of mine was a well-known expert in tissue culture. She was able to detect abnormalities in cultured tissues before others could, and was thereby able to troubleshoot and terminate failing experiments quickly to save time. To be able to ‘read’ the response of living things, we need to be close and sensitive observers.

Are failures a prelude to success?

It is comforting to think that failure is a prelude to success. Unfortunately, this is rarely so. One of the rare examples of being rewarded by persistence is Paul Ehrlich's discovery of salvarsan, an arsenical compound for treatment of syphilis. Ehrlich was motivated by the discovery that some dyes selectively stain bacteria and protozoa without staining human cells. He thought that such stains might be formulated to act as magic bullets to kill targeted bacteria in the human body without harming the body. Ehrlich tested one formulation after another without success and his friends tried to get him to give up. He succeeded with the 606th compound that he tested (Beveridge 1950). Despite Ehrlich's eventual success, scientific discovery is not an elimination game in which, by elimination of failures, we get closer to success. The method of elimination only works if the number of possible solutions is fixed and the correct one must be one of them.

There is a story in Malaysia (which I am unable to verify), that a team of scientists attempted to find out why it was so difficult to rear catfish in pond culture. The catfish population would build up to a peak and then mysteriously decline. The scientists devised a system for monitoring water quality, feedstock, weather, and so on. The data failed to provide an answer. It was then discovered that a water snake had taken up residence in the pond and gobbled up the fish as they emerged from their mudholes. The snake would go into hiding when the data collectors appeared.

As a personal example of repeated failure, I can cite my experiments on *Hibiscus rosa-sinensis,* which is Malaysia's national flower. The first description of this flower was in a Chinese book written in AD 304 by Chi Han, that has been translated into English by Li (1979). This flower was recorded in cultivation in the Moluccas by Rumphius (1628-1702) of the Dutch East India Company, who gave it the Latin name of *Flos festalis* (festive flower), which is a direct translation of its Malay name *Bunga Raya.* The flower was used for decoration at festivals because picked flowers could last a whole day without wilting. It was very popular throughout South East Asia and was grown everywhere as a hedge plant in Malaysia in the 1950s when it was selected as Malaysia's national flower.

Unfortunately, it does not produce any seeds in Malaysia, so we have not been able to improve it by breeding. Many new varieties have been created in Hawaii, Australia and Thailand, but the imported new varieties are not well adapted to the Malaysian climate, being very susceptible to insect attack and diseases and many suffer from severe rates of flower bud abortion. I grew dozens of different varieties and species of *Hibiscus* and carried out hand-pollination at different times of the day and night. I carried out hundreds of experiments over a period of about ten years. This was not difficult to do because the flowers of *Hibiscus* are large and easy to pollinate. The pollination of one flower using pollen from another flower counts as one experiment. I eventually gave up. There may be something like a snake in the water that I was not aware of.

I have lost many rare potted plants because of misplaced belief in Charles Darwin. In a typical example, a plant that has been thriving begins to weaken visibly. My first thought used to be that the plant needed fertilising or moving to a sunnier or shadier place. Then I discovered that when a previously healthy plant weakens and dies, the cause is most likely to be an earthworm in the container. Charles Darwin, in praising the role of earthworms in improving the aeration of agricultural soils is responsible for the myth that earthworms are plant-friendly. Within the confined space of a container, earthworms are definitely detrimental to the roots. It took me years to decide that Darwin was wrong. Now, whenever I have a potted plant that begins to decline for no apparent reason, I tip out the pot and would usually find one or more earthworms. In tropical rain forests, earthworms are found on exposed stream banks, not in deep forests. The forests do not need them.

Confirmation of discovery is by repetition

If a newly published claim is important to us, but has not yet been independently confirmed, we need to check it out by quick experiment before we can fully trust it. Those claims that we cannot check, we should only accept provisionally, not unreservedly.

The idea of experimentation as a tool of learning and discovery is not confined to scientists. Master cooks experiment to create new culinary delights. Artists experiment to develop new forms of expression, entrepreneurs experiment with business ideas, parents experiment in raising children. Discoveries are confirmed by repetition: cooks and artists repeat their creations, successful parents repeat with other children, and successful entrepreneurs are repeat entrepreneurs. The validity of any claim depends entirely on whether it can be consistently confirmed. What scientists do is to convert personal experience into global knowledge by developing theories that are published for public evaluation and improvement.

Chapter 6

Time

Research is important but almost never urgent. Urgent things get done first, while research can nearly always be postponed till tomorrow. In the end, we run out of time for research unless we adopt a strategy that makes effective use of time. This has to take into account the fact that in scientific inquiry, failure is much more likely than success, and failures eat up time.

Suggestions for the strategic use of time

My suggestions for making strategic use of time in research are as follows.

1. Multitask and maintain a large and diversified research pipeline.
2. Avoid unnecessary precision.
3. Minimise travel time.
4. Employ the subconscious mind.
5. Raise the bar for signals.
6. Screen to bypass unnecessary details.
7. Be alert for time-saving options.
8. Avoid the pitfalls of long-term experiments.
9. Master the language of science.

Multitask and maintain a large and diversified research pipeline

Many researchers think it is better to make one study at a time to avoid distraction. This is a mistake for several reasons.

If we carry out one investigation at a time, we have no way of telling how well the investigation is progressing because we have no basis for comparison. If we run several studies in parallel, the rate of progress will inevitably be different for different studies. The smart thing to do is to complete the fast-moving studies quickly while allowing time for the slow-moving studies to develop. The investigations that are slow may require new inspiration or new external inputs that have not yet appeared and may never appear. It may be a matter of luck. By multitasking, we improve our luck.

We also need to run multiple studies simultaneously in order to publish one or two papers a year, This publication target is a useful measure of our personal effectiveness as scientists, and we need such targets as personal clocks or time-keepers.

At the start of any inquiry, we cannot predict how the inquiry will end. By maintaining a big pipeline of research projects, the successful ones will compensate for the unsuccessful ones.

Avoid unnecessary precision

Scientists can make measurements to very high levels of precision but in practice we work to a practical level of precision. For example, to determine the rate of height growth of a plant, the nearest cm is good enough. To measure in mm would be time-consuming and unnecessary and to measure in microns would be expensive, time-consuming and silly. The same applies to every other measure. There is an appropriate level of precision and there is no need to exceed that level.

Minimise travel time

Research should be kept close at hand so that we can turn to research whenever we can make a couple of hours available. Administrative duties are nearly always urgent because there may be others waiting for an administrative decision from us before they can get on with their own work. If our research has to be done at some distance from our office, requiring hours of travel time, our research is bound to suffer because we cannot easily recover from last-minute cancellations due to administrative duties like having to attend a meeting at short notice.

I used to be a reviewer for projects funded by the International Foundation for Science and it was obvious that many scientists were using research grants to supplement their incomes by claiming travel allowances. Their experiments were located far away from their home bases so they had to go on the road at least once a month. These experiments were likely to fail because they were not monitored closely enough.

Employ the subconscious mind

As an administrator I had a whole lot of matters to attend to every morning and most of these had to be cleared by the end of the day. Most matters are routine and easy to deal with. Now and then a matter crops up that is new. I find it is best to briefly study the matter and then keep it aside for tomorrow. On the next day I look at it again and if there is no good solution, I shelve it for another day. Finally, usually within two weeks, the solution will appear. This is because my subconscious mind has been working on it. This is a lot more efficient than trying to force a solution with insufficient information. The subconscious mind is like a personal assistant working all the time in the background.

To empower the subconscious mind, we need to deliberately alert it to a problem, by feeding it with information. People who are genuinely interested in many things have a much larger body of information in the subconscious mind than those with narrow interests.

To the public, an expert is one who specialises in a narrowly defined area of interest, but the narrow expert is a myth. All the experts that I know are knowledgeable about many things and are able to link many different ideas together in the search for solutions.

Raise the bar for signals

To make a discovery is often like trying to identify a particular voice from a babble of other voices in a crowded room, The voice that we want to identify is like a signal and the rest is noise.

In a study in which the signal is strong relative to the noise, the study can be completed easily. If the signal is weak, we have to make a big effort to detect the signal and even then, we might find it difficult to prove that the signal is real. In biological papers attempts to magnify a weak signal often take the form of complicated mathematical manipulation of data. It is better to raise the bar for discovery so that the discovery is evident once it is discovered and pointed out. Raising the bar ensures that we do not waste time and effort over small effects.

If a lot of effort has been put into designing a large and expensive experiment, a small effect, if shown to be statistically significant, becomes very important to the experimenter, out of proportion to its value in practical terms. Journals get loaded with such papers reporting research of low value that should have been terminated early.

Screen to bypass unnecessary details

Screening is a rapid way to identify items of special interest from a large body of items. In screening, one defines the signal and treats everything else as noise, to be ignored.

In pharmaceutical research on plants, one could develop a rapid method for detecting desired properties e.g. presence of alkaloids and then screen the flora of a country to identify those with alkaloids. Then one does not get distracted by other details.

If a disease spreads through a field of crop plants, a scientist might search for survivors and use them to build up a new disease-resistant variant.

There is a well-known story that orange carrots were developed by Frank Cuthbertson for the Campbell Soup Company by a combination of screening and breeding. Carrots used to be pale yellow in colour, but some carrots had sporadic splashes of orange. The company decided that orange carrots would be more attractive than pale yellow ones. It asked the Morse Seed Company to produce a consistently orange carrot, and Morse assigned the job to Cuthbertson. Cuthbertson and his team grew thousands of carrots and checked every one by pushing a small glass tube into each one to extract a core of tissue for examination. Carrots that were more orange than usual were replanted and allowed to seed. Such seeds were used to grow more carrots for testing. After eight years and many generations of carrots, he finally had carrots of the desired colour.

Be alert for time-saving options

Mendel's experiments were on garden peas that had a generation time of a few months. This enabled Mendel to track the fate of heritable traits through multiple generations over a few years at the rate of one generation a year. Progress in genetics shot ahead at a much faster rate when Thomas Hunt Morgan used as his study model, the fruit fly *Drosophila melanogaster,* with a generation time of about 14 days only. Fruit flies are very easy to breed and maintain in a laboratory and they produce offspring prolifically. The properties that make the fruit fly such a pest to the fruit farming industry also make it ideal for genetics research.

I was for many years a breeder of *Canna generalis*, a garden plant with many ornamental varieties. The flowers are large and easy to hand-pollinate, and seeds are readily produced. I accumulated 20 cultivars. Some cultivars were sterile and useless for breeding, but the fertile ones, after pollination, produce fruits that mature in 3-6 weeks. The seeds have hard impermeable seed coats that, if nicked with a sharp blade, will imbibe water and germinate in 1-4 weeks. The time from seed germination to flowering and maturation of seeds is 8-16 weeks. In Malaysia's climate, I can grow three or four generations of plants in one year. This is three to four times better than Mendel's rate with garden peas in his monastery garden in Europe. I managed to produce some new forms that were pretty but not outstanding. To produce truly outstanding flowers through breeding, I would have had to generate and evaluate thousands of plants, but I did not have the space and manpower for this.

The oil palm, needing three years from seed-germination to fruit-bearing will need nine years for assessment of three generations. A dipterocarp, needing 10-30 years to reach flowering stage will need 30-90 years for assessing three generations. The long generation time and the size of land needed for trees make plant breeding impractical for trees.

Fortunately, there is an escape route. Trees have multiple shoots and the bud at the tip of each shoot may mutate naturally. Most mutations are of no value but if a superior mutation occurs, an alert person may spot and clone the mutant shoot. This is the origin of many new garden flowers and improved fruit trees. Mutation is the origin of most cultivated seedless bananas.

Avoid the pitfalls of long-term experiments

Forest research organizations have had a long history of involvement in long-term experiments but such experiments have rarely been worth the effort. Most trees take decades to grow to timber size, so a tradition has developed in forestry for growth experiments to be institutionalized. The initiator for an experiment establishes the experiment and opens a file with instructions for periodic, usually annual measurements. The file is passed on as research officers get transferred or retired. In time, hundreds of such files accumulate and burden whoever is the current person made responsible for them. Most of these experiments come to an end without any closing report and nothing is learnt.

Each experiment is a response to a specific interest, but with time, conditions change and an experiment or research project can become irrelevant. For example, in the 1960s, tropical countries were encouraged to grow *Pinus caribaea* to support paper manufacturing industries. The driving force was a World Bank projection in the 1970s that the world would be facing a paper shortage due to the global rise of literacy in developing countries, especially among women. *Pinus caribaea* was identified as the best species to grow in the tropics for making paper. While the pine experiments were being conducted all over the tropics, paper-making technology changed, making it possible to use mixed tropical hardwoods for paper. Then the invention of personal computers reduced the global need for paper. The global research on *Pinus caribaea* was abandoned after three decades of world-wide effort.

I took 25 years to produce my manual of tropical fruits seeds and seedlings (Ng 1910, 1991). I planned it this way because I only had an hour or two per week available and the work had to be a single-author work to ensure consistency in the interpretation of data. It was possible to stretch out the work in small installments over a long period of time because each species is a separate sub-project and the final unification only needs to be done at the end. Hence the definition of effectiveness varies with the particular situation. The important thing is to avoid drifting along without a sense of time, which is like having no sense of purpose.

Permanent data-collecting exercises differ from long term experiments. The most important are the data from meteorological stations that all countries maintain according to international standards. Such data is meant to be used by anybody who needs to study anything in relation to climate and weather, and these can range from food production to transboundary air pollution, climate change, life expectancy and so on.

There is also an international network of forest plots in which all trees of one cm diameter and above are monitored periodically (Davies *et al*. 2021) The data from such international efforts can be used in many different ways, e.g. to track global climate change and threats to global biodiversity.

Master the language of science

A person highly competent in English may take 10 minutes to read a paper compared to one who has to spend one hour on the same paper. More importantly, the person with a good command of English may in two minutes decide that a paper is not worth reading, whereas one with a poor command has to work through it, and in the end, still not understand it.

The meaning of time for organizations and countries

William Brock (1992) has described how during World War II, it was critical to find a way to produce penicillin in large quantities to fight life-threatening infections. About 1000 chemists were mobilised at Oxford, Illinois and five other universities and seven pharmaceutical companies, in a massive effort to find a way to chemically synthesise the penicillin molecule. The chemists were confident they would succeed. At that time, chemical synthesis was the most high-tech of all scientific methodologies and chemists were the most respected of all scientists. In a separate approach, several pharmaceutical companies in the US cooperated to find ways to mass-produce penicillin biologically by growing *Penicillium notatum*. Making penicillin using fungal cultures sounded like ancient low tech, like brewing beer. As it turned out, the biologists, under Howard Florey, won the race. It was long after the war, in 1957, after other chemists had given up, that a lone chemist, John Sheenan discovered a way to synthesise penicillin.

We learn from this that success in scientific discovery is not a matter of mobilising and concentrating huge amounts of money and brainpower. Big projects are just as likely to disappoint as small ones. The problem of mass production of penicillin was overcome because there were two separate approaches—chemical and biological running in parallel. It was strategically important to pursue both approaches simultaneously because time was of utmost importance. It did not matter which approach would win.

Developing countries never had any time-management strategy in Science and Technology and were ill-advised by international development agencies in the post-colonial period. There were three fallacies they promoted, which were:

1. Leave 'pure research' to the rich countries and concentrate resources on local problem-solving applied research.
2. Do not duplicate research because duplication is a waste of resources.
3. Build on established comparative advantages.

Fallacy 1: Pure versus applied research

The idea that research can be usefully divided into 'pure' and 'applied' components, so that developing countries can make faster progress by concentrating on applied research, has created a disastrous division between thinking and doing. This idea may have had its roots in the colonial division of labour between western master thinkers and colonised providers of labour. After independence, UN agencies provided technical experts to help the newly independent countries develop independent capabilities but the colonial model was the only model they knew.

Scientists in developing countries were not expected and not encouraged to be global thinkers. Seventy years after independence, most developing countries have contributed little to theory in science, demonstrating clearly that doers who are not thinkers can only perform as marginal players in science. Only independent-minded scientists can now break the mental habits that have been entrenched for two or three working generations.

Fallacy 2: Duplication of research

To avoid duplication, research is parcelled out to designated institutions which then designate one scientist to head the study and no competition is allowed. This ignores the fact that different scientists have different capabilities and motivations. Without allowing for competition, we are held hostage by whoever is given a responsibility and there can be no change until that person retires, unless independent-minded individuals decide to take matters into their own hands.

The reason why I am into avocado research is that an international collection of about 20 clones were donated by an overseas well-wisher to the Agriculture Department of Malaysia in the 1980s to help improve the diet of undernourished Malaysians. The collection was donated to the agricultural station in Serdang and placed under a scientist who, it turned out, was not interested in avocados. It was his research assistant who took care of the plants. One day, in January 1991, this research assistant paid me a surprise visit. He came to my office on his little Honda Cub motor cycle from Serdang to Kepong, a journey of several hours, with a set of avocado plants that he had grafted, to pass on to me. He said he was about to retire, and since his boss and other scientists in the agricultural station had shown no interest, he thought the only way to save the collection was to make a set for me at FRIM. I had already retired from FRIM and was staying on for an extra month to finish some work, but I managed to have the collection planted. When I returned after several years overseas, I expanded the trials, made new clonal selections and am now on track to producing excellent clones that are well-adapted to non-seasonal tropical climatic conditions.

Fallacy 3: Building on established comparative advantage

To build on comparative advantage means that if we are successful in growing rubber, we stick with rubber and do not waste resources on other things. If we are a spice country, we stick with spices. This advice assumes that the world is static and economic circumstances do not change. But the world does change, and countries and organizations will sooner or later be stuck with obsolete activities if they do not diversify.

The strategic use of time

How we use time is a fundamental part of scientific strategy. For the individual scientist, it is a matter of trying to do as much research as possible within a personal lifetime. For a country, it is a matter of encouraging individual scientific drive and maintaining a diversity of research activities so as to keep multiple options open for long-term security.

Chapter 7

Organization

Recruitment and separation

Discussions about the relative strength of countries in research are focussed on the numbers of scientists employed and the size of national research budgets. However, the most important contributor to scientific strength is the quality of scientists. Productive scientists are immensely better than average ones, hence the really important issue is how to raise the quality of scientists.

In the recruitment of scientists, I think it is most important to select candidates who have good English writing skills and good command of basic mathematics. Candidates have to write well in English in order to publish and make impact in science, while a firm grasp of mathematics is needed because scientists need to be totally at ease with mathematical concepts such as area, volume, mass, ratio, rate of change, correlation, concentration, dilution, probability and so on.

In spite of the best of intentions, mistakes are often made in recruitment. The wrong candidates are selected, and of the candidates who accept appointment, some find that research does not suit them. In an ideal situation, the organization and the employee would arrange for an amicable separation so that mismatches are corrected quickly and painlessly. The worst situation is when an unhappy person keeps the job until retirement, thereby preventing a more suitable person from getting the job. The organization then has to pay a salary for 20 years or more, without getting any benefit. The whole field of research that has been allocated to that individual is condemned to stagnation. It would be better for the organization to offer a generous cash settlement to allow unhappy individuals to leave with dignity and start new careers elsewhere.

Obsolescence

The greatest danger facing scientists and their organizations is obsolescence. I saw an example in the 1990s when I was in the FAO. The FAO had been instrumental in establishing forestry colleges in developing countries in the 1970s. In the 1990s we received requests from such colleges for assistance to upgrade their curriculums. I thought it was strange why a college could not revise its own curriculum. Then it became clear that the lecturers, who had been trained in the 1970s, never did any research and had been teaching the same things for 20 years. They had become obsolete and were hoping for another round of training overseas. But now, they were 20 years older, closer to retirement, and had already shown lack of ability or interest in keeping themselves up-to-date.

Scientific knowledge goes out of date, and if we are not a member of the knowledge-making camp, we would automatically be in the opposite camp, being made increasingly obsolete.

The first attempt by a non-European country to catch up with Europe was Egypt under Muhammad Ali Pasha al-Mas'ud ibn Agha in 1805-1848. In order to modernise Egypt's cotton industry, Muhammad Ali bought five hundred power looms from Galloways in Manchester, England. The British did not fear competition from Egypt. William Huskisson, president of the British Board of Trade predicted that the machines sold to Egypt would be 'knocked to pieces' (Landes 1998). However, the machines were not 'knocked to pieces' because Muhammad Ali Pasha took care to pay for their maintenance, but they suffered wear and tear and became obsolete because the British were continuously improving their machines. Muhammad Ali Pasha would have had to keep importing new models to stay competitive. He did what was necessary to keep his imported machinery running, but that was not enough. It was necessary to improve the machinery and innovate better ones. What was lacking was a body of scientists, technicians and technocrats to drive the economic development of Egypt.

When I was a student in Oxford University, the electron scanning microscope had just been invented and my supervisor, Frank White, suggested that I use it to study the pollen and epidermis of *Diospyros*, the topic of my thesis. The instrument was in the Physics Department. Frank White made a phone call to the Professor of Physics and a time was fixed for me. I went to the Physics Department and met the technician in charge of the scanning microscope. He showed me what to do and then walked out of the room. I was shocked to be left alone with such advanced equipment, but had no choice but to proceed on my own. I coated my specimens with a microscopic layer of vaporised gold as instructed, and used the scanning microscope to take the photographs that I needed for my research.

In Malaysia, an expensive piece of equipment would be jealously guarded by whoever is responsible, and used as little as possible for fear of damaging it. When the equipment is purchased, technical training would be provided by the vendor, but the equipment is used so sparingly that the people who are trained can never become expert in maintaining it. After a few years, the equipment becomes obsolete and useless.

Any country that takes science and technology seriously has to get into the development of equipment, and the best places to nurture such capabilities are the research organizations and universities. S&T (Science and Technology) becomes a meaningless acronym when scientists and students are afraid to use expensive equipment for exploration, and technicians have no encouragement to develop skills in the maintenance and repair of equipment.

At one time, nearly all the cars in Malaysia were British-made and they performed badly under Malaysian rainfall conditions. That was when Toyota of Japan was able to break the British hold on the car market because their cars did not stall when water covered the road surfaces. Similarly, the local climate makes agriculture, animal husbandry and biodiversity different in the tropics, so there is ample room for research to develop technologies for tropical conditions.

Creating a knowledge-based society

In a knowledge-based society people have easy access to scientific information and are able to use such information in whatever way they like. Scientists and their institutions help to develop such a society by making scientific information available to the public. Such information would include information on flora, fauna, soils, geology, timbers etc. Of special importance are meteorological data obtained through continuous monitoring of rainfall, temperature, sunshine, wind speed and so on. It is important to make such information freely available because information is like equipment. Information can be used inventively in many different ways and combinations. If not used, information has no value and would degrade. Unfortunately, there is a disturbing trend to keep information as national secrets without understanding that information degrades and becomes useless if not kept alive by public use. It is not only the general population that is kept ignorant. The government itself becomes ignorant because unpublished information gets forgotten and misplaced.

For several years, I helped the Sarawak Biodiversity Centre (SBC) to collect information on the local uses of forest plants. Sarawak is a fascinating place for the study of how indigenous knowledge is developed because of Sarawak's headhunting history, in which heads were collected as trophies. Trust between communities would have been low in the past because one could lose one's head to some stranger approaching from behind. To what extent was knowledge shared between communities?

We went into the forests to make contact with local communities, interviewed community elders and made collections of the plants useful to them. At night, we followed up with group discussions. We projected pictures of the plants on a big screen and everybody joined in the discussions. I greatly enjoyed such sessions and the local communities were happy to tell us about their local practices without expecting anything in return. I urged

SBC to publish the information but this was not done. The information was to be kept secret to ensure that the communities would get an 'equitable share' of any future commercialisation income.

Without publication, the information collected would not get reviewed, and problems would not get identified and sorted out while the elderly informants were still alive. For example, there was a plant with leaves that members of a Bidayuh community chewed with betel nuts. The chewing of betel nuts is an ancient cultural practice spread over an immense area, from Nepal and India to China and across South East Asia to Polynesia. The ingredients used everywhere are the same: lime, betel leaves and slices of betel nut. Instead of betel leaves, the local community was using leaves of a species that I had not seen before. Why did they substitute betel leaves with another species? I could have investigated this and the result would have been a short paper, with credit given to the Bidayuh community. This had to be done quickly because betel chewing is a dying custom that will soon be dead.

There was another plant that, when cut, exuded water from the stem that was used as a lotion to clean the eyes. Why did they need to clean their eyes?

These were just some of the questions an independent-minded scientist might ask. One inquiry may lead to another and open up new possibilities. Since the information was locked up, supposedly to protect the intellectual property of the local communities, it had no future, so I ended my involvement.

The price of secrecy

I once had a conversation with a PhD student in Singapore and she told me of a fellow student who had a great idea but the idea was stolen by another student. The story had spread around the campus and made everybody secretive. What a tragedy—young eager people with great potential, learning to be secretive before learning how to use ideas to develop their intellectual capabilities and relationships. A creative person generates new ideas all the time and enjoys passing ideas around to build friendships and professional networks.

The best way to lose friends in science is to be secretive. I have been a scientist long enough to have recruited scientists and watched how their careers developed until they retired. The secretive scientists were disliked by the others and were left isolated with their secrets. Eventually those secrets lost their value because of new developments around them and by the time they were ready to publish, the information that they finally made available no longer had the impact they had expected. They would have done better by sharing information with others while their information had value.

When scientists meet and discuss scientific matters, ideas are bounced around and shaped in the process. If we worry about the ownership of ideas and stay out of discussions, we would stunt our own intellectual development. In exchanging information, we do not lose anything. Our information is still with us. Through exchange, we get information or ideas from others that we did not have before. Information exchange takes place in the course of a friendly conversation, during which we size up each other. If the other party proves to be calculating and secretive, we may decide not to develop the relationship any further. It is the secretive ones that lose.

The value of a scientist is not limited to the knowledge that the scientist has acquired. It includes the personal network that the scientist has built up. A good network means that one can write to a friendly scientist in one's network for information and get a prompt reply. At the same time, one has to reciprocate by responding quickly to calls for help from network friends.

Research boundaries

Our organizations put us into boxes that define our working boundaries. The idea seems to be that each worker should have a territory different from everybody else's. In a research organization, such boundaries discourage intellectual growth. In the end, all go down in mediocrity. The organization itself will decline because it will be promoting into managerial positions individuals from a pool of scientists with narrow rather than broad interests.

It would be better if the boundaries between boxes are made porous so that all scientists have a core area of responsibility but are encouraged to spill over freely and become cross-transferable. All professional scientists should have two professional targets (i) to become world-class scientists and (ii) to prepare for possible promotion to head their organizations.

For a professional person in any field of endeavour, it does not make any sense to settle for second-class status. This applies to doctors, engineers, architects, lawyers and members of all professions. It also applies to athletes, artists, craftsmen and other skilled workers. It must apply to scientists.

For a research organization to ensure healthy institutional succession, each scientist needs to develop a broad vision from the day they join.

Public relations

The relationship between a research institution and the public is based on how useful and friendly the institute is to the public. I was very impressed when I was in a farm in Tasmania and a team of scientists from the CSIRO (Australia's Commonwealth Scientific and Industrial Research Organization) arrived to examine a new species of weed that the farmer had reported the day before. The interaction between the farmer and the scientists showed clearly the high level of trust that existed between the agricultural community and the government scientists.

Close interaction between government scientists and the private sector is very important because the private sector has access to information through interaction with their trading partners and competitors, and are constantly challenged to improve their competitive status. Scientists in government organizations and universities live sheltered lives and have limited production experience, but have the advantages of access to laboratory equipment, and more time for research. Close collaboration would be mutually beneficial, but scientists should initiate the collaboration.

In the past, government scientists were welcomed for bringing new information to illiterate farmers and budding manufacturers. Now, farmers and manufacturers are educated, or have children who are well-educated. They can find information by themselves. As a plant scientist, I have found that the most successful farmers, fruit growers and gardeners are good at experimenting and refining their methods of production. They know many things that scientists do not know. Similarly in industry, I find that many people who go into manufacturing, mining, metallurgy, etc often have no academic training. Most amazing are people who retire from engineering and start a farm, using their engineering skills to automate their farms, devise cooling systems to grow plants that need cool weather, and so on. I find that such people are happy to exchange information with scientists so that can move ahead with any new information they get. They do not view scientists as competitors because success in business does not depend on scientific knowledge alone. Knowledge of the market, skill in management of staff, and ability to deal with suppliers, buyers and regulatory agencies are important, and scientists are not involved in such matters.

The independent-minded scientist

The world of science is a world of endless wonders, open to anyone with an exploratory independent mind, but it is difficult to predict how any scientific investigation will turn out. National planners are driven by big ideas, but success is not guaranteed by the size of funding, nor by the number of scientists employed. Big ideas may flop and small ideas may triumph.

All through the history of science, big contributions have been made by independent-minded scientists acting on their own initiative. Independent-minded scientists contribute what planners cannot plan and money cannot buy. National science budgets keep scientists employed, but the spirit of inquiry that drives science has to come from the scientists themselves.

The challenge for scientists is to raise their performance by adopting the thinking and working habits of independent-minded scientists. This book tries to explain what these habits are. I hope it will help and encourage readers in their own journeys of personal discovery in science.

Those who do not wish to go the full distance as scientists on the global stage, through publication, may still find this book useful as a guide for making discoveries for personal satisfaction.

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Index