EXCELLENCE IN ECOLOGY

16

O. Kinne, Editor

Louis Legendre

Scientific Research and Discovery: Process, Consequences and Practice

ECOLOGY INSTITUTE

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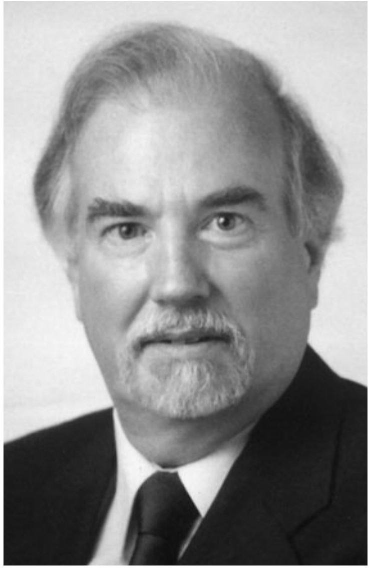
About the Author and the Book



Louis Legendre

Professor Louis Legendre has been elected by the Jury of the international Ecology Institute (ECI), under the chairmanship of Professor Richard T. Barber, as the winner of the ECOLOGY INSTITUTE PRIZE 2001 in marine ecology. Louis Legendre is Director of the Laboratoire d'Océanographie de Villefranche-sur-Mer, France. He has excelled by a blend of scientific excellence, originality, probing intellect, energy and personal charm. The pairing of his high professional performance with a unique personal aura makes him one of the most memorable marine ecologists of his generation. Born 1945 in Québec, Canada, Louis Legendre's professional career is impressive. His scientific activities bridge disciplines, latitudes and continents. After working at the Université Laval for two decades, leaving his imprint in ecology, limnology and oceanography, Louis left Québec in 2000 to accept directorship of the Laboratoire d'Océanographie de Villefranche-sur-Mer. He has received important honours: Knight of Malta, Fellow of the Royal Society of Canada, Honorary Doctor of the University of Liège of Belgium, and G. Evelyn Hutchinson Award of the American Society of Limnology and Oceanography.

EE Book 16 focuses on achievements and shortcomings of modern marine ecology. It also analyses the crux of all science: human capabilities and limitations of conducting research — of critically perceiving the world in and around us. Louis Legendre further examines the relationships between science and culture and underlines the significance of ethics, especially eco-ethics.

About the international Ecology Institute (ECI)* and the book series Excellence in Ecology (EE)

The ECI, a not-for-profit organization of leading research ecologists, honors outstanding, sustained performance in research ecology; furthers exchange among marine, terrestrial and limnetic ecologists; promotes advancement in environmental sciences; and attempts to bridge the gap between ecological science and its application in political and administrative decision making for the benefit of nature and humanity. In order to approach these goals the ECI annually sets out two international prizes, the ECI and IRPE (International Recognition of Professional Excellence) Prize, and it supports — via the Otto Kinne Foundation (OKF) — promising young environmental scientists in Eastern European countries by providing financial assistance.

ECI Prize recipients author an EE Book, taking into account ECI's aims and freely presenting their personal views, insights, concepts and criticisms. Thus, EE Books represent a unique stage for top performers.

EE Books are distributed worldwide at cost price; a considerable number of books are donated to places where they are needed most: libraries in poor countries.

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EXCELLENCE IN ECOLOGY

OTTO KINNE
Editor

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Louis Legendre

SCIENTIFIC RESEARCH AND DISCOVERY: PROCESS, CONSEQUENCES AND PRACTICE

Introduction (Otto Kinne)

Louis Legendre: A Laudatio (Richard T. Barber)

Abridged electronic edition

Available at:

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‘Every discovery in pure science is potentially subversive.’

Aldous Huxley

[*Brave New World* (1932)]

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Introduction

Otto Kinne

International Ecology Institute, Nordbünte 23, 21385 Oldendorf/Luhe, Germany

About the Book

In EE Book 16 Professor Louis Legendre fully explores the unique opportunity this book series offers to ECI Laureates: the use of a worldwide stage for freely evaluating and formulating scientific issues in the light of personal experience and insight. Normally not acceptable in scientific journals or book publications, personal views — especially those of acknowledged high performers — stimulate creativity and challenge current thinking. They can add more flavor to the soup by promoting what is often missed in scientific publications: courageous intellectual constructs, challenges of traditional concepts and visionary views into the future.

Much of Louis Legendre's book focuses on the heart of science: human capabilities and limitations of conducting research. He deals with creative discovery and the driving forces behind knowledge production and truth finding. Louis also pays attention to the effects of science on culture and the possibilities of science to counteract and control the increasingly detrimental effects of modern human societies on nature; i.e. to support and strengthen the concept of eco-ethics.

Louis hypothesizes that creative activities — scientific, literary or artistic — involve the same basic components, namely intuition, craftsmanship and pleasure. Of particular significance are his thoughts on the abilities and restrictions of the human mind to achieve truth. After considering the views of different authors he writes: 'I will take the pragmatic position, in the present book, that science cannot attain absolute truth, at least for the time being' (p. 19).

Referring to the importance of quality control in science, Louis Legendre underlines the need for critical and constructive review procedures: 'Nobody in the scientific community ... should want the dissemination of poorly tested, or even untested, information. This is, however, what happens more and more in mass communication media, especially the Internet, where the best is often next to the worst …'. He continues: '… the wide circulation ... of manuscripts prior to peer review is not a progress, but a regression away from high-quality standards' (p. 54; see also Mar. Ecol. Prog. Ser. 2000, Vol. 192: 305-313).

ECOLOGY INSTITUTE PRIZE 2001

In Marine Ecology

Professor Louis Legendre

(Observatoire des Sciences de l'Univers, Université Paris VI-CNRS, Station Zoologique, BP28, 06230 Villefranche-sur-Mer, France)

has been elected by the Marine Ecology Jury of the International Ecology Institute as the winner of the 2001

ECOLOGY INSTITUTE PRIZE

Professor Louis Legendre has advanced our understanding of how ocean ecosystems function. Elegantly integrating observation, experimentation and theory, Prof. Legendre's work contributes both pragmatic and theoretical advances. He pioneered the concept of hydrodynamic control of biogenic carbon fluxes in open ocean and coastal regimes, an advance that has importance for the future course of carbon partitioning in a world significantly altered by anthropogenic activities. His wide-ranging investigations relating physical processes to biological responses led him to develop the concept of "Dynamic Biological Oceanography." Based in part on the seasonal physical progression that characterizes high temperate and polar oceans, this broad concept involves a mechanistic understanding of species succession, photoadaptation, nutrient limitation, temperature responses, grazing and sedimentation. That Prof. Legendre's contributions are characterized by unusual quantitative rigor is evidenced by his participation in the creation and development of the new discipline of "numerical ecology." In conclusion, we cite Prof. Legendre's long-term project of developing a unified theoretical framework for biological oceanography, an ambitious undertaking that is still in progress.

ECI Marine Ecology Jury 2001:

Professor R. T. Barber, Beaufort, NC, USA (Chairman)
Professor S. W. Chisholm, Cambridge, MA, USA
Dr D. H. Cushing, Lowestoft, UK
Professor V. Kasyanov, Vladivostok, Russia
Professor S. W. Nixon, Narragansett, RI, USA
Professor R. T. Paine, Seattle, WA, USA
Dr F. Rassoulzadegan, Villefranche-sur-Mer, France

ECOLOGY INSTITUTE

The Director

Professor Otto Kinne

Oldendorf/Luhe, Germany, August 2, 2002

Ecology Institute Prize 2001 in the field of marine ecology. Reproduction of the prize awarding document

Considering the importance of mathematics in science, Legendre supports the more or less generally accepted view that mathematical expressions are constructs of the human mind; while providing key instruments for analyzing quantitative aspects of nature, they usually do not deal with nature herself.

Another major aspect Legendre brings out in his book concerns the relationship between science and culture. He looks at how the two have become increasingly separated: 'The gulf between science and culture opened during the 20th century' (p. 106). He proposes ideas which he hopes '… would reintegrate science into culture. This may turn out to be crucial not only for the scientific community, i.e. to attract bright youngsters to scientific careers, and ensure the public funding of research …, but also for society as a whole …' (p. 108).

In addition to considering such important topics as science and the public, research funding, research and politics, and careers in scientific research, Louis Legendre devotes special attention to eco-ethics. On p. 187 he writes: 'Eco-ethics appears so important and reasonable that it should have aroused strong interest in the scientific community, intellectual circles and the general public …' (see also: Inter-Research journal 'Ethics in Science and Environmental Politics' [ESEP], www.esep.de; ESEP Books 1 and 2 by John Cairns Jr. (2002, 2003); Eco-Ethics International Union [EEIU], www.eeiu.org). Legendre continues: 'I suggest that the community of interested environmental researchers sets as its central objective the definition of eco-ethics rules of conduct' (p. 118).

Louis Legendre's book is an important addition to the EE Book series. It is likely to receive much attention, to stir discussion and to promote progress in the development of the conceptual basis of environmental sciences.

About the International Ecology Institute

The international Ecology Institute (ECI) was founded in 1984. It is a non-profit-making organization of research ecologists, sponsored by Inter-Research Science Publisher. The ECI's aims and activities have been described in detail in my introduction to EE Book 3 (Gene E. Likens, *The Ecosystem Approach: Its Use and Abuse*, 1992). The ECI strives to achieve its aims by setting out awards to honor outstanding scientists: the ECI Prize (with associated EE Books) and the IRPE Prize. The Institute also supports postgraduates in eastern European countries via the Otto Kinne Foundation (OKF).

ECI and IRPE Prizes. The ECI Prize honors the sustained high performance of outstanding research ecologists. It is awarded annually, in a rotating pattern, for the fields of marine, terrestrial and limnetic ecology. We realize that the division into such general fields is not very satisfactory; however, so far it has worked quite well. Laureates are elected by a jury of seven ECI members appointed by the ECI Director.

The IRPE (International Recognition of Professional Excellence) Prize honors a young (not more than 40 years of age) research ecologist who has published uniquely independent, original and/or challenging papers representing an important scientific breakthrough and/or who must work under particularly difficult conditions. The prize recipients are elected by the ECI Jury mentioned above.

Details of Prize Winners and their books are available at the following websites:
http://www.int-res.com/ecology-institute/eci-prize/
http://www.int-res.com/ecology-institute/irpe-prize/
http://www.int-res.com/book-series/excellence-in-ecology/

OKF. The Otto Kinne Foundation supports promising young environmental scientists in eastern European countries. It aids postgraduates — without distinction of race, religion, nationality, or sex — by providing financial assistance for research projects, educational travel, and purchase of scientific equipment or published information. Details are available from the President of the Foundation: Dr. Anna F. Pasternak, Moscow, Russia (Email: pasternakanna@hotmail.com).

Further details can be found at http://www.int-res.com/ecology-institute/okf/

Nominations. Nominations for ECI and IRPE Prizes (accompanied by the nominee's CV, list of publications, and a statement why, in the opinion of the nominator, the nominee qualifies for the prize) are invited from research ecologists worldwide. They should be sent to the chairperson of the respective ECI Jury (see http://www.int-res.com/ecology-institute/call-for-nominations/) or, alternatively, to the ECI's director, who will then forward them to the chairperson. Eligible are all ecologists engaged in scientific research (except the ECI's director, the Jury's chairperson, and previous Laureates; Jury members nominated will be replaced by other ECI members). The Jury selects prize winners using the nominations received as well as their own knowledge of top performers and their own professional judgement.

Nominations for OKF Fellows, to be addressed to Dr. Pasternak (email given above) and accompanied by a letter of support as well as a brief documentation of the nominee's performance, are invited from scientists worldwide.

Ecology Institute Staff: see http://www.int-res.com/ecology-institute/staff/

Louis Legendre: Recipient of the Ecology Institute Prize 2001 in Marine Ecology. A Laudatio

Richard T. Barber

Nicholas School of the Environment and Earth Sciences, Duke University, 135 Duke Marine Lab Road, Beaufort, North Carolina 28516-9721, USA

In a scientific field of memorable and intellectually imposing individuals, Louis Legendre stands out by virtue of his blend of originality, intellect, energy and charm. He has advanced and molded modern marine ecology, but it is the pairing of his intellectual contributions with a unique personal style that makes him the most memorable marine ecologist of his generation. After an encounter with Louis Legendre, one's view of marine ecology is altered. Is it his original thinking, his enormous marshalling of evidence or his charm that leaves such an impression? I believe it's the combination of these that distinguishes Louis Legendre.

Louis Legendre's career has ranged across disciplines, latitudes and continents. Born in Québec, he studied at the Université de Montréal, obtaining both a B.A. (1964) and a B.Sc. (1967) with honors in zoology. At this early stage, his capacity for work and passion for accomplishment were already apparent. He received his Ph.D. in oceanography from Dalhousie University (Halifax) in 1971 with a dissertation entitled 'Phytoplankton structures in Baie des Chaleurs.' Following a NATO postdoctoral fellowship at the Station Zoologique de Villefranche-sur-Mer (France, Université de Paris) from 1971 to 1973, he returned to Canada to join the Department of Biology, Université Laval (Québec City), as Assistant Professor, after which he rapidly rose through the academic ranks to became Associate (1977) and Full (1981) Professor. Louis worked at Université Laval for two decades, moving freely among the disciplines of ecology, limnology and oceanography, and leaving his imprint in all three areas. In 2000 he left his beloved Québec to take a position at the Laboratoire d'Océanographie de Villefranche-sur-Mer (LOV), France, one of the flagship laboratories of the Centre National de la Recherche Scientifique. In 2001 he was named to the prestigious position of Director of LOV. The trajectory of Louis's professional positions is impressive, but it indicates only one facet of his character. Note as well these honors: he is a Knight of Malta; Confratello of the Illustrissima Confraternita del Pesce Stocco of Italy; Chevalier of the Confrérie du Franc-Pineau of

France; Fellow of the Royal Society of Canada; Honorary Doctor of the University of Liège of Belgium; and G. Evelyn Hutchinson Award of the American Society of Limnology and Oceanography.

During the early years of his research, Louis studied the phytoplankton ecology of the St. Lawrence Estuary, a marine environment dominated by physical processes, such as tides, internal waves, topographic wakes, intense turbulence and periodic stratification. All of the themes of modern hydrodynamic studies were in play in the environment he was studying. To understand the control of phytoplankton production and community structure, he blended phytoplankton ecology with hydrodynamics, which led to an important synthesis in his well-known paper with Serge Demers entitled, 'Towards dynamic biological oceanography and limnology.' Although his career started in the St. Lawrence Estuary, it soon expanded to include a number of locations in Arctic waters, lakes around the world, the equatorial Atlantic Ocean and coral atolls in the South Pacific. In this extensive range of environments he observed the same hydrodynamic processes he had come to understand in the St. Lawrence Estuary, but with widely varying degrees of strength and influence. Always a prolific writer, his many publications in marine ecology had a unifying focus: the physical regulation of biogeochemical processes in aquatic and marine environments. His tireless approach to observation and experimentation in the field provided abundant fuel for what appears to be his strongest drive, development of a theoretical framework for biological oceanography. Synthesis and unification are trademarks of Louis Legendre's voluminous professional productivity.

Since the beginning of his career, Louis has collaborated with his brother Pierre (Université de Montréal) to create and nurture a new discipline known as 'numerical ecology'. In addition to their many papers and book chapters on the topic, the first edition of their book *Ecologie numérique* was published in French in 1979. It was followed by *Numerical Ecology* in English in 1983 and, in 1984, a second French edition; a second English edition of *Numerical Ecology*, twice the size of the first, was published in 1998. Louis's brand of marine ecology combines physics and biology; in a parallel manner, the discipline pioneered by the Legendre brothers is a long overdue synthesis of theoretical ecology and experimental ecology.

Louis's prolific and creative nature makes him a natural teacher and mentor. He has, in his busy career, found time for a legion of graduate students, both Master's and Ph.D., and postdoctoral fellows. These fortunate individuals have experienced first hand his restless, energetic intellect and have watched a master in action. In his new position as Director of a major laboratory, the effects of his creativity, high energy and charm will influence an even larger arena as he begins the middle phase of a very active professional career.

On behalf of the 2001 ECI Jury for the Prize in Marine Ecology, I congratulate Louis Legendre for his contributions to marine ecology in the development of dynamic biological oceanography, the synthesis of a unifying theoretical framework for biological oceanography and the creation and nurturing of numerical ecology. I laud him for the leadership and direction he provides marine ecology through service in countless international activities and for his teaching, which ensures the continuity of our profession.

Preface

Winning the Ecology Institute (ECI) Prize is a great honour. For me, one of the main privileges attached to the prize was finally meeting the legendary Prof. Dr. Otto Kinne. The meeting took place at the International Ecology Institute, in Oldendorf/Luhe, Germany, where I was invited for the ECI Prize ceremony. I was somewhat apprehensive to make the great man's acquaintance, because I knew Prof. Kinne's treatise *Marine Ecology* (1970–1984), I had published extensively in three of the journals he founded (*Marine Biology*, *Marine Ecology Progress Series* and *Aquatic Microbial Ecology*), I had read several of the books he edited in the *Excellence in Ecology* Series and I was member of the *Eco-Ethics International Union* he created. To my great relief, I found Otto Kinne to be a most pleasant man, full of energy and interesting ideas about the role of science in society, dedicated to altruistic causes and generally enjoying life. I feel that we got on famously together. During the lavish Prize ceremony, I gave a talk on 'Scientific research and discovery', which was a preview of the present book. The following morning, I was having breakfast with Otto, his charming and dynamic wife Helga, my wife Mami Ueno and our friend Fereidoun Rassoulzadegan, who had kindly made my laudatio the previous afternoon, when Otto told me: 'I am curious to know how you developed the thoughts you presented us yesterday afternoon'. I could not provide him with a satisfactory answer there and then. I will try to do it here, as it may interest some readers. Others may go directly to the last few paragraphs of the Preface.

Researchers do not have many opportunities of presenting to specialised or non-specialised audiences personal ideas outside their own scientific fields. This is a bit strange, given that artists are often invited to talk about themselves or general topics of interest to society, and we hear business people as well as politicians expressing opinions about everything nowadays. Prize and award ceremonies provide researchers with unique opportunities to speak publicly on general topics, because they have then a captive audience that is ready to receive words of wisdom from the (ephemeral) great person of the day. This is a bit like the few minutes of fame centenarians sometimes get on their 100th birthday, when they are invited by reporters to share the secret of their longevity. I am personally disappointed every time an awardee chooses to spend the minutes of his/her acceptance speech on disciplinary fine points instead of sharing with the audience some general ideas that s/he cherishes deeply. Anyway, this is to introduce the fact that some prize and award ceremonies, over the last few years, provided me with opportunities to progressively test and improve some of the central ideas that I develop in the present book.

In 1997, I won the Quebec Prize in Pure and Applied Sciences. During the dinner-jacketed Prize ceremony, I was allowed a two-minute acceptance speech. Two minutes' time is very short, but my address was broadcasted live on television! I then introduced the idea that pleasure drives creation in all disciplines, including science. Because that viewpoint was well received, I further developed it in 1999, during the acceptance speech of a medal at the University of Québec in Rimouski. I then proposed that creative imagination combines intuition, methodology and pleasure in the process of producing original works: this is the central idea of Chapter III in the present book. Again, my thoughts were received with interest. When Prof. Dr. Kinne informed me in Autumn 2001 that I had won the ECI Prize, I knew that it was accompanied by the writing of a book. I rapidly decided that my book would be dedicated to further exploring the above ideas. I thus started developing the topic, in preparation for the talk I would give during the ECI Prize ceremony. In the meantime, I won the 2002 G. Evelyn Hutchinson Award of the American Society of Limnology and Oceanography. I used my acceptance speech (Legendre 2002) in Victoria, Canada, to introduce topics that are now in Chapters III and VI. My approach was generally well received by the ASLO members present. Some colleagues then provided me with useful suggestions, especially Prof. Peter Jumars whose text *Creating hypotheses*[1] led me to write Chapter VIII. Finally came the Oldendorf/Luhe lecture, during which I tested my thoughts a last time before starting to write the book as it now stands. I then introduced topics to be found in Chapters II, III, V and VI. After the Prize ceremony, German colleagues told me: 'You should try to convince our Minister of Research of your ideas'. I could not do that, of course, but I decided to include in the book chapters on the funding of scientific research (Ch. VII) and on the relationships between researchers and politicians (Ch. XI). The above suite of unexpected circumstances was therefore important for me, as they led to the progressive testing and development of my thoughts. This is one of the reasons why we must nominate colleagues to professional honours, which often provide the awardees with opportunities to organise and express general ideas. I am therefore deeply indebted to all colleagues and friends who nominated me in the past.

[1] This interesting text can be found on Peter Jumars' Internet site: http://www.umaine.edu/marine/people/sites/pjumars/science/create.html

The unique circumstances reported in the previous paragraph played an essential role in convincing me that my ideas about research could be of interest to colleagues. The origin of my approach goes back to the late 1960s and the 1970s, when as a young scientist I borrowed Arthur Koestler's books from my father, the late ichthyologist Vianney Legendre. I then read, among others, *The Sleepwalkers* (1959) and *The Act of Creation* (1964). These books showed me that scientific research was very different from the process described in most science textbooks, which was exceedingly rational and barely human. During the following years, as my career in biological oceanography and numerical ecology progressed, I was lucky enough to meet colleagues who liked discussing general ideas about science. Some of these colleagues kindly accepted to read the manuscript of the present essay, and they provided me with detailed comments and suggestions that led to significant improvements: my brother and co-author of numerical ecology works Pierre Legendre, the editor of the *Excellence in Ecology* series Otto Kinne and, in alphabetical order, Peter Jumars, Claude Pinel and Warwick F. Vincent. I also thank Ms. Martine Fioroni for library assistance in Villefranche-sur-Mer and the Inter-Research staff for their excellent editing of my book. During the months I was writing this book, I discussed with various people the ideas on which I was then working. Some of their questions, remarks and objections led me to develop ideas that I had not initially considered. I therefore wish to thank for their help those colleagues and students with whom I had the pleasure of discussing my approach, and who may find with some surprise results of their contributions here and there in the book.

Parts of the book, e.g. Chapter X, which deals with international research, were developed from talks I gave to students during my professor's career at Laval University, Québec City, Canada, or that I prepared for specialised workshops. Chapter IV, on scientific theories, comes from my long-time interest in theoretical aspects of biological oceanography, on which I am committed to write another book. Chapter IV also comes from my frustration with the often negative comments of reviewers on manuscripts with theoretical content that I regularly submit to primary oceanographic journals (there is no journal devoted to theoretical aspects of oceanography).

Finally, I wrote this book as a companion to Rigler and Peters' *Science and Limnology*, which was published in the *Excellence in Ecology* series in 1995. I like that book, as I also like the two other books of the late Rob Peters[2]. *Science and Limnology* analyses the scientific method, and briefly discusses scientific creativity. In a somewhat complementary manner, I focused the present book on scientific creativity, of which I consider the scientific method to be but one component.

Because the scientific method can be analysed rigorously, on the basis of written documents, it is a central topic of the philosophy of science. This is in contrast with the analysis of scientific creativity, which cannot be conducted very rigorously because only few researchers have described the complete process that led them to discoveries. Even in cases where researchers have described their own paths to discovery, the introspective reports of creative thinking may be unreliable because '…self-reports are informed by the person's tacit theories, or prejudices. Introspection is looking into one's own mind, and it shares an important feature with looking into anything else: to a large extent, you see what you expect to see' (Boden 1992, p. 242). The only part of the discovery process on which readers of the scientific literature are normally informed is that pertaining to the scientific method.

I could not use the philosophical approach in the present book because I could not rigorously document, deduct and demonstrate my assertions about scientific creativity, and also because I have not really mastered philosophy. Some of the ideas in this book come from my personal experience of research, and that of creative colleagues who shared with me their thoughts about discovery. I borrowed additional ideas from texts reporting and analysing the discovery experiences of well-known researchers. My book is the work of a practitioner, who tries to analyse his personal experience and that of other researchers in view of exploring practical consequences of the discovery process.

[2] The two other books of Robert H. Peters are: *The ecological implication of body size* (1983) and *A critique for ecology* (1991). I shall refer to Peters' three books in the present essay. I like these books not because I necessarily agree with all the author's ideas, which is not the case, but because his writings show him as he truly was: a deeply dedicated and honest researcher, a sharp thinker and a cultured humanist

Preface to the Electronic Edition

The full, printed version of this book was published in 2004 in the series "Excellence in Ecology" (Legendre 2004). Writing the book followed from winning the Ecology Institute Prize 2001 in Marine Ecology.

Since the publication of the book, some colleagues told me that although they had themselves liked reading the book, they thought it was too "scholarly" to capture the interest of university students. In agreement with the Editor of the "Excellence in Ecology" series, Prof. Dr. Otto Kinne, I thus decided to prepare an abridged version of the book, for the benefit of university students and other readers. Prof. Kinne agreed to publish the abridged book electronically, and to make it available on the Inter-Research Internet site free of charge:

http://www.int-res.com/book-series/excellence-in-ecology/ee16/

The electronic edition of the book is about two-thirds the size of the printed edition. I shortened the text by condensing and removing sentences and paragraphs in all chapters. I eliminated most quotations that were in the original text and incorporated their substance in the abridged text. I also removed the historical and etymological notes, and some tables and figures. This led to a reduction in the number of references cited. However, the abridged edition conveys all the important ideas and information that were in the original text, although providing fewer examples and developments. The electronic edition includes one table and one figure that were not in the printed edition. Readers interested in delving deeper into the subject, and getting the flavour of the original quotations from the many authors who inspired the book are encouraged to acquire the full, printed version of the book:

http://www.esep.de/eebooks/index.html

I would be pleased to receive comments and suggestions from readers at the following email address:

legendre@obs-vlfr.fr

I THE SCIENTIFIC ACTIVITY

In this first chapter, I examine some basic approaches that are not generally thought to be connected with the scientific activity. One of these, which is called ‘high-knowledge work’, comes from the field of economics; another, called ‘creation’, is more often associated with the field of arts than that of scientific research. I show that these two approaches can be viewed as two complementary aspects of research, and I combine them to define ‘scientific creativity’. I also discuss two controversial questions: Which limits does the human mind impose on knowledge? Is science coming to an end?

In the following three chapters, I shall explore the nature of scientific research and discovery, scientific creativity, and scientific theories. In the remainder of the book, I shall examine practical consequences of the ideas developed in the first four chapters.

Knowledge Work

Modern societies are characterised by a wide variety of work activities. When considered from the viewpoint of *economics*, these activities include manual work, service work, and knowledge work. *Manual work* comprises resource-based activities, crafts and industrial jobs. *Service work* includes the distribution of goods, and commerce. *Knowledge work* includes technological, professional and research activities. Because all work activities are essential to the functioning of societies, there is no inherent hierarchy among them: each activity is relevant or not to the situation at hand, e.g. for repairing a leaking pipe, a reasonably competent plumber is infinitely better than a Nobel-prize astrophysicist.

The present Section (largely inspired by Drucker, 2001) focuses on KNOWLEDGE WORK[3], which consists in the application of theoretical knowledge to practical issues. Knowledge workers include technologists, professionals and research scientists. Examples of *technologists* are computer technicians, laboratory technicians, manufacturing technologists, paralegals and software designers. Examples of *professionals* are accountants, chemical engineers, high-school teachers, lawyers and medical doctors. Examples of *research scientists* are biologists, demographers, economists, oceanographers, psychologists and sociologists. The latter two categories, i.e. profes-

[3] Throughout the book, each word or expression in small capitals introduces a definition. All these words and expressions, together with their definitions, are collected in the Glossary.

sionals and research scientists, are known as 'high-knowledge workers'. KNOWLEDGE refers to the body of information acquired by humankind.

Here are some characteristics of knowledge work, in a nutshell. (1) Knowledge itself is the *key resource* in knowledge societies. Hence, knowledge workers collectively control the economy in such societies. (2) Because modern knowledge is specialised, knowledge workers operate in organisations in which specialists from various fields work together toward a common end product; these organisations include research and teaching institutions, and corporations. In their organisations, knowledge workers see themselves as equal to those who retain their services; in other words, knowledge workers think of themselves as *professionals*, not employees. (3) Knowledge workers tend to *identify themselves with their specialties* instead of with the organisations where—and not, for which—they work. As a consequence, they are highly mobile among organisations, regions and countries. (4) Knowledge workers often consider *professional performance and achievement* to be as important as money, if not more. They see their jobs as a life, not a mere living. (5) Because knowledge must be acquired anew and progressively improved by every individual (i.e. it cannot be inherited or bequeathed), every person involved in knowledge work has the possibility of continuously *moving upwards professionally*. Of course, a small fraction only of the knowledge workers are outstanding successes, but a very large number are reasonably successful. (6) There is a high price to pay for the upward mobility in knowledge societies: *severe competition*, which may end in personal failure. Hence, knowledge workers often develop, aside from their professional lives, non-competitive lives and communities of their own, which provide them with opportunities for personal contributions and achievements other than professional.

One noteworthy aspect of knowledge work is that it can be done equally well by *men and women*. In many countries, for example, the majority of university students in fields that were traditionally dominated by men are presently women, e.g. medicine, law, high-school teaching and some fields of scientific research. Conversely, men are increasingly present in some activities that were previously reserved for women, e.g. nursing. In other words, all knowledge workers irrespective of gender apply the same knowledge, do the same work, are governed by the same standards and judged by the same results.

Even if the above characteristics of knowledge work are borrowed from economics, and therefore refer to the general labour market, I find that most of them correspond well to the research environment that I know. I will therefore use some of these elements in the present book, when analysing scientific research and creativity.

Creation

The word 'creation' generally means 'to bring into being, or form out of nothing'. However, in the context of this book, creation refers to a more specific human activity, i.e. the *creative act*, which is not to create something out of nothing, but instead to uncovers, select, re-shuffle, combine and synthesize already existing facts, ideas, faculties, skills. Within the context of the present book, I define CREATION as the production of *original works* through *imaginative skills*.

Most, if not all, people are creative to various extents. The present book does not deal with general creativity, which is a broad topic, but focuses instead on 'professional' creativity. *Professional creators* are people who make a living out of their creativity; they comprise the artists, the specialists of Humanities and the scientific researchers. Hence, within the context of the present book, creative works belong to the arts, the Humanities and the sciences. The ARTS include music, performing arts (dance, opera, singing, theatre, etc.), visual arts (cinema, drawing, painting, photography, sculpture, etc.) and writing (literature). The HUMANITIES cover the classics, history, history of art, language, literature and philosophy. Some specialists put the arts among the Humanities. The SCIENCES comprise the natural and social sciences and mathematics. Contemporary philosophers often work on questions formulated within the context of other disciplines, e.g. the arts, languages or sciences (philosophy of science is discussed in Chapters 2 and 4). *Scientific researchers* are not only high-knowledge workers, but also professional creators.

I explained in the previous Section that scientific research belongs to high-knowledge work, and I will show in Chapter II that it is a highly creative activity. Hence, even if creation is not generally recognised by economists as a separate type of activity, I consider that it is the *extreme case of high-knowledge work*.

I will briefly report here on *three different and complementary approaches to creation and creativity* that span the 20th century. These are the approaches of Poincarré and Hadamard, Koestler, and Boden.

The *French mathematician Henri Poincarré* (1854–1912) wrote several books on the philosophy of science during the last decade of his life. Many of his views were later reported by the *French mathematician Jacques*

Hadamard (1865–1963), in his 1949 book on the psychology of invention in mathematics. According to Poincaré/Hadamard, creation in mathematics proceeds in four steps. (1) During the first phase, called *preparation*, the researcher consciously attempts to solve the problem using proven methods. (2) When this turns out unsuccessful, the mind is put to other questions or problems, but the ideas continue to be unconsciously combined with much more freedom than during the first phase; this is the *incubation* phase. (3) After minutes, months or even years, there is 'sudden illumination': this is the *illumination* phase. (4) Following the third phase, the new idea is subjected to testing. This fourth phase has been called *'verification'*, or *'evaluation'*. Poincaré could not explain the unconscious work that goes on during the second and third phases, but he knew that this work had to be preceded and followed by periods of conscious work, corresponding to the first and fourth phases, respectively. This four-phase approach can be broadly applied to both artistic and scientific creation.

Hungarian by birth, the *British novelist and philosopher Arthur Koestler* (1905–1983) took up the question of creativity from where Poincaré/Hadamard left it, and tried to account for their second phase: incubation, which leads to illumination. In *The Act of Creation* (1964), he made very insightful suggestions explaining how creation happens. It is important to note that Koestler, contrary to some previous authors, did not think that intuition is mysterious or superhuman.

Koestler's analysis of creation was based on the idea that the creative process consists in the *discovery of hidden similarities*. His analysis is based on the concept of 'bisociation', which he defined as the sudden interlocking of two previously unrelated skills, or matrices of thought. In his book, he systematically compared three types of creation—humour, science and arts (visual and literary)—which he explained in terms of bisociation. Let us take the three types in turn. (1) In *humour*, a given situation or fact is presented in two contexts that are usually incompatible; the sudden shift from one context to the other creates laughter or amusement. One example from Kostler's book: 'A convict was playing cards with his gaolers. On discovering that he was cheating, they kicked him out of gaol'. (2) In *science*, there are many unrelated types of reasoning; scientific discoveries result from the synthesis of two previously unrelated lines of reasoning. One example is Newton's discovery of gravitation, through the bisociation of the falling of an apple to the ground and the revolution of the Moon around the Earth. (3) In *arts*, creation consists in finding new relationships between the subject of the work and its means of expression. One example, in poetry, is the bisociation of sound and sense, rhythm and meaning. Hence, the approach of humour is comic comparison, that of science is the discovery of hidden analogies and the approach of arts is the revelation of poetic images (Table 1, which also includes Koestler's viewpoint on emotions, to which I shall refer in Chapter III). Even if Koestler's approach does not cover all aspects of discovery, I find it to be very useful.

British psychologist Margaret Boden thought that Koestler's account of creativity was not fully satisfactory. In her book *The Creative Mind* (1992), she took up the question of creativity from where Koestler left it. As Koestler did, she considered that the creative processes are fundamentally *similar in the arts and sciences*. Her central thesis was that creativity is based on everybody's abilities, but *creators draw on these abilities crucially*. The abilities involved in creativity include noticing, remembering, seeing, speaking, hearing, understanding language and recognizing analogies. Hence, according to Boden, creativity would be a general human characteristic, which relies on the gathering of knowledge and experience.

I explained at the beginning of this Section that my book does not deal with general creativity, but focuses instead on *professional creators*, i.e. people who make a living out of their creativity. So, my interest in the above theories of creativity does not lie in their fundamental aspects, which are interesting in themselves but belong to psychology, but instead in understanding what makes some people so creative that they can make a living out of it. In other words, I want to understand which characteristics people must have to be professional creators. Hence, within the context of this book, it does not really matter whether other people actually or potentially share (or do not) the same abilities as those of professional creators. What matters to me here is that professional creators actually have the abilities out of which they make a living.

Table 1. Three characteristics of creation in humour, science and arts; according to Koestler (1964)

Characteristics	Humour	Science	Arts
Approach	Comic comparison	Discovery of hidden analogies	Revelation of poetic images
Two components of bisociation	Two incompatible contexts	Two unrelated lines of reasoning	Subject of the work and means of expression
Emotions	Explosion of tension	Explosion and catharsis	Catharsis of self-transcending emotions

Boden provided some information on the *special abilities of creators*. She wrote that, even if creators do the same things as other people, they do them better. This is because creators have expertise, which is essential, i.e. if one does not know the rules, one can neither break nor bend them. Professional creators may have more wide-ranging, more many-levelled and more richly detailed mental structures than other people, and their exploratory strategies may be subtler and more powerful. They can generate possibilities that other people cannot imagine. For professional creators, motivation is crucial, and strong commitment prevents them from dissipating their energies on other things than creation. Often, this commitment involves not only passionate interest, but also self-confidence. In addition, some special abilities (e.g. musical, mathematical or graphic) may be innate to some extent.

The above combination of ‘ordinary’ abilities that are pushed by creators to extraordinary levels, combined with perhaps innate abilities, is what I will call INTUITION in the remainder of this book. Professional creators achieve the high standards described in the above paragraph, because these abilities are absolutely essential for them. I consider that *intuition* is largely innate, and can be either cultivated or squandered.

Another interesting book on the general topic of this Section is *Creativity: Flow and the Psychology of Discovery and Invention* (1997), written by *American psychologist Mihaly Csikszentmihalyi*. The book is based on interviews with 91 highly creative individuals, from the arts and Humanities, the sciences, business and politics, and 3 inventors. Using these interviews, the author illustrates what creative people are like, how the creative process works, and what conditions encourage or hinder the generation of original ideas. For him, creativity requires the interaction between three elements of a system: (1) the *domain*, which consists of a set of symbolic rules and procedures (e.g. oceanography), (2) the *field*, which includes all the individuals who act as gatekeepers to the domain (e.g. researchers, editors of oceanographic journals, funding agencies) and (3) the individual *person*. His thesis is that *creativity occurs* when a person, using the symbols of a given domain has a new idea or sees a new pattern, and when this novelty is selected by the appropriate field for inclusion into the relevant domain. Hence, creativity is any act, idea, or product that changes an existing domain, or that transforms an existing domain into a new one, but a domain cannot be changed without the explicit or implicit consent of the field responsible for it. The author showed that a genuinely creative accomplishment is almost never the result of a sudden insight, a light bulb flashing on in the dark, but comes after *years of hard work*.

Absolute Limit to Human Knowledge?

According to the definition given at the beginning of the previous Section, creation is the product of the imaginative skills of human beings. Hence, creation reflects to a large extent the functioning of the *human mind*. How far does the human mind influence or limit our creation potential? I shall illustrate my question with two examples drawn from science.

I borrow the first example from physics. Physicists have observed that, according to circumstances, light behaves as either a continuous electromagnetic wave—visible light occupies the wavelengths between 400 and 700 nm in the electromagnetic spectrum—or discrete particles, called 'photons'. Although the two descriptions are mutually exclusive in terms of traditional physics and philosophy, the theory works remarkably well. This approach, although satisfactory for theoretical physicists, does not make the dual phenomenon intelligible to human minds. In other words, even if we accept a scientific theory stating that two apparently incompatible states can co-occur, this does not contribute to our understanding of the underlying natural phenomenon. For me, the paradox illustrates *one of the limits of the human mind*: we do not necessarily succeed at understanding what the weight of evidence forces us to accept.

The second example comes from geometry. The German mathematician Georg F. B. Riemann (1826–1866) purposely created a geometry in which the first Euclidean postulate does not apply (that postulate states that 'there is a unique line through a given point that is parallel to a given line'). This and other non-Euclidean geometries were intended as pure mathematical constructs, with no bearing on Nature. However, less than a century after its creation, Riemannian geometry played a key role in the development of the theory of General Relativity. This suggests *another possible limit of the human mind*: even if we try very hard, we may be unable to make an intellectual construct that is outside our mental representation of Nature.

The limits that the human mind imposes on knowledge are at the core of a very stimulating book by the *pioneer of sociobiology and biodiversity Edward O. Wilson*. The central idea of his book *Consilience. The Unity of Knowledge* (1998) is that all tangible phenomena—belonging to science, the Humanities or the arts—are based on material processes that are ultimately reducible, in some instances through long and convoluted sequences, to the laws of physics. According to Wilson's thesis, it should be possible to link facts and fact-based theory across disciplines, so as to create a unified groundwork of explanation. He called the convergence of knowledge from different disciplines CONSILIENCE. Of course, consilience across the main branches of knowledge does not presently exist, but Wilson (1998) explored how it could be achieved eventually. The belief in the *unity of science* goes back to the Greek scientist and philosopher Thales of Miletus, who lived in Ionia from the late 7th century to the early 6th century BC (the region of Ionia is located on the eastern shore of the Aegean Sea; it was part of Greece in the Antiquity and is presently in Turkey). By reference to Thales, the conviction that the world is orderly and can be explained by a small number of natural laws is called the IONIAN ENCHANTMENT.

Fig. 1 summarises my understanding of Wilson's thesis: it sets the different types of human creative activity in order of increasing complexity, from the natural sciences to the social sciences, the Humanities and finally the arts. In that scheme, the 'simplest' natural sciences, i.e. physics and chemistry, provide bases for the more complex Earth and Life sciences. Even if this is not generally the case presently, the natural sciences could provide bases to the social sciences, these to the Humanities, and the latter to the arts. In the present situation, links exist among various creative activities, but their extent varies widely, from complete uncoupling to merging into interdisciplinary fields. If *consilience* were achieved, all creative activities would be interlinked, and the laws governing them would be unified. In Fig. 1, the progressively larger white and grey envelopes in the upper and lower panels refer to the same creative activities. In the present situation (upper panel), creative activities are only partly connected, as represented by envelopes that are connected at their bottoms only. If consilience were achieved (lower panel), the disciplines would be fully interconnected, as represented by envelopes that are connected at both top and bottom. The arrows and the dark diagonal band symbolise the unification of the laws governing all activities, i.e. consilience.

One crucial question concerning the eventual unification of knowledge is whether it is possible or not for humans to attain *absolute truth*. I am not questioning here the reality of observed phenomena. The question under discussion is: can we accept as absolutely true any law, theory or paradigm developed by researchers for interpreting the observed phenomena? (The words 'phenomenon', 'law', 'theory' and 'paradigm' are defined in Chapter II, Section 'The Nature of Scientific Discovery'). By ABSOLUTE TRUTH, I mean interpretations of observed phenomena that would be the same for all researchers, and for all times.

The *philosopher of science Sir Karl Popper* (1959) is largely responsible for convincing the scientific community that we cannot verify a theory; we can only *falsify* it. He held that science progresses by discovering mistakes and correcting them, not by establishing truths. Hence, the general opinion of most scientists and philosophers is that achieving absolute truth cannot be, and never will. In other words, our laws, theories or models are all provisional and ephemeral.

Contrary to general opinion, Wilson (1998) argues that the answer to the question 'Is it possible to attain absolute truth?' could well be 'yes'. He thought that it could be possible to *diagnose and correct the misalignment* that occurs between freestanding reality (outside our heads) and the reconsti-

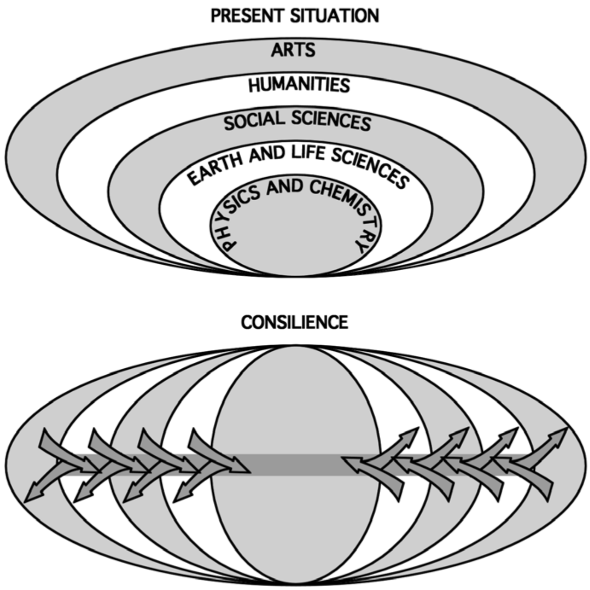


Fig. 1. Schematic representation of Wilson's (1998) thesis on consilience: relations among different types of human creative activity, presently (upper panel) and after the achievement of consilience (lower panel). Creative activities are set in order of increasing complexity, from the natural sciences to the arts. Physics and chemistry provide bases to the Earth and Life sciences; the natural sciences could do the same for the social sciences, these for the Humanities, and the latter for the arts. Presently, there are links among the various creative activities, but the extent of these links varies widely, from complete uncoupling to merging into interdisciplinary fields. If consilience were achieved, all creative activities would be interlinked, and the laws governing them would be unified. The progressively larger white and grey envelopes refer to the same creative activities in the upper and lower parts of the figure. Present situation (upper panel): the envelopes are only partly connected (i.e. at their bottoms). Consilience (lower panel): the envelopes are fully interconnected (i.e. at their tops and bottoms); the arrows and the dark diagonal band symbolise the unification of the laws governing all activities, i.e. consilience (Original)

tution of reality based on sensory input and the self-assembly of concepts (inside our heads). The distortion of the alignment of outer reality with its inner representation would exist because the human brain developed through evolution to maximise survival in the Earth's environment, and only incidentally to understand the world at a depth greater than needed for surviving. If research eventually led to full definition of the biological processes of concept formation, it might then be possible to use that knowledge to diagnose the misalignment between the outer reality and its mind representation, and using the diagnosis to correct the misalignment, i.e. attain absolute truth.

One aspect of the above thesis, which was not discussed by Wilson (1998), is the fundamental difference between *diagnosis and cure*. Indeed, even with a perfect diagnosis of the misalignment (*sensu* Wilson 1998) between the outer reality and its mind representation, it may not be possible to correct the diagnosed misalignment and thus attain absolute truth. Fig. 2 schematically illustrates two hypothetical effects of the human mind on the perception of outer, true reality. In the figure, the outer reality is the geometric shape at the top. On the left-hand side, the human mind is 'blind' to 25% of reality (shaded areas in the upper diagram), which causes a partial mind representation (middle diagram); the most parsimonious model built from the available elements (bottom diagram) is quite different from reality. In this hypothetical case, even if the functioning of the human mind were perfectly known (i.e. which are the areas the human mind cannot see), it would be impossible to reconstruct the outer reality from the elements available to the mind. On the right-hand side, the human mind has a 30° slant relative to reality (upper diagram), which causes a distorted mind representation (middle diagram); a simplified model derived from the distorted picture (bottom diagram) is quite different from the outer reality. In this second hypothetical case, if the functioning of the human mind were perfectly known, it could be possible to reconstruct the outer reality from the distorted picture in the mind. This schematic example shows that the misalignment between the outer reality and its mind representation may be such that, even if the functioning of the human mind were perfectly known, it would not be possible to retrieve absolute truth from the distorted mind representation.

It may be that only some people, who we call 'creators', catch brief glimpses of 'true' Nature, when they make discoveries. If this were the case, it might not be possible to reconstruct the whole of Nature from these brief glimpses.

So, could the human brain eventually achieve absolute truth, as defined above? Prof. Otto Kinne published his views on the matter in the rather

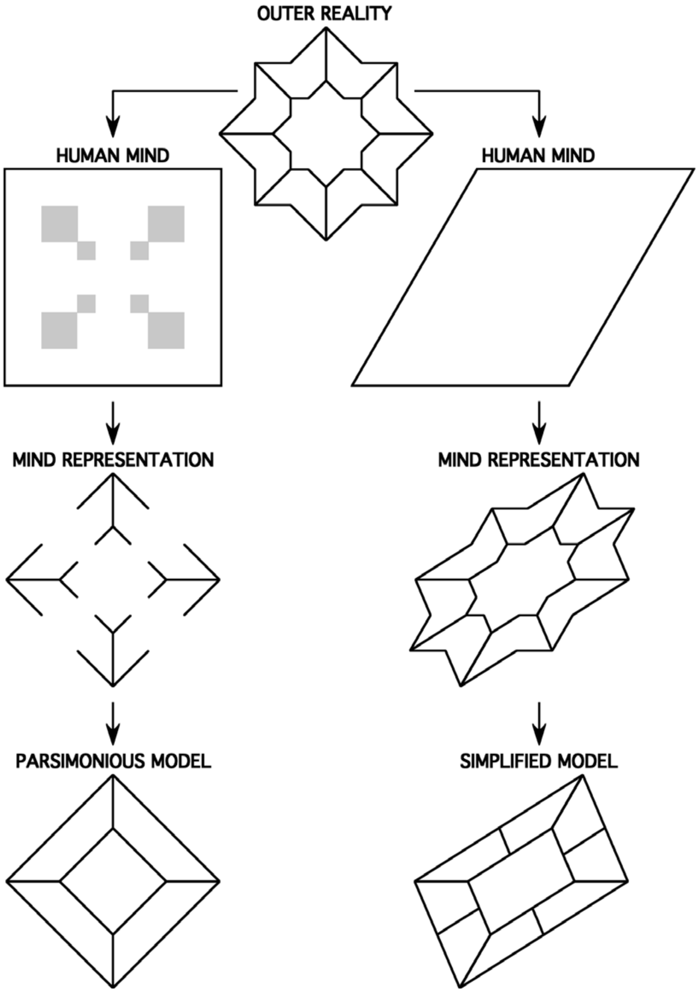


Fig. 2. Schematic illustration of two hypothetical effects of the human mind on the perception of outer, true reality. Outer reality is the top geometric shape. Left: the human mind is ‘blind’ to 25% of reality (upper diagram, shaded areas), which causes a partial mind representation (middle diagram); the most parsimonious model built from the available elements (bottom diagram) is quite different from reality; even if the functioning of the human mind were perfectly known, it would be impossible to reconstruct outer reality from the elements available to the mind. Right: the human mind has a 30° slant relative to reality (upper diagram), which causes a distorted mind representation (middle diagram); a simplified model derived from the distorted mind picture (bottom diagram) is quite different from reality; if the functioning of the human mind were perfectly known, it could be possible to reconstruct outer reality from the distorted picture in the mind (Original)

unusual form of a novel, *Suchen im Park* (1996; *Searching in the Park*). In a nutshell, Kinne's thesis is as follows: (1) Life on Earth is billions of years old. The resulting ecosystems can function successfully only if each of their countless members has its own *unique functions and structure*, and its own *unique 'window'* on the world. Such differences and restrictions are the prerequisites for successful long-term coexistence and co-evolution of millions of different life forms, e.g. the world of an earthworm is different from that of a bat, and the world of a fish is different from that of a bird. The same differences and restrictions also pertain, of course, to the recent species *Homo sapiens*. (2) Our structures and functions are constructed and programmed as parts of ecosystems. The *unlimited recognition of the real world* is neither part of the construction nor part of the programme. Our sensory equipment regulates the recognition and evaluation of each phenomenon and problem. Our brain formulates its own questions, provides its own answers, and breeds its own mistakes. It would be a catastrophe if *Homo sapiens—*or any other species—could overcome the restrictions and be able to 'look behind the scene', i.e. to see and comprehend the real world and to use the insight thus gained for maximizing its own capacity and dominance. (3) Nature protects her ways by granting each of her creatures access only to that *fraction of the real world* that is pertinent for its existence. (4) We presently witness a *growing conflict* between Nature, which must keep her ways intact, and *Homo sapiens*, who strives to break its ecosystem chains. We cannot solve this conflict, only live with it—as long as it does not get out of control.

The answer to the above question 'could the human brain eventually achieve absolute truth?' may well be negative, but nobody knows for sure.

The End of Science?

In his book *Consilience. The Unity of Knowledge*, which I discussed above, Wilson (1998) foresees the achievement of absolute truth—the whole reality—within the first half of the 21st century. If this happened, it would mean the *end of science* as we know it.

Similar views have been expressed, to various degrees, by other people, on the basis that we could *not continue to discover* profound new truths about the universe forever (e.g. Horgan 1997, *The End of Science*). If science were continuously accumulating *truths*, it would rapidly come to an end. Of course, I agree that many and even most of the phenomena evidenced by scientific research are true, although their descriptions are often refined or modified, but I do not think that one can demonstrate the truth of scientific laws, theories or paradigms (see Chapter II, Section ‘The Scientific Method’).

The idea that science is coming to an end has been around for a long time. More than 23 centuries ago, the *Greek philosopher Aristotle* thought that humanity had come to the end of technological developments. A little more than a century ago the foremost *German biologist Ernst Haeckel* published a book entitled *The Riddle* of *the Universe* (1905; http://www.archive.org/stream/riddleofuniverse00haecrich) in which he enumerated seven Great Riddles of the Universe, of which he considered that six were “definitely solved” and the seventh had no real existence.

The fundamental questions raised by the above discussion are: Can we demonstrate that some of the mechanisms, laws, theories or paradigms we propose to explain observations are absolutely *true*? If so, are we close to the end of science, or in other words, is the era of major, fundamental scientific discoveries over? The matter is open to discussion. I tried to show in the previous Section that absolute truth might be beyond the reach of the human mind. The matter will be further discussed in Chapter II. Be that as it may, I will take the pragmatic position, in the present book, that science cannot attain absolute truth, at least for the time being, and therefore *science is not coming to an end*, at least soon.

Scientific Creativity

The various types of knowledge work are generally identified by their characteristic activities, not their aims. For example, the aim of medicine is the maintenance of health and the prevention, alleviation, or cure of disease, but medical practitioners are called physicians, not healers. In the same way as medicine, the aim of scientific research is discovery (see Chapter II), but the practitioners of science are generally called researchers, not discoverers. This indicates a focus on the *activities of scientific research instead of its aim*, i.e. discovery. Consistent with this focus, philosophers of science have devoted much attention to the ‘practice of research’, i.e. the SCIENTIFIC METHOD, and significantly less to the ‘practice of discovery’, i.e. SCIENTIFIC CREATIVITY. However, because searching is part of discovering, the scientific method is part of scientific creativity (Fig. 3). The latter point will be further developed in the remainder of this book.

I already pointed out that researchers are both high-knowledge workers and creators. The *scientific method* is central to their education as *high-knowledge workers* (first Section of the present Chapter). *Scientific creativ-*

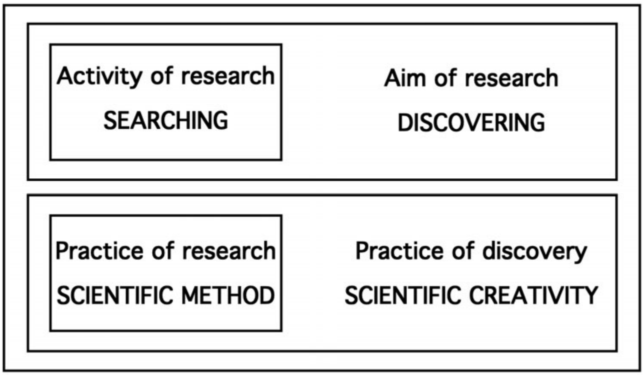


Fig. 3. Searching, which is the activity of research, is part of discovering, which is the aim of research. Hence, the scientific method, which is the practice of research, is part of scientific creativity, which is the practice of discovery (Original)

ity is central to their successful training as *creators* (next Chapters).

I will devote the present essay to the analysis of *scientific creativity as a whole* instead of focusing on its methodological component, i.e. the scientific method. My rationale for that choice is as follows. On practical grounds, there already exist several excellent books on the scientific method written by researchers, whereas there are few books presenting the viewpoint of researchers on scientific creativity as a whole. There are also fundamental reasons to my choice, which are the bases of this book. (1) *Discovery* is the aim of scientific research. However, because only a few researchers have analysed the process of discovery, this key process remains poorly understood. (2) *Scientific creativity* is an essential condition of discovery. Because discoveries largely determine progress, be it intellectual, social or economic, it is therefore crucial to understand scientific creativity. (3) The lack of understanding of scientific creativity can have negative consequences in various aspects of life. These include: *education*, through which the creativity potential of youngsters can be damaged by improper approaches; *science communication to the public*, in which poor communication can both discourage youngsters from becoming researchers and erode public support of research; and the *funding of science*, by which research funds can be wasted by using inappropriate assessment criteria. In order to avoid these serious problems, we must explore the implications of scientific creativity. (4) Even if scientific creativity is personal and partly innate, it can be *triggered and cultivated*. This requires understanding the mechanisms of scientific creativity. (5) The scientific community can and must contribute to improving the lives of people on our Planet; this requires the development of *eco-ethics*, based on the nature of scientific creativity, and more generally the reintegration of science into modern *culture*; it also calls for strong *international research*, in which clear ideas on the motivations and characteristics of that special activity are needed for achieving discovery; it finally requires that researchers find ways to efficiently *communicate with politicians*. (6) It is important for researchers to realise that scientific creativity is *only one component* of research careers and researchers' lives.

The present essay is structured along the six points in the previous paragraph (Table 2). *Chapter I* has investigated various aspects of the scientific activity. *Chapter II* analyses the nature and process of scientific discovery, and specifies the role—crucial, but limited—of the scientific method in scientific discovery. *Chapter III* analyses scientific creativity, which requires creative imagination; the latter is of deep significance not only to researchers but also to society as a whole. Discoveries may be derived from observations or proceed from theoretical approaches*; Chapter IV* examines the sometimes difficult relationships between the two approaches, and explores why some scientific fields are light in theory. The next three chapters consider some consequences of the analysis of scientific creativity. *Chapter V* deals with education, both general and scientific. *Chapter VI* examines the public

Table 2. The present book is structured along six aspects of scientific creativity

Aspects of scientific creativity	Chapter titles (Ch. number)
Discovery is the aim of scientific research	The Scientific Activity (I) Scientific Research and Discovery (II)
Scientific creativity is an essential condition of discovery	Scientific Creativity (III) Scientific Theories (IV)
The lack of understanding of scientific creativity can have negative consequences	Consequences: Education (V) Consequences: Science and the Public (VI) Consequences: Funding of Scientific Research (VII)
Scientific creativity can be cultivated	Developing and Using Creative Skills (VIII)
The scientific community must contribute to improve the life of people	Science, Culture and (Eco-)Ethics (IX) International Research (X) Researchers and Politicians (XI)
Scientific creativity is only one component of research careers	Focusing Creativity on Scientific Research as a Career and/or Other Fulfilling Activities (XII)

response to science, showing that the usefulness of discoveries plays little role there, and discusses how science should be communicated to youngsters and the general public. *Chapter VII* refocuses the funding of research on discovery, and defines efficient criteria for funding research and assessing its quality. The next two chapters look at practical aspects of scientific creativity. Chapter *VIII* proposes ways for developing and using creative skills in research, through the mastering of some key intellectual tools, writing and communicating with pleasure. Chapter *IX* generalises the discussion of scientific creativity to culture, ethics and eco-ethics. Chapter *X* discusses the motivations and conditions of successful international research and how to prepare for it and Chapter *XI* examines the often difficult relations between researchers and politicians. Finally, Chapter *XII* shows how the creativity of most researchers is focused not only on scientific research, but also on other creative activities.

II SCIENTIFIC RESEARCH AND DISCOVERY

This Chapter initiates the discussion of what is, for me, the core of scientific research: *discovery*. I believe that the discovery process, although largely personal, obeys general rules that can be analysed, and on which action can be based. The analysis of discovery is the cornerstone of my book.

Some Basic Rules of Logic

Before starting the discussion of scientific discovery, it is useful to recall some basic rules of logic, which will be used in this Chapter and the next. These rules draw *logical inferences* from a statement of the type 'if A, then C', in which A stands for 'antecedent' and C for 'consequent'. The four logical inferences discussed here are summarised in Table 3, where they are illustrated with an ecological example.

There are two *valid* logical inferences: (1) *Denying the consequent* (or modus tollens, a Latin expression, meaning 'suppressing mode'). This inference states that 'if A, then C', then wherever C does not obtain, A will not obtain either. (2) *Affirming the antecedent* (or modus ponens, a Latin expression, meaning 'setting mode'): wherever A obtains, C will also obtain.

There are two converse, *fallacious* inferences: (3) *Affirming the consequent*: asserting A when C is observed. (4) *Denying the antecedent*: concluding that C does not hold when A is not observed. Inferences belonging to the two fallacious types are sometimes encountered in scientific publications, in which cases the scientific conclusions drawn from the study are not valid logically.

The Nature of Scientific Discovery

The central aim of scientific research is 'discovery'. The general definition of *discovery* is 'getting knowledge of something that existed before but was unknown'; the word 'discovery' also designates something that is discovered. I define SCIENTIFIC DISCOVERY as: finding, with imaginative skills, new phenomena, new mechanisms, new laws, new theories or new paradigms, without taking any assumption as being true *a priori*. Hence, scientific discoveries include new phenomena, mechanisms, laws, theories and paradigms. For simplicity, 'scientific discovery' will often be abridged below as 'discovery'.

Table 3. Four logical inferences from the proposition ‘if A, then C’; A stands for ‘antecedent’, and C for ‘consequent’. Two of these inferences are valid, and two are fallacious. The four inferences are illustrated with an ecological example: marine planktonic organisms that can efficiently collect particles 5×10^3 to 5×10^4 smaller than themselves are called large microphagous zooplankton; they include salps and appendicularians (tunicates), pteropods (molluscs) and euphausiids (crustaceans) (Fortier et al. 1994, their Table I; see also Ch. VIII, Sec. ‘Dimensional Analysis, Theoretical Analysis, Development of Concepts and Models’). The ecological proposition is: *if there are salps (A), then some of the organisms present can feed on particles 5×10^3 to 5×10^4 smaller than themselves (C)*

Wherever	Then	Validity	Logical inference
C does not obtain *The observed organisms cannot feed on particles 5×10^3 to 5×10^4 smaller than themselves*	A does not obtain *Organisms present do not include salps*	Valid *Because the observed organisms cannot feed on particles 5×10^3 to 5×10^4 smaller than themselves, they cannot be salps*	Denying the consequent (*Modus tollens*)
A is observed *Salps are observed*	C obtains *Some organisms present can feed on particles 5×10^3 to 5×10^4 smaller than themselves*	Valid *Salps can indeed feed on particles 5×10^3 to 5×10^4 smaller than themselves*	Affirming the antecedent (*Modus ponens*)
C obtains *Some of the observed organisms can feed on particles 5×10^3 to 5×10^4 smaller than themselves*	A obtains *Some of the organisms present are salps*	Fallacious *Pteropods, appendicularians and euphausiids can also feed on particles 5×10^3 to 5×10^4 smaller than themselves*	Affirming the consequent
A does not obtain *No salps are observed*	C does not obtain *No organism present can feed on particles 5×10^3 to 5×10^4 smaller than themselves*	Fallacious *Pteropods, appendicularians and euphausiids can feed on particles 5×10^3 to 5×10^4 smaller than themselves*	Denying the antecedent

There is no general agreement on the meaning of 'discovery'. On the one hand, some colleagues restrict the expression 'scientific discovery' to *new phenomena*, and prefer to use instead the word 'explanation' for new mechanisms, laws, theories or paradigms. I think that this restrictive definition of the word 'discovery' goes against usage, e.g. most people would say that Newton 'discovered' the laws of gravitation. The essence of science lies not in discovering facts, but in discovering new ways of thinking about them. Be that as it may, I will use 'scientific discovery' in the broad sense of *new phenomena, mechanisms, laws, theories or paradigms*. On the other hand, some people think that a 'discovery' must be a major finding, and therefore use such words as 'advance' or 'progress' to qualify more modest achievements. Because I think that all discoveries share the same key characteristic—novelty—*I will not distinguish between major and minor discoveries*. Researchers must not be unduly humble when it comes to discovery.

There are three components in the above definition of scientific discovery. (1) *Finding with imaginative skills* corresponds to the definition of 'creation' given in Chapter I, i.e. production of original works through imaginative skills. Scientific discoveries are true creations, like the artistic, musical or literary creations. Of course, the different types of creation are not identical (see Chapter III), e.g. the creations of artists, musicians and writers are works of arts, musical pieces and literary works, respectively, whereas those of researchers are discoveries. (2) *New phenomena, mechanisms, laws, theories or paradigms* enumerates different types of scientific discovery. (3) *Without taking any assumption as being true a priori* distinguishes science from other intellectual activities. In the latter activities, one or several basic assumptions are generally taken as being absolutely true, which is never the case in science.

One example, among many others, of *non-scientific intellectual activity* is astrology. Given that astrology regards its basic tenet—the positions of celestial bodies influence human affairs—as absolutely true, i.e. it cannot be questioned under any circumstance, that activity is not part of science. Another example is theology, which is based on the belief in God(s) and, often, on sacred texts. Because the existence of God(s) and the perfection of sacred texts cannot be questioned in theology, that discipline is not part of science. Many people derive pleasure, comfort or inner peace from the practice of non-scientific intellectual activities. There is no reason why science should be in conflict with any such activity (e.g. astrology, theology), except in cases in which either charlatans disguise non-scientific activities under a cloak of pseudo-science in order to swindle naïve people out of their money, or groups try to prevent or control scientific research based on their beliefs.

Does the third component in the definition of scientific discovery—without taking any assumption as being true *a priori*—apply to all sciences, including mathematics? The answer is obviously yes for the natural sciences, is not always evident for some schools of thought in the social sciences and is not immediately obvious in mathematics. The latter is because mathematical theorems, once demonstrated, are absolutely true. In *mathematics*, researchers formulate conjectures, consistent with the premises—or axioms or postulates—of a given mathematical system. CONJECTURES are assertions based on patterns observed in several instances, which are believed, at least by some, to be generally true but have not been proved. A well-known example is *Goldbach's Conjecture*, which was set forth by Prussian mathematician Christian Goldbach in 1742; the conjecture asserts that every positive even integer ≥4 is the sum of two primes; almost three centuries later, Goldbach's Conjecture remains to been demonstrated. Once demonstrated, a conjecture becomes a theorem, which is true forever. However, even if *theorems are absolutely true*, no mathematician thinks that the bases of mathematical systems are inherently true. For example, a base-two system of numeration is not inherently 'truer' than a decimal system: both are arbitrary. This is contrary to what the practitioners of non-scientific intellectual activities believe for their systems of thought. Hence, mathematics rightly belongs to science.

In the present text, the word SCIENCE will refer to the *universal knowledge* acquired through scientific discoveries, and the expression SCIENTIFIC RESEARCH (or simply RESEARCH) will designate the activity of creating that knowledge, through scientific discoveries. In other words, science is the universal knowledge, acquired by imaginative skills, of new phenomena, new mechanisms, new laws, new theories or new paradigms, without taking any assumption as being true *a priori*.

Sciences include the *natural and social sciences*, which both concern Nature, and *mathematics*, which is a pure construct of the mind. Even if the word NATURE covers both the physical environment and living organisms, including human beings, the present book focuses on the natural sciences, and will touch only marginally the social sciences. No distinction will be made between *fundamental and applied research*, except in Chapter VII, which is devoted to funding, because most scientific fields have fundamental and applied components.

In the definition of discovery given at the beginning of this Section, the observed facts or events are called PHENOMENA (Table 4). Sets of phenomena

Table 4. Relationships among the various types of scientific discovery. Even if all discoveries obey the same general conditions and follow the same general process (Ch. II), they can be divided into two broad groups: new phenomena, mechanisms and laws mostly follow from observations or experiments, whereas new theories and paradigms are based on theoretical approaches. In research, theories and observations are closely interlinked (Ch. IV)

Type of scientific discovery	Summary definition
Phenomenon	*Observable* fact or event
Mechanism	Combination of the *fundamental processes* involved in or responsible for a set of *phenomena*
Law	Statement of a *relation among phenomena* that, so far as is known, is invariable under given conditions
Scientific theory	Body of, at least partly, *hypothetical statements*, which represents as simply and completely as possible the relevant *phenomena*, *mechanisms* or *laws*
Paradigm, or research programme	Hard *theoretical core*, and *set of rules* that are used to progressively improve the *theory*

are interpreted in terms of *mechanisms or laws*; phenomena and their associated mechanisms are often represented by *models*. A MECHANISM is the combination of the fundamental processes involved in, or responsible for a set of phenomena. A LAW is a statement—often a mathematical function—of a relation among phenomena that, so far as is known, is invariable under given conditions. In science, a MODEL is a simplified representation of Nature. A SCIENTIFIC THEORY is a body of, at least partly, hypothetical statements, which refers to a small number of principles, and represents as simply and completely as possible the relevant phenomena, mechanisms or laws. A PARADIGM—often called RESEARCH PROGRAMME by science philosophers and also some researchers—consists of: a 'hard core' of theory, 'auxiliary hypotheses' that form a protective belt around the core, 'rules' that specify which paths of research to avoid and which to pursue. Of course, the hard core of any paradigm is developed progressively, through trial and error. *Paradigms and theories* will be further discussed in the Section 'Paradigms, Theories and Tautologies' later in this Chapter.

Research may be driven by sole curiosity, or targeted at problems or needs, or devoted to the resolution of practical problems, but its aim is always the same: *discovery*. Given that the number of natural phenomena is very large, perhaps infinite, a discovery never results from a random assemblage of information. The tool used for making discoveries is the mind of the scientist. What determines the direction of science is primarily the *human creative imagination* and not the universe of facts which surrounds us.

The definition of discovery given at the beginning of the present Section stresses the fact that the central characteristic of discovery is *novelty*. Hence, discoveries cannot be predicted from existing knowledge. Discovery requires something more than deductive logic: it requires *imaginative skills*. Of course, the range of discoveries is very wide, e.g. from finding a new chemical reaction or new biological species to proposing new theories or paradigms such as plate tectonics or biological evolution. However, all discoveries share a common characteristic, i.e. novelty; hence, they all require the same ability, i.e. imaginative skill. Because all discoveries obey the same general conditions and follow the same general process, I will make *no distinction between types or levels of discovery* in the present book. However, several examples I shall use will correspond to major changes in science, simply because these are well documented in the literature, which is generally not the case with more modest accomplishments.

I mentioned above that discoveries cannot be predicted from existing knowledge, and thus require more than deductive logic. It could therefore be objected that discoveries are not possible in *mathematics*, because that field is a construct of the mind in which any finding can be logically deduced from the premises or axioms or postulates of the mathematical system. However, in mathematics as in other scientific fields, progress in knowledge is generally achieved *not by deduction but by leaps of imagination* that produce conjectures or lead to demonstrating theorems. Of course, once imagination has led to a mathematical discovery, it is always possible to *rationally reconstruct* the change in knowledge that took place. This is the case not only in mathematics, but also in *all fields of science*.

Because science is the universal knowledge acquired through discoveries (see above), and not the compilation of the personal quests of discovery of individual researchers, *what we find in scientific literature are always reconstructions*, not reports of how discoveries actually took place. As a consequence of the fact that only logical reconstructions are published, most people (often including science students) think that science proceeds rationally from what is already known to the unknown, and too few are aware of the *essential role played by imaginative skills* in discovery.

The Process of Scientific Discovery

The process by which science progresses, i.e. the process of scientific discovery, is often analysed by reference to the HYPOTHETICO-DEDUCTIVE METHOD. It must be noted that this method is one view only, among others, of the discovery process, which is disputed by some science philosophers. It depicts research as an alternation between phases of *hypothesis creation* (hence, 'hypothetico'), and *evaluation of deductions from the hypotheses* (hence, 'deductive'). Fig. 4 shows that the hypothetico-deductive approach considers the two phases as being of different natures: the *synthetic phase*, during which hypotheses are created, is private and informal; the *analytic phase*, during which predictions deduced from the hypotheses are tested, is public and formal.

The first phase of the hypothetico-deductive method is where *intuition or inspiration* enters science. The scientist looks at the facts and these, in some way called *induction*, suggest a generality to the scientist. Because of the apparent lack of logic of induction or intuition, most researchers interested in the scientific process have focused on the second, deductive phase of the

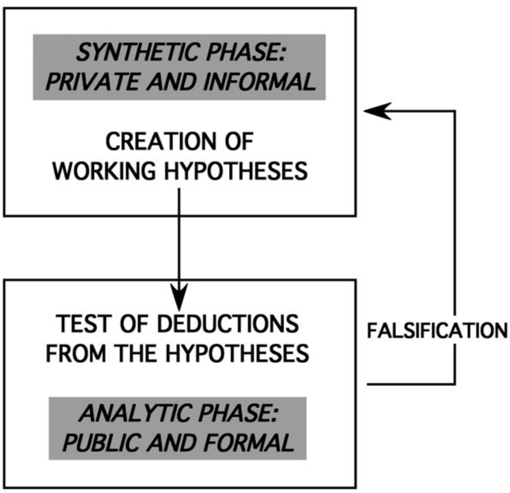


Fig. 4. Schematic diagram summarising the hypothetico-deductive method, which comprises two phases of different nature: the synthetic phase, during which hypotheses are created, is private and informal; the analytic phase, during which predictions deduced from the hypotheses are tested, is public and formal (Simplified from Peters 1991, his Fig. 2.2, by permission of Cambridge University Press)

hypothetico-deductive method. I showed in Chapter I (Section 'Creation') that intuition may not be as mysterious as most think. Even if intuition were mysterious, it would nevertheless be an integral part of scientific creation. Hence, contrary to the usual approach of science philosophers—which restricts creation to the first, synthetic phase of the hypothetico-deductive method, and focuses on the second, analytic phase—I promote the idea in this book that the process of scientific discovery, i.e. *scientific creation*, includes the two phases described above, and more to be introduced later.

Most research efforts do not lead to any discovery, but researchers sometimes make discoveries. How do they proceed in the latter case? The answer to this question is not easy, because the *process of discovery* most likely varies among individuals. Nevertheless, most scientific discoveries probably obey the same general conditions. These have been summarised by Csikszentmihalyi (1997, *Creativity: Flow and the Psychology of Discovery*), as follows: (1) *Preparation*, i.e. becoming immersed, consciously or not, in a set of problematic issues that are interesting and arouse curiosity. (2) *Incubation*, during which ideas churn around below the threshold of consciousness and unusual connections are likely to be made. (3) *Insight*, the instant when Archimedes cried out "Eureka!" In real life, there may be several insights interspersed with periods of incubation, evaluation, and elaboration. (4) *Evaluation*, when the person must decide whether the insight is valuable and worth pursuing. (5) *Elaboration*, which is probably the stage that takes the most time and involves the hardest work. The five stages in reality are not exclusive but typically overlap and recur several times before the process is completed. The first four phases are those already identified by Poincarré/Hadamard during the first part of the 20th century (see Chapter I, Section 'Creation').

The above analyses of discovery may be correct, but they do not really address the fundamental question: What are the conditions required for researchers to sometimes make discoveries? Or, in other words: What is the process by which researchers sometimes make discoveries? My initial answer, to be completed in the following chapters, is that there are at least four components in discovery (Fig. 5):

(1) A necessary element for making a scientific discovery is formulating a *pertinent question*. Some pertinent questions arise from recent progress in science, whereas others have been asked for centuries or millennia before being answered, sometimes only partly. As example of partial answer, Newton's law of gravitation mathematically describes the mutual attraction of moving bodies, but the physical phenomenon itself remains unexplained: the

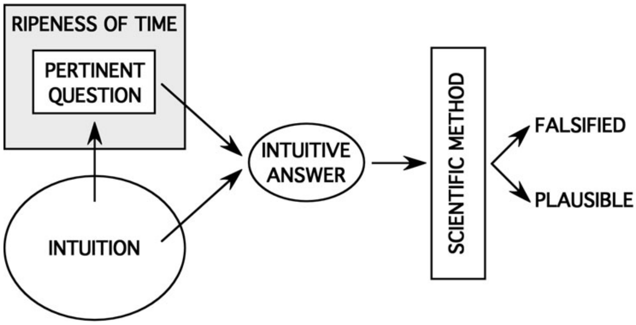


Fig. 5. Four components of scientific discovery: when a *pertinent question* has been formulated, which strongly involves intuition, and the *time is ripe* for answering the question, one or several researchers find an *intuitive answer*; the *scientific method* is used to determine if the intuitive answer is falsified (i.e. rejected) or can be accepted as plausible (Original)

mechanism of gravitation is not understood as yet. A pertinent question is a necessary but insufficient condition for a scientific discovery to occur.

The first key to scientific discovery is not so much to find the right answer, but to ask the right question, i.e. to see a problem where nobody saw one before. The formulation of a pertinent question plays a key role in the discovery process. This crucial step strongly calls on *intuition* (see Chapter I, Section 'Creation'). The role of intuition in the formulation of fertile questions is true in all fields of science.

(2) Another component of the discovery process is the requirement that the *time must be ripe* for answering the question: some preliminary discoveries must have been made, some techniques must have become available and/or a proper intellectual or social environment must have developed. As time ripens, the likelihood of the discovery increases, to the point that discoveries are often made almost simultaneously by independent researchers. However, all the needed elements have occasionally been in place for a long time before a discovery is made. For example, farmers have bred and selected plants and animals for millennia, but it was only during the second half of the 19th century (1866) that Gregor Mendel (for the original text in German and its translation in English see 'Mendel' on http://www.esp.org/foundations/genetics/classical/browse/) discovered the first laws of genetics: the time for such laws was not ripe until then. Even Mendel was too early, as his discoveries were forgotten until re-discovered decades later by Hugo de Vries and collaborators (1900; for a translation in Engish see 'Vries, Hugo de' on http://www.esp.org/foundations/genetics/classical/browse/). Hence, both a

pertinent question and the ripeness of time are necessary but insufficient conditions for a scientific discovery to occur.

For me, ‘the time must be ripe’ corresponds to the possibility of either, in a retrospective manner, identifying which factors made an actual discovery possible, or in a prospective approach, taking advantage of newly available elements to propose an original angle for approaching an already existing question that previously resisted resolution. I will show in Chapter VII (Section ‘Funding of Research: Efficient Criteria’) that the ripeness of time can be translated into an operational criterion for assessing research proposals.

(3) When the question is pertinent and the time is ripe, one or several researchers suddenly see the answer, without any preliminary logical demonstration. The answer is *intuitive*. Intuition plays a key role not only in formulating scientific questions (first component, above), but also in answering them. As a matter of fact, intuition often—or most of the time—leads to answers that must be later rejected. However, without intuition, no answer can be found.

Using intuition to make discoveries may appear very glamorous, but one must remember that proposing an original answer that may rapidly be proven incorrect (which occurs most of the time) is risky both for oneself (discouraging, or worse) and for one’s career (when it occurs publicly). Hence, it has been suggested that riskier problems in science are more typically addressed either by established scientists who can afford to do so, or by those not established at all who have very little to lose.

(4) Finally, the *scientific method* is used to reject or accept as plausible the answer provided by intuition, as explained in the next Section. Hence, it is only at the last step of the discovery process that the rational component of research—the scientific method—takes over. Koestler (1964) noted the apparent paradox that a branch of knowledge that operates predominantly with abstract symbols, whose entire rationale and credo are objectivity, verifiability and logicality, turns out to be dependent on mental processes that are subjective, irrational and verifiable only after the event. I will discuss various aspects of this apparent paradox, and some of its consequences, in the remainder of this book.

The first two components of discovery—pertinent question and ripeness of time—are prerequisites. They often (but not always) exist independently from the researcher who makes the discovery. The third and fourth components—intuition and method—are abilities that a researcher must have to make a discovery. Intuition and method are two components of *creative* *imagination*, which is discussed in Chapter III. Koestler (1964) stressed the different roles played by the prerequisite components and the researchers' abilities in discovery. Some discoveries are facilitated by *ripeness*, i.e. these discoveries were "in the air"—meaning that the various components were all lying around and waiting for the trigger action of chance, or the catalysing action of an exceptional brain, to be assembled and welded together. Other discoveries are *major breakthroughs* in the history of science, which represent such dramatic tours de force that 'ripeness' seems a very lame explanation, and 'chance' no explanation at all. As example of the latter, he cites Einstein's discovery of the principle of relativity, which was unaided by any observation that had not been available for at least fifty years before.

Three additional components of discovery might be added to the above four: the ability to efficiently use intuition, chance and technological advances. I discuss in turn these three potential components of discovery.

(i) *Ability to efficiently use intuition*. Some creative scientists think that the difference between them and their less creative peers is their ability to separate bad ideas from good ones, so that they do not waste much time exploring blind alleys. In other words, these researchers often 'know' in advance what will work.

(ii) *Chance* or *luck*. One could argue that chance or luck is an important component of discovery, which is missing from the previous paragraphs. In other words, making a scientific discovery would also require luck. Of course, being in the right place at the right time is important, but most people do not realize that they are standing in a propitious space/time convergence, and thus cannot seize the opportunity when it occurs, whereas creative people do.

In her book *The Creative Mind* (1992), Boden analysed three possible meanings of 'chance' in reference to discovery. (1) *Randomness*. I explained in the previous Section that a discovery never results from a random assemblage of information. (2) *Serendipity* is the finding of something valuable without its being specifically sought. Serendipity does not involve any inherently improbable event, but someone without a question in mind could not take advantage of the event when it occurred. For example, if Newton had not been seeking an explanation to the revolution of the Moon around the Earth, he would not have drawn any special conclusion from the falling of apples to the ground. Similarly, if Fleming had not been looking for a germ-killer for years, he might not have discovered penicillin when wind blew through the window of his laboratory a spore of the mould *Penicillium notatum*, which happened to settle in a culture dish of staphylococci.

(3) *Coincidence* is a co-occurrence of events having independent causal histories, where one or more of the events is improbable and their (even less probable) co-occurrence leads directly or indirectly to some other, significant event. Although serendipity is sometimes due to coincidence, they are not the same thing. Near-simultaneous discoveries are sometimes interpreted as coincidences, but such discoveries were explained above by the ripeness of time. This is very different from chance. So, the role of coincidence in discovery is questionable. In fact, when the time is ripe for a discovery to be made, there is not much need there for the helping hand of chance.

Let me summarise the above ideas on the role of *chance* in discovery. On the one hand, I showed that *randomness* has nothing to do with discovery, and I think that *coincidence* plays a very small role. On the other hand, *serendipity* consists in the enlightening observation of a normally occurring event by someone who is seeking an answer to an apparently unrelated question. Someone without a question, or being at the wrong time, or without the ability of creating an intuitive answer would not make anything of the event, hence no serendipity. The *founder of microbiology, Louis Pasteur* (1822–1895), said: 'Fortune favours the prepared mind'. It follows that serendipity depends much more on components (1) to (3) above—pertinent question, ripeness of time, intuitive answer—than on the occurrence of an inherently improbable event, i.e. 'chance'. I therefore consider that the part of discovery that is sometimes ascribed to 'chance' is imbedded in components (1) to (3). However, people who think that luck plays a significant role in discovery could insert 'serendipity' between components (2) and (3), above.

(iii) *Technological advances*. It is sometimes stated that discovery is driven mostly by, or reflects, new technology. It is true that many discoveries could not have occurred without technological advances, and this is why I listed new techniques among the elements of the ripeness of time (component 2, above). I do not think, however, that technology drives discovery directly, for at least two reasons. Firstly, new technologies generally are direct or indirect results of scientific discoveries. Hence, they are among both the causes and the effects of discovery. Secondly, technology in itself does not lead to discovery: it is the creative use of technology, by someone who is trying to answer some question, which leads to discovery. The interaction between technology and discovery can probably be described as a co-evolution of concepts and instrumentation.

Scientists must recognise and insist on a very crucial point that generally is misinterpreted by the public and sometimes misunderstood by researchers.

Except in mathematics (see next Section), one can never conclude that the answer given to a scientific question is *true*. The answer to a scientific question can only be rejected, i.e. falsified, or accepted as *plausible*. This is so because, in the natural and social sciences, there is no way to ever demonstrate that an answer is true. As explained in the next Section, the scientific method can only reject (or not) a given answer; if it is not rejected, then the answer is accepted as being *plausible, not true*. This may not be fully satisfactory, but it is the best scientific research can do, at least for the time being, and perhaps forever (see Chapter I, Section 'The End of Science?').

The Scientific Method

The approach of the scientific method is to state some hypothesis that could be rejected. I will follow here the general custom of using the word 'falsify', and its derivatives (e.g. FALSIFICATION), to mean 'reject' when discussing hypotheses. A HYPOTHESIS that could be rejected is therefore called FALSIFIABLE. A hypothesis is said to be falsifiable if there exists at least one possible *alternative hypothesis*. Some hypotheses in the scientific literature are—most of the time not deliberately—drafted in such a way that they cannot ever be falsified.

The idea of falsification is related to the *rules of logic* discussed at the beginning of the present Chapter, and summarised in Table 3. Logic teaches us that the only valid inference from C to A that one can make from the comparison of predictions with observations (i.e. C in Table 3) is to reject the hypothesis (i.e. A in Table 3): wherever C does not obtain, then A does not obtain; this is called *denying the consequent*. Trying to demonstrate that a hypothesis is true, i.e. inferring that wherever C obtains, then A also obtains, would be *affirming the consequent*, which is *logically fallacious*.

Statistical tests are performed on a special type of falsifiable hypothesis called NULL HYPOTHESIS. A null hypothesis (symbolised H_0) specifies a model that can be used to generate realisations of H_0; the distribution of these realisations is used to test H_0 for significance. The null hypothesis must be accompanied by an ALTERNATIVE HYPOTHESIS, which is symbolised H_1. When the null hypothesis is falsified, the alternative hypothesis is provisionally accepted. Specifying the alternative hypothesis is essential because the eventual falsification of the null hypothesis may open the possibility of *several alternative hypotheses*.

In accordance with the *denying the consequent* rule, a statistical decision can only lead to rejecting H_0, or not rejecting it. It can never lead to accepting the null hypothesis. Indeed, further observations, under the same or different conditions, could lead to rejection of previously non-falsified H_0. *Rejecting the null hypothesis* simply indicates that the observations do not contradict H_1, but the rejection of H_0 does not imply that the mechanisms invoked in the formulation of H_1 are correct. In cases where a statistical approach is appropriate, H_0 is *tested statistically*: the probability of the data under H_0 (P) is computed and compared with some predetermined level, which is symbolised α (e.g. α = 5%). When P is smaller than α, H_0 is rejected, i.e. falsified, and H_1 is then accepted as plausible.

An apparently simple example of *null and alternative hypotheses* concerns the Earth's day-and-night cycle (Fig. 6). For millennia, it was hypothesized that the phenomenon could be explained by H_0: the Sun revolves around the Earth. One may think that the alternative hypothesis to the revolution of the Sun around the Earth is H_1: the Earth revolves around the Sun. However, this is not the appropriate H_1 for the day-and-night cycle. Indeed, we now explain the latter by H_1: the Earth rotates on itself. This is because the revolution of the Earth around the Sun explains the cycle of seasons, not that of days and nights. Hence, there are at least two H_1 to the above H_0: one alternative hypothesis for the day-and-night cycle, and a different one for the seasonal cycle. This shows that H_1 depends on the purpose of the study. Hence, stating H_1 is almost never a trivial matter. The major shift in cosmology paradigms, from geocentric to heliocentric, was a very complex process that cannot be reduced to the simple rejection of H_0 and acceptation of H_1. The purpose of my simplified presentation of this major paradigm was to illustrate the real difficulty of specifying alternative hypotheses. Readers

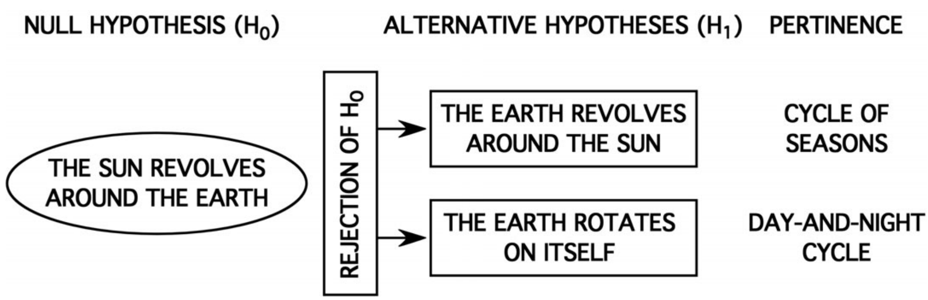


Fig. 6. Example of hypothesis testing. Rejecting the null hypothesis (H_0) that the Sun revolves around the Earth was accompanied by two alternative hypotheses (H_1): the Earth revolves around the Sun, which concerns the cycle of seasons, and the Earth rotates on itself, which explains the day-and-night cycle (Original)

interested in the adventure of cosmology over two millennia are encouraged to read *The Sleepwalkers* (Koestler 1959).

One must always remember that the scientific method *per se* cannot generate answers. The answers always come from researchers' intuition, as explained in the previous Section. The role played by the *scientific method* is to determine if the answers arising from intuition must be rejected—which happens most of the time—or can be accepted as plausible—which occurs only rarely. Fig. 7 combines several ideas discussed so far in this Chapter. The two phases of the hypothetico-deductive approach (Fig. 4) provide the general framework of the figure. The synthetic phase (upper box) includes the formulation of a pertinent question, and the intuitive statement of a possible answer (Fig. 5). The intuitive answer is the null hypothesis (H_0), together with the accompanying alternative hypothesis (H_1). Figures 4 and 5 were discussed in the previous Section ('The Process of Scientific Discovery'). In the analytic phase (bottom box), H_0 is tested: the hypothesis can be used as long as it is not falsified, and it cannot be used once falsified. Upon

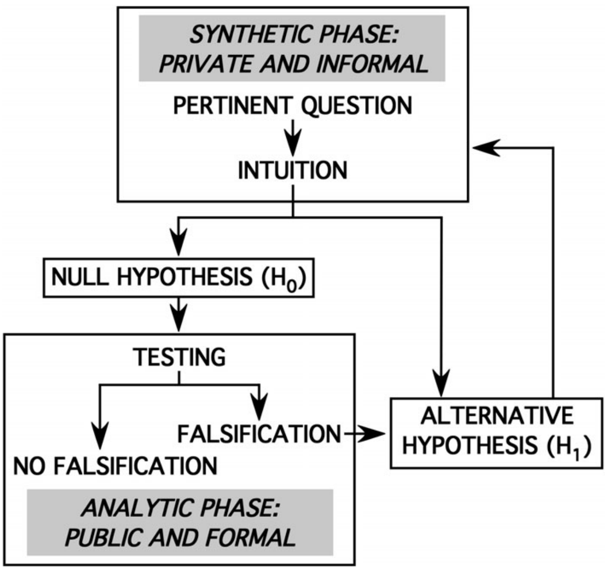


Fig. 7. Combination of ideas discussed in Chapter II. General framework: hypothetico-deductive approach (two phases, Fig. 4). Synthetic phase (upper box): formulation of a pertinent question, and intuitive statement of a possible answer (Fig. 5). The intuitive answer is the null hypothesis (H_0), and the accompanying alternative hypothesis (H_1). Analytic phase (bottom box): H_0 is tested (Fig. 6): the null hypothesis can be used as long as it is not falsified, and it cannot be used once falsified. Upon falsification, H_0 is replaced by the alternative hypothesis (H_1), which opens a new cycle (Original)

falsification, H_0 is replaced by the alternative hypothesis (H_1, Fig. 6), which opens a new cycle.

It must be stressed that the purpose of the scientific method is not to question the reality of observed phenomena, as done by some philosophers, but instead to *test the interpretations of phenomena* made by researchers in terms of laws or theories (see Chapter I, Section 'Absolute Limit to Human Knowledge?')

It is extremely difficult for researchers to coldly apply the scientific method to their own, intuitive answers, because the very aim of the method is to reject these answers. Given that obvious difficulty, an extremely strict approach has been developed over time to ensure that published discoveries have been subjected to *rigorous testing*. This approach involves both the training of future scientists and the use of peer review. On the one hand, *future scientists are educated* in such a way that they do, or should, understand the purpose and limits of the scientific method, and use it appropriately and systematically. On the other hand, manuscripts submitted for publication are *reviewed by peers*, who recommend that the Editors accept or reject manuscripts, the acceptance being conditional or not on improvements.

The rigour of *peer review* must be explained to the general public and even some researchers, because it is sometimes interpreted as a means used by conservative scientists to prevent the publication of discoveries they do not like. This occasionally happens, given that researchers, who are generally progressive individuals, tend to be conservative collectively, like most other groups of people. However, the rapid progress of science shows that sound discoveries almost always find their way into publication, although sometimes after meeting difficulties. Overall, it is surprising how well, in general, the peer review process works.

The alternative to peer review would be uncontrolled publication of all answers that have not been rejected after the researchers' own application of the scientific method. Unfortunately, we know from experience that self-testing, even when very honest, may not be fully satisfactory, not to mention cases where self-testing has been less than rigorous. Nobody in the scientific community, or presumably the general public, should want the dissemination of poorly tested, or even untested, information. This is, however, what happens more and more in *mass communication media*, especially the Internet, where the best is often next to the worst, and where an increasingly large number of people get most of their information. In this respect, I think that the wide circulation on the Internet of manuscripts prior to peer review is not a progress, but a regression away from high-quality standards.

It follows from the previous discussion that a scientific discovery is a creation of the mind: it never results from the sole use of technology or data analysis, although these often provide key information leading to discoveries. Scientific research is an *intellectual activity*, which aims at making discoveries. Scientific discoveries are *transient*, because the answers to questions about Nature are only plausible, not certain. As a matter of fact, new discoveries, new techniques and/or new intellectual or social environments will eventually lead to challenging the answers that we presently accept as plausible. Some of the present answers will then be found incomplete or will be falsified, and therefore give way to new answers, which will themselves be only plausible (see Chapter I, Section 'The End of Science?'). The only scientific field where answers can be absolutely true is mathematics, but truths are limited to the context set by the arbitrary bases of the mathematical system.

Paradigms, Theories and Tautologies

In the Section 'The Nature of Scientific Discovery', I defined *scientific theory* as 'a body of, at least partly, hypothetical statements, which refers to a small number of principles, and represents as simply and completely as possible the relevant phenomena, mechanisms or laws'. My definition of a *paradigm* was: a 'hard core' of theory, 'auxiliary hypotheses' that form a protective belt around the core, 'rules' that specify which paths of research to avoid and which to pursue. The present Section develops these and associated concepts. The testing of theories using observations will be discussed in Chapter IV (Section 'Theoretical Science and Scientific Theories').

Paradigms

Paradigms are also called *research programmes*. One example of a successful research programme is Newton's gravitational theory (Newton 1687). The hard theoretical core of the paradigm consisted of Newton's three laws of dynamics and his law of gravitation. Around that core, there was a protective belt of auxiliary, observational hypotheses and initial conditions, which was progressively improved to account for observed anomalies; these diverted the rule of *denying the consequent* from the hard core. The rule of logic called *denying the consequent* was explained at the beginning of this Chapter, and summarised in Table 3, where A and C (used in the next sentence) are defined. What is meant by 'diverting the rule of *denying the consequent* from the hard core' is that in paradigms, auxiliary hypotheses protect the hard theoretical core (A) from differences between predictions following from the core theory and observations (C): in a paradigm and contrary to the general rule of logic of *denying the consequent*, the falsification of a theoretical prediction—called denying the consequent—does not lead to rejection of the hard theoretical core, but instead to improvements of the protective belt around the core.

One example of paradigm in physio-ecology is ecological energetics, where the hard theoretical core consists of the laws of thermodynamics. The law of conservation of energy, which is part of the hard core, predicts that the energy budget of any organism must be balanced. According to the rule of *denying the consequent* (Table 3), if the energy budget of one organism failed to balance, this should falsify the law of conservation of energy. Of course, it does not, because as explained in the previous paragraph, the rule of *denying the consequent* would be diverted in such a case from the hard core: failure to balance the energy budget of an organism would be interpreted as resulting from measurement errors, or an incorrect formulation of the budget equation for that organism, not as a failure of the law of conservation of energy.

Theories

There is no agreement among science philosophers on the definitions of 'theory' and 'paradigm', which are often used as synonyms in scientific literature. In addition, researchers do not agree on the meaning of *theory*.

For some researchers, theories are constructs that make potentially falsifiable—or testable—*predictions* about natural phenomena (falsification was discussed in the Section 'The Scientific Method', above). It must be noted that 'law', 'hypothesis' and 'theory' are sometimes used as synonyms for such predictive constructs. Because a scientific theory is a generalization that goes beyond the observations, it has the power to predict about specific cases on the basis of a general statement. Within the context of scientific theories, prediction means to *foretell an unknown state*, not simply a future state, as explained below.

Most people understand 'predict' as 'to tell in advance', i.e. prediction is about future events, as opposed to past events. In that context, some researchers argue that the main objective of science is not to tell in advance what will happen, but to *explain* how it is possible for things to happen as they do. A side effect of most scientific explanation is prediction, but for these researchers, prediction is not essential to scientific theories.

A key difference between the above two opposite concepts of scientific theories may a different understanding of 'prediction', i.e. telling in advance future facts (second concept), or more generally foretelling an unknown state (first concept). It will be shown in the coming paragraphs that prediction is necessary for *testing theories*, but this does not mean that science should be restricted to its predictive constructs only.

There are two types of constructs that are useless in science: non-falsifiable theories and *ad hoc* theories. These are discussed in the following paragraphs.

Non-falsifiable theories. The only useful theories in science are those that have the potential to be wrong. If no conceivable observation could ever show a theory to be wrong, then the theory predicts every possibility and therefore tells nothing useful. In other words, to qualify as a *scientific theory*, a statement or set of statements must be potentially *falsifiable*. This general principle is illustrated in Fig. 8. A falsifiable theory predicts that, if A obtains, then some outcomes are possible and others are not. The theory will not be falsified as long as the outcome belongs to those predicted as being possible. It will be falsified if any outcome that was predicted to be impossi-

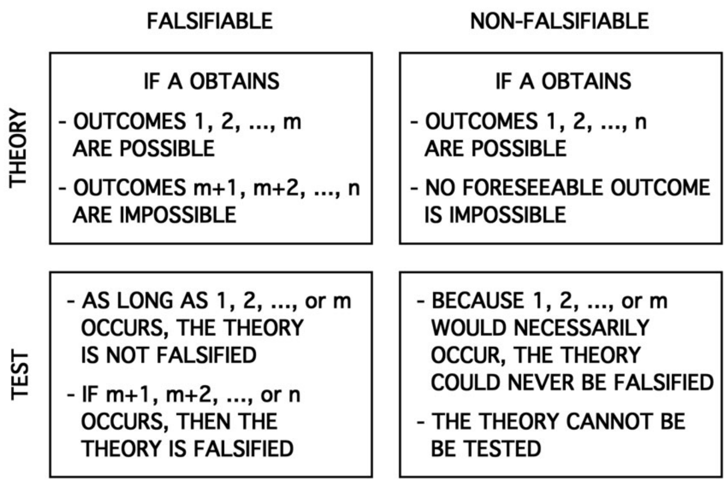


Fig. 8. Difference between falsifiable and non-falsifiable theories. A falsifiable theory predicts that, if A obtains, then some outcomes are possible and others are not. Hence, the hypothesis is not falsified as long as the outcome belongs to those predicted as possible, and it is falsified if any outcome that was predicted to be impossible actually occurs. A theory which predicts that, if A obtains, then all foreseeable outcomes are possible, is not falsifiable. Such a hypothesis could never be falsified, i.e. tested, because all outcomes would necessarily belong to those predicted as being possible (Original)

ble actually occurs. A theory which predicts that, if A obtains, then all foreseeable outcomes are possible, is not falsifiable. Such a theory could never be falsified, i.e. tested, because all outcomes would necessarily belong to those predicted as being possible. It would be *true, but useless*.

Ad hoc theories. The Latin expression *ad hoc* means 'for this'. It refers to a proposition (e.g. a theory) devised for the particular end or case at hand, without consideration of wider application. Before discussing *ad hoc* theories, it is useful to examine the difference between the Latin expressions *ad hoc* and *post hoc*, both of which are applied to theories in the scientific literature. Even though the two expressions have very different meanings in logic, *post hoc* is often used in the scientific literature to mean *ad hoc*. This is incorrect, because in logic, *post hoc* refers to the fallacy of arguing from a temporal sequence to a causal relation. For example, interpreting the succession of species *A* to species *B* as evidence of a causal effect of *A* on *B* would be fallacious, *post hoc* reasoning. Hence, even though the two Latin words *post hoc* translate into English 'after this', it is not correct to use the expression *post hoc* for qualifying a theory formulated after making the observations.

Ad hoc 'theories' or, better, *rationalisations* are devised by researchers to explain some specific observations. An *ad hoc* rationalisation explains only what was observed, and additional or alternative observations that would contradict it are explained away by differences in some details. In other words, no observation would ever be accepted as compelling enough to reject the *ad hoc* rationalisation. This is in contrast with *scientific theories*, which not only predict some observations, but also prohibit others. Because they cannot be falsified, *ad hoc* rationalisations tend to accumulate in the literature, whereas scientific theories, and explanations based on theories, can be falsified and forgotten. (It will be seen in Chapter IV, Section 'Theoretical Science and Scientific Theories', that the actual rejection of theories is not as straightforward as stated here.) Because an *ad hoc* rationalisation is devised to account for a single set of observations, or the results of a single experiment, such a 'theory' (1) *cannot be tested*, because it predicts exactly the one set of observations available, and (2) *is useless*, because it cannot be applied to other sets of observations.

Tautologies

In addition to paradigms and theories, science also includes non-predictive constructs called 'tautologies'. The word 'tautology' does not refer here to the usual definition of 'a needless repetition of the same sense in different words' or 'redundancy, repetition, and circular reasoning within an argument or statement', but instead to a specific concept of logic: a statement that is *true* regardless of the truth-values of its parts. The opposite of a tautology is a 'contradiction', i.e. a statement that is always *false*. Consistent with the previous definition, a TAUTOLOGY identifies, in scientific research, the *range of possibilities* under given premises. In contrast, *a theory* identifies the *smaller set of probabilities* within that range. Tautologies are logical constructs that organise scientific knowledge, allow researchers to see the complete implications of their premises and ensure that every possibility has been considered. An example of tautology is the periodic table of chemical elements, which includes all elements.

Tautologies are *non-predictive constructs* that serve science as logical devices form which science is constructed. In addition, I will explain in Chapter IV (Section 'Theoretical Science and Scientific Theories') conditions under which tautologies can lead to *discoveries*.

Mathematics, Reductionism and Holism

Mathematics is a pure construct of the mind (Section 'The Nature of Scientific Discovery'), which is not concerned with Nature, but provides essential tools for studying it. Researchers interested in discovering the Nature's secrets ask questions (see previous Section 'The Process of Scientific Discovery'), which must be formulated in such a way that they could be answered by using observations or experiments (see Chapter IV, Section 'Scientific Theories and Observations'). The answers to researchers' questions are generally whispers of information. Even if mathematics does not deal with natural phenomena, it is the 'language' that researchers must use to decode the information whispered by Nature, and thus get hints of its secrets. In many cases, mathematics is also the best language that researchers can use to formulate these questions, and to translate their answers into theoretical statements. As stated by Galileo (1564–1642), 'The book of Nature is written in the mathematical language. Without its help it is impossible to comprehend a single word of it'. Researchers in the natural sciences must master mathematical tools, but many scientists find mathematics difficult, even frightening.

The learning of *foreign languages* may provide information applicable to the study of mathematics. Each human language uses a limited number of words that represent distinct concepts, and a set of grammatical rules that prescribe how to assemble the words. Learning a foreign language is especially difficult when there is undue emphasis on either memorisation of words and grammatical rules, or the analysis of the structure of the language. Learning a language may be much easier when there is a need or desire to communicate. In the second case, what appeared to be an almost impossible and/or boring chore may suddenly become a challenging and exciting endeavour. For example, children are often reluctant to learn foreign languages at school, because they do not really believe deep down inside that these languages are actually used by real people, but they pick up a foreign language easily when their family travels or moves to a country where people speak it.

Similar to the mental block about languages, a shockingly large number of bright young people become paralysed when they encounter a mathematical expression. This is often a consequence of *inadequate teaching*, e.g. excessive memorisation of formulae and theorems, or conversely premature exposure to the arcane of mathematical theory, e.g. set theory. Because they cannot relate to such approaches, a large number of youngsters shut their minds to mathematics, and as the gulf between their mathematical abilities and school expectations widens, their fear or detestation of mathematics grows. Such fear or detestation is not only encountered in students inclined to arts or Humanities, but it can also affect students and researchers in sciences, e.g. in biology, ecology, behaviour. The problem is not irreversible, i.e. when the interest exists (e.g. deciphering one's own data in view of obtaining information about Nature) and teaching is appropriate (e.g. synthetic presentation of existing methods, discussion of advantages and limits, and analysis of applications to real research questions), most graduate students and professional researchers not only grasp easily the mathematical approaches, but enthusiastically devote time to master the methods they wish to use. I write in Chapter V (Section 'General Education') '…what is studied without pleasure vanishes rapidly, and what is learned with pleasure is cherished for life'. This is especially true of mathematics.

It is useful to end this chapter on 'Scientific Research and Discovery' with a brief discussion of two broad approaches that exist in scientific research: reductionism and holism. On the one hand, REDUCTIONISM is the *decomposition of complex phenomena or systems into simpler components*, which are themselves governed by general laws. These laws often come from physics or chemistry. Most scientific research follows the reductionist approach, which has demonstrated its effectiveness for more than two centuries. On the other hand, HOLISM treats *complex systems as whole entities*, because systems have properties that are different from those of their individual parts. According to the theory of systems (e.g. von Bertalanffy 1968), new properties appear, i.e. *emerge*, as one goes from low levels of organisation to higher ones. Because the *emergent properties* of the whole system and of its subsystems reflect all the interactions among their parts, these properties cannot be predicted solely from those observed on individual parts or at lower levels of organisation. For example, it can be argued that, when going from atoms to ecosystems, knowing all the properties of individual atoms is not enough to predict the properties of chemical elements, or knowing all the properties of individual chemical elements is not enough to predict the properties of organic molecules, and so forth until considering all the properties of an ecosystem (e.g. a forest, or a lake), which cannot be predicted from those of their constituent species, populations and environmental physical, chemical and geological characteristics. When conducting research at a given level of organisation, one generally considers the emergent properties at that level, and forgets most of the properties at previous levels.

Even if the holistic approach is quite appealing, it is often more difficult to use than reductionism. It follows that in general, researchers prefer *reductionist, explanatory theories* to *empirical, holistic theories*, even though the two types of theory may be equally predictive. One might think that scientists would find it advantageous to use the two approaches, either simultaneously or at successive stages of research. In practice, however, reductionists and holists are generally fiercely opposed. I refer readers interested in the development of the two approaches in biology and ecology to the book of Rigler and Peters (1995) *Science and Limnology*.

III SCIENTIFIC CREATIVITY

There is a metaphor that likens scientific creation to the construction of a cathedral. In that metaphor, theorists play the role of architects who provide the design, field and laboratory researchers provide the materials and all, working together over many generations, build the cathedral. The alternative view is that discoveries are not assembled step-by-step by committees or by the accretion of published knowledge, but result from the *insights of creative individuals*: creative research is not done in isolated pieces, but requires instead a clear view of which information is needed, how this information will be used, and what this use will tell us about Nature.

Discoveries are not made by committees and do not result from accretion of knowledge. Discoveries are products of the *imagination of creative researchers*. In this chapter, I examine what creative imagination is, and what the significance of creativity is, with special reference to science.

Creative Imagination

Everybody can see Nature, but the reality of facts is generally so powerful that it is difficult to imagine something beyond them. I explained in Chapter I that creation is the production of original works through imaginative skills. One needs *imagination* to leap from the reality of Nature that surrounds us to the production of original works about Nature.

In the following discussion, I will compare *creativity in science* to that in writing and arts—visual and musical. For scientists, *original works* include new phenomena, new mechanisms, new laws or new paradigms. For writers, they are literary works, and for artists, visual or musical creations. The main aspects of the discussion below are summarised in Table 5.

My view is that: (1) scientific creativity, like creativity in writing and arts, is rooted in factors such as *fantasy, imagination and intuition*; (2) all creators *combine intuition with craftsmanship*; (3) creation is accompanied by *pleasure*. Concerning the latter, Koestler (*The Act of Creation*, 1964) considered that creation is accompanied by two types of emotion, i.e. the triumphant explosion of tension, which has suddenly become redundant since the problem is solved (“I” made a discovery), and the gradual catharsis of the self-transcending emotions, which is the contemplative delight in the truth that the discovery revealed (a discovery has been made). Table 1 shows that humour is dominated by the first type of emotion, the arts by the second and scientific creation involves the two. By reference to Table 1, PLEASURE could

Table 5. The three components of creativity in science, writing and arts: (1) specific elements of Nature are selected and assembled by intuition; (2) translating the results of intuition into original works requires craftsmanship, i.e. the mastering of tools; (3) pleasure sustains creators, and makes the whole society interested in their works

	Elements of Nature selected and assembled by intuition	Tools of the craft	Works of pleasure
Science	Phenomena, variables, processes, mechanisms	Scientific method	Scientific discoveries
Writing	Characters, situations, ambiance	Language	Literary works
Arts	Images, feelings, themes	Tools of the art	Works of art

be defined as the state of gratification resulting from the explosion of tension, and/or the catharsis of self-transcending emotions. Three key components of creativity are: *intuition, craftsmanship and pleasure*. This is the case for *all types of creation*—scientific, literary or artistic. The three components of creativity are discussed in turn in the following paragraphs (for my definition of intuition, see the end of Section 'Creation' in Chapter I).

Intuition

As already discussed in Chapter II, the keystone of creativity is *intuition*. People who create *original works* select and assemble in new ways specific elements of Nature that contain, for them, information that had not been seen by others. That selection process, which is very personal, is based on intuition.

Intuition is the cornerstone of creativity in science, writing and arts. Creative scientists select, for example, some phenomena, variables, processes or mechanisms that are most often already known, but in which they sense new information. Everybody knows the story of Newton seeing an apple falling from a tree, which led him to discover the laws of gravity. Another example is the observation by Fleming of bacteria killed by mould in his laboratory, which put him on the trail of penicillin. In these two cases, the observed phenomena were not new, but the information sensed in them by Newton and Fleming was original. In the field of writing, authors select by intuition such elements as characters, situations or ambiance in which they sense a story, of which they often do not know the unfolding when they start writing. For artists, the selected elements of Nature may be images, feelings or themes, which become the thread of their work. In all cases, the elements selected by intuition already exist in Nature and are thus available to many or all people, but only *true creators intuitively select* the right elements. As explained in Chapter II, the answers arising from intuition are most often *falsified*, in which case the creation process aborts because the elements selected were not appropriate. In other words, *intuition most often misleads creators*, which is very hard on them. However, there is no creation possible without intuition.

Craftsmanship

Intuition alone is not enough to achieve original works. Creativity also requires *craftsmanship*, i.e. the mastering of a method or craft. This is true in all domains: science, writing, arts, and so on. For example, scientists must learn from other scientists how to formulate and test hypotheses, writers must learn from other writers how to use the written language and artists must learn from other artists how to chisel marble or compose music. Mastering a method is an essential component of creativity because, without craftsmanship, a so-called scientific discovery could in fact be a figment of imagination, a text may prove painfully unreadable or unbearably pedestrian, marble may break under the chisel or music may be an uninspired cacophony. Only the *mastering of a method* makes it possible to *translate intuition into original works*. It follows that intuition alone is not enough for creation. The latter also requires craftsmanship. Conversely, craftsmanship without intuition may produce sound works, but not original ones.

Here is an example concerning the famous *painter Pablo Picasso* (1881-1973). In his youth, Picasso spent much time imitating the styles of the great masters of painting. In his maturity, the artist was asked by an interviewer why he had done so. Picasso is supposed to have answered: “If I had not imitated them, I would have had to spend the rest of my life imitating myself.” Picasso, who revolutionised arts in the 20th century, recognised that without mastering the best achievements of a domain, one is left with only one’s naked talents, having to reinvent the wheel without tools. Intuition and craftsmanship are integral components of creation.

Pleasure

In addition to intuition and craftsmanship, an important, and probably essential, characteristic of original works is *pleasure*. The pleasure is shared by creators and those who enjoy their works.

On the one hand, *authors experience pleasure* in using their intuition and craftsmanship to produce original works. Pleasure is especially important because creation requires both a great deal of curiosity and an almost obsessive perseverance. Even if perseverance may be largely personal, pleasure reinforces it. Without the perseverance of creators, many potential discoveries would never come to light. In this context, pleasure can be seen as part of a functional mechanism, i.e. pleasure rewards creators for their perseverance. The idea was nicely summarised by Feynman, commenting on his Nobel Prize in Physics: ‘The prize is the pleasure of finding the thing out, the kick in the discovery, the observation that other people use it [my work]—those are the real things’ (Feynman and Dyson 2001, *The Pleasure of Finding Things Out*).

On the other hand, *pleasure is also experienced by the people* who are filled with enthusiasm by the new works, e.g. scientific theories, books, sculptures, musical pieces. Their pleasure is, of course, sometimes delayed because original works often shock contemporaries. Almost all original works, however, finally manage to make other people experience the pleasure felt by their authors. Again, the idea developed in the present paragraph was nicely summarised by Feynman: ‘Another value of science is the fun called intellectual enjoyment which some people get from reading and learning and thinking about it, and which others get from working in it’ (Feynman and Dyson 2001)

Pleasure is very important for at least three reasons. (1) It *sustains the creativity* of authors against the difficulties they often meet when producing original works. (2) Pleasure drives the authors to *share their works* with others. (3) It leads *society to support* the authors of original works. Hence, pleasure plays an essential role in transforming creation, an *individual pursuit*, into an activity of interest to *society as a whole*.

Creative Imagination and the Discovery Process

Original works result from the combination of three main components: intuition, craftsmanship or methodology and pleasure. This combination is called CREATIVE IMAGINATION. Creative imagination calls on three main characteristics of human beings: *non-rationality*, which corresponds to intuition, *rationality and/or dexterity*, which correspond to methodology and/or craftsmanship, and *feelings*, which correspond to pleasure. Intuition and craftsmanship were already discussed in Chapter II as components of discovery (Fig. 5). I explained in the above discussion that pleasure is an additional

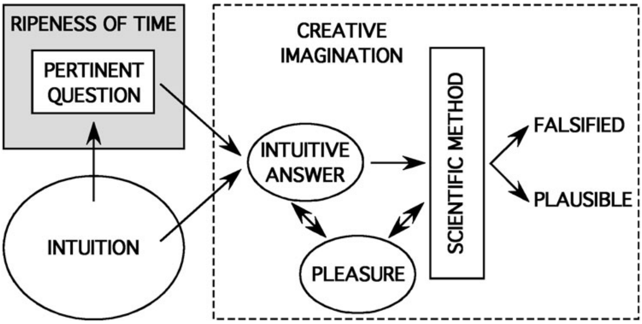


Fig. 9. The process leading to discovery requires a pertinent question, which strongly involves intuition, the ripeness of time and creative imagination; the latter combines intuition, the scientific method and pleasure. Modified from Fig. 5 (Original)

component of discovery. This is illustrated in Fig. 9, which shows that the DISCOVERY PROCESS in science consists of five components: *a pertinent question, the ripeness of time, intuition, the scientific method and pleasure*. The last three components make up creative imagination.

Significance of Creativity

Creative imagination, which has its origin in the intuition of creators, reaches society as a whole. It is interesting to briefly examine how creation progressively extends from individual creator to society, by reference to the three components of creative imagination discussed above. In Fig. 10, the reach (or range) of creative imagination is plotted as a function of its three components, as discussed in the following paragraphs.

(1) *Intuition*. I explained above that intuition is a *personal trait*, which allows creators to see information where other people do not, and to use that information for selecting relevant elements of Nature and assembling them in original ways. This is generally done *individually*: it being recognised that interactions with other people, friendly or antagonistic, sometimes provide the spark that fires imagination, creation typically starts as an individual pursuit.

(2) *Craftsmanship*. Even if creation is largely individual, creators are generally not alone. Because crafts or methods must be learned from peers, creators become part of peer groups during their years of formal education, which often include some form of apprenticeship. The *peer group* grows and changes as the career progresses. It includes a few close friends, sometimes bitter enemies, and a circle of colleagues that creators meet more or less regularly.

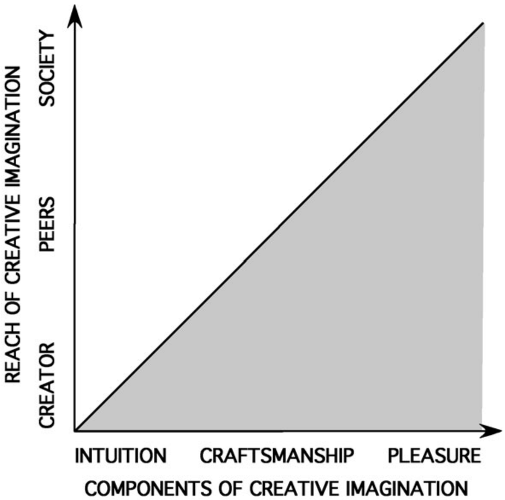


Fig. 10. The reach of creative imagination, from the individual creator to society as a whole, is plotted as a function of the three components of creativity: intuition, craftsmanship and pleasure. Intuition is a personal trait, craftsmanship refers creators to a peer group and pleasure links creators to society as a whole. This framework is conceptual: it is not based on actual data (Original)

(3) *Pleasure*. The reach of creative imagination extends beyond peers, to society as a whole. I explained in the previous Section that the factor *linking creators to society* is pleasure. The *pleasure experienced by authors* during the production of original works most often incites them to share their creations with the public. When the *public receives the new works with pleasure*, this generally pleases the authors, who are thus encouraged to produce more works. This generates a *positive feedback* loop based on pleasure, which is mutually beneficial to the creators and the general public. This is not to say that there is necessarily no creation in the absence of pleasure. For example, some authors are spurred by harsh reviews from critics or negative reactions from the public, and others manage to produce masterworks while remaining unknown from the public during their whole lives. However, pleasure, when present, creates strong links between creators and society.

It follows from the discussion in this Chapter that *creativity is not a social luxury*, but plays instead a fundamental role in societies. This is why we can trace it back to our earliest ancestors. This is also why creativity will likely be with us as long as human societies exist. This is finally why the maintenance of civilisations requires societies to support sustained creativity. I will further develop these ideas in Chapter X, Section 'Culture and Eco-Ethics'.

IV SCIENTIFIC THEORIES

I explained in Chapter II that scientific discovery is finding, with imaginative skills, new phenomena, new mechanisms, new laws, new theories or new paradigms, without taking any assumption as being true *a priori*. New phenomena, mechanisms and laws are based on *observations or experiments*, whereas new theories and paradigms proceed from the *theoretical approach* (Table 4). In the present Chapter, I examine the sometimes difficult relationships between the researchers involved in observations or experiments and theoreticians, and I try to understand why some fields of science are lighter in theory than others.

Theoretical Science and Scientific Theories

In Chapter II (Section ‘Paradigms, Theories and Tautologies’), there were two definitions given for ‘theory’. The *first one* is quite general: a body of, at least partly, hypothetical statements, which refers to a small number of principles, and represents as simply and completely as possible the relevant phenomena, mechanisms or laws. The *second* definition is more specific: a construct that makes potentially falsifiable predictions about natural phenomena.

In *mature scientific fields*, the theoretical and observational (or experimental) components of research are generally strongly linked. For example, cosmologists investigate the origin, structure and space-time relationships of the Universe. In order to do so, they develop and progressively modify general laws and theories about the Universe, to account for the observations of celestial phenomena made by fellow astronomers. Conversely, observational programmes in astronomy are often triggered by cosmologic theories. For example, in 1845 and 1846 the French theoretical astronomer Leverrier predicted the presence of an eighth planet in the solar system, from observed disturbances in the motion of the seventh planet, Uranus; planet Neptune, which nobody had seen before, was discovered in 1846 by the German astronomer Galle, who directed his telescope at the celestial region that Leverrier’s calculation had identified. However, cosmologic theories are sometimes challenged by the observations resulting from programmes they triggered. Another example of the strong link between theories and observations is nuclear physics, in which there are continuous exchanges between the two aspects of research. According to circumstances, theoreticians and researchers involved in observations or experiments see their relationships in term of collaboration or competition.

All theories corresponding to the *first definition* (first paragraph of this Section) contain non-predictive elements, called ‘tautologies’; these include *classifications* (Chapter II, Section ‘Paradigms, Theories and Tautologies’). Even if tautologies are nonpredictive by definition, classifications may lead to predictions in mature scientific fields. Classifications acquire *predictive ability* (which is the key component of the second definition of theories above) when their classes become associated with characteristics beyond those that were used to define the classes. For example, the periodic classification of chemical elements by the Russian chemist Mendeleyev was originally based on observable properties of elements; it is only later that the sequence of elements in the periodic table was found to correspond to their atomic numbers. The periodic table allowed for elements that had not then been discovered at the time, or do not exist naturally on Earth. The missing elements were later found in Nature, or artificially made in the laboratory. Hence in mature scientific fields, theoretical science, including its tautological components, is closely linked to predictive theories.

Concerning the *second definition* of ‘theory’ (first paragraph of this Section), the claim for predictive power can be checked by asking if the premises or conclusions could be falsified by observations (Table 6). The answer is *negative* in the case of *tautologies*, which have therefore no predictive power, because tautologies cover the whole range of possibilities (Chapter II, Section ‘Paradigms, Theories and Tautologies’). The answer is *positive* in the case of *hypothetico-deductive theories* (Fig. 4), which therefore have predictive power. For example, the ‘prediction’ that all chemical elements could be classified in Mendeleyev’s periodic table of elements,

Table 6. Difference between tautologies and theories, according to Peters (1991). In theories, the premises or conclusions can be falsified by observations; in tautologies, they cannot. Tautologies have no predictive power because they cover the whole range of possibilities

	If observations were contrary to prediction	
Premises	Could be falsified	Could not be falsified
Conclusions	Could be falsified	Could not be falsified
Then	Theory	Tautology
Predictive power	Yes	No

which is a tautology, cannot be falsified by observations; hence, that 'prediction' is useless under any circumstance. On the contrary, the prediction of specific physical or chemical properties for an unknown element could be falsified once the element is found or made in the laboratory; hence, that prediction is useful as long as it is not falsified.

The explanation of observed phenomena is the key to understanding Nature. Because any *scientific theory* that explains a phenomenon can also predict it, philosophers of science reject as *non-scientific* those explanatory theories that cannot achieve prediction. In other words, without predictive power, there is no way to check the validity of explanations.

The process by which theories or paradigms can be *falsified* is not a straightforward matter. Indeed, no scientific theory can be considered above the threat of disproof in future tests (Chapter II, Section 'Paradigms, Theories and Tautologies'). Conversely, falsification is rarely, if ever, unquestionably complete. The process of paradigm or theory falsification is the object of an active and fascinating debate in philosophy of science, but this topic as a whole is beyond the scope of the present essay.

One aspect of the above debate is the role played by *observations* in falsification. Opinions range from the position that observations have *little influence* on the development of theories to the other extreme that observations *drive* theoretical advances. For example, some philosophers of science think that the crucial element in falsification is whether the new theory offers any *novel, excess information* compared with its predecessor and whether some of this excess information is *corroborated*. In other words, *falsification alone is not sufficient* for elimination of a theory, and a falsified theory is abandoned only if there exists a *better alternative*. Other authors give more weight to observations, i.e. a theory becomes successful because it is effective in accurately *predicting the outcome* of experiments. An intermediate opinion is that a successful theory not only explains the observations that gave rise to it, but can also *fit new observations*.

Scientific Theories and Observations

The largely *philosophical debate* about scientific theories, although very interesting, is beyond the scope of the present essay. The following discussion examines the *operational relationships* between theoreticians and the researchers involved in observations or experiments, with the purpose of understanding why these relationships are sometimes difficult, and at other times very fruitful.

Possible interactions between theory and observations or experiments are summarised in Fig. 11. On the one hand (abscissa), *researchers involved in observations/experiments* may think that their results have precedence over theory, or they may take advantage, to various degrees, of theoretical advances in their observational or experimental work. On the other hand (ordinate), *theoreticians* may think that theory has precedence over observations/experiments, or they may take advantage, to various degrees, of observations/experiments for theoretical progress. In general, but not always, scientific progress is more rapid when each approach takes advantage of advances in the other. The fundamental relationship between observations and theory was stated by Charles Darwin: ‘No one could be a good observer unless he was an active theorizer’ (Darwin and Seward 1903, Vol. 1, p. 195; cited in Koestler 1964, p. 135):.

Even if most scientists easily agree on the general idea that *continuous exchanges between theories and observations* are essential to the progress of science, the actual coexistence of the two approaches is not always easy. Theoreticians sometimes consider their work as qualitatively superior to observational or experimental results, and take the latter as a necessary evil. Conversely, researchers dedicated to observations or experiments sometimes

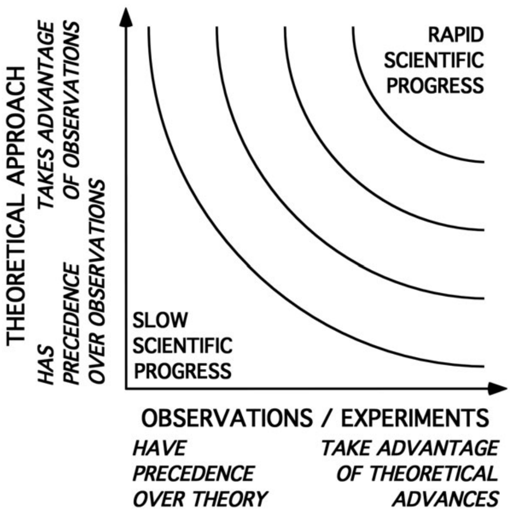


Fig. 11. Possible interactions between theory and observations/experiments. Abscissa: theory as seen by researchers involved in observations/experiments. Ordinate: observations/experiments as seen by theoreticians. In general, scientific progress is more rapid when each approach takes advantage of advances in the other (Original)

consider their work as the only solid science, and see theories as somewhat superfluous. The existence of theories, which are never perfect, sometimes leads to the rejection of otherwise acceptable observations. Conversely, the existence of observations, which are never complete or final, sometimes leads to the rejection of otherwise acceptable theories. Here is one example of each type of situation.

Concerning the *prevalence of theory on observations*, the famous German geophysicist and meteorologist Alfred Wegener failed to convince the scientific community of his continental drift theory during the first part of the 20th century, following the publication of his book *Die Entstehung der Kontinente und Ozeane* (*The Origin of Continents and Oceans*) in 1915. His observations of overwhelming correlations between the morphology, geology, flora and fauna of continents located on opposite sides of ocean basins, e.g. Africa and South America, were dismissed by fellow researchers as irrelevant. The prevailing theories then assumed that continents and ocean basins were occupying fixed positions. Wegener's observations were not accepted at the time because the explanatory model he proposed assumed that continents moved through the ocean bottom, which is not possible because the latter is too rigid to allow the passage of continents. Hence, Wegener's theory was almost unanimously rejected by Earth scientists, until new geophysical observations showed that the upper part of the Earth exhibits movements of expansion, subduction, compression and translation. The synthesis of these observations in the late 1960s, based on the concept of plates, provided the global mechanism that Wegener lacked. In this example, the *inadequate theory* of continents' fixed positions prevailed over Wegener's *sound observations* in wait of an acceptable alternative theory: plate tectonics. By the way, people now often confuse Wegener's theory that continents moved (i.e. continental drift) with the later theory that explained these movements (i.e. plate tectonics).

Concerning the *prevalence of observations on theory*, Paul Dirac (British) and Erwin Schrödinger (Austrian) received the Nobel Prize of Physics in 1933 for the creation of quantum mechanics. Dirac explained later that Schrödinger initially developed his well-known wave equation of the electron by pure thought. When he tried to apply his equation, he obtained results that did not agree with experimental data, because the spin of the electron was not known at the time. He therefore initially published a non-relativistic approximation of his equation, and came back to the original formulation only later. This delayed the progress of physics. In this example, *imperfect observations* prevailed over a *sound theory*.

In spite of the above real difficulties, science normally progresses through the *alternation of theories and observations*. Theories continuously give rise to new observations or experiments, and are compared with the resulting new data. Conversely, the results of new observations or experiments that disagree with existing theories often lead theoreticians to modify their constructs, and sometimes change their paradigms. Here is one example of each type of situation.

Concerning the *influence of new theories on observations*, we know that Darwin's theory of evolution marked a radical change from the prevailing view that all species had been created simultaneously. Many people in the late 19th century were especially shocked by the idea that humans had animal ancestors. The horrified people were then told that our ancestors were apes; what would they have thought of the present suggestion that we descend from Archaea? Darwin's original theory was progressively modified, in the following decades, by discoveries in other fields of science, such as genetics, comparative anatomy and embryology, ecology and molecular biology. Successive versions of the theory launched new types of observation and experiment, made by proponents, sceptics and opponents.

Concerning the *influence of new observations on theories*, most marine biologists until the mid-18th century believed that there were no living organisms in the deep ocean. This idea came from studies on the vertical distribution of marine life by the *British naturalist Edward Forbes* (1815–1854), who observed that animal concentrations decreased with depth and suggested, from dredge hauls in the Mediterranean, that the limit of life was 300 fathoms (ca. 550 m). After Forbes' death, his followers developed the theory that life could not exist in deep waters, because of the high pressure and absence of light and oxygen, even if this contradicted previous observations of deep-water benthos by other people. The abyssal theory was rapidly challenged by an increasing number of observations of life in deep waters. Those observations not only swept the abyssal theory, but were also one of the factors that led to the *Challenger* expedition of 1872–1876, which marked the creation of modern oceanography.

I close this Section with two sentences from *Consilience. The Unity of Knowledge* (Wilson 1998): 'Nothing in science—nothing in life for that matter—makes sense without theory" and 'Science, to put its warrants as concisely as possible, is the organized, systematic enterprise that gathers knowledge about the world and condenses the knowledge into testable laws and principles'.

Scientific Fields that are Light in Theory

Scientific fields can be divided into unidisciplinary (or simply, disciplinary) and multidisciplinary. Examples of the first are: astronomy, biology, chemistry, geology and physics. Examples of the second are: biogeochemistry, climatology, limnology and oceanography.

Most *disciplinary fields* rely on both theories and observations, as reflected by the existence of numerous journals dedicated to theory in basic disciplines. In contrast, most *multidisciplinary fields* are light in theory, as shown by the small number or even absence of theoretical journals in these disciplines. There would be little progress in astronomy without the concurrent observation of space and development of cosmology. Similarly, there would be little progress in geophysics without concurrent field observations of the Earth and theoretical developments in plate tectonics. Curiously, several multidisciplinary fields rely almost entirely on observational or experimental approaches, with little interest in building up theoretical foundations. The history of sciences shows that progress in various scientific fields requires not only sound observations, but also strong theoretical foundations and active research on theoretical aspects. I am therefore persuaded that the lightness in theory of many multidisciplinary fields slows down their overall progress.

Scientific fields that are *light in theory* are not likely to predict the existence of unsuspected phenomena, or generate fruitful hypotheses to be translated into questions that can be answered by observation or experiment. Fields lacking the intellectual tools provided by a strong theoretical background cannot progress rapidly in gathering knowledge about Nature, and condensing it into testable laws and principles. These fields develop few hypotheses and have therefore little theoretical direction to design observational or experimental programmes; they do not have the proper theoretical background to interpret and synthesise the data; and they cannot successfully organise natural phenomena into mechanisms, laws and theories (Table 4). As a consequence, scientific fields that are light in theory generally progress slowly.

The lack of *journals dedicated to theory* in several multidisciplinary fields makes it very difficult for researchers interested in developing theoretical approaches to publish their works. The main outlets for publishing theoretical papers in fields without theoretical journals are books, conference proceedings, or journals with accommodating Editors. However, these three possibilities are not really satisfactory. Firstly, it is difficult to write books without previously publishing papers on the same topic, and without access to specialised journals. Secondly, conference proceedings are not ideal for disseminating theoretical knowledge, because of their generally small and specialised circulation. Thirdly, publishing a theoretical paper in a non-theoretical journal is never easy, even with the support of the Editor, because the reviewers of such journals are often reluctant to recommend the publication of theoretical works. A provisional solution could be the creation of sections devoted to theoretical developments in non-theoretical journals. Another approach could be for Editors-in-Chief of leading disciplinary journals to appoint Associate Editors to deal with theoretical contributions. These two approaches have been implemented in some leading non-theoretical, disciplinary journals in recent years.

There may be several reasons that explain the lack of theoretical approaches in some fields. One is the *stage of development of the discipline* (Fig. 12): young fields often focus initially on observations and experiments, to which theory is progressively added as they mature. This could explain the low theoretical content of several multidisciplinary fields, which are relatively young. Another reason is the *intrinsic difficulty of developing theoretical approaches in multidisciplinary fields* relative to their disciplinary counterparts. Whatever the explanation, it is important for their own progress that fields generally light in theory actively favour and encourage the development of theoretical approaches.

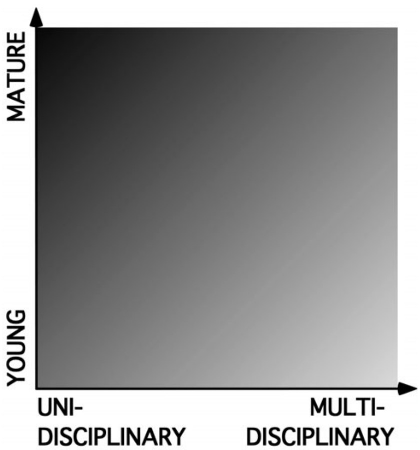


Fig. 12. Theoretical content of scientific fields: from high content (dark shade) in mature unidisciplinary fields, to low content (light shade) in young multidisciplinary fields. This framework is conceptual: it is not based on actual data (Original)

V CONSEQUENCES: EDUCATION

I showed in Chapter III that the production of original works involves the three components of *creative imagination*: intuition, methodology or craftsmanship and pleasure. *Education* plays an important role in preserving, acquiring and favouring these three components in young people. (1) Even if *intuition* is largely innate, it can be either cultivated by enlightened educators, or repressed by excessively rigid education (see Chapter I, Section ‘Creation’, for the meaning of ‘intuition’). (2) *Methods or crafts* must be learned, through formal education or tutoring. According to circumstances, formal education may either repel or attract students to learning. (3) *Pleasure* is a healthy response to imaginative works. Progressive educators may promote pleasure as reward, whereas conservative educators may use the negation of pleasure as punishment. It follows that education, at home and at school, plays essential roles in the emergence of *creative people*.

I will briefly review how the three components of creative imagination can be promoted in both *general and science education*, remembering that educators include parents and teachers. The ideas discussed in this Chapter are summarised in Table 7 and Fig. 13.

General Education

The first component of creative imagination is *intuition*. Intuition is not only too little praised or encouraged at home and in formal education, from a young age to university, but it is sometimes repressed because it is seen as opposed to rationality. I showed in Chapters II and III that intuition and method, i.e. rationality, are not opposed but instead *complementary*, and that creativity requires both. Intuition is often looked at suspiciously from kindergarten to graduate studies at university (or the equivalent level of training in arts), when it suddenly becomes a requirement. It is not logical to look down on intuition until the graduate level, and then turn round demanding students to be creative. Hence, given its key role in *creativity*, intuition must be protected, praised and encouraged in children, youngsters and adults.

All school subjects can be used to *promote and develop intuition*: arts, literature, sciences, sports, etc. Which subject(s) will actually be best largely depend(s) on the individual tastes and abilities of *youngsters*, and the special skills of *teachers*. Nobody, youngster or teacher, is equally good, and interested, in all subjects. The key point is that each student should have the

Table 7. Promotion of the three components of creative imagination in general and science education

Creative imagination	General education	Science education
Intuition	Protect, praise and encourage intuition	Go from the experience of Nature to general scientific concepts
Craftsmanship	Build on individual tastes and abilities in all subjects	Provide quality teaching in basic subjects, and elite training in preferred subjects, including science
Pleasure	Promote pleasure as normal gratification for creation and its sharing	Put pleasure in science at the centre of education, as the key to in-depth learning

opportunity to meet, from time to time, at least one teacher who allows and helps him/her to use intuition in creative activities. Most teachers can do it, and many actually enjoy doing it, but not necessarily with the same students as their colleagues.

The second component of creative imagination is *craftsmanship*. Crafts must be learned. From young age to university, educators must progressively teach methods or crafts that fit individual tastes and abilities. This is a long process, in which errors at critical steps of training may slow down progression toward the acquisition of crafts. One of the main problems here is that formal education often tends to ignore, or even reject, the different tastes and abilities of young people. Such an attitude in some educators or education systems pushes many teenagers out of the school system. Ignoring individual tastes or abilities partly results from the genuine need of teaching a common body of knowledge to all students. However, what is considered to be *basic knowledge* sometimes extends too far into subjects that are not essential for operating in society, in which case the curriculum becomes so cluttered with peripheral subjects that there is no time left for responding to individual tastes and abilities. I think that the common body of knowledge should include: speaking, reading and writing one's mother tongue and at least one foreign language; mathematics and basic sciences; history, geography and civics; bases in visual arts and music; some sports.

A complementary aspect to the previous paragraph is the training of the various types of worker. I explained in Chapter I (Section 'Knowledge Work') that there is no inherent hierarchy among the different work activities, because they are all essential to the functioning of societies. It is there-

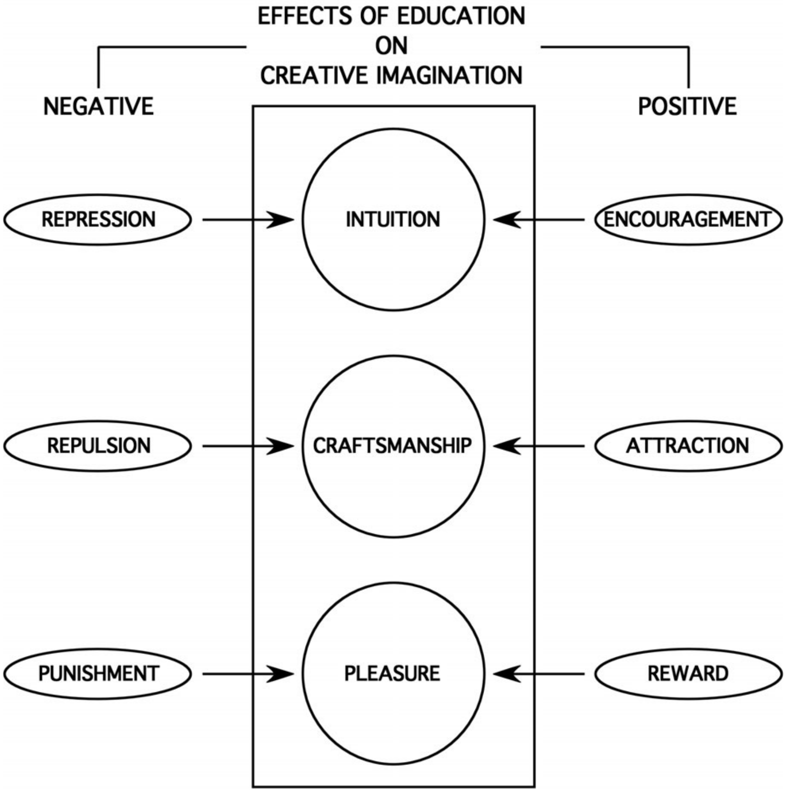


Fig. 13. Possible effects of education on the three components of creative imagination, i.e. intuition, craftsmanship and pleasure (Original)

fore essential that the whole population, the educators and the youngsters themselves be convinced that various types of work and training are *equally important to society, honourable and personally fulfilling*. Youngsters must not be forced to follow the same curriculum irrespective of their tastes and abilities. For example, those interested in manual or service work could be offered training by apprenticeship or in specialised schools, whereas those aiming at knowledge work must attend university or some other school of high knowledge. Again, what really matters is that all youngsters be offered an education corresponding to their tastes and abilities, and not be forced to follow the same curriculum, in which many students are discouraged and humiliated and from which they too often drop.

The third component of creative imagination is *pleasure*. Even in societies in which pleasure is not accepted as a healthy response to imaginative works, educators often use it as a reward for success, accompanied or not by punishment for failures. Educators must promote pleasure as a *normal gratification* not only for academic accomplishments, but also for the creation and the sharing of original works by young people. This is true for visual arts, music, writing, science, sports and, generally, all subjects susceptible to interest youngsters. Using pleasure as the *legitimate reward* for creation assumes, of course, that creative imagination is a component of education, as explained in the previous paragraphs. To summarise, what is studied without pleasure vanishes rapidly, and what is learned with pleasure is cherished for life.

Pleasure is, unfortunately, too often *absent from school*. In many countries, increasing numbers of youngsters—especially boys—attend school reluctantly. As a consequence, they make life difficult for other students and the teachers, who themselves become increasingly reluctant to go to work. This creates a positive feedback, which is disrupting school systems in some countries: most teachers retire as early as allowed by their pension plans, and many youngsters drop out as soon they can.

Science Education

The above ideas about general education are especially important for *science education*, which must call on intuition, cultivate individual taste for scientific subjects and bring pleasure to students. The three aspects are briefly exemplified in the following paragraphs (see also Table 7).

It will be explained later in this book that science is part of *culture* (Chapter X, Section 'Science and Culture'). The fact that science is part of modern culture explains why it is taught to all students, irrespective of their future professions. The purpose of pre-university science education is not to recruit or train future researchers, but instead to expose all students to science as *part of the cultural matrix* of modern societies. It follows that, at pre-university levels, science education must both interest as many students as possible, and offer those who especially like science the opportunity of delving deeper into their favourite subject.

Concerning the first component of creative imagination, i.e. *intuition*, the teaching of natural sciences must avoid going from general laws to observable phenomena, because laws are part of a rational construct in which there is little space for intuition. Youngsters must be introduced to natural sciences through *direct experience of Nature*: collecting rocks, not being taught plate tectonics; observing and/or raising plants or animals, not being taught the theory of evolution; observing planets and stars, not being taught cosmology; and so on. Educators must encourage students to develop their own explanations of natural phenomena, and progressively guide them from observations to general scientific concepts. A central concept of science is that the human mind, and therefore researchers, cannot discover absolute truth about Nature (Chapter I, Section 'The End of Science?'). The latter is a very unsettling idea for most people, and especially youngsters, who are generally confident in the abilities of adults to answer questions. This real difficulty can be turned around by making young people progressively discover that, because knowledge and science can never be perfect, they can contribute personally to their development. This is an exciting perspective! In addition, youngsters must be shown that professional researchers are imaginative people, who deeply enjoy what they do.

The second component of creative imagination is *craftsmanship*, referring here to the cultivation of *individual taste* for scientific subjects. In the teaching of methods or crafts, it must be recognised that different youngsters prefer different school subjects: arts, languages, literature, natural sciences, mathematics, and so forth. In general, young people like most subjects to various degrees, and dislike a few, sometimes strongly. Educators must try to convince children and teenagers to dedicate minimum efforts to all basic subjects (cited in the previous Section), and encourage them to go deeper in the subjects they prefer. Schools must not only provide basic teaching in scholarly matters, but also offer special programs and/or extracurricular activities in disciplines that the youngsters especially like, including sciences. Ideally, all youngsters should have access to *quality teaching* in all basic subjects, and *elite training* in the few subjects they prefer.

For those students especially interested in science, school teaching must be accompanied by science clubs and/or summer camps. Some educators or education managers—in school boards, or Ministries of Education—think that such activities are unnatural for youngsters, and therefore insist that science be a small component only of the daily activities in clubs or camps. The same people, however, have no problem with clubs or camps devoted almost exclusively to sports or arts. It must be accepted that sports, arts, sciences and other scholarly subjects can be *equally formative*, and that extracurricular activities must use those subjects that youngsters especially like for enhancing their personal development. This does not necessarily mean that youngsters particularly interested in science—or arts, or sports—will become professional scientists—or artists, or athletes. Indeed, what is important for children to become creative *is intense involvement* in some subject(s) they especially like.

The third component of creative imagination, i.e. *pleasure*, should be at the centre of science education. Pleasure does not mean absence of authority. As a matter of fact, there is little or no pleasure under continuous anarchy, or under its counterpart, dictatorship. One way to enhance pleasure in education is to use the subjects that youngsters prefer as means for approaching those they do not like much. At the primary and secondary school levels, for example, science-oriented youngsters can be brought to literature by reading fictionalised descriptions of scientific discoveries or biographies of fascinating researchers. Similarly, high school or university students in sciences may become interested in languages and humanities within the context of preparing for international research (see Chapter X). Another way to enhance the pleasure derived from science in high schools is to favour students' participation in science fairs and/or other types of public activity. These provide youngsters with opportunities to use their creative imagination for producing small science works, and to share the pleasure of their discoveries with others. The same is true at the university level, where science days or meetings are occasions for students to present their works publicly. At all ages and for all subjects, pleasure is the key to true learning.

Despite its difficult nature, *teaching* is generally a rewarding activity. Seeing the spark of understanding in the eyes of a student is a great joy. Talking with a student who has a passion for a subject is stimulating. Seeing a young person develop his/her intuition, knowledge, skills and enthusiasm for learning is a great privilege. This often more than makes up for, on the one hand, those youngsters who attend school reluctantly and eventually drop out, as discussed in the previous Section, and on the other hand, the occasional genius who is bored and does rather badly.

The building-up of creative people, over twenty years or more of education, must pay attention to intuition, craftsmanship and pleasure. *Intuition* must be protected and encouraged, *craftsmanship* must be taught in a personalized manner and *pleasure* must be valued.

Even if the teaching of science may have evolved in the right direction in some countries during the last decades, my feeling is—I may be wrong—that sciences are still taught in many instances as a dusty heap of theorems instead of re-creating the excitement of discovery. This partly explains the present disaffection of youngsters for the science component of culture (Chapter IX, Section 'Science and Culture') and for scientific careers (Chapter VI, Section 'Communicating Science to Youngsters and the Public', and Chapter XII). The latter is a worldwide phenomenon, which threatens the future welfare of several societies.

VI CONSEQUENCES: SCIENCE AND THE PUBLIC

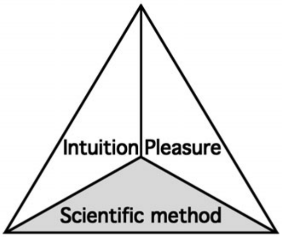
The *public* does not generally understand the process of scientific discovery and the central role played by creativity in the discovery process. I think that the lack of public understanding of that process creates a very risky situation, because the relationships between the scientific community and the public largely determine both the funding of research and the appeal of science to young people. I also think that researchers are, for a large part, responsible for this unfortunate state of affairs, because many do not pay serious attention to either the attraction of the public to science or to the professional communication of science. In this Chapter, I propose an approach to analyse these two aspects.

Responses of the Public to Science

The *public* generally admires original works in the arts, literature and science, but it relates more easily to the former two than the latter. When people visit art museums, they are enthralled by the exhibited works. Similarly, when people read novels or poetry, they are taken by the written texts. In contrast, when people visit science museums or read about scientific discoveries, most are awed by the scientific achievements but they generally do not respond emotionally to the discoveries. Nevertheless, some scientific topics fascinate the general public, for example astronomy and marine sciences. Why is that so?

One of the main reasons for the much lower emotional response to science than arts or literature may be that the public *understands*, or thinks it understands, how artists and writers produce their works, but *does not understand* how scientists make discoveries. *Artists and writers* are generally seen as imaginative, talented and enthusiastic creators, whereas *scientists* are often imagined as very logical, highly trained and unnaturally cold individuals. However, the creative process is largely irrational, not only in art (where we are ready to accept it) but also in exact sciences. In other words, for most people, artists and writers are exciting, whereas scientists are dull or even frightening. All scientific fields that are well liked by the general public have talented *communicators*, who share their passion with the public.

Why does the public generally misunderstand science? There are, of course, various explanations (Fig. 14). I personally think that the main rea-



Scientific field	Usefulness	Dream pot.	Public support
Astronomy	+	+++	+++
Marine sciences	++	+++	+++
Medical research	+++	+++	+++
Molecular biology	+++	++	++
Chemistry	+++	+	+
Nuclear physics	+++	+	+

Fig. 14. Two reasons contributing to the misunderstanding between the scientific community and the public. Left: insistence on the methodological component of creative imagination (shaded), whereas the latter is also based on intuition and pleasure. Right: insistence on the usefulness of science (shaded), which in fact does not attract the public much (shaded) compared with the dream potential of science, as discussed in the text using the example of the six scientific fields listed here (Original)

son for the misunderstanding between scientists and the public is the focus of both scientists and science philosophers on the *scientific method* instead of *scientific creativity* as a whole. I explained in Chapter III that the scientific method is only one of the three components of *creative imagination*, the other two being intuition and pleasure. If writers or artists thought of themselves and/or were seen primarily as specialists of a tool (grammar for writers, chiselling for sculptors, musical notation for composers, etc.), they would probably not arouse much excitement in the general public. *Writers and artists* think of themselves, and are seen by the public as *imaginative creators*. Curiously enough, many *scientists* prefer to think of themselves and to be seen as *specialists of a tool*, i.e. the scientific method, instead of recognising themselves and showing others what they are truly: *imaginative creators*. Of course, some scientists are well-known as passionate human beings. Examples include the physicist and astronomer Galileo Galilei, the biologists Charles Darwin and Louis Pasteur and the theoretical physicists Albert Einstein and Stephen Hawking. Even with such famous examples, it is not generally known among the public that *scientific discoveries* involve intuition and pleasure.

In addition, I am convinced that the public support of scientific research, when it exists, does not primarily depend on the *usefulness of discoveries*. My conviction comes not only from the analysis of scientific creativity in the previous chapters, but also from the examination of the different responses of the public to various scientific fields. For example, nowadays, astronomy, marine sciences and medical research are strongly supported by the public, whereas molecular biology, chemistry and nuclear physics are much less in favour. Let us briefly examine in the following paragraphs these six research fields, as examples.

Concerning *astronomy*, celestial bodies have fascinated people since the beginning of humanity, but their present study has little immediate usefulness. As a matter of fact, the observation of stars developed in the past as a utilitarian activity, i.e. the prediction of future events, but as astrology progressively gave way to astronomy, the field lost most of its utilitarian character. The little practical relevance of astronomy does not prevent it from being very high in the public esteem. As a consequence, that field is very well funded by governments and private foundations.

The case of *marine sciences* is similar to that of astronomy, in that oceans have fascinated people for thousands of years. The passion for oceans increased tremendously after the advent of movies on the underwater world. Several discoveries in the field of oceanography are very useful (ocean circulation, tide predictions, migrations of commercially important fish, etc.), but it is only recently that global problems such as climate change have shown the true relevance of oceanographic knowledge. The public support, and therefore the funding of marine research, is very high, even if part of the knowledge on oceans has been unforgivably misused in recent decades, e.g. the destructive management of several commercial fish stocks, and even if most ocean research is curiosity driven.

The public interest in *medical research* comes from the fact that health has been forever, and continues to be, a central preoccupation for most people. Scientific medicine is, however, in competition with alternative healing approaches. Even if the latter are quite popular with part of the public, discoveries in scientific medicine always create a sensation in the general public, which strongly supports medical research.

Contrary to the above three fields, *molecular biology* is a relatively young science. It became popular when the Nobel Prize of medicine publicly acknowledged the discovery of the double-helix structure of DNA in 1962. In the following decades, discoveries in molecular biology attracted a lot of public interest, culminating with the Human Genome project initiated in the 1990s. However, in many countries the general public sees very negatively some recent applications of molecular biology, e.g. the creation of transgenic organisms. Hence, paradoxically, the public support of molecular biology seems to be decreasing, at least in some countries, as its potential usefulness increases.

Chemistry followed a path similar to that of molecular biology, although on a much longer time scale. That science emerged from alchemy during the 16th century, to become one of the dominant sciences that largely drove the 19th century industrial revolution. The medieval public was fascinated by the alchemic quest for the transmutation of base metals into gold, a universal cure for disease and the water of youth, whereas the modern public is often repelled by the real achievements of chemistry to which it attributes, not always rightly, such global problems as pollution. Here again, the public support of chemistry evolved inversely to the true usefulness of the field.

Nuclear physics followed the same path as molecular biology and chemistry: thousands of years of interrogations about the structure of matter, followed by decades of strong interest and support by the public during the first half of the 20th century, as discoveries went increasingly deeper into the core of matter. Since the mid-20th century, there has been progressive erosion in the public support of nuclear physics, corresponding to the development of such applications as nuclear arms and nuclear power plants.

These six examples clearly show that the support given by the public to various scientific fields has no relationship, direct or inverse, with their respective *usefulness*. In other words, the correlation between the public support and the usefulness of scientific fields is null.

So, if it is not for their usefulness, why are some scientific fields popular, and others not? *Curiosity* has largely led and still leads the scientific quests of humankind. I think that the degree of public fascination by various fields depends fundamentally on their *dream potential*. For example, astronomy makes people dream about the Universe, marine sciences about the most mysterious environment on our planet, and medical research about a better life, if not immortality. Other fields such as molecular biology, chemistry or nuclear physics could also make people dream, and they often did in the past, but the recent focus on their utilitarian facets has *killed the dream*. Dreaming beyond day-to-day contingencies is essential to both individual sanity and social progress. One must therefore be very careful not to kill the dream quality of science by focussing primarily on its utilitarian facets.

In addition, all scientific fields that meet popular favour have talented communicators who share their passion with the public. Public support of a scientific field requires that some people work hard at promoting its discoveries and discoverers. Most scientists underestimate the crucial importance of *professional science communication*, which must be focussed on passion, not utilitarian considerations. Of course, it does not hurt to show the useful facets of science, when these exist, but *usefulness must sustain dreams*, not try to replace them.

Communicating Science to Youngsters and the Public

The above considerations have profound implications for the *communication of science*. This, in turn, has implications for both the attraction of bright youngsters to scientific careers and the public support for the funding of research.

Science communication to youngsters and the public is often largely or exclusively based on three aspects of science that, in my view, turn people off. These are: the insistence on the rationality of the scientific approach, the awed admiration of the magnitude of scientific knowledge and the focus on utilitarian facets of science. The three aspects are discussed here, together with possible solutions (Table 8).

We already examined the *first aspect* of science communication that turns people off: the *insistence on the highly rational nature of the scientific approach* (Chapters II and III). We have shown that *discovery* requires the combination of intuition, pleasure and, of course, the scientific method. Insisting on the latter facet of creativity without simultaneously stressing the other two provides a very distorted picture of science and scientists. The reluctance of researchers to recognise and acknowledge that *intuition and pleasure* are as important in discovery as the *scientific method* may be the result of their academic training up to the doctoral level, which focuses on the scientific method. It follows that the roles of intuition and pleasure in discovery are often a well-kept secret among creative scientists, who fear that these two components of discovery might be seen as sins against rationality. As a consequence, when researchers communicate with the public,

Table 8. Aspects of science communication that turn people off, and possible solutions

Science communication: negative aspects	Possible solutions
Insistence on the rationality of the scientific approach	Show researchers as true creators; insist on the role played by creative imagination in research
Awed admiration of the body of scientific knowledge	Explain that scientific knowledge is transient, and discoveries are the fact of normal human beings
Focus on the utilitarian aspects of science (see Fig. 15 later in Section)	Insist on the dream quality of science (see Fig. 14)

they often show only their rational facet and hide their intuition and pleasure. This unfortunate situation is amplified by the fact that most modern communication with the public is mediated by professionals, who often do not know about the complex nature of scientific creativity, or may suspect it but find it easier to show its methodological component only, or might even wish to explain to the public how discoveries are really made, but hesitate to do so because most researchers do not wish it. Whatever the reasons, showing only the *rational facet of discovery* misleads the public as to the nature of scientific creativity. This produces a gulf between researchers and other people, who generally admire scientists but have no wish to emulate them.

As a *solution to the first problem*, science communication must show researchers as *true creators*, who make discoveries by combining intuition, the scientific method and pleasure (i.e. creative imagination, Chapter III). Both historical examples and living scientists can be used to illustrate that researchers are not only logical, highly trained and rational individuals, but also imaginative, talented and enthusiastic creators. Communicators must insist on the fact that the aim of research is *discovery*, and that scientific discoveries are *creations of researchers' imagination*, not of technology or data analysis (Chapters II and III). This change in approach may not be easy. Indeed, science communicators sometimes needlessly complicate their message by using specialised jargon, invoking convoluted theories, displaying impressive-looking equipment, showing abstruse graphs and/or interviewing researchers who look and/or behave like weirdoes. The opposite existing trend is to simplify the message about research to the limit of platitude, and/or make the naïve researcher hungry for media exposure to act as a foil to the professional communicator. Either situation may delight those who think of science communication as a show, but both reinforce the public's opinion that scientific research is a difficult and mysterious, if not dangerous, activity, and researchers are abnormal people. It is much more difficult to centre science communication on the people responsible for discoveries, and show them as the *creators* they truly are. In other words, communication must convey the message that scientific research is an exciting activity, conducted by interesting people.

The *second negative aspect* of science communication is the *awed admiration of the magnitude—quantity, diversity and complexity—of scientific knowledge*. The latter is generally presented to the public as an immense and complex body of firmly established and interconnected laws, at the periphery of which discoveries are made. Because of that approach, people are awed by the magnitude of what is already known, with the consequence that most think that improving such a formidable construct is nearly impossible. Of course, scientific knowledge is the result of thousands of years of discovery and is, thus, a formidable achievement of the human mind. However, there is no such thing as a large body of theoretical knowledge growing by discoveries made at the periphery. If this was the case, one should master most of the existing knowledge before attempting to improve or enhance it, which would be a nearly impossible task. Scientific knowledge must be shown as it truly is: a construct of human minds, in which all present answers are provisional. Because *all scientific discoveries are transient* (Chapter II), there is no hierarchy within scientific knowledge from certain to provisional, i.e. all scientific knowledge is provisional. It follows that scientific discoveries are within the reach of those who are willing to use creative imagination.

As a *solution to the second problem*, science communicators must explain that the body of scientific knowledge, *although formidable, is transient*. In addition, researchers must avoid at all costs behaving as if they possessed absolute truth, since the answers of science are only plausible, never certain (Chapter II). Hence, science is a continuously evolving product of human minds, and discoveries are within the reach of those who are ready to apply *creative imagination*—intuition, method and pleasure—to scientific questions. As for the first problem of science communication, discussed above, the change in approach I describe here may not be easy. Indeed, the human mind prefers certainty to doubt and permanent conditions to transient situations. Uncertainty and transience make human beings insecure. For example, science teachers know how difficult it is to convince students, even at the university, that scientific knowledge is transient. Once convinced, often reluctantly, many students ask the professor, in desperation: ‘OK, we do not know the answer for sure, but what is your own opinion?’ Teachers must turn such situations around, and use them to explain to students that the transient nature of scientific knowledge is their *window of opportunity*: because of it, any researcher, even—or especially, in some fields—a young one, can contribute to the progress of science. As in the case of science education (see Chapter V), communication must avoid going from general laws to observable phenomena, because laws are part of a complex, abstract construct in which there is apparently little space for creative imagination. In other words, communication must convey the message that science is interesting, and scientific discoveries are made by normal human beings, not weirdoes.

The *third aspect* of science communication that turns people off is the *sometimes exclusive focus on the utilitarian facets of research*. It may be a reflection of our societies that science agencies, science managers and even scientists themselves have been convinced to think that the best way to promote scientific research is to keep repeating to the public that science is useful. Of course, there is no technological progress without scientific discoveries, but the utilitarian aspects of science are not integral to *creative imagination* except when the usefulness of discoveries brings *pleasure* (Chapter III). Indeed, many researchers derive deep satisfaction from the fact that their discoveries are useful to fellow human beings or to society as a whole, e.g. in the fields of medical research or social sciences. Quite often, however, scientists pay lip service to the utilitarian aspect of science, because their managers want it (Fig. 15). The managers insist on the importance for scientists of stressing the usefulness of science because they are

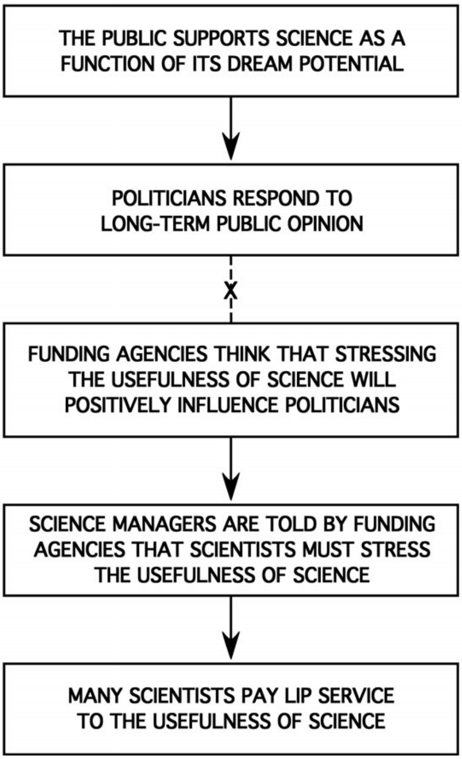


Fig. 15. Funding agencies, science managers and researchers often focus on the utilitarian aspect of science in the hope of influencing politicians; the latter, however, respond to the public, whose interest in science is not primarily driven by its usefulness (see Fig. 14). Hence, the whole construct is based on an illusion (Original)

told so by the funding agencies. The agencies stress the usefulness of science because they think this will positively influence their political masters, given that the latter respond to public opinion. Hence, the whole construct is based on the widely held assumption that the public primarily supports science because it is useful. I have shown in the previous Section that this is not the case. I therefore conclude that the *whole construct is based on an illusion*.

The influence of public opinion is not the only, or even the main, factor that determines the *public funding of research*. For example, governments may decide to put more funds in some fields in response to national emergencies, international competition or lobbying. However, fashions tend to change relatively rapidly. For example, buzzwords in the 1990s included: biotechnology, computer technology, energy, information systems, new materials and pollution. At the beginning of the 2000s, the buzzwords of the previous decade have disappeared, and the new list includes: bioengineering, bioinformatics, encryption, genomics, greenhouse gases mitigation, nanotechnologies, nutraceuticals and sustainable development. The fields in transient fashion may receive high funding for a few years, after which they are replaced by the fields corresponding to new fashions. In contrast, some scientific fields are successful at obtaining sustained public funding over several decades. These fields generally cultivate long-term public support, as explained above for astronomy, marine sciences and medical research.

As a *solution to the third problem*, it is clear that there is no point in focussing science communication exclusively or even primarily on the *utilitarian facets of science*. The best way to attract people to something is to tell them that it is pleasurable, not that it is useful. Publicists understood this a long time ago. Even do-it-yourself is promoted more as a pleasurable pastime than a utilitarian activity! Life is full of utilitarian activities. For adults, these include work, domestic chores and civic duties. For youngsters, the most obvious utilitarian activity is school. All people want to escape the utilitarian aspects of life into private or collective dreams at least a few hours every day. It is a great mistake to think that people would generally devote their free time to utilitarian activities if they can avoid it. Successful science communicators have understood this. As mentioned above, the useful facets of discoveries must be presented in such a way as to sustain pleasure, not replace it. In other words, communication must present science as sustaining our dreams about Nature, and show that many useful facets of discoveries are fascinating.

A recurring topic of discussion among professional scientists worldwide is the present *difficulty of attracting bright youngsters to science*. Part of it may be nostalgia for 'the good old times', when the scientists themselves were students, but there is undoubtedly strong competition for bright young people among the various *knowledge-based disciplines*—technology, professions and scientific research (Chapter I)—and the various *creative activities*—visual arts, music, writing and science (Chapters I and III). Scientific research is at a disadvantage relative to *other knowledge work*, because it requires longer university training than technology or professions and offers work conditions that are often less favourable in terms of salaries, working hours, promotions, and so on. Science is also at a disadvantage relative to *other creative activities*, because creativity is more generally recognised as an integral component of arts and writing than of science. Hence, only few of the bright young people attracted to knowledge work, and only few of those attracted to creative activities would choose scientific research. Not much can be done about the often-unfavourable ratio, in scientific research, of work conditions to training requirements, but this disadvantage could be compensated by the *higher component of creativity* offered by research compared to other types of knowledge work. If the latter, crucial information on scientific creativity were *communicated to youngsters and the public in general,* the attraction of scientific research would be enhanced relative to other knowledge work. In addition, creative young people would know that research offers a *means of personal expression* that is as rich as the arts and writing.

The three conditions of science communication discussed above are *necessary, but perhaps insufficient* to attract bright youngsters to scientific research. Hence, the question of how to best achieve that key objective remains partly unanswered here. Finding answers to this worldwide problem is becoming a priority in many developed societies, because their future progress and well-being and often the pensions of retiring people are threatened by the present low attraction of young people to technological and scientific careers.

Efficient professional communication is essential not only to attract students to scientific research, but also to ensure sustained public funding of the same, because most research funds come directly or indirectly from governments (see Chapter VII), which are controlled by politicians. Politicians generally respond to what they think or know the public wants. This must indeed be the case in democracies, as long as politicians consider long-term public opinion and not short-term fashions. Even private foundations are very sensitive to public opinion. Hence, efficient *communication with the public* is crucial to the long-term funding of research.

VII CONSEQUENCES: FUNDING OF SCIENTIFIC RESEARCH

The main characteristics of the discovery process and creative imagination were discussed in Chapters II and III. These characteristics provide a basis for setting criteria to be used for efficiently *funding scientific research*. Such criteria translate the ideas discussed in the first chapters into the day-to-day practice of research.

My first-hand experience with funding systems in different parts of the world has convinced me that some funding approaches or systems achieve significantly more innovation per money invested than others. In this Chapter, I analyse the *conditions* for efficient funding, and derive from previous Chapters *criteria* for achieving high return on the investment in research.

Funding of Research: Myths and Reality

In some countries and regions, researchers receive significant funds from public sources, corporations and private sponsors (e.g. foundations). In other areas, research funds are for the most part public. In many countries, a large part of the funds controlled by private foundations come from tax exemptions, which is in fact putting public money into private hands. For convenience, *funding sources* are grouped here as follows: private corporations, private groups (foundations, groups of interest, associations, etc.), the public sector (ministries, state agencies and public utilities) and funding agencies; the latter are mostly public. The various sponsors have different approaches to the allocation of research funds.

On the one hand, *private corporations and some private groups* fund research for their own purposes or according to their own objectives. This is also true for most of the *public sector*, which funds research in support of specific mandates. Corporations, the public sector and also sometimes private groups generally use a *top-down approach* to research: they know what they want, and they employ or contract researchers to do it.

On the other hand, researchers can often access funds from *public and private sources* following a *bottom-up* approach: they propose research projects or programmes, which are accepted or not after a review process, by peers or others. According to the funding context, the proposed research may be on topics that primarily interest the *researchers*, or it must be targeted at problems or needs that have been predetermined by the *sponsors* in a top-down manner.

Different combinations of the bottom-up and top-down approaches, by researchers and sponsors, define *three broad types of research* (Fig. 16): curiosity-driven, targeted and applied. In CURIOSITY-DRIVEN RESEARCH, researchers set the work objectives without any constraint from the sponsors. In TARGETED RESEARCH, researchers set the objectives in accordance with problems or needs that have been predetermined by the sponsors; these predetermined problems or needs are the targets. In APPLIED RESEARCH, researchers work at resolving well-identified problems or fulfilling precise needs.

The three types of research are funded differently (Fig. 17), with a wide variability in funding strategies according to countries and regions. The main sponsors of *curiosity-driven research* are public funding agencies and private foundations, but there are cases in which private corporations fund curiosity-driven research as long-term investment. *Targeted research* can be supported to various degrees by all funding sources, i.e. most funding agencies devote at least part of their budgets to research that is targeted on society's problems and needs, which are identified in house or by governments; private foundations often fund research in specific areas only, e.g. wildlife, health; and public utilities may fund curiosity-driven research in areas corresponding to their mandates, e.g. agriculture, energy, marine resources; the latter is sometimes also true of corporations. *Applied research* is mostly funded by private corporations and the public sector, but funding agencies may contribute to applied research, often in partnership with corporations.

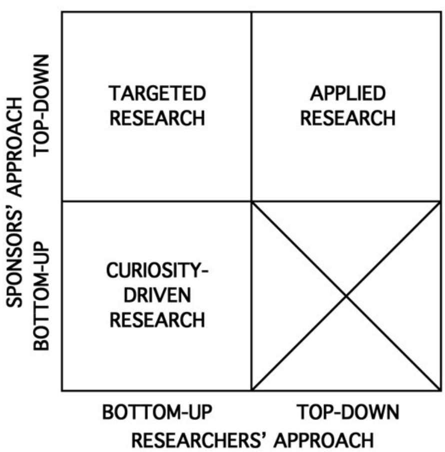


Fig. 16. Combining the bottom-up and top-down approaches used by researchers and sponsors defines three broad types of research: curiosity-driven, targeted and applied (Original)

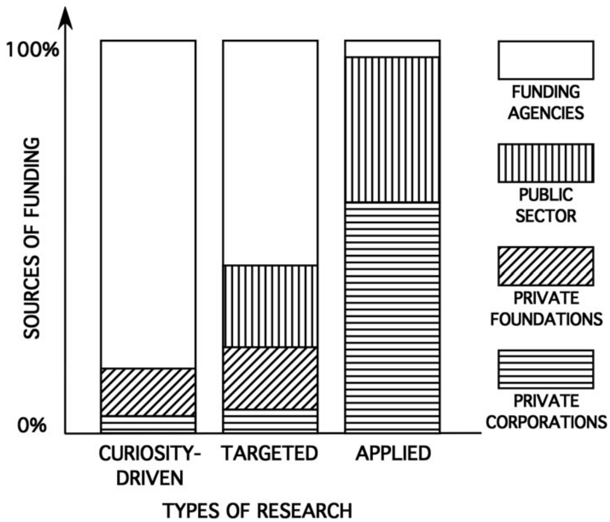


Fig. 17. Contributions of private corporations, private foundations, the public sector and funding agencies to curiosity-driven, targeted and applied research. The proportions vary among countries and regions. This framework is conceptual: it is not based on actual data (Original)

There is a *continuum* between curiosity-driven, targeted and applied research. This has been shown so many times that I always wonder why some countries neglect the first type of research—I refer here to curiosity-driven research—and put most funding in the other two. This is a sure recipe for scientific, economic and social disaster.

The *funding of research* is surrounded by complex conditions: top-down or bottom-up selection of topics or targets, different objectives of the various sponsors, and so on. This often creates confusion as to the *purpose of research*, which is sometimes seen by sponsors as one tool among others to be used for resolving problems or achieving socio-economic objectives. This view sometimes prevails even in Government Departments in charge of research. I re-state here my earlier position that 'Research may be driven by sole curiosity, or targeted at problems or needs, or devoted to the resolution of practical problems, but its aim is always the same: *discovery*' (Chapter II, Section 'The Nature of Scientific Discovery').

The purpose of all scientific research is *discovery*. Because scientific discovery is the *only useful outcome* of research, it should be the *central funding objective* for all sponsors of research: private corporations, private groups, the public sector and funding agencies. I explained in Chapter II

(Section 'The Nature of Scientific Discovery') that the central characteristic of discovery is *novelty*. Because scientific innovation is a condition of socio-economic progress, it is obviously a waste of money and efforts to fund or conduct research that has low or no innovation potential. Indeed, research that repeats what has already been done or finds what is already known has no use for the scientific community, or for the public or private sectors. Curiously enough, the key point of *innovation* is often forgotten when designing *criteria* for funding research. Indeed, these criteria are frequently bureaucratic rules, which have no bearing on discovery and therefore result in squandering the available research funds.

It follows from the above discussion that, for both researchers and sponsors, the only useful outcome of research is *discovery*. Hence, the most crucial aspect of any research proposal is its *innovation potential*. There is a wide range of research proposals: at the 'low' end, we find proposals that have no discovery potential, and at the 'high' end, proposals that have a very high potential. On the one hand, results at the *low end of the spectrum* are, for the most part, known or predictable before conducting the research, e.g. studies repeating previous work. Such projects offer *safety* to sponsors, but a prospect of *socio-economic return* that is null. These proposals are sometimes preferred by bureaucratic systems that are not accountable for the way they dispense funds. On the other hand, the outcome of research at the *high end of the spectrum* is not predictable. Funding proposal for such studies is therefore *risky* for the sponsors, but there is also a possibility of *high socio-economic return*. These proposals may be preferred by entrepreneurs who are accountable for the way they invest funds. Of course, there is no guarantee that the second type of proposals would lead to any discovery, in which case the research investment would be lost. However, losing the investment is only one of two possible outcomes of *high-risk proposals*, the other being high return. The lost investment may also serve the intermediate step of sorting hypotheses, and selecting the most promising one; this may turn out to be a key step towards discovery. In contrast, there is only one outcome *to totally safe projects*: wasting the research funds.

To summarise the previous paragraph, proposals for which results can be predicted have no innovation potential, so their return is very low or null, whereas the outcome of proposals with high innovation potential, which therefore offer the possibility of high return, cannot be predicted (Fig. 18). Most sponsors would like to have the best of both worlds: *high predictability of results and high return in terms of innovation*. This is a myth. Indeed, as on the stock market, there is a direct relationship between possible return

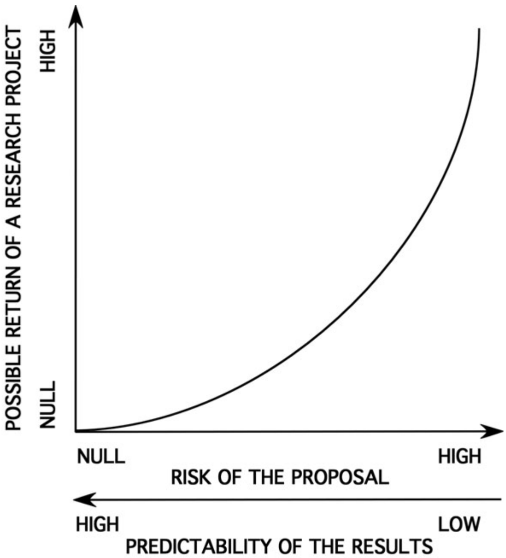


Fig. 18. Possible return from a research project as a function of the risk of the proposal and the predictability of its results. This framework is conceptual: it is not based on actual data (Original)

and risk: a high prospect of return is accompanied by high risk. In contrast, scientists in dynamic research systems would like to have a different best of the same two worlds: *funding with no strings attached, to freely pursue discovery*. This is another myth. Indeed, as for investments, those who fund research require the researchers to lodge some securities as collateral; in other words, the sponsors exert some control on the research activity, e.g. through reviewing and/or reporting. Obviously, some compromise must be found between the two mythical approaches. This compromise varies according to countries, social context, political systems and economic sectors. The harsh reality is that the efficiency of the compromise can be judged from the *amount of innovation*—numbers of papers published in international journals, patents, international honours, etc.—*per money invested in research*; the latter must include the overheads of governments, funding agencies and research institutions, which vary widely among funding systems.

Funding of Research: Efficient Criteria

The efficient funding of research requires that appropriate *criteria* be developed for reviewing proposals. These criteria must be designed primarily to assess the innovation potential of proposals, for any research: one must not only *discover sound research*, but also *search for discovery potential*. It is relatively easy to discover proposals describing sound research, but it is much more difficult to search for proposals potentially leading to discovery. Hence, the primary criteria must not only ensure that *proposals offering sound research* will be discovered, but also guide the search for the subset of *proposals with discovery potential*. Additional, secondary criteria could be used to assess the adequacy of proposals to the different objectives of *sponsors* (see the previous Section). The primary criteria would be the same for all types of research, whereas the secondary criteria would be different for curiosity-driven, targeted and applied research.

I explained in Chapter II (Fig. 5) that there are four components in *discovery*: a pertinent question, the ripeness of time, the formulation of an intuitive answer and the scientific method. I also explained in Chapter III (Table 5) that discovery is based on *creative imagination,* which combines intuition, craftsmanship or methodology and pleasure. Hence, efficient criteria must primarily assess research proposals by reference to the above *five components of the discovery process*: pertinence of the question, ripeness of time, intuition, scientific method and pleasure (Fig. 9). These criteria could be formulated and implemented as follows (Table 9).

Primary criterion 1: The proposal addresses a pertinent question. Most research proposals review the literature with the purpose of showing that they address a pertinent question. Pertinence must specifically refer to the proposal's *discovery potential*. Hence, the key aspect here is the novelty of the question: reviewers must be convinced that the question has not been answered already in a satisfactory manner, or if some answer already exists, that the proposal could lead to an alternative, original answer. A second important aspect of pertinence concerns the application of the *scientific method* to the eventual answer: the question, which is often formulated as a null hypothesis (see Chapter II), must not be such that it leads to a single possible answer. In other words, the question or hypothesis must be such that it could be tested or falsified (see Chapter II).

Primary criterion 2: The time is ripe for discovery. The proposal must show that the likelihood of answering the question is higher than before because, for example, of some new discovery, new technique and/or new intellectual or social environment. There must a clear link between the question at hand and the new factor(s) invoked in support of funding.

Primary criterion 3: The researcher is able to produce intuitive answers. The only way to judge the intuition ability of a researcher, other than some psychological test that does not exist and that nobody would want to use

Table 9. Primary and secondary criteria for assessing research proposals

Criterion	Implementation
Primary	
Proposal addresses a pertinent question	Question has not already been answered, and could be falsified
Time is ripe for discovery	Likelihood of answering the question is higher than before
Researcher is capable of intuition	Track record, and reading publications of researcher (or citation indices, to be used with care)
Researcher masters the scientific method	Proposed work could answer the question
Proposal has pleasure components	Degree of enthusiasm of researcher
Secondary	
Curiosity-driven research	Degree of responsibility and realism of researcher
Targeted research	Appropriateness of the proposed curiosity-driven research to the target(s) specified
Applied research	Likelihood of resolving the problem(s) or fulfilling the need(s)

even if it did, is his/her *track record*. Indeed, the likelihood that a researcher will successfully use intuition to approach the question at hand is directly proportional to the *innovation ability* s/he demonstrated in the past. Examining the track record is the only approach available for reviewers to determine if a researcher has the potential for using intuition to answer scientific questions, even if one can never be sure that the researcher will be successful in doing so for the question at hand. Assessing previous discoveries by reviewers generally requires that they actually read a few papers or patents selected by the proponent in view of establishing his/her innovation ability (see the discussion of communication criteria in the next Section). An alternative approach to reading papers of the proponent is to use citation indices (e.g. the h-index), but one must be careful with indices as citations conventions differ widely among scientific fields.

Primary criterion 4: The researcher masters the scientific method. All research proposals have a methodological section, in which the proponent explains how s/he intends to conduct the research. The reviewers must assess not only the specific methodology, but also two crucial aspects of the proposal: the likelihood that the proposed work could answer the question at hand, and the possibility of falsifying the hypothesis. Hence, reviewers must examine *possible falsification* within the context of both the initial question (*Primary criterion 1*) and the proposed methodology (*Primary criterion 4*).

Primary criterion 5: The proposal has pleasure components. Pleasure, especially in scientific research, is valued in some cultures, whereas it must be hidden in others. Hence, according to location, it may be quite easy or very difficult for reviewers to assess if a proposal includes pleasure components. Reviewers can generally determine the *degree of enthusiasm* of proponents when meeting them face to face, e.g. during site reviews, even if the degree of interactions allowed in such circumstances varies widely according to tradition and individuals. It is often much more difficult to assess enthusiasm from written proposals. However, without initial enthusiasm, it is unlikely that researchers will achieve discovery. In the absence of specific evidence concerning pleasure, the researchers' *track record* (see *Primary criterion 3*) can be used as indirect evidence of pleasure: those who were successful at discovery in the past likely enjoy doing research.

The *secondary criteria* vary widely according to the objectives of sponsors. Hence, it would not be realistic or even useful to discuss such criteria here, except in very general terms. I therefore propose only one general secondary criterion for each type of research: curiosity-driven, targeted and applied.

General secondary criterion for curiosity-driven research. In curiosity-driven research, researchers are invited by sponsors to propose research on any topic they wish. Hence, the array of proposed topics is potentially very wide, with little connection between them. Given that situation, the proponent must convince the sponsors that s/he is a *responsible and realistic researcher*. The approach for doing so requires the researcher to set the proposal within the context of her/his *long-term objectives*, which calls for a discussion of the track record and possible course of future research. The proposed criterion here is therefore the consistency of the proposed study with the researcher's long-term objectives. Of course, changes in research direction are not only acceptable, but also normal during the course of one's career, but they must be justified in a convincing manner.

General secondary criterion for targeted research. The general criterion here is obviously the *appropriateness* of the proposed curiosity-driven research to the target(s) specified by the sponsor. However, embarking into targeted research only because funds are temporarily available could be a major mistake for a researcher who would thus compromise his/her long-term objectives for accessing short-term funds. Conversely, sponsors should not wish to temporarily attract to their targets lukewarm or reluctant researchers. Hence, for both researchers and sponsors, it is important to assess the consistency of the targeted project with the researcher's *long-term objectives* (see previous paragraph).

General secondary criterion for applied research. The general criterion here must be the likelihood that the proposal will *resolve the problem(s) or fulfil the need(s)* specified by the sponsor. As for the previous criterion, consideration of the researcher's *long-term objectives* may be important for applied research, especially when the project is very demanding on the researcher's time.

No proposal ever fully satisfies all criteria. Hence, the decision of funding a proposal or not is based on judging whether the lower satisfaction of some criteria jeopardises the likelihood of discovery. This would be the case if any of the criteria was not met at all, or if several criteria were poorly satisfied. Even if the above criteria are not generally stated as such, or under the same form, the funding systems that achieve high innovation return per money invested in research do apply such criteria, whereas those with poor return on investment apply only part, or even none of them.

Assessing the Quality of Research: Communication Criteria

Assessing the *quality of past research* is generally included in the review of proposals. For example, it was recommended in the previous Section to examine the track record of researchers by reference to *Primary criteria 3* and *5* for reviewing proposals. The quality of research can also be examined independently from proposal reviews, e.g. for promotions. I discuss here *criteria for assessing the quality of research*, based on communication (Table 10). These criteria are not exhaustive, because the output of research includes not only communication, but also patents, and so on.

Professional communication among scientists—written texts, oral presentations and posters—is the mandatory result of research (see also Chapter VIII, Section 'The Pleasure of Communication'. (1) Discoveries, and more generally, results of research, that are not communicated do not really exist; in other words, *communication* is an intrinsic component of research. (2) Scientific information that has no elements of novelty is useless; in other words, *novelty* is also intrinsic to scientific information. (3) Written texts that are not read, oral presentations that are listened to with only half an ear and posters that do not attract visitors have no purpose; in other words, *effi-*

Table 10. Communication-based criteria for assessing the quality of research; the four criteria are nested (Fig. 19)

Criterion	Implementation
Scientific information is communicated	Existence of written texts, oral presentations and posters
Information has elements of novelty	Reading a few publications of researcher; citation indices (to be used with care)
Communication is efficient	Reading publications of researcher (if not, combine with next criterion)
Community uses the information	Citations, and other evidence of influence on the field

ciency is a central characteristic of scientific communication. (4) Communicated information that is not used by other researchers is as good as nonexistent; in other words, real efficiency in scientific communication requires that the *community use* the new information.

The above discussion provides four *communication criteria* for assessing the quality of research. These are: (1) scientific information is communicated, (2) the information has elements of novelty, (3) communication is efficient and (4) the scientific community actually uses the new information. As shown in Fig. 19, these criteria are nested: each criterion must be satisfied before going to the next. Communications that do not meet the four criteria do not belong to research. In other words, people who do not communicate their results, or whose communications lack novelty, or are inefficient or useless to the community are not really doing research. Nobody has to do research if s/he does not wish, but using research money without satisfying the four communication criteria is unacceptable.

The *four communication criteria* are often used to assess the quality of research. This is done in various ways according to countries and research sponsors.

Communication criterion 1: Scientific information is communicated. Actual communication can be determined from lists of written texts, oral presentations and posters. The effort invested in communication varies with media, e.g. it is more demanding to write a book than a paper, or to write a paper than prepare a meeting presentation or a research seminar. The choice of communication media may vary with circumstances, and change as the career develops.

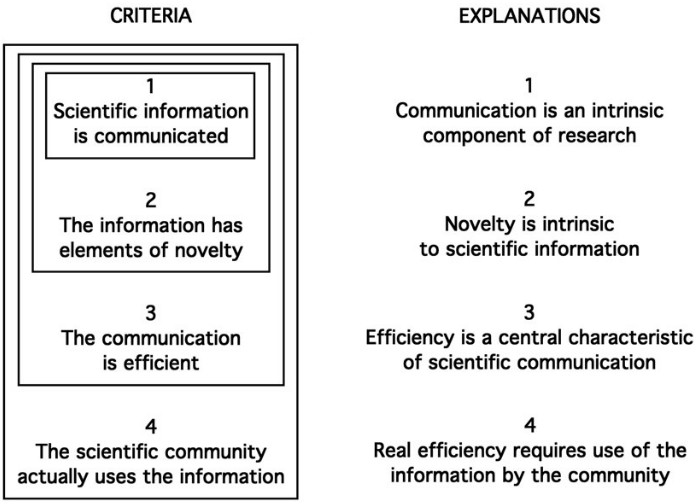


Fig. 19. Four communication criteria for assessing the quality of research, and corresponding explanations. The criteria are nested, i.e. each criterion must be satisfied before going to the next (Original)

Communication criterion 2: The information has elements of novelty. Novelty is often judged from reading a small number of papers submitted by the author, as practiced in some funding systems. This provides a benefit to reviewers, who thus often read interesting papers they would have missed otherwise. As explained above (*Primary criterion 3,* in the previous Section), an alternative to reading papers is to use, with care, citation indices. This criterion applies not only to primary publications but also to books. Reviewers generally do not have enough time to read the books written by people they assess. However, they could generally determine the novelty of books from their tables of contents, reviews published in professional journals, citations, and so on.

Communication criterion 3: The communication is efficient. Reading papers selected by the researcher allows reviewers not only to judge the novelty of the information (see *Communication criterion 2*), but also estimate the efficiency of the communication. In cases where papers are not read, this criterion can be combined with *Communication criterion 4*.

Communication criterion 4: The scientific community actually uses the information. This use can be quantified from the citations of published works (see *Primary criterion 3*). Reviewed researchers can provide additional, qualitative evidence on the influence of their work on the development of their scientific field.

During the discussion of *Communication criterion 2*, I mentioned the writing of *books*. Some funding agencies and research directors consider books inferior to journal papers. I strongly criticise that ill-informed attitude, and the resulting career penalties imposed on researchers who dedicate great efforts to *synthesise* into textbooks the increasingly scattered scientific information. As a consequence, reputed researchers in countries in which such policies exist seldom write textbooks, which in turn lessens the influence of these countries on the development of international research. The writing of textbooks sometimes leads to discoveries but, more importantly, textbook syntheses raise the discovery potential of the scientific community. *Imaginative textbooks* stimulate the curiosity and creativity of undergraduate and graduate students. *High-level syntheses* provide both general ideas and specialised information that facilitate discovery to graduate students and professional researchers. This is especially important because, in the midst of an information explosion, scientists have over-emphasized production and neglected digestion and foresight; hence the need for syntheses.

VIII DEVELOPING AND USING CREATIVE SKILLS

Creative imagination was defined in Chapter III as the combination of intuition, craftsmanship or methodology and pleasure that is used for the production of original works. I stated at the beginning of Chapter V that, even if intuition is largely innate, it can be cultivated or repressed, methods or crafts must be learned and pleasure is a healthy response to imaginative works.

I write the present Chapter for the benefit of university students and scientific researchers who show intuition, are conversant with the scientific method and enjoy research. My aim here is to provide these readers with practical suggestions for developing and using their creative skills in research.

Heuristics

Professor Peter Jumars (University of Maine at Orono, USA) considers *solving scientific problems* a creative enterprise, and offers very useful suggestions for this activity. I recommend reading Prof. Jumars' suggestion. On his Home Page, he stresses that a small number only of books or papers actually discuss how hypotheses are generated (http://www.umaine.edu/marine/people/sites/pjumars/science/create.html). He lists a few interesting references on the subject, most in the field of mathematics and traceable to the seminal work of Pólya (1988). Some of the references provide generalisations beyond mathematical applications. Professor Jumars suggests several approaches to the *solution of scientific problems*, which include drawing a diagram, writing an explicit equation, reformulating the problem, examining special cases, simplifying or generalising the problem, constructing an analogous problem and exploiting related problems in neighbouring scientific fields. When using such approaches has borne tentative solutions, he suggests comparing these with independent data, and checking if they predict anything that has not been observed but could be. Professor Jumars remarks that the approaches he recommends provide us with the ability not only to *answer posed problems*, but also to *pose answerable problems*, i.e. to devise testable alternative hypotheses. In other words, the learning, doing and teaching of *creative problem solving* is probably the best means toward learning, doing and teaching *hypothesis generation*.

The above suggestions belong to the general category of 'heuristic' techniques. A HEURISTIC (noun) is a technique that provides a way of thinking about a problem, which follows the paths most likely to lead to the goal, leaving less promising avenues unexplored. Most heuristics are *pragmatic rules of thumb*; there is a reasonable chance that they will help solve the problem, but they can sometimes have the opposite result. There is even the general heuristic of ‘dropping the heuristic’, in order to break the grip of an irrelevant rule, and also ‘heuristics for changing heuristics’, which are required in some situations.

The *visual or spatial representation of observations* is associated with heuristics. This is because the visual system has evolved to notice spatial relations; for example, the gaps in the periodic table prompted chemists to search for new elements (see Chapter IV, Section ‘Theoretical Science and Scientific Theories’). However, any representation can block creativity as well as aiding it, i.e. people can be trapped not only by a frozen heuristic, but also by a frozen representation.

It has been stated in the literature that the prerequisite of originality is the *art of forgetting*, at the proper moment, what we know, i.e. without the ability of forgetting, the mind remains cluttered up with ready-made answers, and never finds occasion to ask the proper questions. In that sense, under propitious conditions, inexperience can be an asset, as it may lead to asking questions that nobody has asked before, or seeing a problem where nobody saw one before. This “naivety” is a key characteristic of true creators.

Published examples of heuristics drawn from the ecological literature are provided in the full, printed version of this book. These illustrate how some of the heuristics recommended by Prof. Jumars produced new ideas, and even led to discoveries. Follow a few comments on two heuristics, i.e. writing an explicit equation and simplifying the problem.

Writing an explicit equation. Some researchers are reluctant to write equations because they do not like mathematics. However, writing an equation is one of the best ways to clarify one’s ideas about a problem. The very fact of writing equations compels us to move from vague ideas to *rigorous statements*. It can therefore pave the way for a solution, and possibly discovery. In addition, equations are amenable to *dimensional analysis* (see below), which provides a very powerful tool for checking the thoroughness of one’s approach, and sometimes developing original solutions.

Simplifying the problem. It is often useful to simplify the problem at hand. Difficulties arise because specialists are often reluctant to sacrifice details to which they have devoted much effort, even when the purpose of the exercise is to gain a better understanding of the *key elements* of the problem. However, putting aside details does not necessarily mean discarding them. It could mean developing instead a more powerful model than existing ones, in which the available information would be better used.

Dimensional Analysis, Theoretical Analysis, Development of Concepts and Models

In addition to heuristics discussed in the previous Section, research problems can be sorted out with the tools of dimensional analysis, or approached from a theoretical angle. Using such methods often favours the development of original concepts and models. In this Section, I briefly examine in turn *dimensional analysis*, *theoretical analysis* and the *development of concepts and models*. A more detailed presentation of these topics, including examples drawn from the ecological literature, can be found in the full, printed version of this book.

Dimensional Analysis

It was mentioned in the previous Section that *dimensional analysis* is a way of both solving problems and assessing the value of tentative solutions. In the following paragraphs, I summarise some key aspects of dimensional analysis.

Legendre and Legendre (1998, their Chapter 3) provide an introduction to dimensional analysis, and illustrate that powerful approach with applications to ecology. Dimensional analysis concerns the *general forms of equations* that describe natural phenomena. All fields of natural sciences rest on *abstract entities* such as mass, length, time, temperature, speed, acceleration, radioactivity, concentration or energy. These entities, which can be measured, are called *quantities*. The *dimensions* of quantities are represented by symbols in square brackets, e.g. the dimension of mass is [M]. The *International System of Units* (SI, Table 11) is based on seven quantities, to which are associated seven *base units*, and it also recognizes two *supplementary units*, which are dimensionless (see below). All other units, called *derived units*, are combinations of base and supplementary units; their dimensions are *products*, *powers* or *combinations of powers* of the dimensions of fundamental units, e.g. the dimension of acceleration ($[LT^{-2}]$) combines the dimension of length ([L]) and the –2 power of the dimension of time ($[T^{-2}]$).

Although most constants and variables have dimensions, and are thus called *dimensional*, some are dimensionless. Examples of *dimensionless constants* are: π, the Napierian base *e* (natural logarithms) and all exponents.

Table 11. The seven base and two supplementary units of the *International System of Units* (Sl). Note that: (1) unit names are written with small letters only (e.g. ampere, kelvin, pascal), the sole exception being the degree Celsius; (2) unit symbols are written with small letters only, except the symbols of units that are surnames, of which the first letter is a capital (e.g. A, K, Pa); (3) unit symbols are not abbreviations, hence they are never followed by a point; (4) the unit symbol of the second is *not* sec (Modified from Table 3.1 of Legendre and Legendre [1998])

Fundamental quantity	Dimension symbol	Base unit	Unit symbol
Mass	[M]	kilogram	kg
Length	[L]	metre[a]	m
Time	[T]	second	s
Electric current	[I]	ampere	A
Thermodynamic temperature	[θ]	kelvin[b]	K
Amount of substance	[N]	mole	mol
Luminous intensity	[J]	candela	cd
Planar angles		radian	rad
Solid angles		steradian	sr

The latter property provides a tool for checking theoretical developments: any such development leading to an equation with a dimensional exponent has something wrong. Examples of *dimensionless variables* are: angles, relative density (i.e. ratio of two densities) and dimensionless products. The latter combine several quantities in new entities that have no dimension. For example, the Reynolds number (*Re*), which is often used in aquatic and atmospheric sciences, combines the relative velocity (*V*) and linear dimension (*L*) of the object under study with the density (ρ) and dynamic viscosity (η) of the surrounding medium in the following product:

$$Re = VL\rho/\eta$$

The dimensions of *V*, *L*, ρ and η are $[LT^{-1}]$, [L], $[ML^{-3}]$ and $[ML^{-1}\,T^{-1}]$, respectively. Hence, the resulting product is dimensionless:

$$([LT^{-1}]\,[L]\,[ML^{-3}])/[ML^{-1}\,T^{-1}] = [1]$$

Dimensionless products play a central role in dimensional analysis. They are used in many applications, including the scaling of results from small-scale models to full-scale prototypes, e.g. in hydraulic flumes.

One fundamental rule of science is that all *equations of theoretical nature* must be dimensionally homogeneous. This is because additions and subtractions can only be performed on quantities that have the same dimensions.

Hence, any equation of the general form $a + b + c + \ldots = g + h + \ldots$ is *dimensionally homogeneous* if and only if all quantities $a, b, c, \ldots g, h, \ldots$ have the *same dimensions*. This property does not necessarily apply to empirical equations. The principle of *dimensional homogeneity* is very useful in itself. For example, checking dimensional homogeneity is essential when writing and developing equations: equations must be dimensionally homogeneous at each stage of development. When they are not, this indicates that some necessary variable was not included at the start, or dimensional variables or constants were lost during the development. Another application of dimensional homogeneity is the resolution of problems by transforming all their components to the same dimension. The most powerful way to achieve and maintain dimensional homogeneity is to make all variables dimensionless.

The greatest achievement of dimensional analysis probably lies in its ability to find the general form (i.e. the equation) of the relationship among any set of variables. The requirement to do so is that all pertinent dimensional variables and constants be identified at the start of the study, which is not always easy or even possible. Dimensional analysis does not in itself lead to discovery, but it provides tools that put or keep the minds of researchers on the *discovery track*.

Theoretical Analysis

Another approach to the solution of research problems is to consider them from a theoretical viewpoint. *Theoretical analysis* often provides original solutions to old problems. In the following paragraphs, I summarise some key aspects of theoretical analysis.

Contrary to dimensional analysis, above, the expression 'theoretical analysis' does not refer to a well-organised scientific method. I use that expression here to stress the usefulness of *approaching several scientific problems theoretically* before analysing the data, instead of charging blindly in the data set, or simply trying to use existing elements of theory. This may be especially important in those scientific fields that are light in theory (see Chapter IV). In several instances, analysing the data based on the existing theoretical background would lead to adequate, but not original conclusions, while approaching the same data from a novel theoretical viewpoint could lead to discovery.

'Theoretical analysis' includes five steps: (1) summarise the existing knowledge using existing conceptual models; (2) revisit the topic by applying simple equations to existing data; (3) combine into a single, new conceptual model the information from (1) and (2); (4) develop equations representing the new conceptual model; and (5) apply the new equations to existing data, and compare the results with those from other, more classical approaches. The five steps are illustrated by examples in the full, printed version of this book.

Development of Concepts and Models

The above discussion on dimensional and theoretical analyses illustrates how concepts and models develop (Fig. 20). The development of concepts and models can proceed as follows: (1) generation of new concepts by combining available information with theory, (2) building of a conceptual model to structure the new concepts, (3) translation of the model into equations, (4) model implementation, using data that are different from those at the origin of the model or used for its development. Each phase needs creativity. At the end of the process, if the scientific community likes a proposed model, it may use it for various applications.

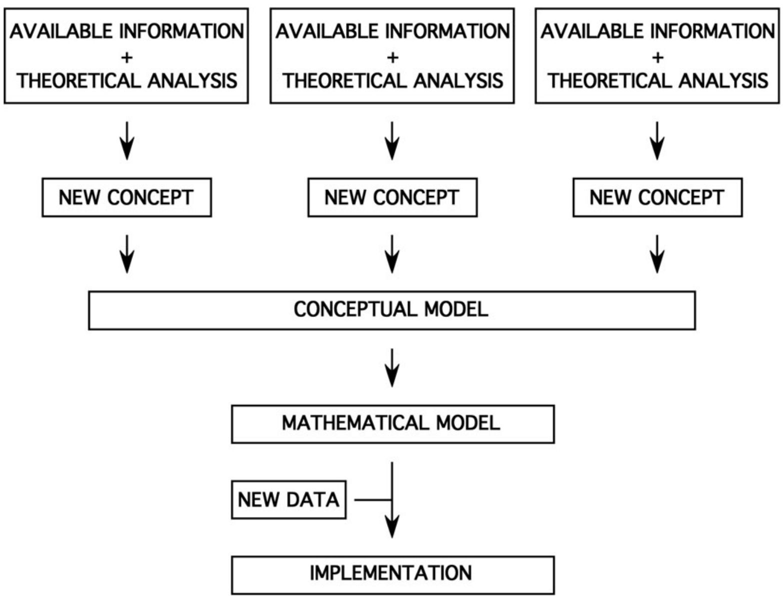


Fig. 20. Development of concepts (by combining available information with theory) and models (to structure the new concepts). The conceptual model is formulated into equations, and the resulting mathematical model is implemented using new data (Original)

The approach summarised in the previous paragraph is becoming more and more popular in environmental research, because of the rapidly increasing availability of *large databases* about the marine, continental and atmospheric environments. These databases are the products of past and ongoing collaborative projects (see Chapter X). The 'mining' and re-analysis of historical and recent data that exist in the literature and in databases, within the framework of syntheses, leads to both evolving descriptions of the Earth's changing environment and new models of its functioning. The data available in the literature and databases were generally collected for answering specific questions raised by individual researchers or within the framework of collaborative projects. Information that goes beyond the aims of the original studies, in terms of geographical coverage and scientific breadth, is extracted by mining and synthesising data coming from many, often disconnected projects. This is generally not a trivial exercise.

The *successive steps of synthesis studies* described in the previous paragraph frequently go as follows: a conceptual model is developed, followed by translation of the model into equations; based on the model, data are mined from the literature and databases; the resulting data must be validated and standardised, which often requires the intervention of specialists of the relevant field(s); the data are exploited within the framework of the model; this leads to regional or planetary generalisations of the model, or its partial or total reformulation (Fig. 21).

Researchers in some countries are reluctant to *analyse and synthesise existing data* because they fear, sometimes rightly, that this type of research will not be recognised by their funding agencies as equal to research based on the *production of new data*. This has at least three negative consequences. (1) Publications resulting from syntheses often exert *long-lasting influence* on the international scene. Hence, countries that contribute little to the synthesis work miss a major opportunity for influencing the development of international science. (2) Databases include records acquired at great cost by many countries, and synthesis projects *extract new information* from the existing data. Countries that are absent from the synthesis phase of international research miss a significant part of the discovery return on their own funding investments. (3) Without synthesising the results from previous projects, proposals for new projects are often *repetitive of past work*. Hence, countries lacking synthesis activities may waste research funds on repetitive research.

Here are a few recommendations, aimed at funding agencies, to help them overcome the problem discussed in the previous paragraph. Concern-

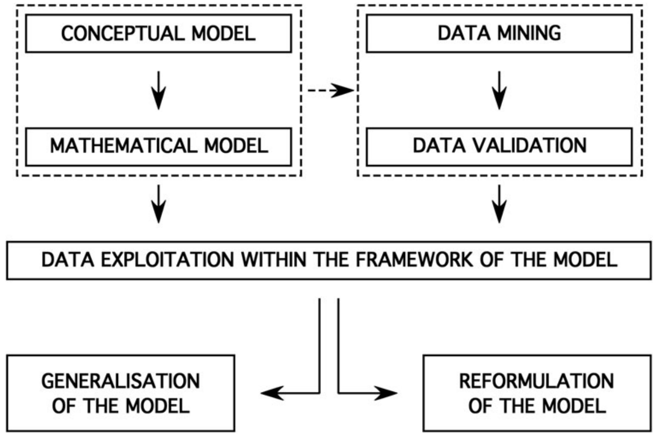


Fig. 21. Steps in synthesis studies: a conceptual model is developed, followed by translation of the model into equations; based on the model, data are mined from the literature and databases; after being validated the data are exploited within the framework of the model; this leads to generalisations of the model, or its partial or total reformulation (Original)

ing *long-term orientation*, the agencies should develop policies of active participation in international synthesis activities, and fund data analysis and synthesis. Concerning the *evaluation of research quality*, they should recognise first-class syntheses as positive indicators of overall quality. Concerning the *funding of research proposals*, they should make it clear that a funding criterion for all large-scale projects is a phase of synthesis, which could start at the very beginning of the project or even precede it, i.e. synthesising existing data, and must continue after the publication of primary analyses.

Writing in Support of Creative Imagination

People who are not involved in professional creation may think that the *production of original works* follows from well-developed concepts. For example, people may imagine that writers know the unfolding of the plot before writing a book, or that artists have a relatively clear vision of the final work when they begin chiselling a block of marble or writing down music, or that researchers know what the main ideas of the manuscript will be when they start writing. It may come as a surprise to many people that it is not always, or perhaps even generally, the case. Indeed, even if most creators proceed from a seed idea, which may sometimes be quite precise, the final work is often *very different from the initial concept*.

For example, the final work of a painter may have no resemblance with its starting point, because of unexpected interactions between the painter's imagination and his/her unfolding work. Similarly, the characters in a book may acquire a life of their own as the writing of the book progresses. In the same way, Inuit (Eskimo) sculptors in the Canadian Arctic believe that a spirit or a form inhabits the stone to be carved, their role as the sculptor being to bring it out. The latter describes very well the feeling of creators that *their works somehow drive them*.

These examples stress the fact that the three components of *creative imagination*—intuition, craftsmanship and pleasure (see Chapter III)—not only interact continuously with one another during the creation of an original work, but also that the *work itself* becomes a term of the interaction (Fig. 22). As the work progresses, the intensities of the interactions among the four terms change continually. This process is called INSPIRATION, which is defined as the *creative drive* of artists, writers and researchers.

Inspiration can occur when writing a scientific paper. I have experienced it many times, and several colleagues have told me they also have. Suddenly, in mid-sentence, a *novel idea* seems to flow from the pen or to drive the fingers hitting the keyboard, in a totally unexpected manner. The idea seems to *emerge from the text itself*, and the new angle it provides sometimes becomes the main thrust of the paper. When this occurs, the Results and the previously written Discussion are seen in a *new and more original light* than before; this often leads to the reorganisation of the whole manuscript, reanalysis of the data and rewriting part of the text. I used purposely the word 'emerge' a few lines above, both as the image of an *idea rising from the text*, and by reference to the *theory of systems*. As explained at the end of Chapter

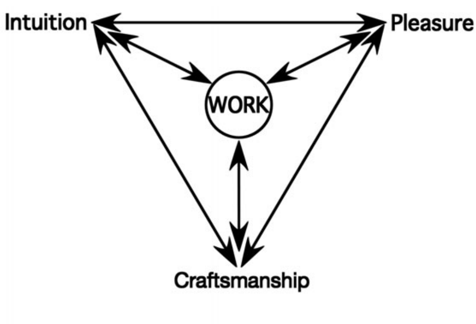


Fig. 22. During the creation of an original work, the three components of creative imagination — intuition, craftsmanship and pleasure (Fig. 9) — continuously interact with one another and with the work; this process is called *inspiration* (Original)

II (Section 'Mathematics, Reductionism and Holism'), new properties generally appear, i.e. emerge, as one goes from a low level of organisation to a higher one; the emergent properties cannot be predicted solely from those at lower levels of organisation. Within that context, the act of writing could be seen as a *progression towards higher organisation of the ideas*, which sometimes favours the *emergence of original thoughts* that could not be predicted from those at lower levels of discussion.

The role of *inspiration* in the writing of scientific texts is not something that researchers generally admit in public, discuss openly with colleagues, or even recognise themselves. One reason is that inspiration seems to bring into the process of scientific research an *irrational component*. Another reason is that inspiration seems to operate *outside the accepted framework* of scientific research, described in Chapter II (Section 'The Scientific Method'). However, it must be remembered that discovery is an iterative process, which involves the two phases illustrated in Fig. 7. I interpret the phenomenon of inspiration as a rapid *alternation between the synthetic and analytic phases of research*, once or several times. Because each *iteration* involves the rejection of the existing null hypothesis and the formulation of a new one (together with its alternative hypothesis), the result of the process may be quite different from the hypothesis considered at the onset of the study. The end product of inspiration may not be the end of the story, as reviewers may disagree with the interpretations of authors, thus forcing an additional iteration. Because the occurrence of inspiration cannot be predicted, it cannot be taken into account when reviewing research proposals. However, reviewers must make sure that researchers would be able to take advantage of inspiration if it took place. This can be done by applying, during the review process, *Primary criteria 3* to *5* described in Chapter VII (Section 'Funding of Research: Efficient Criteria').

It follows from the previous paragraph that *inspiration* is neither irrational nor outside the accepted framework of scientific research. Hence, there is no reason why researchers should hide it. On the contrary, the concealment by researchers of the role of inspiration in the production of scientific works reinforces the idea, widely held in the public and by young people, that research is not a creative activity (see Chapter VI).

My interpretation of inspiration in scientific writing is that *interactions between the four terms* described above—the researcher's intuition, the act of writing (i.e. craftsmanship), the pleasure of interpreting the data and of developing theoretical explanations and the unfolding of the discussion (Fig. 22)—create conditions required for the *emergence of new ideas*. An alternative, simpler interpretation could be that the conditions favourable to the emergence of a novel idea are the result of the *extreme focusing of the mind* engendered by the act of writing. Whatever the explanation, *writing is central to creative imagination* in science.

I therefore think that one of the most efficient ways to develop *original scientific ideas* is to *write*. The sooner one starts writing during the course of a study, the better. Writing as early as possible goes against a natural tendency of researchers to consider the data at great length before starting to write, with the hope that the data would somehow generate new ideas. Of course, analysing the data contributes to providing ideas (see the previous Section), but I am convinced that the most original or interesting ideas in a large proportion of studies appear at the time of writing.

Many researchers *do not like writing*, and therefore delay or even avoid it as much as they can. It is natural to dislike activities one does not master. As is the case for any other craft, becoming good at writing requires actually *doing it*. Hence, the more we write, the better we become at it, and thus the more we *enjoy it*. The difficulty is in launching the positive feedback. I would say, as a half-joke, that the toughest phase for anybody is writing his/her first two hundred thousand words, which is equivalent to ca. thirty substantial scientific papers. We must put these painfully written first two hundred thousand words behind us as soon as possible. This is why it is so important to write as much as possible during pre-university years. All words written before university—essays in literature or philosophy, poetry, articles in school newspapers, etc.—bring youngsters closer to *breaking the two hundred thousand words wall*. Conversely, every word short of two hundred thousands that was not written before university years must be written then, which is hard. The situation is even worse when Master's and Doctoral theses are part of the first two hundred thousand words, not to mention colleagues who are unfortunate enough to start their professional career with a *deficit of written words*. Education systems and schoolteachers who do not train youngsters to write properly, and do not offer them the opportunity to actually write and be corrected, are unforgivable. In cases in which writing skills have not been mastered before reaching the university, the latter must provide undergraduates with remedial teaching early during their curriculum.

Writing can be a *great joy*. When this skill leads to discovery, it provides *extraordinary pleasure*. The individual pleasure of discovery is enhanced by peer recognition, and by reaching readers all over the World. I personally never tire of the pleasure of writing scientific texts, of having manuscripts accepted for publication and of hearing colleagues sometimes tell me 'I enjoyed reading that paper of yours!' I wish to offer here a few suggestions to enhance the overall pleasure in the scientific community: as an author, start writing early during the course of projects; as a reviewer, be fair and open-minded; as a fellow human being, tell colleagues whenever appropriate: 'I enjoyed reading that paper of yours!'

The Pleasure of Communication

I explained in Chapter VII (Section 'Assessing the Quality of Research: Communication Criteria') that *professional communication among scientists*—written texts, oral presentations and posters—is a key component of research, and is therefore a duty for researchers. I will show in this Section that communication is not only a duty, but can also be a pleasure.

The purpose of scientific communication is to convey and share specialised information. Researchers can distinguish at least *three levels of communication*. (1) *Communication among researchers*. Communication within the international research community is now largely in English. However, there are people, usually not researchers, in some non-English speaking countries who do not agree with the omnipresence of English, and would prefer that researchers use their respective national languages on the international scene for communicating among themselves. This is a useless debate on a non-existing problem, because that level of communication is limited to specialists, who are spread thinly over the World and speak a wide variety of national languages. All of them know English. Of course, regional communications among researchers sharing a common language other than English often take place in that language. (2) *Communication from researchers to university students*. Science textbooks with a wide readership, e.g. undergraduates, can be usefully written in any major language, whereas those with a small, specialised readership cannot be published economically in languages other than English. (3) *Communication from researchers to the public*. In order to be effective, communications directed to the general public must be done in vernacular languages. The present Section does not deal with the latter, which was addressed in Chapter VI, Section 'Communicating Science to Youngsters and the Public'.

The *amount of information transmitted* depends mostly on two factors: the amount of information in the message and the efficiency of communication. As conceptually illustrated in Fig. 23, the amount of information transmitted is low when either factor is low, and it is maximum only when the two factors are maximum. Hence, researchers must not only make sure that they

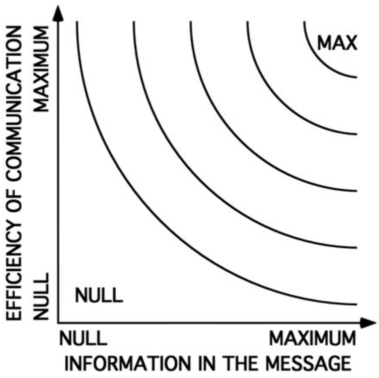


Fig. 23. Amount of information transmitted, from null (lower left) to maximum (upper right) as a function of the amount of information in the message and the efficiency of communication. This framework is conceptual: it is not based on actual data (Original)

communicate high-quality information, but also pay much attention to the way they communicate, which largely determines the efficiency of communication. This is not always the case, unfortunately. Indeed, some researchers do not care much about the quality of their communications, based on the wrong idea that colleagues will spend time searching for treasures hidden in the muddle, whereas nowadays most people barely have time to look at treasures even in jewel boxes. I propose to resolve that problem by putting pleasure at the heart of communication.

Both the author and the colleagues with whom s/he is communicating can *derive pleasure from communication*. The author derives pleasure from positive reactions of colleagues to her/his work. The colleagues receiving the information discover with pleasure the new, original work. This shared pleasure depends to a large extent on the *quality of the communication*. Poorly presented information, even of high quality, not only lowers the efficiency of communication but seldom causes enthusiasm in colleagues; this lack of enthusiasm may, in turn, be very disappointing for the author. Conversely, the careful preparation of flawless work not only enhances the efficiency of communication, but also provides pleasure to the author; there is pleasure both in the anticipation of a positive reaction from colleagues, and when the positive reaction actually occurs. Similarly, colleagues enjoy original works that are well communicated, thus ensuring efficient communication. For example, the researchers who generally attract large crowds during scientific meetings are those who present original works very well; this is pleasurable for them and their colleagues. These ideas will be further developed in the next paragraphs for written texts, oral presentations and posters, respectively.

The purpose of writing *scientific papers* is, for a researcher, to be read, to influence the readers and thus to contribute to the overall development of science in her/his field. One way to achieve this is writing not only interesting science, but also using a pleasant style. Good science must have appeal, so that other scientists will read the work and remember it. Also, in order to achieve acceptance by a prestige journal, the author must not only present significant, well-planned and well-executed research, but s/he must also offer this in a concise, clear style.

I do not agree with those who think that scientific texts must or should be written in dull, withered language. It is possible to *write both precisely and elegantly*. This enhances the *pleasure* not only of the readers, but also or even primarily of the authors themselves. In order to do so, one must master the written language. Vocabulary and grammar must be learned, and writing must be practiced under the supervision of teachers. Once this is done, writing well requires, above all, *reading well-written texts*. According to personal taste, these can be novels, poetry, essays (on philosophy, politics, science, etc.), magazines or others. It does not really matter what one reads, as long as it is well written. Reading interesting texts is a complementary pleasure to good writing, and writing well is a complementary pleasure to doing good research.

The purpose of *oral presentations* is to capture the attention of listeners, to provoke discussion in the scientific community and to be remembered. One way of achieving this result is to prepare with care not only the contents of presentations but also the visual aids. There is *pleasure in preparing* simple, clear and elegant visual material. Speakers should also take into account the fact that many meetings include participants who are not native English speakers, sometimes in large numbers. When it is known in advance that there will be non-native English speakers in the audience, speakers should increase significantly the amount of *written information in their visual aids*. The same advice applies to speakers whose English is strongly accented. Low-quality visuals and poor understanding of the speaker cause listeners to almost instantly switch off for the duration of the talk, which is generally felt by the speaker and thus destroys his/her pleasure. Using appropriate visual aids helps prevent the situations described above, which are unfortunately quite frequent, and thus enhances everybody's pleasure.

The purpose of *poster presentations* is to attract visitors, to interest them and to trigger discussion of the research work. Some of my suggestions here are similar to those for oral presentations: authors should aim at simplicity, clarity and elegance. I do not know many people who are attracted to crowded, confusing and/or ugly posters. Posters must successively: catch the eye from a distance, catch the attention of those approaching, catch the interest of those glancing at the text and illustrations and finally catch the imagination of those reading. Elegance catches the eye, clarity catches the attention, simplicity—meaning here straightforward design—catches the interest and original content catches the imagination. Looking at a good poster is pleasurable. Seeing a potential visitor hesitating, moving towards the poster, scanning it and finally reading it provides great pleasure to the author. The *pleasure* of both the authors and visitors is generally enhanced by discussion of the work.

There are many books on scientific communication that provide useful advice on scientific writing, oral presentations and/or poster preparation. In addition, several scientific societies have prepared short texts on oral and poster presentations. Hence, there is no point in repeating such information here. I shall therefore end with a small piece of advice, which may seem very simple but is often quite difficult to put into practice. In every communication, there should be *a single central idea*. As readers, we all prefer papers with a clear focus. This is also true for talks and posters. As authors, however, we often wish to pack several messages into a single communication, because we have many ideas to convey. Even if it is difficult, we must convince ourselves that doing so is counterproductive, and must therefore be avoided. This becomes easier when we remember how much pleasure we derive, as readers, from well-focused papers, as listeners, from well-focused presentations and, as visitors, from well-focused posters. When a difficult decision must be made, such as selecting which information to include in a scientific communication, *pleasure* generally provides a useful guideline.

IX SCIENCE, CULTURE AND (ECO-)ETHICS

The German philosopher Immanuel Kant (1724–1804) was the first to use 'culture' (*Kultur*) to mean the whole intellectual aspects of a civilisation. I showed in Chapter II that scientific research, which aims at discovery, is an *intellectual activity*. It follows that science is among the intellectual aspects of civilisation, and is thus *part of culture*. Within the context of this book, I therefore define CULTURE as the whole intellectual aspects of civilisation, including science. In modern, developed societies, it could be argued that science is not only part of culture, but is one of its dominant aspects. However, *science and culture* are often thought of as distinct, if not opposed, aspects of civilisation in modern societies. Why is that so, and what are the consequences of this view?

Science and Culture

At the end of the 19th and beginning of the 20th century, scientific discoveries created great excitement in the general public. Science was then part of culture. Deep interest of the public in scientific discoveries progressively declined during the course of the 20th century, as a gulf opened between science and culture. Possible reasons for the progressive widening of this gulf will be discussed below.

Let us first examine the present relationships between science and culture at the international level, in national governments and in universities.

On the *international* scene, education, science and culture are considered to be complementary components of civilisation. The *United Nations Educational, Scientific and Cultural Organization* (UNESCO) was created in 1946 as a specialised institution of the *United Nations*. Its main objective is to contribute to peace and security in the world by promoting collaboration among nations through education, science, culture and communication, as explained at the beginning of Chapter X.

Within *national* governments, science is seldom associated with culture. According to countries or fashions, scientific research may have its own Ministry or Agency, or be grouped with such government activities as education, technology, industry or even commerce. Alternatively, research may be spread among several ministries, with sometimes a more or less efficient coordination structure, the efficiency of the coordination depending on who actually controls the research monies. That situation may be seen as favouring scientific research, in the short term, because the budgets allocated to culture by most governments are much smaller than those going to research. However, it contributes to pushing science into a ghetto, where it generates little excitement in the public. I showed in Chapter VI that such a situation might jeopardise the long-term public support of research.

The situation of science relative to culture varies widely in *universities* and schools of higher education. At one end of the range, one finds universities or schools that specialise in a single or a small number of subjects, i.e. scientific, non-scientific—arts, languages, literature, etc.—or professional—agriculture, business, engineering, forestry, law, medicine, and so on. At the other end of the range, there are universities that are composed of only two large faculties, i.e. Arts and Sciences at the undergraduate level and Graduate Studies, plus professional schools. In the mid-range, universities may have several scientific and non-scientific faculties and professional schools, on the same campus. There exist a large number of intermediates between these three broad models. Hence, some universities focus on scientific or non-scientific or professional subjects only, whereas others integrate to various degrees scientific and non-scientific subjects.

The previous paragraphs show that there is no agreement in developed societies on the situation of *science relative to culture*. Science may be seen as a utilitarian activity, completely distinct from culture, e.g. science put into Ministries of Industry or Commerce, or as the complement of other activities that include culture, e.g. science as the complement of education and culture in the UNESCO, or as part of culture, e.g. Faculties of Arts and Sciences in some universities. The general situation in international organisations, governments and universities reflects the fact that, for most people in modern societies, *science and culture are distinct*, and even opposed, high-knowledge activities (see Chapter I). Culture is often understood as covering such activities as visual arts, music, literature and philosophy, to the exclusion of science.

The gulf between science and culture opened *during the 20th century*, despite the existence of numerous popular books and magazines as well as radio and television programmes on science and technology. This is all the more difficult to understand or accept because the two groups of creators, in science and arts, are actually very similar (Chapter III, Section 'Creative Imagination'). Why is that so? I think that the main reasons for the situation described here include, as explained in Chapter VI, at least *three components*. (1) There is the way society and even researchers themselves *think of* *scientists*: too often scientists are imagined—and/or think of themselves—as very logical, highly trained and cold individuals; in other words, scientists are imagined as dull or even frightening people. (2) *Scientific knowledge* is generally seen as an immense and complex body of firmly established and interconnected laws that is almost impossible to penetrate. (3) Research is often marketed as a primarily *utilitarian activity*, which is of interest for technologically oriented people, but unbearably boring for non-specialists. This situation is all the more dangerous because most politicians have little or no knowledge of science, whereas addressing the most pressing problems that confront our societies requires some understanding of the processes of Nature (see the next Section, and Chapter XI).

The situation described above contains the elements of a *positive-feedback, downward process*, as explained in the remainder of this paragraph. The three factors mentioned above—researchers are imagined as dull or frightening people, scientific knowledge is considered to be almost inaccessible and research is seen as a mostly utilitarian and boring activity—concur to bring about a devastating result: the *public and young people* withdraw from science. As a response, *researchers* retreat into more specialisation and isolation, which pushes the *public and young people* to further withdraw from science, and so forth, in a downward spiral (Fig. 24). This *feedback process* would explain why the public response to science progressively spiralled down, from general excitement at the end of the 19th and beginning of the 20th century to overall indifference, except for a few scientific fields (see Chapter VI), at the end of the 20th and beginning of the 21st century. I am not sure that the process I describe here actually took place or, if it did, that the process played a significant role in the disaffection for science, but I am sure that the public and young people increasingly withdraw from science. This must be stopped, and reversed.

Possible solutions to the three problems cited above are discussed in Chapter VI, and there are undoubtedly many other aspects in the relationships between researchers and non-researchers that could be improved. These *solutions* include: science communicators and researchers themselves must show scientists as *true creators*; science communication must explain that the body of scientific knowledge, although formidable, is *transient*, and researchers must behave accordingly; science communication and researchers must avoid focusing exclusively or even primarily on the *utilitarian* facets of science. In order for this to occur, and thus permanently bridge the present gulf between science and other aspects of culture, researchers must change drastically the way they see and show themselves.

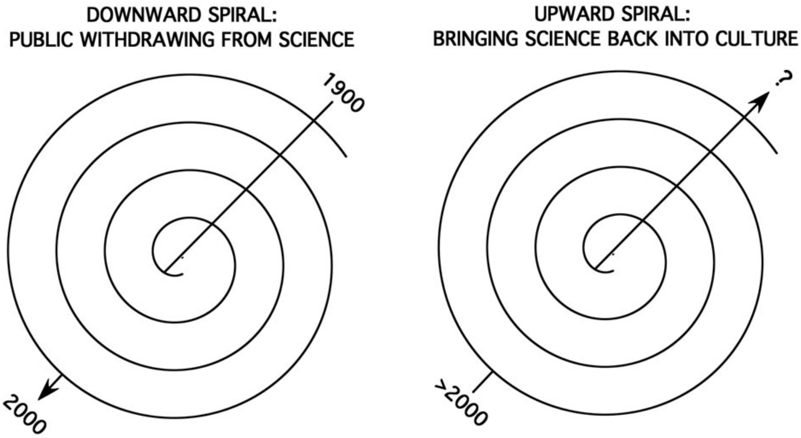


Fig. 24. Left: the public withdrew from science during the 20th century because, progressively, researchers were imagined as dull or frightening people, scientific knowledge was considered almost inaccessible and research was seen as a mostly utilitarian boring activity; this led researchers to retreat into more specialisation and isolation, hence a positive-feedback process and downward spiral. Right: proposed upward spiral toward reintegration of science into culture, by bringing back discovery and creative imagination to the centre of research; this could start a new positive-feedback process, and thus an upward spiral (Original)

Such a new attitude will require, in turn, a change in the *training of researchers* (see Chapter V), which will result, among other consequences, in attracting to science some of the bright youngsters who presently avoid it. The presence of these new people will contribute to modifying the way science is seen by *researchers and society*. This will, hopefully, initiate a *positive upward feedback process* (Fig. 24).

What I proposed in the previous paragraph is to reverse the downward spiral, and start an upward trend. I think that we could decelerate and stop the downward spiral of the 20th century, and initiate an upward spiral in the early 21st century by bringing back *discovery* (Chapter II) and *creative imagination* (Chapter III) to the centre of research.

The approach proposed here, if successful, would *reintegrate science into culture*. This may turn out to be crucial not only for the scientific community, i.e. to attract bright youngsters to scientific careers, and ensure the public funding of research (see Chapter VI), but also for society as a whole, as mentioned in Chapter III (Section 'Significance of Creativity') and discussed in the next Section.

Culture and Eco-Ethics

In this Section, I will develop the idea, proposed by others, that the survival of our species might depend on a new approach to the environment—called *eco-ethics* or *environmental ethics*—to be rooted in science, knowledge and compatibility between Nature and humanity (Kinne 1997, 2002, 2003).

ETHICS is the philosophical theory of moral; it provides *rules of conduct and behaviour*. MORAL is the *theory of human actions*, as subjected to duty and aiming at good. Because ethics takes into account intellectual progress, it can change with time and its rules may differ among cultures. Hence, the rules of conduct based on ethics *evolve*. This is contrary to the approach of most religions in which the rules of conduct are often immutable, because their basis is dogma. However, there are as many sets of religion-based rules as there are religions, and within a given religion, new interpretations of religious traditions or texts sometimes lead to changes in rules of conduct. As a consequence of the fundamental difference between ethics and religions, the rules of conduct based on ethics sometimes conflict with those from religions.

It was proposed that *ethical precepts* are principles of the social contract hardened into rules and dictates, i.e. the behavioural codes that members of a society fervently wish others to follow and are willing to accept themselves for the common good. Before focusing on eco-ethics, it is useful to examine one well-known example of successful application of ethics to everyday life: *medical ethics*. 'Medical ethics' is sometimes called 'bioethics', but medical ethics is only one component of bioethics.

It is now generally accepted that all steps leading from biomedical research to the treatment of patients must obey rules of medical ethics (Fig. 25, left-hand side). These steps include biomedical research, the interactions between researchers and companies that make and market medical products, e.g. drugs and medical equipment, the use of biomedical discoveries by companies, the interactions between companies and physicians, the use of medical products by physicians and the interactions between physicians and patients. Double arrows in Fig. 25 identify interactions. Examples of unethical practices during interactions include: researchers trying to get funding or employment from companies at the expense of their scientific integrity, or companies trying to convince researchers to doctor their results; companies offering personal advantages to physicians who prescribe their products, or physicians demanding such advantages from companies; physicians behaving unethically with their patients, or patients requesting unethical acts from physicians.

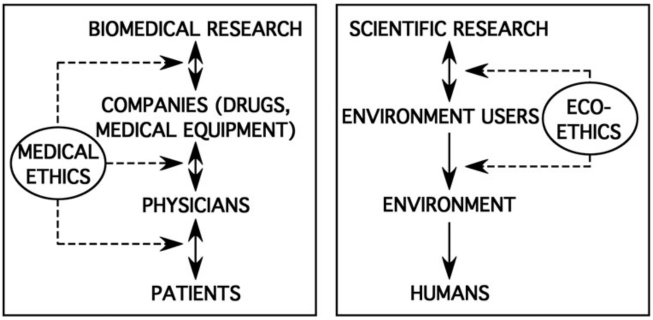


Fig. 25. Roles of medical ethics (left) in controlling the steps from biomedical research to the treatment of patients, and of eco-ethics (right) in the steps from scientific research to humans. Solid arrows identify interactions (double arrows) and unidirectional actions (single arrows). Dashed arrows refer to the role of ethics (Original)

As summarised in the central part of Table 12, the improvement of human health benefits from *biomedical discoveries*. These reach patients through companies that make medical products, and physicians who use these products or apply some of the discoveries directly. If there were no external con-

Table 12. Role of medical ethics in improving human health, and possible role of eco-ethics in ensuring human progress and survival

	Human health	Human progress and survival
Knowledge base	Biomedical discoveries	Natural sciences discoveries
Users of knowledge	Companies (drugs, medical equipment, etc.)	Environment users (companies, communities, farmers, etc.)
Actors	Physicians	Environment
Threatened party	Patients	Humans
Controlling the threat	*Medical ethics*	*Eco-ethics*
Representatives of parties	Associations of biomedical researchers, companies, physicians and patients	Scientific, professional and industrial associations, environmentalists and politicians
Other specialists	Social scientists, philosophers, lawyers	Social scientists, philosophers, lawyers
Coercion	National laws and regulations, professional codes	International treaties, national laws, professional codes

trol exerted on both companies and physicians, corporate or personal interests could threaten the health of patients. This has been understood for a very long time, as evidenced by the oath embodying the code of medical ethics devised by the *Greek physician Hippocrates* (from ca. 460 to 377 BC). The *Hippocratic Oath* was taken by those about to begin medical practice more than two millennia ago, and is still taken in many countries nowadays. In modern societies, establishing *rules of medical ethics* generally involves discussions among representatives of interested parties (associations of biomedical researchers, companies, physicians, patients, etc.; Table 13), and other specialists (social scientists, philosophers, lawyers, and so on). Involving in the exercise a wide array of people takes advantage of their diversity of expertise and opinions; it also helps in developing consensus in the community. In most countries, rules of medical ethics are embodied in national laws and regulations, and some are part of professional codes. Hence, medical ethics is not a matter of sentiments: its rules are implemented with necessary coercion by governments and professional bodies.

The rules of medical ethics often vary among countries, i.e. among cultures. Medical ethics both *prescribes* some courses of action, and *forbids* others. Except for a few extremists who wish total freedom for themselves (often dictated by greed), most biomedical researchers, companies that make medical products and physicians realise that the absence of medical ethics would threaten not only the patients but also their own professions. I suspect that a similar, realistic reasoning, and not only or primarily idealism, led to the Hippocratic Oath, twenty-four hundred years ago, because the Hippocratics, who devised wonderfully precise rules of medical ethics, were not idealists but followed a materialist philosophy.

Table 13. Parties involved in the improvement of human health, and their representatives for establishing the rules of medical ethics

	Parties	Representatives
Knowledge base	Biomedical researchers	Associations of biomedical researchers
Users of knowledge	Companies (drugs, medical equipment, etc.)	Company associations
Actors	Physicians	Physician associations
Threatened party	Patients	Patient associations

Concerning eco-ethics, we know that humans are presently modifying the environment of Planet Earth at an accelerating pace, which threatens the very survival of the human species. The *Eco-Ethics International Union* (EEIU) proposed that ethics provides the approach to face that major threat (http://www.eeiu.org).

As far as I know, there is no formal definition of ECO-ETHICS or environmental ethics. I propose to define it as follows: the theory of human actions, as subjected to duty toward Nature—to which humans belong—and aiming at compatibility between Nature and humanity, which provides *rules of conduct and behaviour* for interacting with the *natural environment*. It must be remembered that Nature consists of the physical environment and living organisms, including human beings (Chapter II, Section 'The Nature of Scientific Discovery'). The definition of eco-ethics stresses the fact that human beings both belong to Nature and often act on the natural environment as if they were not part of Nature. This almost schizophrenic attitude is largely responsible for the problems discussed here. In the remainder of this Section, I will analyse the idea of eco-ethics and discuss how I think it could be implemented.

Fig. 25 compares the roles of *medical ethics* (left-hand side) in controlling the steps from biomedical research to the treatment of patients (discussed above), and of *eco-ethics* (right-hand side) in controlling the steps linking scientific research to humans. The steps involved in eco-ethics include scientific research, the interactions between researchers and those who use the environment, the utilisation of scientific discoveries by the environment users, the action of users on the environment and the action of the environment on human beings.

The two sides of Fig. 25 show major differences. On the left (medical ethics), all steps are *tightly coupled* by interactions (double arrows). In order to remain in operation, such a coupled system must have well-defined rules, which probably explains why *medical ethics* appeared early in human civilisations. On the right (eco-ethics), only two of the steps are interacting (i.e. double arrow between scientific research and users), whereas the other steps are characterised by unilateral actions (single arrows). Because of the *absence of a tight coupling* of the various steps that link scientific research to humans when dealing with the environment, the system has been operating until now *without ethics rules*. Eco-ethics is appearing now because an increasing number of people realise that the build-up of environmental problems is threatening the very survival of our species. I will first discuss the single interaction and the two unidirectional actions on the right-hand side of Fig. 25, before examining the possible role of eco-ethics.

The interaction between researchers and those who use the environment sometimes leads to unethical practices, e.g. researchers trying to get funding or employment from users at the expense of their scientific integrity, or users trying to utilise scientific discoveries for purposes unacceptable to researchers. Hence, there are *rules* in many countries or professional associations that govern this interaction. The situation is very different for the two unidirectional actions.

The *first unidirectional action* is that of users on the environment. Western culture, among others, considers that the natural environment can be used freely for the benefit of human beings, forgetting that humans are themselves part of Nature. We now realise that humans never conquered the world and that, in fact, they do not understand it; until recently, several human societies thought they had control, but they now find this is not the case. Of course, we preserve some parts of the natural environment, which are relatively small, for both future generations and our present enjoyment, e.g. parks, with the feeling that this 'good deed' in favour of Nature allows us to use the remainder of our planet as a supply of resources or a dump for wastes. This attitude did not inflict large-scale or long-lasting damages to the global environment as long as technology was primitive and the human population remained small. This started to change with the beginning of the industrial revolution and the population explosion, about two centuries ago. We now begin to see the consequences of the exponential degradation of the natural environment, caused by the combination of technological developments and rapid population growth. This occurred because of our unidirectional action on the environment: in general, those who exploit the natural environment do not suffer directly from the damages they cause to it. Other people, often far away or in the future, do or will suffer. Hence, the *lack of direct, immediate reactions of the environment on those who exploit it* explains why there are presently no ethics-based rules of conduct governing this action.

The *second unidirectional action* is that of the environment on humans. The functioning of our planet is controlled by a large number of feedbacks, which are governed by natural laws. I first develop a real example of such a feedback, before continuing the discussion of eco-ethics, i.e. the World debate on the *depletion of stratospheric ozone* that occurred during the last decades of the 20th century (Legendre 2007). I use it to illustrate what I mean by *feedbacks and eco-ethics* (Fig. 26). The information comes from

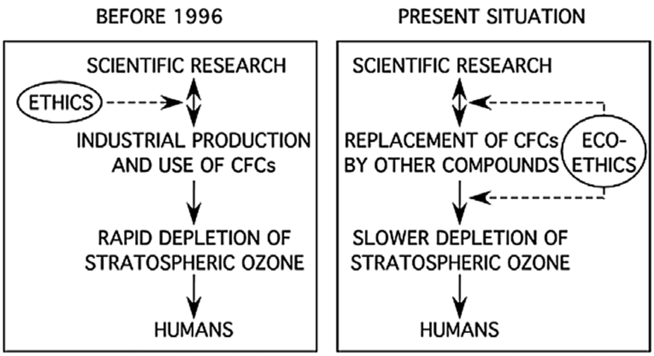


Fig. 26. The depletion of stratospheric ozone caused by the release of CFCs, before and after the Montreal Protocol (signed in 1987, and reinforced in 1996), is used to illustrate the role of eco-ethics in the steps that link scientific research to humans. The general principle of eco-ethics is schematised in the right-hand side of Fig. 25. Arrows as in Fig. 25 (from Legendre 2007)

the Internet site of the U.S. National Academy of Sciences (http://www.beyonddiscovery.org/; see Environmental Issues, The Ozone Depletion Phenomenon).

Ozone is a gas that occurs naturally in the Earth's atmosphere. In the early 1970s, researchers discovered that *chlorofluorocarbons (CFCs)*—which were widely used in refrigeration systems, as propellants in aerosol sprays, and in various industrial applications—could destroy ozone in the stratosphere (i.e. the part of the atmosphere located between 12 and 50 km above the Earth's surface). In the stratosphere, ozone absorbs part of the *ultraviolet (UV) radiation* from the Sun; high doses of UV are dangerous for living organisms. It was feared that reduction of ozone in the stratosphere, caused by the increasing release of CFCs by humans, could lead to an increase in UV radiation at the Earth's surface, which would in turn increase skin cancers and eye damage in humans, and affect terrestrial and marine ecosystems.

The *chain of causalities* linking CFCs to environmental effects of stratospheric ozone depletion was not readily accepted by all researchers, but it struck the imagination of the public and of many politicians. After a hot debate within the scientific community and in industrial societies in general, some countries (including the U.S.A.) decided to ban the use of CFCs in aerosols in the late 1970s. A few years later (1984-1985), there were the first reports of ozone loss over the Antarctic continent during spring (the well-known "ozone hole"). This important finding contributed to the signature of the *Montreal Protocol*, in 1987, which called for eventual worldwide CFC reduction by 50%. The occurrence of the Antarctic ozone hole was confirmed in later years, and the occurrence of a similar “hole” over the Arctic was first reported in 1988. Since 1987, more than 150 countries have signed the Montreal Protocol, which was modified to completely *ban CFCs* from January 1996. Even with this ban in effect, it may take until the middle of the 21st century for ozone levels above the Antarctic to return to 1970s levels.

By reference to Fig. 26, the release of CFCs causing depletion of stratospheric ozone, and the subsequent effects of ozone depletion on humans (directly, or indirectly through the effects on ecosystems) correspond to the *first and second unidirectional actions*, respectively. In the second unidirectional action, the Earth’s environment acts blindly on humans, who do not have any direct mean of countering that action. When human societies banned the industrial production of CFCs in 1996, they acted on the release of CFCs (first unidirectional action) and thus *modified their action on the environment*. They did it because they could not prevent the environmental effects of stratospheric ozone depletion; i.e. they had *no hold on the reaction of Nature on humans*. In left-hand part of Fig. 26, the interaction between researchers and industries was presumably ethical, but the action of industries on the environment was not (it must be noted that nobody, including industries, was aware of possible environmental effects of CFCs until the 1970s). After the CFCs were banned (right-hand part of Fig. 26), both the interaction between researchers and industries and the action of industries on the environment became ethical.

This example shows that the only way humans can prevent or stop environmental disasters—for humanity—is to *modify their own actions on the environment*, i.e. change the first unidirectional action, because they *have no hold* on the second one. This corresponds to the idea of Lovelock (2000, p. xx) that: ‘we are part of the Earth system and cannot survive without its sustenance’.

It is generally difficult for people to see how their actions on the environment (first unidirectional action in Figs. 25 and 26) are linked to the reactions of the latter on them (second unidirectional action), because the two types of action often occur on *different time scales*. Usually, the *actions of humans* on the environment take place on a much shorter time scale than the *reactions of the environment*. A striking example, which was discussed in the previous paragraphs, is the long-lasting effect of CFCs on stratospheric ozone after its ban. Even when the actions of humans on the environment are in the long term, e.g. the steadily increasing release of CO_2 in the atmosphere since the beginning of the industrial revolution, more than 200 years ago, most people do not relate their day-to-day activities to the resulting changes in the Earth's environment (i.e. climate change, see Chapter XI, Section 'Possible Solutions').

There is also a *spatial aspect* to the above two unilateral actions. In *small systems*, those who use the environment are often spatially close to those who would suffer from their abuses. In addition, because spatial and temporal scales are not independent in natural systems, the actions of users on the environment in small systems may be followed rapidly by reactions of the environment on the human community. For example, in a small-lake system, farmers who would release excessive amounts of fertilisers in the watershed, and would thus cause eutrophication of the lake, are physically and socially close, and/or related to, or even among those who use the lake, e.g. for drinking water and recreation. In such a system, there is a potential for spontaneous feedback and the development of community solutions, without the need for resorting to formal rules of eco-ethics. In contrast, in *large systems* those who use the environment are often far in space, time and/or socially from those who would suffer from their abuses, which can lead to the situation illustrated on the right-hand side of Fig. 25 and the left-hand side of Fig. 26. The resolution of actual or potential problems without resorting to *formal rules of eco-ethics* in some small systems stresses the need for such rules in larger systems.

Combining the above paragraphs on the two unilateral actions shows that, because the steps that link scientific research to humans are *not tightly coupled*, on the one hand, there is no immediate incentive for those abusing the natural environment to treat it ethically, and on the other hand, humans generally cannot protect themselves from catastrophic environmental reactions. *The catastrophes would be for the humans*, not for the environment. In that sense, the idea of 'saving' Planet Earth is mistaken, although generous, because *the Earth does not need our protection*. As a matter of fact, our planet does not need human beings anymore than it needed the dinosaurs. In other words, *what we must 'save' is not the Earth, but ourselves*. Fig. 25 shows that the only way humans can avoid catastrophic environmental reactions is to force ethics on the unidirectional action of users on the environment. This could probably be achieved through *international* actions only, although incorporating rules of eco-ethics in *national* laws and professional codes could be a first step in the right direction.

The approach to eco-ethics described above is primarily *anthropocentric*: 'saving ourselves'. However, there are increasing numbers of people who think that human beings have the moral responsibility to act as *stewards of the biosphere*, for present and future generations. As a matter of fact, many researchers feel a strong responsibility to the living world in general, which provides a complementary basis for developing eco-ethics. I wish to point out that ‘saving ourselves’ and ‘acting as stewards of the biosphere’ are, in fact, two sides of the same coin. On the one hand, I think that most people can understand the *urgency of saving ourselves*, and be convinced to rapidly take steps in that direction. By raising the environmental standards in a way to save ourselves, we would improve the likelihood of survival not only for human beings but also for most other species. On the other hand, a number of people may prefer to *base eco-ethics on the idea of a stewardship of the biosphere*. By raising the environmental standards in such a way as to save other species than our own, we would improve the likelihood of survival not only for these species but also for ourselves. Hence, the two approaches are complementary, and they would lead to the *same rules of eco-ethics*. The difficulty lies in finding a way to set the process in motion. Combining the two approaches may be the key to success.

Authors have stressed the facts that humankind has become a *geophysical force* that rapidly alters the Earth’s climate, and the *greatest destroyer of life* since the Age of Reptiles was abruptly terminated 65 million years ago. In addition, we may run out of food and/or water in a few decades because of overpopulation. As a response to the present danger, most people instinctively wish to either *re-create our Blue Planet* as it was before we changed it, or *use technology* to get free from the laws of ecology, which are imposed by the natural environment of Earth. These two dreams are, of course, impossible, which leaves only one course of action: *environmental ethics*. Wilson (1998, p. 287) explained that many people and governments accuse environmentalists of being alarmists, and prefer to save efforts now by making the choice of not taking action. However, if they are wrong and the environmentalists are right, the price to pay will be ruinous. In matters of the environment, as in medicine, a false positive diagnosis is an inconvenience, but a false negative diagnosis can lead to catastrophe. He concluded that we are learning the fundamental principle that *ethics is everything* (p. 297).

The right-hand part of Table 12 summarises how eco-ethics could ensure human progress and survival if the utilisation of discoveries in the natural sciences by the environment users, and their effects on the environment, were subjected to eco-ethics rules of conduct. Such rules already govern the interactions between researchers and some environment users, in a limited number of countries, and there are a few international agreements that regulate the actions of users on the environment, e.g. the Antarctic Treaty, which forbids the exploitation of the Antarctic environment, and the Montreal Protocol, which bans the production of ozone-destructive CFCs (see above). The very existence of such rules shows that *eco-ethics is not a wild dream*, and indicates what the general rules of eco-ethics could be. By reference to the example of medical ethics, discussed above, it is clear that building eco-ethics will require discussions among representatives of *interested parties*. These include: scientific, professional and industrial associations, who will represent the researchers and environmental users, respectively; environmentalists, i.e. researchers and activists, who will 'represent' the environment; and politicians, who will represent the citizens of Planet Earth (Table 14). As in the case of medical ethics, the discussions should also involve other specialists, such as social scientists, philosophers and lawyers. The end result would be rules of eco-ethics, embodied in international treaties, and possibly national laws and professional codes. These *rules would be enforced* by governments and professional bodies. In some cases, the development of eco-ethics rules at national and professional levels could be steps leading to the necessary international actions.

Eco-ethics appears so important and reasonable that it should have aroused strong interest in the scientific community, intellectual circles and the general public, especially in developed countries where the functioning of society is based on exchange of information. However, relatively *few people have actively responded* to the idea so far, although their number is growing. How could this be explained, and perhaps reversed?

I think that a major reason explaining the limited involvement of non-scientist intellectuals and the general public in eco-ethics, so far, comes from the *wide gulf* discussed in the previous Section which exists between researchers and the public, and more generally between science and culture. On the one hand, because of that gulf, the *general public and non-scientific*

Table 14. Parties involved in achieving human progress and survival, and their representatives for establishing rules of eco-ethics (based on Table 18, for medical ethics)

	Parties	Representatives
Knowledge base	Natural sciences researchers	Scientific associations
Users of knowledge	Environment users (companies, communities, farmers, etc.)	Professional and industrial associations
Actors	Environment	Environmentalists
Threatened party	Humans	Politicians

intellectuals are not really interested in social ideas originating from natural scientists, or at best they suspect these ideas to be self-serving. On the other hand, because of the same gulf, few *researchers* in developed countries believe that they could exert significant influence on social conduct or behaviour, except perhaps through political lobbying. For example, many scientific societies in the USA have their headquarters in Washington, where they actively meet and/or lobby senior civil servants and politicians.

Some examples show that the international community is capable of action when there is clear evidence that humanity is endangered. I already cited the example of the Montreal Protocol, which banned the use of CFCs when it was suspected that their destructive effect on the ozone layer could lead to a life-threatening increase of UV at the Earth's surface. The purpose of eco-ethics is not only to *prevent* the occurrence of such catastrophes, but also to *avoid coming close to them*, because at some point in the future last-minute action may happen too late.

I think that one of the problems of present environmental policy is that environmental researchers and activists both aim at wrong targets. *Many environmental researchers* favour *education*, in which they advocate a gentle, ethical approach to the environment. However, as shown in the biomedical field, ethics is not a matter of sentiments, and its efficiency depends on the definition and implementation of rules of conduct. The latter sometimes requires coercion. *Environmental activists* would like to *save the Earth*. However, as I already explained, the Earth does not need to be saved: it existed more than four billion years without human beings, and if we destroyed the conditions necessary for the existence of complex organisms or societies and consequently disappeared, such conditions would probably be restored quite quickly, say, within a few thousand years. The Earth does not need to be saved by us, but we may *need to save ourselves* from life-protecting Earth.

I suggest that the community of interested environmental researchers sets as its central objective the *definition of eco-ethics rules of conduct*. Once this objective is clear, we should approach, as a community, all groups that could become potential partners in establishing these rules (Table 14). It should be clear to all parties involved that *no group alone* has the expertise to set the rules of eco-ethics. Because of the diversity of interests among partners, actual agreement on rules would not be easy, but it could be successful if the objective of the exercise were clear: preventing catastrophic feedbacks of Earth on humans, resulting from our disruption of major natural equilibria. The idea could be appealing even to those who wish to 'save' the Earth, because their objective—bringing under control our actions on the environment—is the condition for eliminating the danger of catastrophic natural feedbacks on humans.

According to the above proposal, the community of environmental researchers should approach potential partners in other fields of activity with a *clear idea* of the problem to address and a *general agenda* for doing it, and it should clearly inform partners that the rules of eco-ethics would be defined *collectively* by all interested parties, e.g. as is done in the case of medical ethics. This could be a major step in *reintegrating science into culture*, as discussed in the previous Section. This would be all the easier if researchers showed themselves to their partners as they truly are: imaginative people, who do not think of themselves as possessing the truth and who put the pleasure of discovery before the utilitarian aspects of research.

X INTERNATIONAL RESEARCH

The present body of science results from thousands of years of discoveries by researchers from all over the World. In addition, the planetary problems discussed in the previous Chapter ignore national boundaries. Hence, science is truly *international knowledge*, and scientific research *one of the most internationalised activities*. There is a generally *free flow of information* among researchers, except in industrial and military environments, and a large component of scientific research is conducted internationally.

Another, complementary aspect of international science is its role in the *maintenance of peace and security* on Earth, as stated in the constitution of the UNESCO: 'The purpose of the Organization is to contribute to peace and security by promoting collaboration among the nations through education, science and culture in order to further universal respect for justice, for the rule of law and for the human rights and fundamental freedoms.'

During the course of my career, I have travelled in a large number of countries, conducted research with colleagues on several continents and participated in many international programmes. In addition to the scientific benefits of these international activities, their *human and cultural returns* were a great reward for me. I progressively learned that *careful preparation* is essential to ensure success in international activities. The present Chapter summarises my personal views on international research, and offers suggestions to maximise the scientific, human and cultural *discoveries* resulting from this challenging activity. I will especially insist on the need of careful preparation.

My viewpoint in this brief chapter is that of a researcher who has had lived and worked in *developed countries* most of his life, and visited developing countries for research only occasionally. It follows that what I write here may be applicable to researchers in developed countries only. Indeed, many people from *developing countries* have been very successful at conducting undergraduate, graduate and postgraduate research in developed countries, showing repeatedly that necessity is the mother of invention. They did not need my chapter to find out how to conduct international research.

Motivations of International Research

The present Section explores the *motivations* of conducting research beyond the national framework. Distinction is made here between *three types of collaborative research*: BILATERAL RESEARCH, which involves two countries;

MULTILATERAL RESEARCH, which involves several countries, outside the framework of an international organisation; INTERNATIONAL RESEARCH strictly speaking, which is conducted under the purview of an international organisation, e.g. the Earth System Science Partnership (ESSP; http://www.essp.org/). There are *various types of international action*: coordinated research projects, which are conducted nationally in each participating country; joint research projects, in which research is conducted by teams of scientists coming from several or all participating countries; international programmes, which are developed under the purview of international organisations and conducted by national or multinational teams. The following discussion shows that the motivations of international research are both *scientific and economic* (Table 15).

There are at least *six major scientific reasons* for conducting research beyond the national framework. These are detailed in the following paragraphs.

(1) Some scientific questions or problems are global. Examples of *global scientific questions* include the possible existence of life on other planets, which could be located in our solar system or outside, and the global biodiversity (see http://www.diversitas-international.org/). Examples of *global envi-*

Table 15. Scientific and economic motivations of international research, and corresponding type(s) of collaborative research (i.e. bilateral, multilateral and/or international)

Motivation	Type of action
Scientific	
Scientific questions or problems of global nature	International
Similar challenges in several countries	Bilateral, multilateral or international
Environments very complex, and/or costly to study	Multilateral or international
National problems that require international help	Multilateral
Sharing of environments or resources	Bilateral or multilateral
International scattering of needed expertise	International
Economic	
Access to competitive research funds	International
Access to dedicated research funds	Bilateral or multilateral
Technology transfer	Bilateral
Access to scientific resources from other countries	Bilateral or multilateral

ronmental problems are: climate change, caused by the anthropogenic release of greenhouse gases to the atmosphere; increasing ultraviolet radiation, resulting from the anthropogenic emission of chlorofluorocarbons that destroy stratospheric ozone (see Chapter IX, Section 'Culture and Eco-Ethics'); progressive eutrophication of coastal marine waters, caused by the massive influx of nutrients from continents, e.g. nitrogenous compounds, as a consequence of increasing populations in coastal areas, the extensive use of fertilisers in farmed lands and, more recently, aquaculture; pandemics, such as the acquired immune deficiency syndrome (AIDS); loss of biodiversity, as a consequence of increasing human pressure. Such global questions and problems cannot be efficiently addressed otherwise than *internationally*.

(2) Some scientific or technological challenges are *similar in several countries*. Examples include the management of natural resources within the context of sustainable development, regional adaptation to the globally changing environment and rapid population growth and/or aging in some countries. Given that these challenges are similar in several countries, the scientific and technological bases to meet them are best developed *bilaterally, multilaterally or internationally*.

(3) Some environments are *too complex and/or costly* to be studied by a single country. Examples of such environments include the Antarctic continent, the oceans and space. These environments must therefore be studied *multilaterally or internationally*.

(4) Some problems are so *complex and urgent* that the countries affected must call upon the international community for help. One example was the deep ecological changes that took place in the Black Sea as a consequence of human activities. These changes included the accidental introduction of carnivorous ctenophores that jeopardised the native pelagic fauna. This led to *multilateral* research activities, e.g. from European countries.

(5) Two or more countries may share the *same environment or resources*. Examples include: most seas (i.e. inter- and intracontinental), e.g. Arctic, Caribbean, Mediterranean; freshwater bodies belonging to two countries, e.g. the North American Great Lakes, which are bordered by the US and Canada; and renewable resources exploited by several countries, e.g. European marine fish stocks. Research on these environments or resources is generally conducted *bilaterally or multilaterally*.

(6) The few people or groups who have the expertise needed to resolve a problem *belong to different countries*. One example is the sudden occurrence of an unknown, highly contagious disease, which requires rapid coordinated action of the few expert groups to prevent an epidemic. The action is gener-

ally coordinated *internationally*, e.g. for infectious diseases, by the World Health Organization.

The *economic motivations* of international research are varied. Four are briefly discussed in the following paragraphs.

(1) A major motivation for getting involved in international research is *accessing competitive research funds*. In many countries, there are specific funds available for *national participation in international programmes*. In others, reference to such programmes in grant applications helps in obtaining national research funds. Some countries, however, do not really favour participation of their researchers in international programmes, especially if these are led by other countries, because they fear a lack of originality in research and loss of independence.

(2) Similarly to international programmes, *dedicated research funds* are often available only within the context of *bilateral or multilateral* agreements. The reasons why countries set up these agreements and funds are varied. For example, two or more countries may encounter similar scientific or technological problems, which could be resolved more quickly and/or cheaply by sharing the existing knowledge in the different countries and/or the cost of developing new knowledge. Another example is the use of science and technology as means for developing long-term relations between countries, aiming at long-term economic spin-offs.

(3) Another economic motivation of international research is *technology transfer* from a developed to a developing country. Such *bilateral* actions generally require specific research to adapt the technology to the new environment. Hopefully, the research and technology transfer should benefit the two countries in terms of employment and economy.

(4) Finally, researchers can have *access to scientific resources* from other countries through *multilateral or bilateral* agreements. These resources include costly infrastructures, e.g. oceanographic ships, telescopes, specialised laboratories and positions (i.e. postdoctoral or permanent, fundamental or applied). Such access is especially crucial during periods of low funding of research in some countries.

I must add that the spin-offs of international research often go beyond those following from the above scientific reasons and economic motivations. One of the broad, long-term results of international research is the development of *permanent collaboration* among the participating individuals and institutions. Interacting with colleagues whose scientific backgrounds and cultures are different often leads to *original perspectives*, which sometimes favour *discoveries* that would not have occurred otherwise; in any case, such interactions can be very pleasant and personally rewarding. More generally, international research is one of the best ways to promote *good relations among countries*, because it is based on free intellectual exchanges and mutual respect among researchers, and it also brings social and/or economic returns to the countries involved. Hence, international research provides countries with a concrete basis for maintaining *peace and security*.

Conducting International Research

International collaboration, like most human activities, has both *advantages and inconveniences*. Hence, international collaboration may be successful or not—i.e. it may or may not lead to *discoveries*—depending on the balance between the advantages and inconveniences. Successful collaboration requires that the partners both *identify and accept the inconveniences* at the start of the project. Failure to do so almost always leads to disaster. Conversely, recognising the inconveniences at the start of the project allows maximum benefits from the collaboration to be drawn, in terms of both human relationships and scientific discovery. The steps involved in organising and conducting international research projects are discussed below, by reference to the advantages and inconveniences summarised in Table 16.

(1) *Preliminary discussions* among potential partners are generally the first step of international research projects. This phase provides the opportunity of *experiencing different scientific cultures and organisations*. In the case of partners without a history of collaboration, these discussions may be fairly *long and tedious*. Another important aspect lies in national differences in the *modes of scientific organisation and funding*: preliminary discussions are generally long when these differences are wide. In several instances, the very length of the initial phase may discourage potential partners, or it may be such that funding opportunities are missed. Potential partners must be fully aware of this constraint, and they must try to realistically assess beforehand the length of preliminary discussions. The existence of *official agreements* between countries involved may help overcome national differences in modes of scientific organisation and funding.

(2) *Scientific programmes* developed for international projects often cover a *wider range of scientific fields* than would be possible within each of the participating countries. A scientifically strong project will attract good researchers, increase the competitiveness of proposals and favour high-quality results. However, *integrating a wide range of fields* often requires long discussions. The duration of this phase is largely determined by the history

Table 16. Typical advantages and inconveniences encountered at various steps of international research projects; steps 6 and 7 are specific to projects that involve fieldwork

Steps	Advantages	Inconveniences
1. Preliminary discussions	Experiencing different scientific cultures and organisations	Relatively long period of preliminary contacts
2. Scientific programme	Wide array of fields	Complex integration of different fields
3. Grant proposals	Expertise of experienced researchers	Combination of national objectives; often several sets of proposals
4. Funding	Diversification of funding: possibility of costly research	Integration of independently funded budgets
5. Implementation	Large pool of well-qualified human resources	Detailed planning of logistics, fieldwork, etc.
6. Logistics	Large pool of logistic resources	Often complex logistics
7. Fieldwork	Interesting human interactions; large pool of equipment	International coordination; often incompatible equipment
8. Analysis of samples	Large pool of technicians and diversified technical expertise	Exchange of samples; different laboratory protocols
9. Analysis of data	Diversified scientific expertise	Non-homogeneous data sets
10. Joint publications	Several multi-authored papers	Possibility of misunderstandings

of collaboration among researchers involved in the various fields, within each country and among the countries involved. In cases in which interdisciplinary collaboration had not previously been attempted, but becomes possible within the framework of the international project, time must be allowed for the new partners to develop mutual trust and find ways for integrating their different approaches. In projects in which some researchers had previously collaborated and others not, special attention must be paid to integrating the latter.

(3) Drafting *proposals* is a critical step in the funding of any scientific project. For international projects, this step is facilitated by the *presence of experienced researchers* who can contribute to the task. However, one complex aspect is often the preparation of two, or several sets of proposals, directed at national funding agencies with somewhat *different objectives,*

procedures and timetables. For example, international projects of high quality sometimes encounter funding problems with the relevant national agencies because of different priorities in the different countries. This constraint must be identified early, so as to design a common, central scientific project that could be further developed into specific proposals that are fundable in the different countries. The preparation of proposals suitable for the different funding agencies may be facilitated by the existence of *official agreements* among the countries involved.

(4) The *funding* of international projects is generally achieved through a diversity of sources in the participating countries. Combining funds from different countries may allow researchers to conduct *costly work*. Funding agencies often see international collaboration positively, and are responsive to *official agreements*. However, the very diversity of funding sources may create difficulties when *integrating the independently funded budgets*. The proposed budgets must therefore be drafted in such a way as to cover all aspects of the planned international activities, including dedicated personnel, exchange of data, and international workshops for implementation and data analysis.

(5) The *implementation* phase is critical for the success of international projects. Indeed, once a project is funded, there must be *detailed planning of the upcoming activities*. The task may be facilitated by the *large pool of available human resources* who have pertinent experience. However, the implementation may be quite complex, and therefore require specific resources, e.g. dedicated personnel and funds, which must have been built into the budgets initially. The implementation phase often requires meetings to refine the science plan, including *sampling and logistics*.

(6) *Logistics* for large international projects is a key to successful fieldwork. The various organisations involved in the project often have their own logistic personnel and resources, so that the *combined pool* from which to draw may be large. However, planning the whole operation may be quite *complex*, especially if fieldwork is conducted at several sites. The most reasonable approach is to dedicate specific personnel to this task, which must have been built into the budgets initially.

(7) *Fieldwork* can be one of the most fulfilling parts of international research, because of strong positive *interactions among participants and access to a diversified pool of equipment*. In some cases, however, it may be a difficult period because of *diverging modes of operation and incompatibilities of equipment*. The chief scientist(s) play(s) a key role in preventing the development of tensions and in favouring positive interactions among par-

ticipants. Detailed planning (steps 5 and 6) is essential for minimising frustrations caused by problems with equipment, e.g. incompatibilities or failures. Successful fieldwork generally leads to strong friendship and further collaboration, e.g. joint participation in other international projects, or sabbatical leaves in one of the other countries.

(8) and (9) *Analysis of samples and data* arising from fieldwork is facilitated by the *large manpower and diversified technical expertise* available in the various laboratories, e.g. for sample analyses, and by the *wide scientific expertise* offered by the participants, e.g. for data analyses. However, exchanging samples and data among laboratories is not a trivial task. The analysis of samples is sometimes made difficult by the use of *different laboratory protocols*. Similarly, the analysis of data is sometimes complicated by the *lack of homogeneity in the data sets*, as a result of different sampling or laboratory protocols, different procedures for species identification, and so on. Data exchanges and multi-laboratory workshops must have been planned at the start of the project (i.e. budget, step 4), and carefully considered during the implementation phase (step 5).

(10) *Joint publications* are the normal output of international collaborative research. Most participants co-author several papers, to which they contribute ideas or data. *Co-authorship* is one obvious benefit of a large-scale international project. However, multi-authorship may create *misunderstandings* among potential authors, especially when publication traditions differ among participating countries. Therefore, some mechanism must be set at the start of the project to prevent or rapidly control potential misunderstandings. A procedure to do so is detailed in the in the full, printed version of this book.

Efficient *coordination* is essential for international collaborative projects, from steps (1) through (10). In order to achieve this, one senior coordinator should be appointed in each participating country. In addition, one junior researcher from each country can be appointed full-time to the project. The coordinators must invite participants from the various countries to convene workshops, working groups, and so on. In large projects, permanent personnel must be hired to ensure the coordination, which must be built into the budgets (step 4).

Preparing for International Research

Until relatively recently, only a *small fraction of the scientific community* was involved in direct, hands-on international research. There were at least four groups of 'international researchers': (1) *elite researchers* from developed countries, which were invited to other countries because of their international reputations, or were invited by foreign or international organisations to join international boards, committees, working groups, etc.; (2) researchers from developed countries who conducted *field work* in various areas of the world, to collect samples or information; (3) researchers from developed countries who *worked in, and with, developing countries* for the purpose of helping these countries progress; and (4), as mentioned in the introductory Section of this Chapter, *people from developing countries* who conducted undergraduate, graduate and postgraduate research in developed countries. Because of the scientific reasons and economic motivations of international research described at the beginning of this Chapter, this activity is expanding rapidly. Hence, increasing numbers of young and not-so-young people wish to prepare for international research. University students and young researchers often wonder how to *prepare for international research*. International research requires both long- and short-term preparation. I briefly describe some elements of that preparation (Table 17).

In the *long term*, the best preparation for international research is to become and remain *highly competent* in one's field of science. The acquisition of strong competence is the very purpose of university education, and it is continuously improved through research. Additional long-term prepara-

Table 17. Long- and shorter-term preparation for international research, and corresponding means

Preparation	**Means**
Long-term	
High competence in one's field of science	University and personal
Mastering the English language	Pre-university and/or personal
Some knowledge of other languages	Pre-university and/or personal
Solid background in world geography and history	Pre-university and/or personal
General culture, incl. arts, literature and table manners	Pre-university and/or personal
Knowledge of international politics and economy	Pre-university and/or personal
Shorter-term	
In-depth knowledge of the collaborative research	Personal; research officers
Characteristics of the countries involved	Personal; Foreign Affairs, embassies
Characteristics of the foreign partners	Personal; science agencies, embassies

tion includes: *mastering the English language*, both spoken and written; getting acquainted with *other languages*; acquiring a *solid background in world geography and history*; being conversant with such aspects of *general culture* as the arts, literature and table manners; and developing a current *knowledge of international politics and economy*. This additional preparation often plays a key role, especially in the many countries where personal relationships are as important, if not more, than scientific competence or achievements. Most universities do not provide much in term of additional long-term preparation, which is considered to be part of pre-university education and/or personal culture.

In the *shorter term*, one must *scrutinise the aims, rationale and research plans* of the intended or on-going international collaboration. This may be facilitated by research officers in funding agencies and organisations such as universities. Additional short-term preparation includes *studying the characteristics of the countries involved* in the collaboration, e.g. geography and history; political, economic and social organisation; culture; food and drinks. Ministries of Foreign Affairs and/or embassies can often provide information that would be difficult to get otherwise. Short-term groundwork also includes developing better *knowledge of the foreign partners*, e.g. organisation of research, stature and key publications of researchers. National science agencies and embassies often prove useful in this respect. However, the key component of short-term preparation is personal work.

The experience of international collaboration is *very rewarding when successful*. It can also be *very frustrating when fruitless*. Given that the scientific and economic motivations of international research described in this Chapter are becoming increasingly prevalent in modern societies, it is surprising that *universities and research organisations* do not actively contribute to preparing students and young researchers to this important component of their career. Such preparation is often part of university programmes in foreign affairs or business, but *not sciences*. Because of this, college and university students interested in international research must seize all opportunities to *improve their knowledge of languages and world affairs, and their general culture*: optional courses, evening classes, special training sessions, and so on. In the most progressive universities, such training could become an *optional component of the curriculum* of undergraduate and graduate science students.

XI RESEARCHERS AND POLITICIANS

In several countries, researchers lack the ability and often the interest to *communicate with the general public and also politicians*. In Chapter VI, I discussed the problems of communication with the *general public*, and some possible solutions. In the present Chapter, I shall examine the often difficult communications of researchers with *politicians*. The lack of efficient communications of researchers with politicians is a serious problem, because the latter have an important say in the *overall funding*, and sometimes the *general direction of scientific research* (Chapter VII). In addition, several of the major challenges with which our societies are presently confronted, e.g. climate change, require that *politicians make decisions* that must take into account *scientific discoveries*. I mentioned in Chapter IX the key role politicians must play in the development and implementation of *eco-ethics*.

There are many examples of *creative people* who, although utterly absorbed in their projects, become deeply involved with *social issues*. It may be that the curiosity and commitment that drive creators to break new grounds in their respective fields also direct them to confront social and political problems. Hence, *social and political issues* are not foreign to researchers.

In the present chapter, I shall examine various facets of the difficulty for researchers to communicate with politicians, and suggest possible approaches to resolve part of the problem. I shall only consider changes that *researchers* could usefully bring to their attitudes towards politicians, and exclude from the discussion changes that *politicians* could possibly bring to their attitudes towards researchers. This is not because researchers are always wrong in their attitudes and politicians are always right, but simply because what I write here might perhaps influence some fellow researchers, but certainly not politicians who will not read my book. It must be clear that I am considering here the activities of researchers as *professional scientists* only, not as private citizens. The professional attitudes of researchers towards politicians fit in this book, whereas their attitudes as citizens belong to their personal inclinations and/or choices.

Contradictions and Differences

I already showed in Chapter VII (Section ‘Funding of Research: Myths and Reality’) that the *funding of scientific research* in most countries comes largely from public funds, directly or indirectly. Because the scientific community is rightly concerned by the funding of research, its members often *try to get the attention of politicians*, who determine the allocation of public funds. In a paradoxical manner, researchers often *distrust politicians*, suspecting them of two contradictory attitudes toward science. Researchers suspect some politicians of sometimes *trying to use scientific discoveries* for their own purposes, whereas other politicians, or the same but at different times, would *not be interested in or could not understand science*. The development of positive communications between researchers and politicians would require the former to resolve their *inconsistent attitudes* towards the latter. Causes for these attitudes include real and not-so-real differences between the two groups, as discussed in the following paragraphs, and summarised in Table 18.

(1) *Objectives*. In many instances, researchers approach politicians with the idea of convincing them to maintain or increase the funding of ongoing

Table 18. Real or perhaps imagined (by researchers) differences between researchers and politicians

	Politicians	Researchers
Objectives	Generally well defined	Often poorly defined
Means	Often poorly defined	Generally well-defined
Approach to research	Generally short-term	Generally long-term
Response to problem	Let's do something now	Let's do more research
Type of answer	Single answer	Probabilities
Deficiency in education	Natural sciences	Social and political sciences
Imagined career[a]	Short-term electoral mandates	Long-term (tenure)
Real career	Suite of re-elections	Often short-term projects
Imagined activity[a]	Dictated by events	Dictated by long-term objectives
Real activity	Generally slowly evolving parties	Often jump on bandwagons
Imagined motivation[a]	Sordid	Dignified
Real life	Partly responsible for research	Includes many tedious tasks
Imagined ability[a]	Easy communication with public	Not easy to communicate science
Real ability	Discourse often rejected	Not interested in communication
Imagined credibility[a]	Low	High
Real credibility	Exert leadership during crises	Feared as sorcerer's apprentices

[a] Imagined by researchers

research efforts. When doing so, researchers often do not clearly state what their overall objectives are, or in other words, what the *discovery potential* of the proposed research is. In contrast, politicians sometimes, perhaps often, have a good idea of their objectives, which include the *resolution of social or economic problems*, but they frequently do not know how to achieve these objectives, especially when the solution involves science.

(2) *Means*. Following from the previous paragraph, the approaches of researchers and politicians are often divergent: the first may know what they *would like to do*, without necessarily *knowing why*, whereas the second may know what they *would like to achieve*, without necessarily *knowing how*. The proper management of this divergence could develop into complementarity.

(3) *Approach to research*. In cases in which researchers have a clear idea of their objectives, they generally ask politicians to finance research that is *long-term*. In contrast, when politicians have a clear idea of how researchers could help them resolving problems, they often propose to finance research that is *short-term*. Resolving that difference may require *mutual education* of researchers and politicians on their respective modes of operation.

(4) *Response to problem*. Related to the above two differences is the contrasting attitude of researchers and politicians when confronted with a social or environmental problem. The initial reaction of researchers is often: 'Let's do more research on the matter', whereas the initial reaction of politicians is generally: 'Let's do something right now to alleviate the problem'. In other words, researchers would like to *improve knowledge* before recommending a course of action, whereas politicians want rapid *resolution of the problem*. The give-and-take solution could be for researchers to accept that their available knowledge is enough to devise a partial solution, and for politicians to recognise that the initial solution would be improved by doing additional research. Providing short-term, partial solutions to problems recognised as important by politicians may be a good way for researchers to show the *relevance* of their work, which would support their argument for longer-term funding.

(5) *Type of answer*. When researchers accept addressing the environmental concerns of politicians, they generally state their answers in terms of *probabilities*, whereas what politicians usually want is a *single answer*, with no or very little uncertainty. Researchers often criticise the lack of ability of politicians to understand that natural phenomena are generally not deterministic, but they must remember the following two facts. *Firstly*, researchers can *educate politicians and the general public* to the inherently stochastic nature

of many natural phenomena. One example is weather forecasting, which is now stated in probabilistic terms in several countries. *Secondly*, even if researchers know that Nature is largely not deterministic, they still *formulate most scientific laws, models and theories in a deterministic manner*. This is because a deterministic approach is simpler than its probabilistic counterpart. Hence, researchers are not innocent of the sin for which they blame politicians, i.e. *reducing stochastic outcomes to deterministic solutions*.

(6) *Education*. Researchers often stress the fact that most politicians *lack education in natural sciences*, which would explain why they do not understand the needs or limits of scientific research. Conversely, the sometimes crude analyses made by researchers of the way politicians reach decisions show that the former often *lack education in social and political sciences*. This is a real difficulty, which often leads to contradictory approaches to problems of mutual concern, e.g. declining fish stocks or climate change (both discussed below). It would be useful to include in the *education of researchers* notions on the social and political roles of science in society.

In addition to the above real problems, I think that several differences between politicians and researchers are not as deep as many researchers think. Here are some examples.

(7) *Imagined and real careers*. Researchers often see politicians as mostly driven by the *need to be re-elected* in the short-term, whereas they are protected from short-term approaches by their *tenure*. There are several aspects to this view. Concerning *politicians*, most of them are re-elected over several mandates, often until they decide to retire. Concerning *researchers*, it is not clear which employment conditions are the most conducive to long- or short-term career strategies. On the one hand, some researchers are *not tenured*, e.g. postdoctoral fellows, research associates and soft-money academics (the latter exist in some countries only). These people must find or firm up employment or salary year after year. One of the best ways to ensure continuous short-term employment or the steady funding of soft-money positions is often to develop unique expertise, i.e. to pursue *long-term research objectives*. On the other hand, many *tenured* researchers often work on *short-term projects*, because funding for such projects is easier to get than for long-term programmes, i.e. in many instances, researchers do not take advantage of their tenure to embark on risky projects. Hence, politicians and researchers may not be that different concerning the short- *versus* long-term nature of their activities.

(8) *Imagined and real activity*. The short- *versus* long-term difference discussed in the previous paragraph concerns not only the duration of elec-

toral mandates or research employment, but also the very nature of politics and research. On the one hand, many researchers think that the objectives of *politicians* are by-and-large short-term, and that their decisions are mostly *dictated by the events* of the day. However, most politicians belong to parties in which the fundamental attitudes toward society go back decades if not centuries, and *evolve very slowly*. On the other hand, *researchers* like to think that their own activity is *dictated by long-term objectives*, but they often jump 'en masse' on the *bandwagons of influential discoveries*. This shows that the time horizons of the two groups may be quite similar.

(9) *Imagined motivation and real life*. Researchers often stress the problem that, even if *research is part of the responsibilities of politicians*, it is not generally part of the reasons that prompted them to enter politics. They imagine these reasons to be quite *sordid*. In fact, the reasons for entering politics include a variable combination of ideals, practical considerations and personal interests. Even if *researchers* may believe that their approach to the scientific career was more *dignified*, practical considerations and personal interests were also involved, as in all human activities. Also, the career of a researcher is not free of its own contradictions. For example, most researchers spend a lot of time on such tasks as serving on committees, writing proposals, managing budgets, writing reports and directing groups, but these *tedious tasks* were obviously not part of the reasons that motivated them to enter research. Hence, *ideals, practical considerations and personal interests* motivate both politicians and researchers.

(10) *Imagined and real abilities*. Researchers often envy the ease with which *politicians communicate with the public*, whereas the inherent complexity of science would make their own communication efforts much more difficult. However, the public, especially young generations, increasingly *reject the discourse of politicians* in many countries, as shown by declining participation in elections and voting for parties outside the main stream. The supposed difficulty of communicating science may reflect more a *lack of interest of researchers or improper approaches* than an inherent problem (see Chapter VI 'Consequences: Science and the Public'). Hence, both groups have problems communicating with the public.

(11) *Imagined and real credibility*. Researchers generally like to think that they are *more credible* to the public than politicians. However, even if the public often does not trust *politicians*, political parties have significant membership in many countries, and politicians can exert *strong leadership at times of crises*. Conversely, even if the public largely respects the integrity and expertise of *researchers*, increasingly large segments of the population

see researchers as *sorcerer's apprentices*—e.g. research on new chemicals, transgenic organisms and nuclear power—and therefore fear them. Hence, the two groups may have high or low credibility with the public, according to circumstances.

In conclusion, *researchers often need politicians, and politicians often need researchers*. The two groups often unknowingly share *similar career problems*. When there are real differences between the two groups, changes in the attitudes and/or approaches of researchers toward politicians could perhaps be beneficial to research and, who knows, to politics as well.

Possible Solutions

The above analysis suggests that the differences—true or apparent—that exist between researchers and politicians could eventually be *resolved, and even used positively*. For the reason I explained at the beginning of this Chapter, I limit the present discussion to changes that researchers could usefully bring to their attitudes towards politicians, and purposely exclude changes that politicians could possibly bring to their attitudes towards researchers.

(1) Researchers must recognise that most politicians are generally *not well-informed about science, or especially attracted to it*. In this, politicians reflect the general attitude of the contemporary society towards science. Researchers should not look down on politicians because of their lack of interest in science.

(2) Researchers must abandon their *contradictory attitudes* towards politicians, i.e. their desire for getting politicians' attention *versus* distrusting them, or their suspicion that politicians have an evil interest in science *versus* reproaching them for having no interest. The pragmatic attitude is probably at the centre of these contradictions: to approach politicians with an *open mind*, while exerting *some reserve* until mutual trust has developed.

(3) Researchers must recognise that their *view of science* is different from that of politicians. *Researchers* often know what they would like to do, without necessarily knowing or explaining to the politicians why, whereas *politicians* may know what they would like to learn or achieve, without necessarily knowing how researchers could contribute. Problems arise when researchers without clear objectives approach politicians with little knowledge of the research process.

(4) Researchers must recognise that their legitimate interest in *long-term approaches* does not fit the eagerness of politicians to have problems

resolved as soon as possible. Hence, politicians generally do not like *long-term funding commitments*. This must be understood when approaching politicians.

(5) Researchers must accept providing *short-term, partial solutions* to the social or environmental problems recognised by politicians, especially since the researchers were often those who pointed out the existence of these problems in the first place. The explicit or implicit counterpart for short-term solutions could be *longer-term funding*, for improving both the knowledge base and the initial solutions. In addition, researchers should devote efforts at educating politicians and the general public about the *inherently stochastic nature of the environment*, so that everybody understands there are no simplistic answers to environmental problems.

(6) The most frustrating aspect of dealing with politicians may be that, for them, *science is only one term* of the complicated and often obscure 'equations' they use for resolving problems. One well-known example is the determination of fishing quotas by politicians, which frequently exceed what researchers know to be the carrying capacity of the exploited stocks. The approach to such situations may be eco-ethics, as explained in Chapter IX (Section 'Culture and Eco-Ethics') and developed in the following two paragraphs.

Fig. 27 applies the model developed for *eco-ethics* to fisheries. The left-hand part of Fig. 27 (same as the right-hand part of Fig. 25) shows one interaction (double arrow) and two unidirectional actions (single arrows). The case of *fisheries* (right-hand part of Fig. 27) fits the general eco-ethics model: *present-day fishing* industries affect the *exploited species*, which in

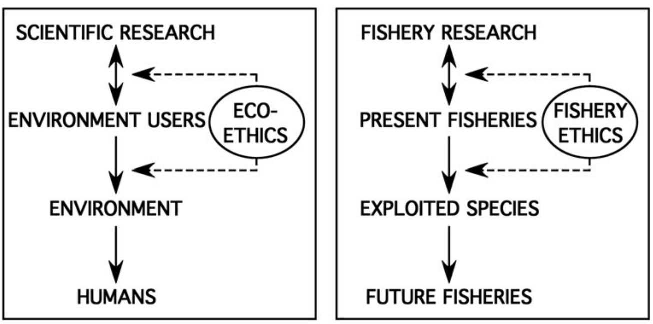


Fig. 27. Possible roles of eco-ethics (left; from Fig. 25) in the steps from scientific research to humans, and of fishery ethics (right) in the steps from fishery research to future fisheries. Solid arrows identify interactions (double arrows) and unidirectional actions (single arrows). Dashed arrows refer to the role of ethics (Original)

turn affects *future fisheries*. For an increasing number of species, intensive fishing leads to rapid depletion of the exploited stocks. This occurs because fishing industries often do not suffer directly or immediately from the damage they cause to the exploited species. The price to pay is the catastrophic effect this has on future fisheries. Because the steps linking scientific research (top) to future fisheries (bottom) are *not tightly coupled*, on the one hand, there is no immediate incentive for those who overfish to treat the exploited species ethically, and on the other hand, because the future fishers are not present, they cannot protect themselves from the upcoming catastrophic collapse of fisheries. The only way this catastrophe can be avoided is to *force ethics on the exploitation of species* by the present-day fishing industries.

The left-hand part of Table 19 (same as the right-hand part of Table 12) summarises how *eco-ethics* could ensure human progress and survival. Concerning fisheries, the right-hand part of Table 19 shows how *fishery ethics* could promote sustainable exploitation, if the use of fishery science discoveries by industries, and the effects of the latter on exploited species, were subjected to ethics rules of conduct. As in the case of eco-ethics, this would

Table 19. Application of eco-ethics to fisheries: possible roles of eco-ethics in ensuring human progress and survival (from Table 12) and of fishery ethics in promoting sustainable fisheries

	Human progress and survival	Sustainable fisheries
Knowledge base	Natural sciences discoveries	Discoveries on exploited species
Users of knowledge	Environment users	Present fishery industries
Actors	Environment	Exploited species
Threatened party	Humans	Future fishery industries
Controlling the threat	*Eco-ethics*	*Fishery ethics*
Representatives of parties	Scientific and industrial associations, environmentalists and politicians	Scientific and industrial associations, environmentalists and politicians
Other specialists	Social scientists, philosophers, lawyers	Social scientists, philosophers, lawyers
Coercion	International treaties, national laws, professional codes	International treaties, national laws, professional codes

require discussions among the representatives of interested parties, contrary to what generally happens nowadays. Too often, there is confrontation among fishers, researchers and politicians, leading to confusion in the general public, which is faced with antagonistic arguments promoted by the ‘friends of the fish’ and the ‘friends of the fishers’. All parties interested in sustainable fisheries must be involved in establishing rules of fishery ethics. The parties include: scientific and fishery associations, who would represent the researchers and industries, respectively; environmentalists—researchers and activists—who would be ‘representing’ the exploited species; and politicians, who should represent future fishers. As in the case of eco-ethics, the discussions should also involve other specialists, such as social scientists, philosophers and lawyers. The end result would be rules of fishery ethics, embodied in international treaties, and possibly national laws and professional codes. These rules would be enforced by governments and professional bodies.

Even if my view of fisheries may be exceedingly naïve, the above very brief discussion of the problem illustrates the fact that *researchers and politicians* cannot escape some degree of *partnership*. This should be possible given that, as shown in the previous Section, the two groups are not as different as most researchers think.

The question is: how can we establish a partnership between researchers and politicians?

(1) Researchers must try to understand *the way politicians make decisions*, which requires some degree of *education in social and political sciences*. This could be done by including in the training of researchers notions on the social and political roles of science in society, as suggested in the previous Section. If this was not done, formal education must be replaced by personal studies and discussions with colleagues.

(2) Researchers must provide politicians with *information on the status and progress of modern science*. A sure recipe for failure in doing so is to invite politicians to attend lectures intended for the general public: very few politicians, if any, would come. In some countries, official scientific bodies, e.g. Academies, are actively involved in that important activity. For example, the *Canadian Partnership Group for Science and Engineering* invites Canadian parliamentarians to meet scientists and engineers during monthly breakfasts called ‘Bacon & Eggheads’ (http://www.pagse.org/en/breakfasts.htm).

(3) Researchers must approach politicians with clear messages as to *what* they wish to achieve, and, more importantly, *why*. Politicians are flooded by

people who want something from them (favourable decisions, contracts, grants, etc.) and they have vast means at their disposal—public service, state laboratories, and so on. Hence, they must continuously *make choices* among alternative possibilities, i.e. *whats*, based on objectives, i.e. *whys*. In other words, they are submerged by *whats*, and look for *whys* in order to keep floating. When researchers come to politicians with *poorly defined projects and vague objectives*, they contribute to swamping them with *whats*. On the contrary, when researchers come to politicians with *clearly defined objectives* they can understand, researchers then provide politicians with *whys* to hold onto.

(4) In the same vein as explained in the previous paragraph, researchers must have clear ideas concerning the *relations between their activity and society*. In general terms, they must remember that the purpose of scientific research is *discovery*, as discussed at length in this book. The immediate *usefulness*, or not, of discoveries is a secondary issue (see Chapter VI). Even when research is determined by the short-term resolution of an immediate problem, I have shown that the only way to generate a pertinent discovery is to use criteria based on *scientific creativity* (Chapter VII, Section 'Funding of Research: Efficient Criteria'). Despite the fact that discovery is their central objective, researchers must come to politicians with clear ideas of the potential short- or long-term *importance for society* of the results of the research they propose politicians to fund. This is illustrated in the following paragraphs, using climate change research as an example.

A large part of the scientific community has been mobilised to study the Earth System, in order to *predict the upcoming change in climate and its effects*. All developed countries are funding climate change research to various degrees. Given the magnitude of the problem, the very large number of researchers and disciplines involved, and the complex organisation of the research (which includes international, regional and national agencies and programmes), there is a need to *clearly link the objectives of the proposed research to the use of potential discoveries in the making of political decisions*. Table 20 summarises a possible classification of Earth System research objectives and corresponding political decisions, based on *space and time scales*. The three rows in Table 20 could, of course, be developed into more detailed classes.

(1) On the *global scale*, the greatest changes will be seen over time scales of a *century or more*. On that scale, research aims at predicting changes in the climate of the whole Planet, and their overall consequences on ecosystems and human beings. That information can be used for developing strate-

Table 20. Relations between researchers and politicians: the example of Earth System research for climate change

Space and time scales	Objectives of research	Political decisions
Global, centuries	Scenarios of changes in planetary climate	Planetary mitigation: international agreements (need for 20× Kyoto)
Regional, several decades	Regional impacts of changing climate: temperature, precipitation, storms, etc.	Adaptation: early, *versus* waiting until major changes take place
National, a few decades	Carbon sequestration in continental reservoirs	Carbon credits (Kyoto Protocol, 1997)

gies to *mitigate* climate change, e.g. by reducing the emissions of greenhouse gases, or sequestering some of the emitted gases in natural reservoirs. This can only be achieved by international treaties, which are the results of political decisions. For example, the 1997 Kyoto Protocol aimed at reducing the overall emissions of greenhouse gases by at least 5% below 1990 levels, in the period from 2008 to 2012. Hence, reaching 100% reduction would eventually require the equivalent of 20 Kyoto Protocols, resulting from a suite of political negotiations and international treaties.

(2) On the *regional scale*, e.g. a continent, the effects of climate change will be felt within *decades*. On that scale, the research aims at predicting changes in the regional climate, and their consequences on ecosystems and human beings in the given area. The direct effects of climate change on people will be changes in regional and local temperatures, precipitations, storm activity, and so on. Since these will be consequences of changes in the global climate, they could generally not be mitigated regionally. On the regional scale, the key word must be *adaptation*, i.e. difficult and costly changes in ways of life, infrastructures, and so on. This will require very tough decisions from politicians, who will increasingly face the dilemma of either initiating adaptation early, on the basis of evolving scientific predictions, or waiting until major environmental changes actually take place, which would then require hurried adaptation of social and economic patterns, at very high human and financial costs. Countries and regions that initiate adaptation early on will not only minimise their own costs, but also develop expertise they could market to those countries waiting for the worst to come.

(3) On the *national scale*, countries can get carbon credits under the 1997

Kyoto Protocol by sequestering carbon in continental sinks, e.g. agricultural soils and forest biomass. The corresponding research activities aim at finding ways to achieve carbon sequestering by human activities. Because carbon sequestration in continental sinks takes place on scales that are relatively *small in space and short in time*, it will not influence the global or regional climate in the long-term. However, it is a step in the right direction, as it shows the determination of the international community to do something to mitigate the change in climate, and paves the way for more significant international agreements. Funding such research should appeal to politicians.

The *Intergovernmental Panel on Climate Change* published its fourth assessment of climate change in 2007, and was awarded the Nobel Peace Prize the same year. The assessment included detailed reports on “The Physical Science Basis”, “Impacts, Adaptation and Vulnerability”, and “Mitigation of Climate Change” (http://www.ipcc.ch/).

The climate change example shows that *researchers and politicians need each other*, and society needs for them to *develop partnerships*. In order to develop efficient relations with politicians, researchers must follow practical rules. This chapter suggested some of them, as a contribution to realistic discussions of that important topic within the scientific community.

XII FOCUSING CREATIVITY ON SCIENTIFIC RESEARCH AS A CAREER AND/OR OTHER FULFILLING ACTIVITIES

I wish to conclude this book with a brief chapter on the *careers of researchers*. I decided to write this chapter because I observed that it is often difficult for people to understand what researchers really do, and also because science students and researchers themselves do not always realise how rich research careers can be. I thought that a discussion of research as a career and of complementary and/or alternative creative activities could be of interest to some people. These may include *students* who are contemplating a research career, *active scientists* who are dedicating much time and efforts to research and colleagues who are in the process of *leaving research*. Because research careers vary widely among countries, disciplines and individuals, the following discussion will be mostly qualitative.

The fact that research careers evolve is normal, but it is seldom discussed openly in the scientific community. In the present chapter, I wish to break this unhealthy taboo, and show that *career reorientation* can be beneficial to both the researchers involved and the scientific community as a whole.

Scientific Research as a Career

It was mentioned in Chapter I (Section 'Creation) that the *strong commitment* of creators involves *passionate interest*. This is certainly true of scientific researchers. However, their careers are seldom limited to research. Indeed, most researchers devote part of their time to high-knowledge creative activities other than research. There is no need to explain here what research is, because it has been the topic of the previous eleven chapters. It is useful, however, to briefly examine here some *high-knowledge activities other than research* that are part of the careers of many researchers: teaching, management and consulting.

(1) *Teaching*, especially in universities, is the normal complement of research. On the one hand, teachers who conduct research not only transmit up-to-date facts in their courses, but also do it within the context of the *scientific method* (see Chapter II, Section 'The Scientific Method'). This way, students can master the scientific method, and use what they have learned for achieving *discovery*. In addition, it is often through personal contacts with active researchers that students find that scientists may be *interesting*

and passionate creators. On the other hand, researchers who have sustained contacts with students are continuously challenged by the latter to *reassess their approaches*. As a consequence, these researchers do not become ossified. In addition, part of the *university teaching is through research*, i.e. students actually *do research* as part of their training, in an increasing proportion as they progress in the university curriculum. This is why, in most countries, a large component of the research is conducted in universities, and the highest-quality research often stems from universities. In most countries, including those where a significant part of research is not university based, non-university research organisations generally collaborate with universities in teaching through research, e.g. by providing supervisors and research facilities for graduate students.

(2) *Management* covers a wide range of activities. The general purpose of these activities is, or should be, to *facilitate research and/or teaching*. They include: the direction of research teams or laboratories; the edition of scientific journals; the organisation of scientific meetings; the leadership of learned or professional societies; the participation in, or chairing of local, national or international committees or working groups; the direction of university teaching programmes or departments; the deanship of faculties; the presidency of universities. The previous examples are only a few among the almost *innumerable non-research tasks* that researchers must do. These tasks are time-consuming, but because most of them require people with *high scientific knowledge, training and experience*, they must be done by *researchers*. I did not include among managerial activities the very demanding task of writing research proposals, because I consider that task part of the research activity (see Chapter VII).

(3) *Consulting* refers to the production of expert assessments, for a variety of organisations. These include: university and research organisations, e.g. reviewing promotion applications, or the performance of teaching or research units; funding organisations, e.g. reviewing research proposals; professional journals, e.g. reviewing manuscripts; learned societies, e.g. reviewing candidates for membership or awards; corporations, e.g. producing impact statements, reviewing book proposals, providing expert advices; governments, e.g. reviewing research activities, assessing policies. As in the case of management, the consulting activities are time-consuming, but they are often an unavoidable duty that requires people with *high scientific knowledge, training and experience, i.e. researchers*. Consulting may be especially attractive when it is rewarded with honoraria, or when it improves the likelihood of obtaining research funds or access to facilities.

Table 21. Evolution of scientific careers: for three stages of the career, proportions of person-years in three types of activity. +++: very large proportion; ++: large proportion; +: small proportion, –: very small proportion; 0: none

	Early career	Mid-career	End of career
Research	+++	+ to ++	–
Other high-knowledge creative activities	+	++ to +++	+
No professional activity	0	–	++

Table 21 provides a schematic representation of the *evolution of scientific careers*. It shows, for three stages of the career, the proportions of person-years in three types of activity. The Table assumes that most of the work (i.e. person-years) of people with a doctoral degree in science is initially dedicated to *research*, and some to the *other high-knowledge activities* discussed above, i.e. teaching, management, consulting. As people get older, the proportion of time of the community dedicated to the latter increases, and retirement progressively takes place. In the present discussion, *people with no professional activity* include not only pensioners but also those who become professionally inactive without actually leaving their jobs.

Table 21 stresses an important point that concerns university training: *even at the beginning of their careers*, researchers must generally devote some proportion of their time to *other high-knowledge tasks than research*. This often comes as a surprise to young researchers, because most universities do not prepare doctoral students to the realities of their careers. For example, they may not know that preparing a new course, for the classroom or the field, takes several months, even when there exists a good textbook. Also, some universities or research organisations tend to assign lots of management tasks to newcomers, who often accept them gratefully as a means to integrate into their new work environment. In addition, journals and other organisations draw heavily on the expertise of young researchers, because these are generally both experts in forefront approaches, and willing to do these tasks to establish their careers. People starting their career must not waste their most precious resource—time—because it is difficult to backtrack when the available time has been allocated to tasks that are not scientifically productive. In any case, the *time resource* becomes increasingly strained as the career progresses. Universities, or at least research supervisors, should address these aspects during the course of doctoral studies, so that future researchers learn how and when to say ‘no’.

A striking feature of Table 21 is the *shift from research to other high-knowledge activities* during the course of the career, i.e. from a very high proportion of person-years dedicated to research at the beginning of the career to a very small proportion at the end. At mid-career (typically 45 years old), the proportion of person-years devoted to research ranges from high to small, according to scientific fields and countries. The reasons that explain the progressive shift from research to other high-knowledge activities involve at least two complementary processes, as discussed in the next paragraph.

The *first process* involved in the progressive shift from research to other high-knowledge activities is driven by the genuine *attraction* of some people to these activities. Such activities are attractive because most of them are designed for people trained as researchers, and they offer alternative possibilities of creative work. As discussed above, most researchers do some teaching, management or consulting at almost all stages of their careers. Over time, many researchers become very good at some of these activities, and are therefore increasingly attracted to them. The *second process* is triggered by the sometimes unrewarding character of research. I explained in Chapter III (Section ‘Creative Imagination’) that pleasure plays a key role in sustaining the creativity of researchers, against the difficulties they often meet in the discovery process. When pleasure fades out, so does the incentive to continue research. This may happen when, for some reason, the delicate process illustrated in Fig. 9 slows down, and eventually comes to a halt. One must understand that most (not all) researchers who have not made a discovery for years become discouraged, and wish to leave research for other creative work. Such activities as teaching, management and consulting may fulfil the wish of people trained as researchers to continue a *professionally creative career outside research*. This is not to say, however, that all researchers who like and excel in other high-knowledge activities than research are not successful at discovery. It is not to say either that all people who completely shifted from research to other high-knowledge activities did it because they were not successful in research; they may have shifted because they preferred the second type of activities. It is not to say, finally, that people who devote most of their time to research would not have the ability of doing other high-knowledge work; they may prefer research.

Table 21 also indicates that some people with a doctoral degree in science become *professionally inactive in mid-career*: these people do not do research, teaching, management or consulting. One wonders why some bright people, who have been trained as researchers, stop all creative professional activities in mid-career or sometimes later. I do not consider here inactivity caused by health problems, either physical or psychological. I have in mind people who no longer find any fulfilment in research, teaching, management or consulting. Such a situation, which is probably quite dramatic for most of those concerned, is also a terrible waste for society. One of the reasons for this may be that, in several organisations, the general shift, as people get older, from research to other high-knowledge activities is not recognised as normal, legitimate and beneficial to the community as a whole. This *lack of recognition* may push some people who quietly leave research to drift into inactivity instead of reorienting their career to other high-knowledge activities. A solution to the problem could be to offer people who are becoming professionally inactive *retraining* that would allow them to use their knowledge and skills for accomplishing fruitful non-research high-knowledge tasks. This would not resolve all problems, but could minimise their numbers, and thus improve both the professional lives of people concerned and the overall efficiency of the organisations to which they belong.

In contrast with the previous situation, there are people who are still *active in research at the time when they become pensioners*, and want to continue their research activities. Their wish is greeted differently according to country. In *a first group of countries*, people must leave their institutions for good the day they become pensioners. Because this is a general rule in these countries, nobody there contemplates the possibility of staying on after retirement. In a *second group*, a small number of top researchers are granted the emeritus status, upon request and for a limited period. As long as they keep their special status, the emeritus pensioners continue to conduct research in their institutions, of course without pay. In a *third group*, active pensioners are welcome to continue conducting research in their institutions, with or without emeritus status and without pay; in these countries, the emeritus status is generally granted for life. *Countries in the first group* consider that retirement is a strictly legal matter. *Countries in the second group* consider that the possibility for pensioners of remaining active in research is a privilege, which must be limited to a very small number of people and granted for a short time only. *Countries in the third group* consider active pensioners a high-quality resource, of which they take full advantage. Most of the latter countries allow pensioned researchers to apply for competitive research grants. These countries also frequently take advantage of the resources neglected by countries in the other two groups, i.e. by inviting active pensioners from the latter countries to conduct research in their own laboratories. The approach followed by countries in the first two groups is wasteful.

The above discussion shows that research careers are multifaceted. Curiously enough, most of the points reviewed above are virtually *never discussed openly* in the scientific community. This may be because the passionate interest of researchers for science, which was mentioned at the beginning of this Section, prevents them from collectively accepting as *genuinely creative* other high-knowledge activities than research. I think that the lack of open discussion on career evolution often causes misunderstandings and frustration, as discussed in the following paragraphs.

How to approach career evolution?

(1) As people get older, the *general shift from research to other high-knowledge activities* must be recognised by the research community as normal, legitimate and beneficial to all. Without such recognition, those who take on a heavier load of non-research high-knowledge work, with or without continuing research, often do so with a sense of guilt. This is because their colleagues and often themselves do not recognise that such work can only be done by people trained as researchers, the whole community needs it to be done, and it may be as *creative* as research. Without proper recognition, the sense of guilt may ruin the *pleasure* that these people should derive from their non-research high-knowledge activities.

(2) The presence of a significant proportion of *mid-career professionally inactive high-knowledge people* should not be considered normal in any organisation. The existence of such people in relatively large numbers in a given organisation is the symptom that the organisation does not properly manage its human resources. The solution to the problem, in organisations in which it exists, may be to offer *retraining* to the ‘drop-outs’. People who are becoming professionally inactive were educated as researchers, and many of them would be delighted to be offered new, challenging high-knowledge tasks after proper retraining. In order for this approach to be successful, however, colleagues must recognise that the general shift from research to other high-knowledge activities as people get older is normal, legitimate and beneficial to the community.

(3) *Pensioners who wish to continue doing research,* without salary, should be greeted with appreciation, as done in some countries, and not simply tolerated with a lack of enthusiasm, as in others. In addition to research, the organisations could ask active pensioners to contribute occasionally to

other high-knowledge activities: teaching, management or consulting. Finally, active pensioners could contribute in filling an unfortunate gap that presently exists in many countries: the writing of high-quality textbooks (see Chapter VII, Section ‘Assessing the Quality of Research: Communication Criteria’); we all know renowned pensioners who are doing it very successfully, in some countries.

Implementing the above ideas, where needed, would require both bottom-up and top-down actions. The *bottom-up approach* would be for professional societies, researcher unions, etc. to recognise and promote the fact that the careers of people initially trained as researchers are multifaceted, and that the various facets are contributing to both the success of the scientific community as a whole and the fulfilment of individuals. The *top-down approach* would be for research organisations to recognise that not fully using the human potential of people who are moving away from research, or have dropped out of high-knowledge work or are still active in research upon retirement, is an unacceptable waste of high-knowledge workers who were trained at great cost. Professional societies, researcher unions, research organisations and other concerned bodies must not only pay lip service to the above, but also recognise *all facets of the researchers’ careers* for promotions, honours, and so forth.

Complementary or Alternative Creative Activities

It was mentioned in Chapter I (Section ‘Creation) that the *strong commitment* of creators prevents them from *dissipating their energies on other things than creation*. Concerning scientists, this could be interpreted as meaning that their lives are exclusively devoted to *research*, and their only form of creation is *scientific discovery*. This is not the case, as discussed in the previous Section, and further developed below.

I explained in Chapter I (Section ‘Knowledge Work’) that researchers not only are *creators*, but also belong to the category of *high-knowledge workers*. I also explained that these workers often develop, aside from their professional activities, a non-competitive life and a community of their own. This *non-professional life and community* provide them with opportunities for personal contributions and achievements other than professional.

The above statements on the characteristics of creators and those of high-knowledge workers may seem contradictory: *creators* are strongly committed to creation, and *high-knowledge workers* often develop a non-professional life. One may wonder how researchers, who are both creators and high-knowledge workers, manage to reconcile these two seemingly incompatible characteristics. The apparent contradiction is resolved when one realises that researchers generally use their *creative abilities* not only in their *professional work*, but also in *non-professional activities*.

Several, if not most, researchers are engaged in some *non-professional creative activities*. I know colleagues who are actively involved in such artistic activities as painting, sculpture, photography, music, amateur theatre, choral singing or novel writing. Others express their creativity through cooking or wine-making. Others like to work manually, e.g. building or renovating houses, making furniture or sewing. These non-professional activities may be occasional hobbies or major passions, and therefore make up variable proportions of the researchers' creative activities.

Fig. 28 provides a schematic representation of *changes in creative activities* as researchers get older. These activities may be both professional and non-professional. The first were discussed in the previous Section. They generally belong to high-knowledge work, and they cover both research and

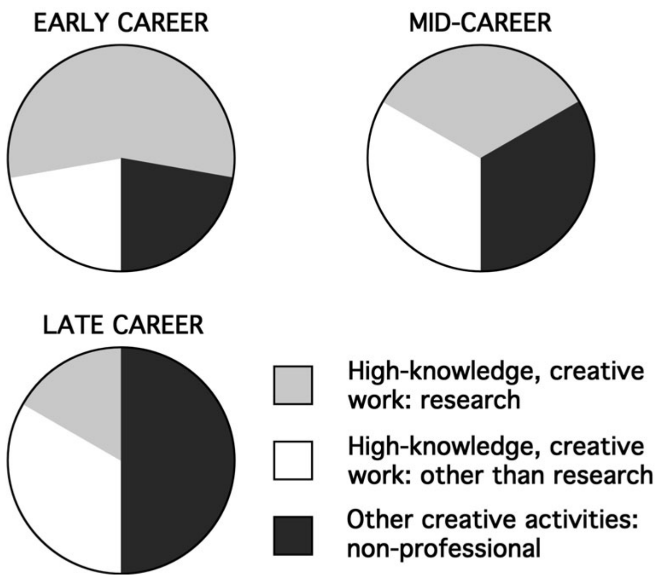


Fig. 28. Schematic representation of changes in creative activities as researchers get older. These activities may be both professional and non-professional. Professional activities, which generally belong to high-knowledge work, include research and activities other than research, e.g. teaching, management, consulting. The relative importance of the three types of activities varies among scientific fields, countries and individuals. This framework is conceptual: it is not based on actual data (Original)

activities other than research, e.g. teaching, management, consulting. The second was briefly described in the previous paragraph.

Even though the relative importance of the *three types of activity* varies among scientific fields, countries and individuals, Fig. 28 suggests some general trends. *In early career*, research generally dominates over other creative activities, i.e. high-knowledge work other than research and non-professional creative pursuits. *As the career progresses*, there is often an increase in the proportion of high-knowledge activities other than research, as discussed in the previous Section, and also an increase in the proportion of non-professional creative activities. *In late career*, the latter may become dominant. *After retirement*, except in the case of pensioners who continue doing research (previous Section) and/or high-knowledge work other than research (e.g. consulting), creativity is fully dedicated to non-professional activities.

The frequently observed increase, as people age, in the proportion occupied by non-professional creative activities reflects a combination of factors. One may be a *progressively growing lack of interest for professional activities*, as discussed in the previous Section. Another factor may be a *mounting interest in non-professional activities*: by practicing such activities, one becomes better at them and thus wishes to devote more time to them. Finally, several people purposely get involved in such activities as *preparation for retirement*, i.e. getting ready to shift from professional to non-professional activities when becoming pensioners. Whatever the nature and proportion of non-professional activities in creative pursuits, they provide much *pleasure* and generally enrich life. Hence, young people interested in science should be encouraged to develop their *creativity* not only in research, but also in other pursuits corresponding to their tastes.

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Glossary

The definitions of words and expressions given below are drawn from the present text; for each definition, the relevant Chapter is identified in parentheses. These definitions are personal syntheses of information from dictionaries, encyclopaedias, books and articles.

ARTS. The arts include music, performing arts (dance, opera, signing, theatre, etc.), visual arts (cinema, drawing, painting, photography, sculpture, etc.) and writing (literature) (Ch. I).

CONJECTURE (mathematics). Assertion based on patterns observed in several instances, which is believed, at least by some, to be generally true but has not been proved (Ch. II).

CONSILIENCE. Convergence of knowledge from different disciplines (Ch. I).

CREATION. Production of original works through imaginative skills (Ch. I).

CREATIVE IMAGINATION. Combination of intuition, craftsmanship (or methodology) and pleasure, for the production of original works (Ch. III).

CREATIVITY, SCIENTIFIC. Practice of scientific discovery (Ch. I).

CULTURE. Whole intellectual aspects of civilisation, including science (Ch. IX).

DISCOVERY PROCESS. The process leading to discovery requires a pertinent question, the ripeness of time and creative imagination; the latter combines intuition, the scientific method and pleasure (Ch. III).

DISCOVERY, SCIENTIFIC. Finding, with imaginative skills, new phenomena, new mechanisms, new laws, new theories or new paradigms, without taking any assumption as being true *a priori*; the central characteristic of discoveries is novelty (Ch. II).

ECO-ETHICS. Theory of human actions, as subjected to duty toward Nature—to which humans belong—and aiming at compatibility between Nature and humanity, which provides rules of conduct and behaviour for interacting with the natural environment (Ch. IX).

ETHICS. Philosophical theory of moral, which provides rules of conduct and behaviour (Ch. IX).

FALSIFICATION (OF HYPOTHESIS). Rejection (of hypothesis; see HYPOTHESIS, FALSIFIABLE) (Ch. II).

HEURISTIC. Technique that provides a way of thinking about a problem, which follows the paths most likely to lead to the goal, leaving less promising avenues unexplored (Ch. VIII).

HOLISM. Treatment of complex systems as whole entities, because systems have (emergent) properties different from those of their parts (Ch. II).

HUMANITIES. The Humanities include the classics, history, history of art, language, literature and philosophy; some specialists include the arts in the Humanities (Ch. I).

HYPOTHESIS, ALTERNATIVE (symbolised H_1). Hypothesis to be provisionally accepted when the null hypothesis (H_0) is falsified, or rejected; the eventual falsification of H_0 may open the possibility of a large number of alternative hypotheses (Ch. II).

HYPOTHESIS, FALSIFIABLE. A hypothesis is said to be falsifiable if there exists at least one possible alternative hypothesis (Ch. II).

HYPOTHESIS, NULL (symbolised H_0). A null hypothesis is a falsifiable hypothesis that specifies a model that can be used to generate realisations of H_0; the distribution of these realisations is used to test H_0 for significance (Ch. II).

HYPOTHETICO-DEDUCTIVE METHOD. Approach that depicts research as an alternation between two phases: creation of hypotheses, and evaluation of deductions from the hypotheses (Ch. II).

INSPIRATION. Creative drive of artists, writers and researchers (Ch. VIII).

INTUITION. Combination of ordinary abilities that are pushed by creators to extra-ordinary levels, and also perhaps innate abilities; intuition is largely innate, and it can be either cultivated or squandered (Ch. I).

IONIAN ENCHANTMENT. Belief in the unity of science, i.e. conviction that the world is orderly and can be explained by a small number of natural laws (Ch. I).

KNOWLEDGE. Body of information acquired by humankind (Ch. I).

KNOWLEDGE WORK. Application of theoretical knowledge to practical issues (Ch. I).

LAW. Statement—often a mathematical function—of a relation among phenomena that, so far as is known, is invariable under given conditions (Ch. II).

MECHANISM. Combination of the fundamental processes involved in, or responsible for a set of phenomena (Ch. II).

METHOD, SCIENTIFIC. Practice of scientific research; rational appraisal of scientific discoveries (Ch. I).

MODEL. Simplified representation of Nature (Ch. II).

MORAL. Theory of human actions, as subjected to duty and aiming at good (Ch. IX).

NATURE. Physical environment and living organisms, including human beings (Ch. II).

PARADIGM (also called RESEARCH PROGRAMME). A paradigm consists of: a hard core of theory, auxiliary hypotheses that form a protective belt around the core, rules that specify which paths of research to avoid and which to pursue (Ch. II).

PHENOMENON. Observable fact or event (Ch. II).

PLEASURE. State of gratification resulting from the explosion of tension, and/or the catharsis of self-transcending emotions (Ch. III).

REDUCTIONISM. Decomposition of complex phenomena or systems into simpler components, which are themselves governed by general laws; the latter often come from physics or chemistry (Ch. II).

RESEARCH, APPLIED. Activity in which researchers work at resolving well-identified problems or fulfilling precise needs (Ch. VII).

RESEARCH, BILATERAL. Collaborative research that involves two countries (Ch. X).

RESEARCH, CURIOSITY-DRIVEN. Activity in which researchers set work objectives without any constraint from the sponsors (Ch. VII).

RESEARCH, INTERNATIONAL. Collaborative research that is conducted under the purview of an international organisation (Ch. X).

RESEARCH, MULTILATERAL. Collaborative research that involves several countries, outside the framework of an international organisation (Ch. X).

RESEARCH PROGRAMME: see PARADIGM.

RESEARCH, SCIENTIFIC. Activity of creating the universal knowledge of science, through scientific discoveries (Ch. II).

RESEARCH, TARGETED. Activity in which researchers set work objectives in accordance with problems or needs pre-determined by the sponsors; these pre-determined problems or needs are the targets (Ch. VII).

SCIENCE, NATURAL AND SOCIAL. Universal knowledge acquired through scientific discoveries. In other words, universal knowledge, acquired by imaginative skills, of new phenomena, new mechanisms, new laws, new theories or new paradigms, without taking any assumption as being true *a priori* (Chs. I and II).

TAUTOLOGY. In research, a logical tool that identifies the range of possibilities under given premises (Ch. II); because it covers the whole range of possibilities, a tautology has no predictive power (Ch. IV).

THEORY, SCIENTIFIC. Body of, at least partly, hypothetical statements, which refers to a small number of principles, and represents as simply and completely as possible the relevant phenomena, mechanisms or laws (Ch. II).

THEORY, SCIENTIFIC (*sensu* Peters 1991). Construct that makes potentially falsifiable predictions (Ch. II).

TRUTH, ABSOLUTE. Interpretations of observed phenomena that would be the same for all researchers, and for all times (Ch. I).