

Concept-Driven Revolutions and Tool-Driven Revolutions: Dyson, Kuhn and Galison

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Abstract: Freeman J. Dyson has introduced the notion of *tool-driven* revolution that stands in contrast to the *concept-driven* revolutions analysed by Thomas Kuhn in *The Structure of Scientific Revolutions*. We study Dyson's thesis, pay special attention in the interesting Dyson's idea of scientific tool and compare Dyson's point of view with Peter Galison's conception, as developed in *Image and Logic*. It seems that the differences between them are slightly stronger than Dyson suggests. Dyson's ideas yield some where between Galison and Ian Hacking, whose notion of disunity of science seems to be related to the abundance of tools and tool-driven revolutions Dyson has pointed.

Freeman J. Dyson, the retired British scientist, who has spent most of his scientific career in Princeton at the Institute for Advanced Studies, speaks in his recent books (Dyson, 1997, 49-55; 1999, 13-21) about the existence of *tool-driven revolutions*, in contrast to the *concept-driven revolutions* analysed by Kuhn in *The Structure of Scientific Revolutions*.

Tool-driven revolutions arise in relation to the invention of new tools or instruments designed to investigate nature and discover new facts that challenge our previous concepts. According to Dyson (1997, 50), whereas Kuhnian revolutions provide some new concepts to understand nature, “explain old things in new ways,” *tool-driven revolutions* allow us to discover “new things that have to be explained.” Dyson states that this kind of revolution has been decisive in the recent development of most sciences, particularly in main fields such as biology¹ and astronomy, and that most of the latest scientific revolutions have been tool-driven.

In *The Sun, the Genome, the Internet*, Dyson (1999, 13-14) compares Harvard's historian of science, Peter Galison² and his *Image and Logic*, with Thomas Kuhn's

¹ It is interesting to compare the point of view of Dyson with that of Steve Rose (1998, 48), who has written: “Biology offers fewer examples of either grand paradigms or paradigms-breaking experiments, presumably because we deal with much more varied and complex phenomena than are found in physics. Our paradigms tend to be rather smaller in scale, more local, less universalistic. There is no equivalent in biology to Newton's laws of motion. At least there seemed not to be until the 1990s, when efforts have been made to elevate so-called ‘universal Darwinism’ to a kuhnian paradigm into which all phenomena of life must be shoehorned. A subparadigm within universal Darwinism is the DNA theory of the gene and replication. Thus, in the afterglow of Kuhn's book the historian of science Robert Olby retold what he called ‘the path to the double helix’ as an account of replacing a previous, protein-based theory of life with the new DNA-based paradigm.”

² Galison (1987, ix) has written: “Despite the slogan that science advances through experiments, virtually the entire literature of the history of science concerns theory. Whether the scholars have looked to the seventeenth-century scientific revolution, to the nineteenth-century field theory, or to twentieth-century relativity and quantum mechanics, the histories they write highlight the evolution of concepts, not laboratory practice. Paraphrasing Einstein, there seems to be an asymmetry in historical analysis not present in the events themselves.”

work, emphasizing their common evolution (first physicists, later historians of science).³ According to Dyson, Kuhn describes physics like a theoretician who focuses on ideas, whereas Galison emphasizes scientific instruments. Dyson believes that Galison's work restores the balance in our vision of science. Dyson states that Kuhn spread the opinion that all scientific revolutions were of a conceptual nature, creating a unilateral vision of science. Some thinkers concluded that science is, to a great extent, subjective, just another battle between conflicting points of view.⁴ Dyson, without denying the value of the Kuhnian view, maintains (1999, 14) that "the progress of science requires both new concepts and new tools."

In this article, after a brief exposition of Dyson's main ideas (and especially his notion of "tool"), we consider his analysis of *tool-driven revolutions*, which involves answering three questions:

What is a tool-driven revolution?

Which episodes in the history of science can be characterized as *tool-driven revolutions*?

Why do these revolutions seem to provoke a lower level of interest than *concept-driven revolutions*?

In Dyson's view (1999, 7-9) modern science originates from the fusion of two traditions, that of Greek philosophical thought and that of the craftsmen who flourished in the Middle Ages. Dyson, in spite of his theoretical training, prefers the craftsmanship of his profession and states that (1999, 14): "Science for me is the practice of a skilled craft, closer to boiler-making⁵ than to philosophy." Dyson (1990, 7) states that science tends, at the moment, to take more and more care of facts and particular phenomena instead of providing a unifying vision of reality. Dyson (1990, 43) sees two different traditions in the history of science: the unifiers, who follow Descartes' trail, and the diversifiers, who lean, rather, towards Bacon. The unifiers try to reduce the lavishness of nature to a few laws and general principles. The diversifiers prefer to explore the details of things in their infinite variety. The unifiers love equations, such as superstrings, whereas the

³ Notice that Dyson's *Imagined Worlds* was published in 1997, before *Image and Logic*.

⁴ Dyson claims that (1999, 16) "Kuhn never said that science is a political power struggle. If some of his followers claim that he denied the objective validity of science, it is only because he overemphasized the role of ideas and under-emphasized the role of experimental facts in science. He started his career as a theoretical physicist. If he had started as a biologist, he would not have made that mistake. Biologists are forced by the nature of their discipline to deal more with facts than with theories." Kuhn himself, on the contrary, stated (1970, ix): "Far more historical evidence is available than I have had space to exploit below. Furthermore, that evidence comes from the history of biological as well as of physical science. My decision to deal here exclusively with the latter was made partly to increase this essay's coherence and partly on grounds of present competence."

⁵ The profession of his grandfather in Yorkshire (Dyson 1999, 8).

diversifiers prefer the peculiarity of unique things, such as butterflies.⁶

This image of science implies that discovery is the fundamental scientific event. Thus, Dyson praises technology because it allows us to make unexpected discoveries that help to formulate new questions. Dyson claims (1994, chap. 3, 43) that there is no illusion more dangerous than to believe that the advance of science is predictable, because if we search for the secrets of nature in a single direction, we will fail to discover the most important secrets, precisely those that our imagination is unable to foresee. In spite of the unpredictability of scientific progress, Dyson believes that a suitable scientific and technological policy can enhance it, and offers us a reflection on the ecological⁷ aspects of technological projects in order to optimise rates of growth and technological investment. Dyson focuses on tools for two reasons; first, advances in instrumental scientific aspects are easier to imagine and to plan and, second (1994, chap. 9, 14; 1997, 89-90), *tool-driven revolutions* follow one another in shorter temporary cycles than any others.

Scientific Instruments and Tool-Driven Revolutions

Many scientists have insisted, in a similar manner, on the importance of instruments and their development for the advance of science. We will mention some recent examples.

Charles Townes, inventor of the maser and the laser, talking about the spectroscopy of microwaves (Sánchez Ron, 2000, 26-29), highlights the fact that historical development worked in exactly the opposite sense to what might have been expected. In most cases it is assumed that pure science develops from principles and ideas, that are soon translated into equipment applications and instruments. But Townes claims that, in this case, the opposite occurred: the tools were developed first, so pure science was considerably indebted to technology.

Chemistry's Nobel Prize-Winner, Max Perutz (1990, 242), has emphasized how Frederick Sanger began to explore the genome of diverse organisms without any prior conception on which to base his discoveries and without any clear idea of how he was going to find out what he wanted to know. Perutz emphasizes that Sanger did not proceed in accordance with the Popperian ideal, but that he devoted himself to inventing new chemical methods able to solve problems that nobody had faced up until that time, problems that were believed to be irresolvable. By proceeding in this way, he did not verify his experiments with previously existing

⁶ Dyson (1990, 14). "Butterflies are at the extreme of concreteness, superstrings are at the extreme of abstraction."

⁷ We have tackled this aspect before: see our paper (2002).

paradigms. He opened up new worlds for which no paradigms actually existed.⁸ Before Sanger mapped the genome of the virus ϕ - X 174, nobody had thought that some genes could overlap. It is interesting to note that Dyson (2000, 48-52) also mentions this very case and insists that Sanger decided to study this genome not because of his interest in the virus, but as an exercise for the new sequencing methods he was inventing and developing.

The complexity of the normal equipment found in a scientific laboratory increased enormously throughout the twentieth century. As C. P. Snow has emphasized, after Rutherford nobody could continue to pursue experimental research with “sealing wax and string.”⁹ The increasing complexity of instruments, along with the growth of the scientific community, has led many men of science to devote themselves professionally to tool manufacturing. Among these professionals we can find numerous testimonies regarding the importance of tools in the development of science. For example, Gerhard Kremer, the former head of the International Bureau of Packard Instruments in Zurich, speaks of “research-enabling technology” (Rheinberger, 1998, 2) when talking about technologies that open up new fields of research and will even allow us, paradoxically, to answer questions that have not yet even been posed. Apart from this new category of tool makers, the ability to make tools or utensils is considered to be a virtue by numerous scientists and is still regarded to be a valuable skill for every experimental worker. In this respect, Otto Frisch (1982, 258) has claimed that he was always more attracted by the design of scientific tools than by the results that could be obtained with them. And Perutz (1990, 209) reminds us how Rutherford was mourned by the poor men who did not have laboratories in which to work.¹⁰

Dyson believes (1999, 9-13) that the construction of innovative tools and the development of new technologies are inherent to scientific research and that there

⁸ Perutz also provides another example that fails to match the Popperian ideal: Dorothy Hodgkin’s discovery of the three-dimensional structure of insulin.

⁹ “Rutherford himself never built the great machines which have dominated modern particle physics, though some of his pupils, notably Cockcroft, started them. Rutherford himself worked with bizarrely simple apparatus: but in fact he carried the use of such apparatus as far as it would go. His researches remain the last supreme single-handed achievement in fundamental physics. No one else can ever work there again –in the old Cavendish phrase– with sealing wax and string” (Snow, 1967, 595). Galison also underlines this tendency (1987, 14): “... makes it impossible to ignore the vast physical changes in the experimental environment during our period of interest. Through the 1930s [...], most experimental work could be undertaken in rooms of a few hundred square feet, with moderate, furniture-sized equipment. But at Fermilab [...] a herd of buffalo actually grazes in the thousand-odd acres surrounded by the main experimental ring, and individual detectors can cost millions or tens of millions of dollars.”

¹⁰ It is interesting to observe that Perutz (1990, 215) also talks about Cavendish’s “sealing wax and string” style and of the poverty of his equipment, that still persisted under the direction of Bragg when he arrived. This poverty was partly due, in Perutz’s opinion, to the strict economy that Rutherford, and also Bragg, imposed. Rutherford never seemed to worry about financing his investigations and Perutz expresses the opinion that Rutherford would have disapproved of the manoeuvres of geneticists to obtain such amounts of money. Dyson (1994, chap. 13) presents an interesting perspective of the change of direction that Cavendish’s laboratory witnessed after Rutherford’s death.

will always be young people ready to build new tools that enable them to stretch the frontiers of science. This task will give rise to new craft industries that furthermore “find uses in the world outside.” Dyson observes that great complexes of craft industries have flourished “around every large centre of scientific research.”

Dyson also presents a highly original and wide-ranging idea of the scientific tool. He not only mentions the classic cases, such as telescopes or microscopes, or the sophisticated instruments that predominate in experimental work today. His conception of the scientific instrument even includes natural entities such as viruses or pulsars. Dyson explains that the virus (1999, 20) “is a tool, not a theory,” because it “is a tool for the practice of medicine as well as for the advancement of science.” The virus allows us to gain a better knowledge of more complex and larger creatures because “to infect a cell with a virus is nature’s way of doing an invasive surgical intervention.” Other qualities such as homogeneity, specificity, malleability, speed of reproduction, cheapness, etc., turn the virus into a suitable tool for biological research. Dyson also sees pulsars as natural accelerators that will provide us with cosmic rays and laboratories to study the properties of matter and radiation.

We can find other interesting shades of this idea in the case of the computer. On the one hand, Dyson (1997, 51) states that “the computer is a prime example of an intellectual tool. It is not a concept but a tool for clear thinking. It helps us to think more clearly by enabling us to calculate more precisely.” Thus, a scientific tool is not only considered to be something that strengthens our senses or is useful in taking measurements, but also as an aid to our understanding. He claims (1997, 51) that the computer

“has also had a revolutionary effect in narrowing the gap between mathematics and theoretical physics.”¹¹

Dyson believes that the computer is potentially able to generate many more scientific tools (and revolutions). In the future many new instruments will originate from the software craft industry. In the future there will be numerous opportunities to design different scientific software programs of considerable use for research. Dyson indicates that, nowadays, digital astronomy, with projects such as the Sloan Digital Sky Survey, or biotechnology, that require cheap, handy,

¹¹ Dyson seems to imply that the facilities of communication, accessibility to publications, facilities for handling data ... that the computer has brought, have led to a revolution in science as a whole. In the same sense it would be possible to say that the letter in the sixteenth century, that constituted the usual means of communication among European scientists, or scientific journals were and continue to be (the letter was replaced by faxes and e-mails and traditional journals are being replaced by digital editions and the rapid digital files of “preprints”) scientific tools that, at the time produced scientific revolutions and changes in style in the way science is pursued. These tools commonly revolutionize science by accelerating its rates of discovery. This idea, represented in more or less implicit form in Dyson, reminds us of the concept of the *killer application* that Bill Gates introduced to explain the enormous popularity of the personal computer.

reliable software programs, digital databases and libraries, are already offering software engineers many opportunities to develop new tools. Thus, Dyson's concept of the scientific instrument not only includes those tools that the scientist uses for the direct study of nature, but also encompasses those that have changed scientists' lives.

However, some scientists do not like this political term and believe it is absurd to talk about science revolutions. It is not difficult to find figures as well-known as Richard Lewontin (1983) who criticize the enthusiasm with which some Lenins of the laboratory have embraced the idea of revolution.¹² On the other hand, in 1998 Steven Weinberg¹³ presented some arguments against Kuhn's ideas that have had a certain influence:

“Nor do scientific revolutions necessarily change the way that we assess our theories, making different paradigms incommensurable. Over the past forty years I have been involved in revolutionary changes in the way physicists understand the elementary particles that are the basic constituents of matter. The greatest revolutions of this century, quantum mechanics and relativity, were before my time, but they are the basis of the physics research of my generation. Nowhere have I seen any of Kuhn's incommensurability between different paradigms. Our ideas have changed but we have continued to assess our theories in pretty much the same way: a theory is taken as a success if it is based on simple general principles and does a good job of accounting for experimental data in a natural way.”¹⁴

¹² “Scientists are infatuated with the idea of revolution. Even before the publication of Thomas Kuhn's *The Structure of Scientific Revolutions*, and with ever increasing frequency after it, would-be Lenins of the laboratory have daydreamed about overthrowing the state of their science and establishing a new intellectual order. After all, who, in a social community that places so high a value on originality, wants to be thought of as a mere epigone, carrying out "normal science" in pursuit of a conventional "paradigm"? Those very terms, introduced by Kuhn, reek of dullness and conventionality. Better, as J.B.S. Haldane used to say, to produce something that is "interesting, even if not true."

¹³ Weinberg counters the “Kuhnian view” that there is no scientific progress outside normal science. Weinberg, along with Dyson, agrees with some Kuhnian ideas: while Dyson focuses on the concept of scientific revolution, Weinberg emphasizes the concept of normal science.

¹⁴ It is also interesting to compare the opinions of Weinberg with the analysis of particle physics that Dyson makes (1994, chap. 4, 1997, 55-61).

Dyson distinguishes two great phases in the history of particle physics. First there was a *Tolstoyan* phase of studying cosmic rays. It was dominated by the European groups of Conversi, Pancini and Piccioni in Italy, Cecil Powell in Bristol and Rochester and Butter in Manchester. They worked with old-fashioned, home-made apparatus, like particle counters, microscopes and photographic plates and cloud chambers. The second and *Napoleonic* phase began with the construction of the first particle accelerators in the USA (Berkeley, Cornell and Chicago). More and more powerful accelerators were constructed to produce new particles. This phase presumably finished with the cancellation of the Superconductor Supercollider (SSC) project in 1993. Dyson also points to diverse innovations in the history of particle physics that could be described as *tool-driven revolutions*, such as, for example, Don Glaser's bubble chamber and Gerard O'Neill's storage rings.

Dyson's claims regarding the role of *tool-driven revolutions* should not be viewed as a complete criticism of Kuhn's ideas (at least at first sight), but as a call to rethink their importance, value and usefulness. As we have already indicated, he offers a large number of possible examples that neither Kuhn considered originally¹⁵ nor subsequent analyses have sufficiently taken into account. It is no small indication of the importance of technology for science that, except for highly biased analyses, it is widely recognized by everyone.¹⁶

In *Infinite in All Directions* Dyson quotes Lynn White's paper "Technological Assessment from the Stance of a Medieval Historian" in order to explain how to evaluate the influence of a particular technology on the development of human life (1990, 137):

"Technology assessment, if it is not to be dangerously misleading, must be based as much, if not entirely, on careful discussion of the imponderables in a total situation upon the measurable elements."

This statement, that we might call the Dyson-White criterion, gives us a clue to understanding Dyson's idea of the scientific tool and how to measure the revolutionary character of the improvements, innovations and changes propitiated by each new tool.

This criterion suggests a set of precise questions to judge the role played in the scientific progress by a certain instrument. Questions such as the following:

Could the same scientific successes have been achieved without the instrument in question or another instrument of the same characteristics? To what extent did this particular tool or instrument accelerate the discoveries? How did it impel theorization? Could the same results have been reached through a mere theoretical approach? These and other similar questions place us on the trail of more complex

In spite of the *disaster* of the SSC, Dyson is quite optimistic and expects a new Tolstoyan phase. This new phase could be dominated by the new underground detectors constructed to study particles from the Sun or by new acceleration techniques based on the laser. Dyson reminds us that the world of particle physics is three-dimensional and that the construction of more and more powerful accelerators advances only in terms of energy dimension, ignoring the parameters of peculiarity and precision.

Weinberg, who without a doubt belongs to the unifying tradition, explained (in 2001) how his vision of the history of particle physics is conditioned by his conception of nature and history, being comparable to that of the Western religions. He has also criticized (see for example Horgan, 1993, 32) the cancellation of the SSC on numerous occasions.

¹⁵ Thomas Kuhn (1970, x) indicates in the preface: "I have said nothing about the role of technological advance or of external social, economic and intellectual conditions in the development of sciences."

¹⁶ Anyway, Kuhn's attitude towards the role of tools was restrictive (1970, 76): "As in manufacturing so in science –retooling is an extravagance to be reserved for the occasion that demands it. The significance of crises is the indication they provide that an occasion for retooling has arrived."

analyses of the advance of knowledge and enlighten us regarding the reciprocal influences between different fields and schools.

These questions and their answers, that can often be too speculative at first, can clarify what the landmark was that produced the revolution. They can show the essentially revolutionary nature of the scientific progress in question and decide whether the intrinsic characteristics of the tools or of the experimental techniques were essential or not.

Nevertheless, in order to speak accurately of *tool-driven revolutions* we must analyse some cases. Dyson cites several examples: Galileo's telescope, the three revolutions (X-rays, crystallography, spectroscopy of microwaves, microwaves, astronomical observations) that John Randall undertook, the computer science programs of the Polish astronomer Alexander Wolszczan (1999, 22-26), the protein and DNA sequencing methods of Frederick Sanger (1999, 26- 33), and so on.

Let us consider the case of Wolszczan by following Dyson's account closely. Alexander Wolszczan is a radio-astronomer and lecturer at Pennsylvania State University, who recorded the existence of an extrasolar planet family for the first time in 1992. The most important of the new instruments that facilitated Wolszczan's discovery was the software he used. It is worth noting that, in many scientific fields, new computer science programs are nowadays more important to research than the most powerful computers. Astronomy is one of these fields; thanks to the new computer science technologies, scientists are making some very interesting discoveries with telescopes that seemed obsolete. For example, the telescope that Wolszczan used for his observations, the great radio telescope of Arecibo (Puerto Rico), is already forty years old, but it was complemented by a new computer science program that had been written specifically to examine the pulsations of irregular and extremely weak radio waves from a millisecond pulsar. Another illustration of the tool-driven character of Wolszczan's discovery can be found in the fact that the credibility of his discovery depended completely on the credibility of his computer science programs. In order to convince his colleagues that the planets were real, he had to convince them that his software programs were entirely flawless.

The revolutionary character of his discovery consists of his ability to disprove the previous belief that planets could not exist by revolving around a millisecond pulsar and of the fact that, over the five years that followed, other astronomers were able to discover new planets thanks to his tools and methods. Strangely enough, these new discoveries differed from Wolszczan's in two respects. First, the planets belonged to stars, like the Sun, and not to millisecond pulsars. Second, the planets had much greater masses, several hundred times the mass of the Earth, unlike Wolszczan's planets, whose masses were only three times those of the Earth. These differences highlight the narrow link that exists between the scientific instrument and the discoveries that it makes possible. It was inevitable that the new planets would have greater masses. For planets revolving around an ordinary

star to be detectable, they must have a large mass when compared to Jupiter's. At the moment, planets with masses similar to that of the Earth can only be detected if they belong to millisecond pulsars: it is no coincidence that the only planets discovered until now with masses similar to the Earth's are those discovered by Wolszczan.

The future of this new field depends on new *tool-driven revolutions*, since after these latest discoveries all astronomers now believe that the universe is full of planets with earth-like masses orbiting around stars like the Sun. These planets are of greatest interest from a human point of view, but it will be impossible to discover them until we have new instruments or new techniques of observation.¹⁷

The case of medical physicist John Sidles, from the University of Washington in Seattle, is also interesting (Brand, 1998). Sidles works on the development of what he has called Magnetic Resonance Force Microscopy (MRFM), a new piece of apparatus designed to examine human tissues and molecules. Sidles' research arises from the deficiencies of present methods within his field. X-rays and Magnetic Resonance Imaging (MRI) are highly penetrative, but have the disadvantage of offering very poor resolution. On the contrary, the atomic force microscope offers excellent resolution but it is only useful in examining surfaces; it has zero penetration. Sidles has designed an apparatus, of which some prototype has already been built, that combines both qualities. In this case his interest and inspiration are not motivated by any theoretical problem, but by a practical one. Furthermore, the development of MRFM technology would allow many new research directions and be distributed world-wide throughout medical centres, eventually replacing the older systems, if it is cheap enough.

¹⁷ In fact, Wolszczan's discoveries can be included within a more general astronomical revolution. Dyson (1997, 66-77; 2000, 52-67) explains how present astronomy is undergoing a revolution based on the union of three different traditions: the old culture of the great optical telescopes, the culture of electronics and that of software engineering (1997, 68). The CCD, or Charge Coupled Device, created in 1948 (like the electronic telescope) by the Swiss astronomer Fritz Zwicky, is a clear example of this symbiosis. Its main advantage is that the images can be recorded on a digital support. It represents an evolution from the chemistry-based qualities of photographs to the physics-based qualities of digital records. The technology of the CCD and digital image-processing have been able to optimise the results of optical telescopes that years ago seemed obsolete. This means that projects such as the Digital Sky Survey can be carried out with a telescope of ample field measuring 2.5 meters built in Apache Point, New Mexico, featuring a total cost (including the telescope) of 14 million dollars and a duration of five years. In addition, the revolution of astronomy has had sociological consequences, changing researching styles. First, the great observatories of the planet have been connected to the Internet and share all their information; they can be used from any place around the globe and, nowadays, it is very easy to coordinate them in original projects that require the almost constant surveillance of huge expanses of the sky. For example, the PLANET project, coordinated by astronomer Penny Sackett, is searching for new invisible planets. Whenever a possible planet microlens is discovered in the Large Magellanic Cloud, the coordination of the four observatories on the project enables a continuous watch of the object to be maintained. Second, thanks to the CCD and the personal computer, the gap between amateur and professional astronomers has been narrowed. Nowadays there are fields of astronomical research in which the abundance of instruments and the almost limitless observation time of amateurs could be tremendously helpful for professional astronomers. In 1992 Dyson proposed Occultation Astronomy as a possible field of collaboration.

In order to understand Dyson's idea of tool-driven revolution, it might be useful to talk about two instruments thought up by Dyson: the table-top DNA-sequence analyser and the protein-structure analyser. The DNA-sequence analyser that Dyson proposes would be based on the physical properties of DNA (for example, its different atomic mass) and not on wet chemistry,¹⁸ like the current methods. The idea would be to save time and money by avoiding some stages of the sequencing process. Dyson's hypothesis is that with rapid and even cheaper methods, sequencing would require less effort and this effort could be devoted to the interpretation of the still greater mass of data that would be obtained. The ultimate aim would be to accelerate the rate of the discovery and hope that these discoveries would be sufficiently abundant and interesting to be able to open up and create new fields.

Another case of tool-driven revolution not mentioned by Dyson can be found during the early days of brain science. Golgi's silver nitrate stain enabled the discovery of Golgi's organ. Furthermore, when improved by Cajal, this was the tool that changed the course of neurology. Cajal's work can be taken as a clear example of tool-driven revolution, even though the microscope, his actual tool of work, did not evolve during his lifetime.¹⁹ In fact, Cajal's main contribution was not to improve the instrument itself, but to establish improvements and new methods to dye the microscopy preparations.

Cajal's contribution was much more technical than doctrinal, but its implications dramatically changed the future of the brain sciences. The effect of Cajal's contribution was to demolish, with technical improvements, the commonest belief among his colleagues, who

“because of the weight of the theory (the main histologists) saw webs everywhere” (1981, 52).

The situation at the beginning of his career was described by Cajal as follows “the analytical resources were very poor to attack this great and exciting problem” (1981, 53). Cajal studied and practiced the techniques available at the time and became aware of their limitations. His diagnosis was quite categorical:

“We lack the powerful tool to open the deep mystery of grey matter”²⁰ (1981, 54).

¹⁸ The goal, as in the case of the CCD, would be to evolve from chemical to physical methods.

¹⁹ As Hacking said (1983, 192): “Many of the chief advances in microscopy have nothing to do with optics. We have needed microtomes to slice specimens thinner, aniline dyes for staining, pure light sources, and, at more modest levels, the screw micrometer for adjusting focus, fixatives and centrifugates.”

²⁰ Both Cajal's quotations have been freely translated by the authors from the original:

-“subyugados por la teoría, los principales histólogos veíamos entonces redes por todas partes;”
-“faltábamos el arma poderosa con que descuajar la selva impenetrable de la sustancia gris.”

His dedication and conviction regarding the need to obtain that definitive weapon were the key to the triumph of neuronal theory over the old reticular hypothesis.

Cajal's case can be seen as an early example of the importance of the instrumental factor in the contemporary development of biology. It is easy to find, in the bibliography that has analysed the success of biology after the discovery of the double helix, statements that support Dyson's analyses. First we might mention Robert Olby's *The Path to the Double Helix* (1974), an accurate history of the discovery of the structure of DNA and the biological "revolutions" witnessed during the second half of the twentieth century. This book, written in Kuhnian language, emphasizes the importance of experimental and instrumental aspects in the shifting paradigm from "the version of the protein of the central dogma" to "the version of the DNA of the central dogma". The work's conclusion includes a section devoted to analysing the role of methods and instruments in the birth of molecular biology.

Olby (1974, 25) has also underlined other important factors in the foundation of molecular biology, such as

"intellectual migrations which brought physicists and structural chemists into biology"

and the fusion of two traditions or schools: the structural school and the information school. These changes were, according to Olby, strongly influenced by factors outside the field of biology, mainly by the financing provided by the Rockefeller Foundation, and they produced and were also accompanied by

"a new kind of *professionalism* (italics in the original) marked by the demand that explanations must stand up to the rigorous standard of the new quantum physics (1974, 29)."

Olby's great work has enormously influenced later research on the history of molecular biology and develops ideas similar to those proposed by Dyson. Another classic study of the development of molecular biology, by Horace Freeland Judson (1996), also describes the origin of the discipline as a scientific revolution²¹. In an epilogue added to the 1996 reprint, Judson reviews the later development of molecular biology. Although at the end of the sixties the pioneers of molecular biology accepted Crick's view (Judson, 1996, 592), that future work in the field would consist of

²¹ Although Judson differs from Olby when characterizing the core of this revolution (Judson 1996, xx), Judson does not consider the key factor to be the changing conception of the nature of the gene. Judson claims that the key is the agreement about what "biological specificity" is. For Judson the elucidation of the molecular nature of the gene and Frederick Sanger's work (that established the sequence of amino acids of bovine insulin in the mid-fifties, denying the possibility that proteins had some type of periodic structure), were decisive because they forged the way towards the concept of biological specificity.

“filling in all of the biochemistry so that what we know in outline we also know in detail,”

the extension and application of the ideas of molecular biology to the study of superior organisms produced new revolutions in which the technological factors were of considerable importance. As examples of these revolutions, Judson mentions the technique of the recombinant DNA that arose from the research undertaken by Baltimore, Temin and Mizutani and the new techniques for sequencing nucleic acids developed by Sanger (who received his second Nobel Prize as a result). Thus, Judson states: (1996, 600)

“much of the most interesting work done from the nineteen-seventies onwards has been in the development of technical methods: recombinant DNA began and defines this shift”

and adds that (1996, 601)

“Overall, the technology of genetic experimental analysis has done more than facilitate research and theory. It has driven research and theory. Sometimes the new technology is the science.”

Judson highlights, furthermore, the difference between classical scientific revolutions (Copernican astronomy, Newtonian physics, relativity and quantum mechanics), and the latest biological revolutions that have taken place by opening up fields rather than overturning them. Judson shows us how these new revolutions changed the style of molecular biology and mentions the importance of agencies and institutions such as the U. S. Department of Energy, that have financed these studies.

In order to understand why *concept-driven revolutions* have attracted so much attention and why *tool-driven revolutions* have been ignored, we might recall Galison’s explanation in the last chapter of *Image and Logic*. Galison states that both the logical positivist and anti-positivist conceptions of science, in spite of their remarkable differences, assigned a secondary role to observation and experience, a function without philosophical relevance. In spite of the differences between these visions of science, both emphasized theory. The positivists consider observation to be objective and progressive and the anti-positivists reduce it to the level of theoretical presumptions, an idea that already existed in Popper’s work. Both assigned observation, after all, the same role in their vision of science: to serve solely as the judge of theoretical predictions, either to confirm or to deny them; observation was subordinated to them.²²

²² Hacking (1983, 261) emphasizes the similarities between them and Kuhn: “Do not expect him to be quite as alien to his predecessors as might be suggested. Point-by-point opposition between philosophers indicates underlying agreement on basics, and in some respects Kuhn is point-by-point opposed to Carnap-Popper” (1983, 7). More specifically, Hacking has written about measurements: “Kuhn’s account of measurement is not so different from Popper’s. Precise measurements turn up phenomena that don’t fit into theories and so new theories are proposed. But

In our opinion, these ideas have been decisive in ensuring that little consideration has been given to *tool-driven revolutions*. In addition, this theoretical prejudice of many historians and philosophers of science has gone beyond academic borders and has also influenced the popular view of science. As Dyson points out (1997, 50), “*concept-driven revolutions* are the ones that attract the most attention and have the greatest impact on the public awareness of science” and he adds (1990, 138) that there exists an “academic snobbery which places the pure scientist on a higher cultural level than inventors” and that overlooks the fact that (1990, 158) “invention is just as creative and just as exciting a way of life as scientific discovery” and that “the life of an inventor also provides ample room for philosophical reflection and for active concern regarding the great problems of human destiny.”

In order to focus even more closely on the idea of tool-driven revolution and be able to incorporate other characteristics and aspects of these episodes, we shall review Galison’s ideas in *Image and Logic* and compare them with Dyson’s.

Galison’s research, that has focused on the history of physics, and more specifically on experimentation over the last century, allows him to draw up a schema (1997, 799) of physicists’ activity based on a division into three main groups: the theoreticians, the builders of instruments and the experimenters. In his view none of these groups is destined to be “the arbiter of progress in the field” or to serve as a “reduction basis.”

Diverse cultures and traditions can be found within each of these groups and they each follow a separate course of development, featuring their own breaks with the past or “revolutions.” Galison’s image shows a laminated structure in diverse cultures featuring different developments. This structure allows local coordination in spite of considerable global differences.²³ Galison borrows concepts from anthropology in order to characterize the relations between these cultures. He is thus interested in the concept of the trading zone, which he defines (1997, 784) as

“the site –partly symbolic, partly spatial– at which local coordination between beliefs and action takes place.”

whereas Popper regards this as an explicit purpose of the experimenter, Kuhn holds it to be a by-product.” Hacking has also noticed that the “history of natural sciences is now almost written as a history of theory” (1983, 149) and that “a theory-dominated philosophy blinds one to reality” (1983, 261).

²³ Galison states the following (1997, 799): “The local continuities are intercalated –we do not expect to see the abrupt changes of theory, experimentation, and instrumentation occur simultaneously; in any case it is a matter of historical investigation to determine if they do line up.” One of the reasons Galison gives to explain this point is that (1997, 798): “Each subculture has its rhythms of change, each has its own standards of demonstration, and each is embedded differently in wider cultural institutions, practices, inventions and ideas.” The idea of “own rhythms of change” can be found in Dyson too, as we have already explained.

Galison also compares communication between the different cultures with the “pidgin” and “creole” languages that linguists and anthropologists have described. These languages arise due to the need for daily communication between two or more groups with different languages. Galison’s laminated and partially independent image of physics leads him to talk about the “disunity of science.” This concept has different connotations and meanings for Galison than the negation of the positivist “unity of science” would. Neither is it the concept of “disunity of science” that the anti-positivists created. For Galison, the “disunity of science” is responsible for the strength, stability and coherence of physics (1997, 844): “It is the disorder of the scientific community - the laminated, finite, partially independent strata supporting one other; it is the disunification of science - the intercalation of different patterns of argument - that is responsible for its strength and coherence.”

Comparing Dyson's notion of tool-driven revolution with Galison's ideas, a number of interesting differences between both concepts can be found. For example, although Dyson speaks of different traditions and styles of investigation, he does not talk, as Galison does, of separate inner revolutions of each tradition or culture. Instead he looks for the cause of the revolution, that he understands on a more global level, within the theoretical or instrumental field.²⁴

In particular, the ideas of tradition, culture or scientific style often appear in Dyson's texts and deserve to be more carefully considered. Dyson makes numerous references to different scientific cultures with varying degrees of independence and interrelation. Thus, Dyson speaks of (1990, 47) unifying and diversifying, of Baconian and Cartesian theoretical traditions, of (1997, 55) Tolstoyan (freedom) and Napoleonic (discipline) science, Athenian and

²⁴ Dyson's ideas are half-way between Galison's and Hacking's. *Representing and intervening*, according to the Canadian philosopher, can be considered a recent landmark in reflections on the importance of instruments and technologies for science. The author begins by focusing on the fact that experiments (1983, xv) “have been neglected for too long by philosophers of science” (and he observes, for example, that although the telescope has enjoyed certain theoretical fame, the microscope has been practically ignored). Hacking states (1983, vii) “experimental science has a life more independent of theorizing than is usually allowed,” and that (1983, 150, 165) “Experimentation has many lives of its own,” (an expression used before by Ernest Nagel) because (1983, 173) “experimenting is not stating or reporting but doing – and not doing things with words.”

Hacking also presents a tripartite division of scientific activity (1983, 212-214): speculation, calculation and experimentation, that can reasonably be compared to the three levels of Galison's scheme: theory, instrument and experiment. The most problematic comparison is instrument-calculation. Calculation is the term with which Hacking designates, in a quite arbitrary and personal manner, the most theoretical facet of the Kuhnian notion of articulation. Therefore, it is not difficult to extend the notion of calculation in order to include the manufacture of experimental apparatus that facilitate the connection between theory and experimentation. To recognize the theoretical aspect of articulation in Galison's instrument category seems rather more difficult; one possibility might be to consider the uses of Monte Carlo and the computers that Galison assigns to the instrumental level. Another important difference is that calculation, derived from the Kuhnian notion of articulation, is typical of normal science, whereas the instrumental traditions that Galison speaks of are independent of the experimental and theoretical traditions.

Manchesterian science, etc. Dyson (1990, 42) opposes, for example, the citing of Rutherford as a typical representative of the Manchesterian current when compared to the Athenian Einstein and affirms that the differences between both were wider than the traditional differences between theoreticians and experimenters, and irresolvable to the point that they were irreconcilable. These differences arose, in Dyson's view, from different visions of the nature and purpose of science.

Dyson himself seems to have experienced at first-hand the differences and relations between diverse traditions. His first great success as a scientist was related to an encounter between two traditions. Dyson received a strong theoretical education at Cambridge during the war. His teachers included Hardy, Littlewood and Mordell and, in fact, his first pieces of research had to do with Number Theory. He also received training in physics from great teachers such as Dirac, Eddington, Jeffreys and Bragg. After his military service, he returned to Cambridge for a year where he was lucky to meet Nicholas Kemmer, who taught him quantum field theory, following the only existing book at that time, *Quantentheorie der Wellenfelder* by Gregor Wentzel. The following year Dyson travelled to Cornell to work on theoretical physics on the advice of Hans Bethe.

At Cornell he found a strong empirical tradition very different from the theoretical tradition he had learned. As he writes (1996, 11):

“The American scientific tradition was strongly empirical. Theory was regarded as a necessary evil, handed down for the correct understanding of experiments but not valued for its own sake. Quantum Field Theory had been invented and elaborated in Europe. It was a sophisticated mathematical construction, motivated more by considerations of mathematical beauty than by success in explaining experiments. The majority of American physicists had not taken the trouble to learn it. They considered it, as Samuel Johnson considered Italian Opera, an exotic and irrational entertainment.”

In those days, the great challenge for American physicists was how to interpret the experiment on the atomic levels of energy that Lamb, Retherford, Foley and Kusch had carried out in Columbia. At Cornell, Dyson was able to determine, thanks to the quantum field theory he had learned with Kemmer, some experimental numbers. There he met Feynman, who, based on his powerful physical intuition, had reworked quantum mechanics and found an original way (now known as Feynman's diagrams) to make the calculations. His results were more precise and easier and faster to obtain than those that Bethe could obtain with a pastiche of classical methods and physical intuition or than those Dyson was able to obtain with quantum field theory.

At that time, Julian Schwinger, who was also learned in quantum field theory, provided a satisfactory theoretical explanation of the experiments. He used quantum field theory to do so, but because he shared the American physicists'

distrust of it, he used the theory in a “grudging” way, preferring the mathematical formalism of Green’s functions.

Dyson harmonized both methods²⁵ and demonstrated that Green’s functions used by Schwinger were essentially the same thing as the propagators devised by Feynman to produce his diagrams. Feynman’s effective method of calculation was then recognized and legitimised. As Dyson explains (1996, 13):

“The effect of the two papers was to make quantum electrodynamics into a convenient tool for practical calculations. By a systematic use of perturbation theory one could calculate physical processes to any desired accuracy.”

It is interesting to compare the birth of quantum electrodynamics with the origin of the Monte Carlo method, that Galison (1996, 1997) has analysed and documented so brilliantly.

Galison describes how the Monte Carlo method was introduced after World War Two by Von Neumann and Ulam to make the calculations required for the design of the hydrogen bomb. Thus, in principle, Monte Carlo was no more than a technique of numerical analysis and the computer a tool with which to carry out the necessary operations. Later on, although the legitimacy of the method was placed in doubt, Monte Carlo was learned by numerous scientists and was applied to other problems. As Galison states (1996, 151; 1997, 746):

“Practice proceeded while interpretation collapsed.”

The result was a new method of science, a new category of scientific activity half-way between experimentation and theory and a change in the conception of the computer; from being merely a laboratory instrument or tool (computer-as-tool) it became a simulation of reality (computer-as-nature) (1996, 121; 1997, 692). Around 1950, a wide range of scientists from various specialized fields (pure and applied mathematicians, nuclear physicists, industrial chemists, statisticians, meteorologists...) began to learn the Monte Carlo method and to master its technical and practical details, and to exchange information regarding their experiences using Monte Carlo, to abstract the procedures and to study the mathematical aspects of the method and its properties. New terms were coined, articles on the peculiarities of the abstract method were written, theorems were

²⁵ At the same time a new version, the relativist quantum field theory of Tomonaga, arrived from Japan. Tomonaga, who had studied in Germany before the war, was trained in classic quantum theory and he based his theory on it even more than Schwinger. Even so, the Green’s functions that Dyson used to create the main link between the works of Feynman, Schwinger and Tomonaga, also appeared in Tomonaga’s work. We might mention the fact that Schwinger and Tomonaga were aware of Wentzel’s work before World War Two. Schwinger states (1991, 681): “And then we became aware, through the published paper, of Wentzel’s considerations on a simple model of the strong coupling of meson and nucleon. I took on the quantum challenge myself. Not liking the way Wentzel had handled it, I redid his calculations in my own style...”

proved, new magazines appeared and congresses were held. So a new trading zone was created alongside the Monte Carlo method and a new pidgin language was also born. Galison focuses on how this pidgin language originated and how, although mathematicians, meteorologists, physicists, etc., produced different interpretations of Monte Carlo, they could share knowledge, communicate and be understood in spite of them (1996, 152):

“A chemical engineer could speak easily to a nuclear physicist in this shared arena of simulation; the diffusion equation for chemical mixing and the Schrödinger equation were, for purposes of their discussion, practically indistinguishable.”

In around 1960, the pidgin language acquired a greater degree of autonomy and it became a creole language. With this change a new discipline arose (1996, 153; 1997, 769):

“By the 1960’s, what had been a pidgin had become a full-fledged Creole: the language of a self-supporting subculture with enough structure and interest to support a research life without being an annex of another discipline, without needing translation into a ‘mother tongue’.”

It is true that many scientists adopted Monte Carlo. They had different interests from those of Von Neumann and Ulam and applied it to very different and new situations. Monte Carlo had a different significance in each field. It is also true, as Galison explains, that some of these scientists changed their fields and their approach to problems, but their use of Monte Carlo was the constant throughout these changes. Galison claims that the language of Monte Carlo was an intermediate language (1996, 155; 1997, 776):

“understood both by theorists and by experimenters.”

In our opinion, to describe the way in which scientists learned this method and applied it to their disciplines as the emergence of a pidgin language might simplify rather too much and, in fact, divest Monte Carlo of all meaning. Considering Monte Carlo as a pidgin language seems quite inadequate when it comes to explaining why Monte Carlo was useful in dealing with the problems that preoccupied nuclear physicists, meteorologists, in fact all the specialists who used it.

From Dyson’s point of view there is one main objection to Galison’s analysis. The question that concerns us is whether it makes sense to speak of pidgin and creole at all. The way Galison uses these concepts could create some misunderstandings and oversimplify our comprehension of the desirable, but complex and difficult,

relations between scientists trained in different traditions and working within different subject areas. As Galison explains, pidgin and creole languages are naturally born when two groups of speakers with different mother tongues meet at the same time in the same place and are forced to communicate. Pidgin is the first stage of an eclectic and practical mixture of the original languages. Both groups use elements of the other's language, but they do not necessarily give these elements the same value or meaning that they had in the original language, and these elements may acquire a new meaning in the communication between the two groups. Curiously enough, they do not need to have the same value for both groups. Pidgin soon becomes totally independent from the original languages and becomes a creole, a new common language with its own grammar and etymology. This process takes place in a spontaneous way and without any conscious control or guidance. Nobody writes down or establishes the grammar since the speakers agree unconsciously.

Another episode in Dyson's scientific career may help us to understand what Galison seems to ignore. In 1952, Dyson (1996, 17) became interested in a practical method to calculate strong iteration processes. In Chicago, a team led by Fermi had obtained some experimental measurements of meson-nucleon scattering. Dyson modified the Tamm-Dancoff method with the hope of obtaining theoretical calculations that agreed with the first experimental data and being able to predict new results. Dyson put his postgraduate students to work on these calculations. After two years' work, when they produced the first results, he travelled to Chicago in order to show them to Fermi. Dyson tells us that:

“As soon as we had some preliminary results, I went for a visit to Chicago and proudly showed our numbers to Fermi, hoping for an enthusiastic response. Fermi was unimpressed. He remarked that a theoretical calculation should either be based on a clear physical picture, which he considered the preferable alternative, or be based on rigorous mathematics. Our calculation was neither physically clear nor mathematically rigorous. Furthermore, the agreement between our numbers and the experimenters was not impressive. I came back from Chicago with my confidence in our whole enterprise severely shaken.”²⁶

²⁶ Later, Dyson convinced himself that the whole attempt was destined to fail (Dyson, 1996, 17): “We now know, forty years later, that the pseudoscalar meson theory on which our calculations were based is wrong, that strong interactions are in fact dominated by vector rather than pseudoscalar mesons, and that any apparent agreement between our results and the experiments was illusory.” A couple of years later, at Princeton, Dyson returned to the problem of meson-nucleon scattering, using a new tool, the Chew-Low equation (Dyson, 1996, 21): “During my first two years as a professor at Princeton, the most successful tool for the understanding of meson-nucleon scattering and other strong interaction process was the scattering equation of Chew and Low. The Chew-Low equation was much simpler than the Tamm-Dancoff formalism that I had been struggling with at Cornell. The Chew-Low equation was also more successful in matching the results of experiments. The Tamm-Dancoff equations were a mutilated version of a relativistic field theory that was based only on a formal analogy with quantum electrodynamics. As Fermi remarked, the Tamm-Dancoff calculations were unphysical. In contrast, the Chew-Low method

Galison seems to forget that, although the Monte Carlo method had different meanings for different scientists, these meanings must have had some value in each case that made it useful. After all, as Galison recognizes, the validity of Monte Carlo was based, for Von Neumann and others, on a physical image, on the idea that Nature is not continuous like differential equations but discrete, (Galison, 1996, 125; 1997, 697):

“Now von Neumann had to defend the validity of this form of machine representation, and he did so with a long string of plausibility claims. First, he turned the tables on the visual “representational” argument for differential equations. In particular, he pointed out that it is the hydro dynamical equation [...] that distorts the world by using a continuum to describe what in ‘reality’ (von Neumann’s term) is a discrete set of molecules. By contrast the ‘beads-and-springs’ ‘corresponds’ to the ‘quasi-molecular description of this substance’.”

On the other hand, Monte Carlo was undoubtedly a method for calculating things and many scientists thought it could be useful in other contexts. Thus, it was necessary to understand the peculiarities of the method in order to generalize it. For this reason, a new specialty was born in order to study the Monte Carlo’s mathematical basis. The mathematical study of the method allowed scientists to determine its characteristics, properties, rank of application, etc.

Understanding Monte Carlo as a tool²⁷, as a mathematical tool, may avoid many controversies about its nature and may also explain the emergence of a new community of specialists. Each new tool requires a technique, one that must be learned. At the same time the virtuosos, those who have knowledge derived from practice, can think up improvements for the tool. The same instrument can also have diverse uses and even some functions different to those for which it was devised. For this reason, in scientific research it is usual to systematically apply

was based on three physical principles, the unitary nature of the scattering matrix, the causality of the scattering process and a simple non-relativistic model of the scattered matter as a fixed heavy object. As soon as the Chew-Low method was invented, Fermi gave his blessing to it. It was a good example of the Fermi style in theoretical physics, simple ideas and simple arguments leading to simple conclusions. Fermi always preferred physical intuition to mathematical formalism.”

In this respect, we might also mention Dyson’s *Scientific American* article (1964), where he discusses three methods (Fields Theory, S Matrix Theory and Group Theory) of theoretical investigation in particle physics, as well as their different philosophical, physical and mathematical advantages and disadvantages.

²⁷ Galison explains that the introduction of Monte Carlo had effects that went beyond the scope of instrument manufacturers or calculators (1996, 154): “The Monte Carlo in some ways was the culmination of a profound shift in theoretical culture, from the empyrean European mathematicism that treasured the differential equation above all, to a pragmatic empiricized mathematics in which sampling had the final word.” Thus, we see how revolutions, as Dyson claims, although they originate in a particular tradition, can frequently have general consequences. On the other hand, this fact shows us that identifying Hacking’s level of calculation with Kuhn’s theoretical articulation and, therefore its assignment to normal science, can be too restrictive (see note 16).

the greatest possible variety of techniques (tools) to each new object of interest. In most cases the application of a new tool will not provide any interesting results but, even in the worst case analysis, when we fail to obtain any new result, we are able to draw conclusions regarding the features of the required tool and regarding what aspects of the phenomena are worth studying.

From Galison's references to pidgin and creole, one might conclude that the communication between scientists from different specialties is spontaneous and non-problematic. We believe that Galison neglects the difficulties of understanding Nature and the theories and ideas of other scientists. Relations between different disciplines are often fruitful, but they require a conscious and exerted effort to understand the "other side's" problems and the principles that are applicable to one's own problems and those that are not.

Pidgin does not just appear from nowhere in order to allow scientists to understand others' problems and achievements. Dyson has mentioned, for instance, the difficulties that he had to overcome in order to understand Feynman and Schwinger. First of all, Feynman's work was highly original and corresponded to a type of "mind" that was very different to that of Dyson himself (1979, 55):

"The reason Dick's physics was so hard for ordinary people to grasp was that he did not use equations. The usual way theoretical physics has been done since the time of Newton was to begin by writing down some equations and then to work hard calculating solutions to the equations. This was the way Hans and Oppy and Julian Schwinger did physics. Dick just wrote down the solutions out of his head without ever writing down the equations. He had a physical picture of the way the things happen, and the picture gave him the solutions directly with a minimum of calculation. It was no wonder that people who had spent their lives solving equations were baffled by him."

At Cornell, Dyson discussed issues many times with Feynman. In these dialogues Dyson learned to appreciate Feynman's way of thinking (1979, 56),

"I stared at the strange diagrams that he drew on the blackboard, I gradually absorbed some of his pictorial imagination and began to feel at home in his version of universe."

Dyson noticed too that, in addition to their different physical perspectives, their ultimate goals were different. The following quotation describes one of their talks during a car trip to Albuquerque in the summer of 1948 (Dyson, 1979, 63-64):

"So Dick and I argued through the night. Dick was trying to understand the whole of physics. I was willing to settle for a theory of the middle ground alone. He was searching for general principles that would be flexible enough so that he could adapt them to anything in the universe. I was looking for a neat set of equations that would

describe accurately what happens in the middle ground. We were arguing back and forth. Looking back to the argument now from thirty years later, it is easy to see that we were both right. It is one of the special beauties of science that points of view which seem to be diametrically opposed turn out later, in a broader perspective, to be both right. I was right because it turns out that nature likes to be compartmentalized. The theory of quantum electrodynamics turned out to do all that I expected of it. It predicts correctly, with enormous accuracy, the results of all the experiments that have been done in the domain of the middle ground. Dick was right because it turns out that the general rules of space-time trajectories and sum-over-histories had a far wider range of validity than quantum electrodynamics. In the domain of the very small, now known as particle physics, the rigid formalism of quantum electrodynamics turned out to be useless, while Dick's flexible rules, now known as Feynman diagrams, are the first working tool of every theorist.

That stormy night in our little room in Vinita, Dick and I were not looking thirty years ahead. I knew only that somewhere hidden in Dick's ideas was the key to a theory of quantum electrodynamics simpler and more physical than Julian Schwinger's elaborate construction. Dick knew only that he had larger aims in view than tidying up Schwinger's equations. So the argument did not come to an end, but left us each going his own way.²⁸

Ann Arbor was the destination of Dyson's trip. He was to attend a five-week course at the University of Michigan, during which Julian Schwinger would explain his just published theories. Schwinger, aside from having avoided the explicit use of quantum field theory, as we saw above, had hidden his ideas behind a certain mathematical elegance. Although his starting-point was closer to Dyson than to Feynman, Dyson had to work hard to understand his ideas. (Dyson, 1979, 66):

"Julian Schwinger's lectures were a marvel of polished elegance, like a difficult violin sonata played by a virtuoso, more technique than music. Fortunately, Schwinger was friendly and approachable. I could talk with him at length, and from these conversations more than from the lectures I learned how his theory was put together. In the lecture his theory was a cut diamond, brilliant and dazzling. When I talked with him in private, I saw it in the rough, the way he saw it himself before he started the cutting and polishing. In this way I was able to grasp much better his way of thinking. The Ann Arbor physicists generously gave me a room

²⁸ Dyson (1979, 63): "The middle ground is an enormous domain, including everything intermediate in size between an atomic nucleus and a planet. It is the domain of everyday human experience. It includes atoms and electricity, light and sound, gases, liquids and solids, chairs, tables and people. The theory of what we called quantum electrodynamics was the theory of the middle ground. Its aim was to give a complete and accurate account of all physical processes in the third domain, excluding only the very large and the very small."

to myself on the top floor of their building. Each afternoon I hid up there under the roof for several hours and worked through every step of Schwinger's lectures and every word of our conversations. I intended to master Schwinger's techniques as I had mastered Piaggio's differential equations ten years before. Five weeks went by quickly. I filled hundreds of pages with calculations, working through various simple problems with Schwinger's methods. At the end of the summer school, I felt that I understood Schwinger's theory as well as anybody could understand it, with the possible exception of Schwinger. That was what I had come to Ann Arbor to do."

We may conclude that in cases such as the birth of simulation methods based on the Monte Carlo method or the birth of quantum electrodynamics, new fields of exchange for concepts and ideas arise. But these interchanges are not spontaneous and easy and only take place thanks to a huge effort on the part of the scientists who participate.²⁹ In the place of Galison's metaphor of pidgin and creole languages, Dyson suggests the metaphor of bridge-building to talk about his work on the unification of quantum electrodynamics (Dyson, 1995, 803):

"When I did my most important piece of work as a young man, putting together the ideas of Tomonaga, Schwinger and Feynman to obtain a simplified version of

²⁹ It can be illustrative to consider another of Dyson's experiences. In the summer of 1956, Freddy de Hoffman brought together, in a small abandoned schoolhouse in San Diego, a group of about thirty scientists and engineers to study the practical possibilities of the peaceful use of nuclear energy and to initiate the development and construction of reactors for civil purposes. Freddy de Hoffman invited Dyson to collaborate. Dyson (1979, 96-97) explains the plan of work: "We assembled in June in the schoolhouse, and Freddy told us his plan of work. Every morning there would be three hours of lectures. The people who were already expert in some area of reactor technology would lecture and the others would learn. So at the end of the summer we would all be experts. Meanwhile we would spend the afternoons divided into working groups to invent new kinds of reactors. Our primary job was to find out whether there was any specific type of reactor that looked promising as a commercial venture for General Atomic to build and sell.

The lectures were excellent. They were especially good for me, coming into the reactor business from a position of total ignorance. But even the established experts learned a lot from each other. The physicist who knew everything that was to be known about the physics reactor learned about the details of the chemistry and engineering. The chemists and engineers learned about the physics. In a few weeks we were all able to understand each other's problems."

Dyson worked in a group whose objective was to develop a "safe reactor that could be given to a bunch of high school children to play with, without any fear that they would get hurt." The idea of a safe reactor was Edward Teller's, who himself led the team. Dyson was to work on the project closely with Teller during the summer. These were their experiences:

"Working with Teller was as exciting as I had imagined it would be. Almost every day he came to the schoolhouse with some harebrained new idea. Some of his ideas were brilliant, some were practical, and a few brilliant and practical. I used his ideas as starting points for a more systematic analysis of the problem. His intuition and my mathematics fitted together in the design of the safe reactor just as Dick Feynman's intuition and my mathematics had fitted together in the understanding of the electron. I fought with Teller as I fought with Feynman, demolishing his wilder schemes and squeezing his intuitions down into equations. Out of our fierce disagreements the shape of the safe reactor gradually emerged. Of course I was not alone with Teller as I had been with Feynman. The safe reactor group was a team of ten people. Teller and I did most of the shouting, while the chemists and engineers in the group did most of the real work" (Dyson, 1979, 98).

quantum electrodynamics, I had consciously in mind a metaphor to describe what I was doing. The metaphor was bridge-building. Tomonaga and Schwinger had built solid foundations on one side of a river of ignorance, Feynman had built solid foundations on the other side, and my job was to design and build the cantilevers reaching out over the water until they met in the middle. The metaphor was a good one. The bridge I built is still serviceable and still carrying traffic forty years later. The same metaphor successfully describes the greater work of unification achieved by Weinberg and Salam when they bridged the gap between electrodynamics and weak interactions. In each case, after the work of unification is done, the whole stands higher than the parts.”

Although we are not enamoured of Galison’s metaphor, we must recognize that Galison pinpointed quite an important problem. This could be summarized in the following way: how and why scientists who have different ways of seeing nature, who are concerned with different problems, who seek different objectives, who work with different technical resources ... can collaborate, can communicate, can work on the same subject and can achieve advances that, although discovered and expressed in different ways, are recognized by all of them. This is undoubtedly a problem that Galison has focused on, although his linguistic approach does not seem to be sufficiently convincing to solve it.

Both Dyson and Galison accept and recognize the existing divisions between cultures, disciplines, styles, traditions, etc., but while Galison underlines these differences and formulates an argument for the global strength of science, Dyson promotes the crossing and demolition of these frontiers as a condition for the outbreak of new revolutions and the emergence of new research fields.³⁰

As we have already mentioned, the detection of fields in which new scientific advances are possible is a main concern of Dyson’s reflections,³¹ reflected as much in his writings on scientific policy as in his reflections on the existence and types of scientific revolutions. Dyson’s philosophical work should be understood as an encouragement for scientists who do not care to open up new fields, who try

³⁰ We find here a new point of contact between Dyson and Hacking. For example, Hacking refutes (1983, 261) Lakatos’ analysis of the Michelson-Morley experiment as an example of “theory-dominated philosophy,” while Galison claims that Michelson had his own purposes as an experimenter, distinct from the theoretical consequences that could be derived from his works. Galison explains how the completion of the experiment extended over a period of decades, and how it had to be repeated and to be redesigned on several occasions to satisfy experimental requirements, ... but at the end he concludes: “Doubtless Michelson is a little like Bacon’s *ant*, a whiz at mechanical experiments and weak on theory – although not ignorant of it. Likewise, Lorentz was (to a lesser degree) a little like Bacon’s *spider*. Both men thought highly of each other. Lorentz encouraged Michelson’s work, while at the same time trying to develop another mathematics which would explain it away.[...] More importantly we see the interaction between two kinds of talents. The stupendous interest of Einstein’s theories of relativity naturally makes the theoretical work the more important in this domain. Michelson too opened up new realms of experimental technique. Science, as Bacon wrote, must be like the *bee*, with the talents of both ant and spider, but able to do more, that is digest and interpret both experiments and speculation.”

³¹ Chapters 6, 9, 13 and 14 (1994) may be especially interesting in this respect.

new clever and original approaches, who cross the borders of traditional fields, and who are interested in the problems of other scientific disciplines and make an effort to exchange ideas with experts from other fields.³²

Another significant coincidence between Dyson and Galison is outlined below. Galison insists that one of the objectives of his research is as follows (1997, xviii):

“By following the all-too-material culture of the laboratory, I want to get at what it meant to be an experimenter and to do an experiment across the century.”

Galison’s conclusion is that, as in the case of Monte Carlo, in the beginning it was considered to be a tool and then came to be considered an artificial reality in which to simulate reality and perform experiments, (1997, xviii)

“the very category ‘experiment’ will not stay still.”

Dyson has a very open attitude regarding the possibility of a complete change within science, driven by all types of *tool-driven* and *concept-driven revolutions* (1997, 90):

“If we try to imagine what science will be doing a thousand years from now, we must face the possibility that science as we know it may have ceased to exist. The thought processes of our descendants a thousand years in the future may be as alien to us as our theories of quantum mechanics and general relativity would be to Saint Thomas Aquinas.”

³² In “Missed Opportunities” (1996, paper 98, A4) Dyson discusses moments in the history of physics and even situations in current physical research that lack an appropriate mathematical theory for the study and description of the physical phenomena which have just been found, thus offering an opportunity for the development of new mathematics. Dyson cites the words Henry Smith, Oxford’s mathematician, pronounced in 1873 to mark the occasion of Maxwell’s electromagnetic theory and his treatise on electricity published that year: “No mathematician can turn over the pages of these volumes without very speedily convincing himself that they contain the first outlines (and something more than the first outlines) of a theory which has already added largely to the methods and resources of pure mathematics, and which may one day render to that abstract science services no less than those which it owes astronomy. For electricity now, like the astronomy of old, has placed before the mathematician an entirely new set of questions, requiring the creation of entirely new methods for their solution, ...” Dyson laments how these words were ignored and the developments they demanded were delayed for almost 30 years. Dyson dares to describe in an abstract way situations in which, as in the previous case, physics offers mathematics the opportunity to advance, as long as mathematicians are interested in what it happens in physics and they are willing to use their talents to collaborate with physicists: “The past opportunities which I discussed have one important feature in common. In every case there was an empirical finding that two disparate or incompatible mathematical concepts were juxtaposed in the description of a single situation. [...] In each case the opportunity offered to the pure mathematician was to create a wider conceptual framework within which the pair of disparate elements would find a harmonious coexistence. I take this to be my methodological principle in looking for opportunities that are still open. I look for situations in which the juxtaposition of a pair of incompatible concepts is acknowledged but unexplained.”

But Dyson differs from Galison regarding the consequences they extract from the existence of *tool-driven revolutions*. Galison draws sociological conclusions and proposes lines of investigation in accordance with this approach (for example, the study of the similarities between the structure and operation of the research groups and the architecture of the buildings that lodges them, or tracing the careers of different scientists through different specialties). Dyson instead proposes an historical research that, as we saw above, connects new scientific achievements with the technical features of the instruments that made them possible.

Tool-Driven Revolutions and Kuhn

Finally, it might be interesting to compare Dyson ideas with Kuhnian views, who did not ignore the importance of instruments and scientific tools and devices, even within his conception of science. First, we might present some of Kuhn's observations (1970, 15) on the birth of scientific fields:

“In the absence of a paradigm or some candidate for paradigm, all the facts that could possibly pertain to the development of a given science are likely to seem equally relevant. As a result, early fact-gathering is a far more nearly random activity than the one that subsequent scientific development makes familiar. Furthermore, in the absence of a reason for seeking some particular form or more recondite information, early fact-gathering is usually restricted to the wealth of data that lie ready to hand. The resulting pool of facts contains those accessible to the casual observation and experiment together with some of the more esoteric data retrievable from established crafts like medicine, calendar making, and metallurgy. Because the crafts are one readily accessible source of facts that could not have been casually discovered, technology has often played a vital role in the emergence of new sciences.”

At any case, it is true that, when one speaks about Kuhnian scientific revolutions it is normal to quote the classical examples such as the Copernican, Newtonian, chemical and Einsteinian revolutions. These are the examples that Kuhn himself gives in chapter seven of his work entitled “Crisis and Emergence of Scientific Theories.” But it is often forgotten that in the previous chapter, “Anomaly and the Emergence of Scientific Discoveries,” Kuhn explains how normal science, whose goal is paradigm articulation and not unexpected novelty, nevertheless causes paradigm change.

Both *discoveries or novelties of fact* and *novelties of theory or inventions*, in fact, induce these changes.³³ Kuhn gives several examples of the former in chapter six and analyses them. One of these examples is indeed the discovery of X-rays.

Although Kuhn and his followers gave too much more importance to *concept-driven revolutions*, the previous observations should make more cautious regarding the usually accepted version of the Kuhnian conception that reduces the value of experimental work in normal science, and that assumes that if this experimental activity produces unsuitable data within a valid paradigm, these data must be accumulated, receiving a lesser or greater degree of attention, until a new and revolutionary theoretical approach assimilates them or includes them among its achievements.³⁴

Another reason to consider experimentation solely the work of normal science and to suppose that revolutions occur at a theoretical level, is based on Kuhn's choice (1970, chap. 3) to speak of experimental tasks before theoretical tasks, to explain the three types of activities that characterized normal science. Some of these could be the measurements that are made to determine magnitudes more precisely, that are emphasized as interesting aspects of a present paradigm or establish the value of constants that indicate the theory.³⁵

One of the examples of experimental discovery that Kuhn mentions is the discovery of X-rays. As Kuhn states, this was the typical accidental discovery, that Roentgen came across during his research on cathode rays, given that this type of discovery

“occurs more frequently than the impersonal standards of scientific reporting allow us easy to realize” (1970, 59).

Kuhn emphasizes, first, that this discovery had not been foreseen by the theoretical paradigms that guided Roentgen's scientific activity and that of his contemporaries, but the appearance of this new type of phenomenon didn't seem to contradict them either. Even so, Kuhn believes that the discovery of this new phenomenon entailed a scientific revolution because he noticed the existence of unknown and uncontrolled factors in the experimental investigation on cathode

³³ Kuhn (1970, 52) claims, when he introduces these concepts: “That distinction between discovery and invention or between fact and theory will, however, immediately prove to be exceedingly artificial”, and he adds (1970, 66): “in the sciences fact and theory, discovery and invention, are not categorically and permanently distinct.”

³⁴ This is, for example, the way to understand the Kuhnian revolution Galison describes (1997, 793) when he states: “Because theory was so central, when theory itself fragmented the whole cloth of scientific activity was rent, effectively turned into unrelated bits. [...] the anti-positivists choose as their central metaphor a picture of scientific change that was grounded in theory.” It seems reasonable to maintain that even Kuhn himself contemplated a greater wealth of possibilities.

³⁵ Thus, Hacking states (1983, 244): “Kuhn's account of measurement is not so different from Popper's. Precise measurements turn up phenomena that don't fit into theories and so new theories are proposed. But whereas Popper regards this as an explicit purpose of the experimenter, Kuhn holds it to be a by-product.”

rays. Experiments in these fields had to be redone and all the methodological procedures and arguments that supported these experiments had to be reviewed to be sure that the new phenomenon did not influence the results of the experiments designed to develop procedures and techniques to control its influence. Therefore, the appearance of X-rays

“denied previously paradigmatic types of instrument” (1970, 59).

Kuhn observes, in addition, that there were two other ways in which the discovery of X-rays was revolutionary. X-rays, as we said before, had not been predicted by previous theories. Yet they did not refute them either. That is why they presented a new field of investigation. It was necessary to explain and to characterize them (1970, 59):

“X-rays, to be sure, opened up a new field and thus added to the potential domain of normal science.”

X-rays indeed entailed a challenge for normal science, but this was not comparable, for example, to finding new chemical elements that filled some of the existing gaps. The appearance of X-rays represented a true revolution that required a shift of paradigm, as much of procedures as of expectations, because for a small scientific community it opened up a new world.³⁶ As Kuhn states (1970, 61):

“We may simultaneously see an essential sense in which a discovery like X-rays necessitates paradigm changes -and therefore change in both procedures and expectations- for a special segment of the scientific community. As a result, we may also understand how the discovery of X-rays could seem to open a strange new world to many scientists and could thus participate so effectively in the crisis that led to twentieth-century physics.”

The last revolutionary aspect that Kuhn mentions (1970, 59) is that the appearance of X-rays forced a rearrangement of scientific communities:

“They also [...] changed the fields that already existed.”

In addition, when explaining his idea of normal science, Kuhn talks about paradigms that define the problems or puzzles that scientists face. But he also talks about some rules that limit the possible solutions to problems and the steps by which these solutions can be achieved. The differences between paradigms and rules, as well as the different scales and divisions among them, offer a peculiar

³⁶ Max Perutz explains (2002, 389) how there was no scientific paradigm that explained the atomic structure of solid matter before W. L. Bragg used X-rays to explain the structure of crystals. Perutz states that the application of X-rays opened up a field that nobody before had ever imagined. He also discusses Popper’s ideas again when he states that the structures established by Bragg are not exposed to any experimental rebuttal. They are, however, definitive and whoever might wish to review them would be disappointed.

topology that could be compared with the structures of the relations and developments of the diverse scientific traditions that Dyson and Galison propose. This topology could be used to sustain a disunified vision of science in the style of Galison and Hacking.

Kuhn's concept of the rule is quite wide-ranging. It not only limits the rank of solutions that admit the problems proposed by a paradigm, but the way to achieve these solutions. In the fifth chapter of *The Structure of Scientific Revolutions* entitled "Normal Science as Puzzle-Solving", Kuhn presents diverse senses of this concept (1970, 40-42):

"explicit statements of scientific law and about scientific concepts and theories"

that help to establish puzzles and to limit acceptable solutions, for example, Newton's laws during the eighteenth and nineteenth centuries;

"a multitude of commitments to preferred types of instrumentation and to the ways in which accepted instruments may legitimately be employed;"

"higher-level quasi-metaphysical commitments"

on, for example, the corpuscular nature of matter, that leads, in addition, to methodological commitments; or the acceptance, without which no individual can get to be a scientist, of the fact that scientists

"must be [...] concerned to understand the world and to extend the precision and scope with which it has been ordered."

Strangely enough, this broad Kuhnian concept of the rule has almost been forgotten and has not received as much attention as the concept of the paradigm. The reason for this forgetfulness can even be traced back to *The Structure of Scientific Revolutions*, because Kuhn himself was very negative with regard to the possibilities of detecting and studying the rules shared by a scientific community:

"the search for a body of rules competent to constitute a given normal research tradition becomes a source of continual and deep frustration."³⁷

Kuhn claims, furthermore, that paradigms and not rules determine scientific communities³⁸ and the course of normal science. In fact, Kuhn states that the

³⁷ He also wrote that (1970, 43): "Anyone who has attempted to describe or analyse the evolution of a particular scientific tradition will necessarily have sought accepted principles and rules of this sort. Almost certainly [...] he will have found the search for rules both more difficult and less satisfying than the search for paradigms."

paradigm can, for some time, guide normal science in the absence of shared rules and that it is the paradigm that determines the rules shared by a scientific community (1970, 42):

“Rules, I suggest, derive from paradigms but paradigms can guide research even in the absence of rules.”³⁹

Kuhn concluded, on the one hand, that the task of the historian of science is to locate and describe paradigms and, on the other hand, that (1970, 49):

“Substituting paradigms for rules should make the diversity of scientific fields and specialties easier to understand.”

Kuhn presented other characteristics of paradigms and rules. Thus, he states that (1970, 49):

“Explicit rules, when they exist, are usually common to a very broad scientific group, but paradigms need not be.”

For this reason, we can talk about greater or smaller revolutions in the sense that they affect larger scientific communities or only small scientific groups. In addition, he explains how changes in rules that manage the achievement and acceptability of solutions to puzzles or scientific problems, never solve the puzzle but redefine or change the paradigm⁴⁰, which, in fact, brings about a scientific revolution.

At this point we might return to the history of quantum electrodynamics in order to underline the differences between rules and paradigms and attempt to see how scientific methods can have a different value for different scientific communities.

³⁸ This opinion is expressed in a firmer (and circular) way in *Something More on Paradigms* (1983, 318): “A paradigm is what the members of a scientific community, and only they, share. It is the possession of a common paradigm that constitutes a scientific community, formed as well by different men in all other aspects.”

³⁹ Other statements reflect these ideas: “Normal science can be determined in part by the direct inspection of paradigms, a process that is often aided by but does not depend upon the formulation of rules and assumptions. Indeed, the existence of a paradigm need not even imply that any full set of rules exists” (1970, 44); “Paradigms may be prior to, more binding, and more complete than any set of *rules* for research that could be unequivocally abstracted from it” (1970, 46); “... paradigms could determine normal science without the intervention of discoverable rules” (1970, 46).

⁴⁰ Other quotations reflect these ideas: “Normal science can be determined in part by the direct inspection of paradigms, a process that is often aided by but does not depend upon the formulation of rules and assumptions. Indeed, the existence of a paradigm need not even imply that any full set of rules exists” (1970, 44); “Paradigms may be prior to, more binding, and more complete than any set of *rules* for research that could be unequivocally abstracted from it” (1970, 46); “... paradigms could determine normal science without the intervention of discoverable rules” (1970, 46).

In *Image and Logic* (1997, 816-827) Galison brings to light a peculiar episode in the invention of quantum electrodynamics: the work on the development of radar that Julian Schwinger carried out during World War Two and the consequences that this episode had for his later contributions to quantum electrodynamics. Galison considers the scientific and technical activity carried out over the years 1942-1946 at the MIT Rab-Lab to be an example of a trading zone, in which, among other interchanges, an exchange between theoreticians and engineers (or instrument designers) took place (1997, 818-820):

“the transfer of a research style from the instrument design of electrical engineers to the arcane, sometimes metaphysical world of particle physics.”

Galison reports that:

“In this instance, the connection is not the trivial one in which theory is applied to the specific case of instrument design. Rather, the instrument makers -here the electrical engineers of the wartime MIT Radiation Laboratory- had a characteristic way of learning about the world, and it was the theorists who came to adopt it.”

Schwinger arrived at the theoretical section of the Rab-Lab and was asked to develop a general and simple account of microwave networks that could be useful in making practical calculations. As a theoretical physicist, Schwinger undertook this task using Maxwell's equations. However, the small wavelength of microwaves in comparison with the size of the radar's electronic components, such as resistors, copper wires and cylindrical capacitors, meant that the application of this classic and theoretical resource to the problem of calculation actually failed. Schwinger soon discovered a surprising solution. He adopted the method by which electrical engineers calculated the circuits of complex electrical systems, such as loudspeakers. The electrical engineers placed the complicated physics of each component in the system into a “black box”, that was replaced in his calculations by an equivalent circuit of purely electrical components. They rejected the physics of the component and to produce the calculations of the system they only used the inputs and the outputs of that component. Schwinger assimilated this method of calculation from the electrical engineers at the laboratory and applied it in order to develop a kind of wave-guide algebra, making the calculations of new microwave circuits simply a matter of routine.

As Galison recalls, Schwinger himself recognized the value of his experiences at the Rab-Lab regarding his research in quantum electrodynamics. Schwinger (1980, 686-687) explained this in a lecture he gave in honour of Tomonaga, who, strangely enough, also worked during the war on a Japanese radar development project:

“During the war, I also worked on the electromagnetic problems of microwaves and wave guides. I also began with the physicist’s approach, including the use of the scattering matrix. But long before this three-year episode ended, I was speaking the language of the engineers. I should like to think that those years of distraction for Tomonaga and myself were not without their useful lessons. The wave guide investigators showed the utility of organizing a theory to isolate those inner structural aspects that are not probed under the given experimental circumstances. That lesson was soon applied in the effective-range description of nuclear forces. And it was this viewpoint that would lead to the quantum electrodynamics concept of self-consistent subtraction or re-normalization. [...] Tomonaga already understood the importance of describing relativistic situations covariantly –without specialization to any particular coordinate system. At about this time I began to learn that lesson pragmatically, in the context of solving a physical problem. As the war in Europe approached its end, the American physicists responsible for creating a massive microwave technology began to dream of high-energy electron accelerators. One of the practical questions involved was posed by the strong radiation emitted by relativistic electrons swinging in circular orbits. In studying what is now called synchrotron radiation, I used the reaction of the field created by the electron’s motion. One part of that reaction describes the energy and momentum lost by the electron to the radiation. The other part is an added inertial effect characterized by an electromagnetic mass. I have mentioned the relativistic difficulty that the electromagnetic mass usually creates. But, in the covariant method I was using, based on action and proper time, a perfectly invariant form emerged. Moral: To end with an invariant result, use a covariant method and maintain covariance to the end of calculation. And, in the appearance of an invariant electromagnetic mass that simply added to the mechanical mass to form the physical mass of the electron, neither piece being separately distinguishable under ordinary circumstances, I was seeing again the advantage of isolating unobservable structural aspects of the theory. Looking back at it, the basic ingredients of the coming quantum electrodynamics revolution were then in place. Lacking was an experimental impetus to combine them and take them seriously.”

According to Schwinger’s testimony, it seems certain that, as Galison concludes, the theoretical physicists assimilated a style of investigation typical of the electrical engineers. However, we would like to point out how the conclusions that Schwinger drew from the electrical engineers’ methods of calculation actually had a very different meaning in quantum electrodynamics. The electrical engineers had an established paradigm for the calculation of electrical systems. Their method of calculation was totally determined by (and integrated in) this paradigm. Schwinger not only learned a new method of calculation, but learned the importance of isolating the non-observable internal characteristics of theories and discovered a

way to find mathematically invariant results (he took this from the construction of the first great accelerators). Both lessons were more significant than just a new method of calculation. Both also entailed, following Kuhn's nomenclature, new rules for quantum electrodynamics, whose adoption necessarily changed the paradigm used before the war and brought about a revolution in the specialty.

During this episode in the history of quantum electrodynamics, we can see how the tools (we consider this new calculation method to be a tool) produced local and global revolutions that went beyond the boundaries of the specialties and the cultures in which they originated. We can also see how the same scientific achievement had a different value for different cultures and specialties. This means that scientists behave wisely, although maybe not deliberately, when they export ideas, tools and calculation methods (with their consequent refinements, adjustments and developments) from other specialties; the movement of ideas, tools or methods from one field to another can sometimes produce extraordinarily good results. Each new tool, method or idea has a series of unique characteristics that, when applied to different problems, can have the capacity to reveal surprising aspects and properties.⁴¹ These properties may be relevant or irrelevant when it comes to solving problems or, on the contrary, quite enlightening and even capable of revealing new problems.

In order to conclude our commentary on Kuhnian ideas, we would like to suggest a new way of understanding the diversity of scientific fields and communities. Instead of substituting rules with paradigms, as Kuhn proposed, we might consider both at the same time. Sometimes rules are adopted thanks to a paradigm and help to regulate it, but in other cases, as we have seen in the Schwinger example (also mentioned by Kuhn), the adoption of new rules can entail true revolutions. Thus, we find in this melting-pot of specialties, communities and sub-specialties, that share paradigms and rules, a positive confirmation of the Galisonian concept of the disunity of science:

“What has been said so far may have seemed to imply that normal science is a single monolithic and unified enterprise that must stand or fall with any one of its paradigms as well as with all of them together. But science is obviously seldom or never like that. Often, viewing all fields together, it seems instead a rather ramshackle structure with little coherence among its various parts. Nothing said to this point should,

⁴¹ Hacking, (1983, 206): “As always a new kind of microscope is interesting because of the new aspects of a specimen that it may reveal. Changes in a refractive index are vastly greater for sound than for light. Moreover sound is transmitted through objects that are completely opaque. Thus one of the first applications of the acoustic microscope is in metallurgy and also detecting defects in silicon chips. For the biologist, the prospects are also striking. The acoustic microscope is sensitive to the density, viscosity and flexibility of living matter. Moreover the very short bursts of sound used by the scanner do not immediately damage the cell. Hence one may study the life of a cell in a quite literal way: one will be able to observe changes in viscosity and flexibility as the cell goes about its business.”

however conflict with that very familiar observation" (Kuhn, 1970, 49).

Tool-Driven Revolutions and the Science of the Twentieth Century

Kuhn claims that the discovery of X-rays opened up⁴² a new world for a small scientific community and played a significant role in the great theoretical crisis that led to the new physics of the twentieth century. This is true, but it may be somewhat brief in its acknowledgments. The discovery of X-rays also meant, for example, that Poincaré questioned the relationship between the fluorescence of the glass that was bombed by cathode rays and the X-ray emissions that Roentgen had discovered. This question was picked up by Becquerel, who discovered radioactivity. Even before this, Roentgen had noticed the capacity of X-rays to transfer bodies of a certain mass and the fact that X-rays were absorbed at different rates by different bodies according to their atomic weight. These properties, along with the fact that the rays exposed photographic plates, inspired him to apply them to medical research, creating the field of radiography. In order to do this, he did not need to wait 15 years for Von Laue to provide a theoretical explanation for X-rays.

Soon other research applications were found for X-rays. Furthermore, radioactivity established a new paradigm of study and created a new scientific community, also being rapidly incorporated and used for research in other fields. For example, Rutherford bombed fine gold plates with alpha particles and obtained new data on atomic structure. Radioactivity also had a considerable impact in biological research; a good example of this importance is the technique of radioactive

⁴² It may be worthwhile to recall that Judson points the discovery of radioactivity as an example of scientific revolution by *opening-up* (Judson, 1996, 586): "The rise of Molecular biology ask for a different model. Copernican astronomy, Newton physics, relativity, quantum-mechanics -but biology has no such towering, overarching theory save the theory of evolution by mutation and natural selection. (And -obviously, but the false analogy is sometimes met- the predecessor the theory of evolution overthrew was not a scientific theory.) Biology has proceed not by great set-pieces battles but by multiple small-case encounters-guerrilla actions-across the landscape. In biology, no large-scale, closely interlocking, fully worked out, ruling set of ideas has ever been overthrown. In the normal way of growth of the science, variant local states of knowledge and understanding may persist for considerable periods. In the tent of our understanding of the phenomena of life, among the panels covered with brilliant pictures that seem to tell a continuous tale, suddenly a new panel begins to appear and grow. Revolution in biology, from the beginnings of biochemistry and the study of cells, and surely in the rise of molecular biology and on the present day, has taken place not by overturnings but by openings-up.

It is not so, further that physics too, even at classical periods, has sometimes gone like that? The discovery of radioactivity in the last decade of the last century was an absolute surprise. It opened an enormous new era-which was exploited much in the manner, fast-breaking and at first localized, then spreading and generating new strains and consequential discoveries, that is characteristic of the guerrilla combat of biology. Be that as, it may, physics in the present era, which it is to say since the second world war, from theories of cosmology to fundamental particles, seems breath takingly agile, promiscuously receptive to new notions, adaptable, and free of doctrinal rigidity. There is a revolution proceeding now in physics, I am told: it seems to be following the model that has been characteristic of the permanent revolution in biology."

isotopes. Therefore, X-rays and radioactivity, whose emergence should be considered quite revolutionary, not only contributed to creating the crisis of physics, that was solved with the great theoretical revolutions of principle of the twentieth century, but also provided biology with tools that enabled researchers to make unexpected progress.

As we shall explain, X-rays and radiation, apart from being physical entities, became experimental instruments in other disciplines. This curious scientific practice has drawn the attention of Ian Hacking who, under the motto (1983, 22-24)

“If you spray them, they are real,”

bases his argument in favour of the realism of theoretical entities on it.⁴³ Hacking’s conclusion is that one has to be realistic about electrons (and positrons) because we are able to spray a ball of niobium with a few. An analysis of the electrons’ role, in this case, agrees with the idea we outlined above; the electrons are being used as tools to study other phenomena.

The birth of this type of experimentation or the use of experimental entities is not particularly distant in time from the discovery of X-rays and radioactivity. Hacking (1983, 227) has highlighted “the rarity of phenomena” that appear in nature by simple observation:

“Outside of the planets and stars and tides there are few enough phenomena in nature waiting to be observed”

and he has spoken in a relativistic way of the effects that are discovered in experimentation :

“I suggest [...] that the Hall effect does not exist outside of certain kinds of apparatus. Its modern equivalent has become technology, reliable and routinely produced. The effect, at least in a pure state, can only be embodied by such devices.”

⁴³ Hacking (1983, 262): “Experimental work provides the strongest evidence for scientific realism. This is not because we test hypotheses about entities. It is because entities that, in principle, cannot be ‘observed’ are regularly manipulated to produce a new phenomena and to investigate other aspects of nature. They are tools, instruments, not for thinking but for doing.” Hacking’s argument about the realism of theoretical entities is related to the notion of disunity of science, that according to Hacking (1983, 183), is the quality “that allows us to observe (deploying one massive batch of theoretical assumptions) another aspect of nature (about which we have an unconnected bunch of ideas).” The disunity of science is, therefore, extremely important in Hacking’s conception, as it was in Galison’s, and his conclusion is that (Hacking, 1983, 218): “The ideal of science is not unity but absolute plethora.”

Avoiding possible controversies about the existence of natural phenomena prior to discovery, Hacking's idea of the creation of phenomena recalls Dyson's thoughts on tool-driven revolutions.

As we have mentioned, Dyson claims that almost all recent revolutions have been *tool-driven* and that these have been very frequent in biology. Of course, the use of entities such as X-rays, alpha particles, electrons, etc., in the creation of phenomena is not very old. This kind of use turns experimental entities into scientific instruments that discover new facts, as well as new concepts, that must all be suitably explained by new theories. This practice gives rise to *tool-driven revolutions*, according to Dyson's description. Furthermore, these new facts are sometimes new entities that can be used in the same way to carry out new research. For example, the idea of the electron as a particle separated from the atomic nucleus appeared when Rutherford bombed an atom with an alpha particle. Later, the electron became a very good projectile and a very useful tool in studying nuclear structure. As we have already mentioned, it is perfectly documented how biology changed when certain physicists arrived on the scene, bringing with them a set of new instrumental skills and devices after World War Two. This kind of effect may be a good explanation for the frequency of *tool-driven revolutions* that Dyson talks about.

Tools are, then, essential in making certain things happen in full view that, although they may occur habitually, we would never notice without them. This idea can be found in Kuhn's work (1970, 65) and is widely supported by Hacking (1983, 206) and, of course, by Galison (1997, 783). What is more, the instrument provides a positive programme of invention, provides a source of inspiration, something always essential to the scientist who has previously tried out all the strategies he can devise in order to find something new. It is in this sense that the disunity of science must be understood, as a factor of progress: there is no single programme, no single paradigm; anything goes and this competition also extends to the way we use and understand the possible contributions of a new instrument, in Dyson's sense of the term.

The ideal of science does not have to lead towards the majestic unity that the positivists dreamed of. It leads instead, as our very image of life does, to a plethora (Hacking, 1983, 218) of research programmes and scientific projects, all competing amongst themselves. This seems to be a quite Kuhnian outlook. It is the image that science offers when it is observed most closely, with greatest attention to its development, as opposed to focusing on an idealistic image of excellence and purity.

Finally, to put it in Dyson's words (1995, 11):

“My message is that science is a human activity, and the best way to understand it is to understand the individual beings who practice it. Science is an art form and not a philosophical method. The great advances in science usually result from new tools rather than from new

doctrines. If we try to squeeze science into a single philosophical viewpoint such as reductionism, we are like Procrustes chopping off the feet of his guests when they do not fit on to his bed. Science flourishes best when it uses freely all the tools at hand, unconstrained by preconceived notions of what science ought to be. Every time we introduce a new tool, it always leads to new and unexpected discoveries, because Nature's imagination is richer than ours.”

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