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Introduction

History, Science and Technology*

Matteo Gerlini†
matteo.gerlini@unifi.it

Recently, the history of science has extended its paradigms to cultural, social, economic and political elements, including their interaction with the development of science or scientific revolutions. There is not enough space here to list all the seminal works that have inaugurated elements of classic social sciences into the history of science, as well as the history of technology. Prominent international academies and scientific institutions have now included in their activities studies and education in culture, science and society, as they already had done with studies in economics and technology.

In this change of paradigm, recent work makes a particular focus on one aspect: specifically, the international dimension of science as a whole. This means, for example, scientific institutions, scientific communities, scientific experiments, and, moreover, the policy of fostering scientific research. Frequently, these researches in the history of science have overlapped with analogous researches in the history of technology. However, only infrequently do such researches coincide with those of international history.

A relatively neglected area of international history studies deals with technological and sometimes scientific issues. Emerging from a background of diplomatic history, such works view the selfsame historical object, but analyse it through the history of science with a different kind of perspective and methodology. This entails an initial overcoming of disciplinary borders in order to develop joint efforts in researching complex historical processes as, for example, nuclear programmes or the development of aeronautical technology. A significant number of pioneering studies have been published, such as the works of John Krige, Dominique Pestre, Gabrielle Hecht, Paul Edwards and Donald McKenzie on the side of studies on science and technology, whereas, on the side of international studies we have David

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† Università degli Studi di Firenze, Italy.

Holloway, Robert Jervis and Gunnar Skogmar, only to mention a selection of names without any presumption of completeness.

The articles presented in this issue of *Humana.Mente* deal with this aspect of science and technology in international history. The authors met in a workshop hosted by the Machiavelli Centre in the School of Political Sciences at the University of Florence, (http://www.machiavellicenter.net/2010/05/workshop-on-international-history-of-science-and-technology-2010/). This took place in June 2010, and one of the aims of the workshop was to debate research on the international history of science and technology, as carried out mainly by Italian scholars. For this reason the reader will find in these pages some worthy studies undertaken by senior as well as junior researchers belonging to an Italian academic framework which still lacks major centres devoted to the study of science and technology from the perspective of the social sciences, similar to those of other countries. The empathy and the pleasure experienced in discussing amongst us a wide range of topics, concerning scientific institutions, the international system, the European integration and the conflicts in aeronautical cooperation, convinced me of the necessity to gather the essays from the principal participants of the workshop.

The opening article on the life of Vito Volterra, more precisely of his exile and political opposition to fascism, characterises the work by Gianni Paoloni, as an example of how an historian of science analyses the biography of a scientist by way of a political perspective.

Following on, three articles deal with the US model of research, the control of knowledge production, and of nuclear policy. Angelo Baracca sums up a piece of long term research as a historian of physics examining two alternative ways in which to perform scientific research. One of these papers, the "Big Science" won on a political level. John Krige, a prominent scholar in the history of science and international relations, explains the co-production of knowledge as an unequal exchange between the USA and the rest of the world, whereby the USA gains control over the knowledge production of other countries.

Also in the collection is an article by Antonio Tiseo, junior fellow in diplomatic history, on former President Jimmy Carter's nuclear policy toward the International Nuclear Fuel Cycle Evaluation, as a consistent endeavour in attempting to control the spread of nuclear technology.

Following on, two articles deal with aero-spatial cooperation as a test for European integration, both offered by historians of international relations.

David Burigana analyses the making of cooperative relations between Europe and the United States, while Filippo Pigliacelli deals with the internal dynamics of aero-spatial research and the making of European integration in itself.

A commentary by Lorenza Sebesta on a recent synthesis on the history of technology and a report by Mauro Elli on the European Science Foundation funded event at Sofia in 2010 close the issue.

I hope that this initiative can contribute toward reinforcing the work of Italian groups and scholars working on the topic of science and technology from an international history perspective, paving the way toward further research and debates.

This border-studies issue is dedicated to my Professor, Ennio Di Nolfo, now in his eighties, who taught me how important Thomas Kuhn's structure of scientific revolutions was in order to approach the history of international relations.

Vito Volterra and the Making of Research Institutions in Italy and Abroad*

Giovanni Paoloni[†]
giovanni.paoloni@uniroma1.it

ABSTRACT

The great mathematician Vito Volterra was a notable figure who had a significant public profile in the early years of the twentieth century.[1] He made an important contribution to political debate and, in particular, to what would become defined as science policy. Volterra's scientific interests were not limited only to mathematics and mathematical physics, but also gave impetus to research in the spheres of oceanography and meteorology. Volterra's career path, characterised by the prominence of the mathematician in the international scientific community, finally reconstituted itself into the position that he assumed toward the fascist regime in Italy. It was the very international acknowledgement of Volterra that caused resentment in Mussolini, toward whose regime Volterra maintained a strenuous opposition – resulting in the ostracism he was subjected to in his own country until his death.

Vito Volterra is generally considered one of the greatest mathematicians of his time: «His most important contributions» according to the *Dictionary of Scientific Biography* «were in higher analysis, mathematical physics, celestial mechanics, the mathematical theory of elasticity and mathematical biometrics. His major works in these fields included the foundation of the theory of functionals and the solution of the type of integral equations with variable limits that now bear his name, methods of integrating hyperbolic partial differential equations, the study of hereditary phenomena, optics of birifrangent media,

* This paper was presented and discussed in the «Tuesday Lunchtime Colloqium» held in Boston, Dibner Institute for the History of Science and Technology, on March 7th, 2006; it partially appeared in Paoloni & Simili (2008).

† Università degli Studi di Roma "La Sapienza", Italy.

1 Goodstein (2007).

the motion of the earth's poles and, in his last years, placing the laws of biological fluctuations on mathematical bases and establishing principles of a demographic dynamics that present analogies to the dynamics of material systems». He was born in Ancona in 1860, and began his academic career as early as 1883, becoming full professor of Mechanics in the University of Pisa when he was only 23. He had been a pupil of Enrico Betti, whom he succeeded upon his death in 1892 on the chair of Mathematical Physics in Pisa, as well as in the direction of the *Nuovo Cimento*, the professional journal of Italian physicists. In 1893 he moved to a chair in the University of Turin; while he taught in Turin, in 1894 was elected a member of the Società Italiana delle Scienze, detta dei XL, in 1895 member of the Accademia delle Scienze di Torino, in 1899 member of the most influential body of Italian academy, the Accademia Nazionale dei Lincei.

In 1900 he moved again, this time to Rome, the topmost University of unified Italy, where he was to become for a number of years (1907-1919) the dean of the Faculty of Mathematical, Physical and Natural Sciences. As soon as he arrived in Rome, he was invited to give the inaugural lecture for the new academic year (a distinguished honor): his lecture "Sui tentativi di applicazione delle matematiche alle scienze biologiche e sociali" (On the attempts to apply mathematics to the biological and social sciences) demonstrated his great interest for the applications of mathematics to the biological sciences and to the social and economic research; this lecture circulated widely, in Italy and abroad, was translated in French in 1906, and both the Italian original and the translation were repeatedly reprinted. Later on, in the Twenties, biomathematics was to become one of his research fields. Before moving to Rome, Volterra had been involved in traditional academic activities: committees, academic elections, professional journals, professional societies; in 1897 he had successfully promoted, with Riccardo Felici and Angelo Battelli, the creation of the Società Italiana di Fisica.

Very keen in international relations, he was in touch with the Swedish mathematician Gustav Mittag-Leffler from 1887, and was involved from the beginning in the organization of the International Congresses of Mathematicians. Volterra established an extensive network of prestigious international correspondents, which included most of the important mathematicians of his time. With some of his French colleagues, and with Mittag-Leffler, he exchanged hundreds of letters. Starting in 1888, he visited many countries in Europe; anyway, his most frequent destination was Paris, where he was regularly invited to lecture. In later years he also travelled to North and South America.

After 1900 his growing interest (both scientific and practical) for the relationship between scientific research and economic and social development, put him in touch with the new technocratic milieu emerging in Rome, under the political leadership of Giovanni Giolitti and Francesco Saverio Nitti. In 1903 Volterra was one of the three members of the commission appointed by Giolitti to study the establishment of a Polytechnic in Turin; in 1905 he became Senator, which represented a consecration of his new role as policy maker in the field of scientific research; in 1906 he promoted the creation of the Società Italiana per il Progresso delle Scienze (Italian Association for the Advancement of Science), soon to become the most influential organization of the Italian scientific community in the first three decades of the 20th century; in 1912 he became president of the newly established Comitato Talassografico Italiano, a national (and soon internationalized) endeavor for marine research in the Mediterranean Sea, which he considered both from the point of view of his biomathematical interests and from that of the Italian fishing industry. In this position, Volterra was also involved in the establishment of the national network for meteorology, in the promotion of studies of the upper atmosphere led by Gaetano Arturo Crocco, and in the early stages of the Italian aeronautics. In these initiatives he could rely on the support of Bonaldo Stringher, economist and member of the Accademia dei Lincei, general director of the Banca d'Italia, a prominent personality of the technocratic milieu, with whom he developed a close relationship.

In 1909 he sailed for his first journey to the United States. He had been invited (with Ernest Rutherford, Robert William Wood and Carl Barus) by Arthur Gordon Webster to lecture in the celebrations of the 20th anniversary of Clark University, near Boston. A few months before leaving Italy he had met the American astronomer George E. Hale, first in Brussels, then in Rome, where Hale had been invited to lecture on his recent research on sun spots. During this first visit in the United States, which was arranged before they met in Europe, Volterra could not manage to reach the Mount Wilson Observatory and reciprocate Hale's visit to Rome, but it was immediately clear that the two gentlemen liked each other. Actually, they were both dealing with a similar set of institutional problems, aimed at establishing a cooperative institutional environment between the scientific community, the government, and industry. There was a big difference, of course, between a country where the corporate industrial system was already strong and far-reaching (US) and a country (Italy) which was at the time a latecomer of industrial development. In histories of Italian economy the years from 1896 to 1914, the so-called "Giolitti period", are usually defined as the years of the "industrial take-off". The need to develop a science-based industry had been stressed by Volterra as early as his inaugural lecture of 1900; as a member of the Turin Polytechnic Commission he had thoroughly studied the German model of university-industry relationship, and had praised it as a model to be pursued in the establishment of the new school. But he was clearly more attracted by the ongoing developments of a different model in the US in the first two decades of 20th century:

> Close cooperation between the industrial interests and the educational institutions of the country, which in Germany was made so effective by the domination of both by the State, can in America be brought about only by a voluntary personal relationship between the executives of the companies and the instructing staff of the institutions. (Noble 1977, 143, n. 80)

Between 1907 and 1920 Hale was a leader in the American scientific community to this aim: he was involved in the origins of Caltech, in the renewal and strengthening of the National Academy of Sciences, and during World War I in the establishment of the National Research Council.

In 1910 a young, brilliant student from Harvard University, Griffith C. Evans (a pupil of William F. Osgood and Maxime Bôcher), obtained a Sheldon Travelling Fellowship, and decided to use it to travel to Europe from 1910 to 1912, where he spent most of his time in Rome, studying with Volterra. The Italian mathematician and his family made a warm welcome to the young American, and Virginia and Angelica (the wife and the mother of Vito) grew very fond of him: with Virginia they were to keep in touch for many years, even after Vito's death, until the beginning of the Sixties; we know from Evans that he spent many Sundays lunching at the Volterra's, in Rome, Via in Lucina, or in Ariccia (a small town near Rome where Volterra had his country house), and that in the afternoon he would talk with Vito not only on mathematical subjects, but also on topics related to university, science policy and politics in general in the United States. In the following years Evans was for Volterra a continuing source of useful information on those topics. In 1912 Edgar O. Lovett (with whom he corresponded since 1903) invited Volterra to lecture in the ceremony for the formal opening of the Rice Institute (other invited speakers were, for science, Hugo de Vries, Emile Borel, Henri Poincaré, William Ramsay, Wilhelm Ostwald, Carl Størmer, and for the humanities, Henry Jones and

Benedetto Croce). This time, Volterra had a full schedule to comply with: first he had to go to San Francisco, to lecture at Berkeley, then he reached Pasadena, where he could visit the Observatory and talk at length with Hale, then he went to Houston, to lecture at Rice. Evans had returned to the United States a few months before. He was still in Rome, when he received offers from Yale, and from the University of California at Berkeley (according to one of his biographers he also turned down an offer from MIT). He discussed these offers with Volterra, as he found them unsatisfactory, both from the point of view of the salary, and of the kind of job he was being offered. Volterra had mentioned Evans to Lovett already when they met in Paris, in January 1912. At the end, Evans accepted an offer from the Rice Institute, as he felt that Rice offered him the greatest opportunities. In his letters he thanks Volterra, saying that he got the job at Rice (where he remained until 1934, when he accepted to go to Berkeley) because of his support. At the Rice celebration Volterra's attendance was given an outstanding acknowledgement: he talked twice in the official addresses, and twice as invited lecturer, as he gave not only the lectures he was supposed to give, but was also asked to commemorate Henri Poincaré, who had been invited to the Rice inauguration and unexpectedly died shortly before.

In 1914–1915 Volterra was very active in the political debate on Italy's position in the European War. He was against the neutrality proclaimed at the outburst of the War, and in favour of an alliance with the French-British Entente against the Central Empires. When Italy broke its neutrality in 1915, May 24th, Volterra entered the Army as a volunteer in the Air Force, and was immediately involved in scientific and technical inter-allied cooperation: in this position he met again with friends, the French Borel and Picard, the British Schuster, and the Americans Hale and Evans. The former was leading the newly created National Research Council, the latter was liaison officer in Paris and Rome. Volterra in 1917 became the director of the Ufficio Invenzioni e Ricerche, from which the Italian National Research Council was to develop after the War, and in 1918–1919 he worked with Hale, Schuster and the Belgian Georges Lecointe in the foundation (led by Hale) of the International Research Council (nowadays the International Council of Scientific Unions) of which he was to become vice-president. In 1920 he was elected president of the Società Italiana delle Scienze detta dei XL, and in 1923 president of the Accademia Nazionale dei Lincei. In the same year he succeeded in obtaining from the government the creation of the National Research Council, and he was appointed president of the new body. It has to be borne in mind that since the previous years he had remained a member of the boards of both the SIPS and the Comitato Talassografico, and immediately after the War he had been appointed president of the Bureau International de Poids et Mésures (of which he would remain president until his death), with Charles Guillaume as Secretary General: under their guidance the Bureau built its new location at Pavillon de Sèvres in 1931 and established the new measurement standards for electricity and photometry.

In November 1919 Volterra sailed again to the United States, this time to lecture in Berkeley. On the way back he visited again Pasadena and Houston, to meet Hale and Evans, and to participate in some social events organized by Evans and Lovett. Before leaving to Europe he also found time to give a talk at Cornell University, which invited him when they learned he was in the States. This was Volterra's last visit to the USA; archival research shows that he was planning at least three further visits, in 1923, 1926 and 1937, but that he failed to leave for different reasons. The interest for the United States shown by these plans let us understand that what he said in one official address at the Rice inauguration was not just said for mere courtesy:

> Allow me to express the feeling of admiration that I experience in visiting this great new country, an admiration that has changed only to increase since my last coming to America. Your high civilization and enterprising spirit have been able to conquer an entire continent, to create as if by enchantment marvelous cities like this which we are visiting now [Houston]. These grow up in a few years. They provide themselves not only with all the modern comforts which make existence easy and agreeable, but also reach a high place in life that is intellectual and moral. [...] You have created institutions from the beginning and at once, universities in which you can accommodate everything to the demands of the present, without the embarrassment of a single relic from the past.[2]

He kept being interested in science policy and in general politics in the US, as is shown in his correspondence with Evans, who sent him comments on Hoover, and on Roosevelt: one of Evans' pupils, C.F. Ross, had become chief economist of the NRA. From 1924 Volterra was also involved in the important Rockefeller Scholarship Program: he met Wickliffe Rose in his travel to Europe, then met and corresponded with Augustus Trowbridge, a Princeton physicist «in charge of the Board's work in this field in European countries».[3]

[2] Rice Digital Scolarship Archive. http://scholarship.rice.edu/bitstream/handle/1911/8864/article_RI034231.pdf?sequence=4, consulted on December 30th, 2010.

[3] Rose to Volterra, 1925.

When the IEB funded the creation of the Institut Henri Poincaré in Paris, Volterra was involved, and was invited to lecture for the inauguration of the Institute.

Three main concerns are remarkable in Volterra's institutional activity: 1) his attention to the relationship between scientific community, politics, and economic development; 2) the desire of overcoming the limits of hyperspecialization and promote the crossing over of disciplinary boundaries; 3) his being involved in the institutional development of disciplinary fields outside mathematics, and especially in three of them, where an important renewal of methods was on the go, i.e. economy, biology, and physics. It is striking that Volterra, long before being himself directly involved in bio-mathematical research in the Twenties, had a leading role in creating the Comitato Talassografico, and through it a whole network of laboratories for the biological, physical and chemical study of the Mediterranean Sea, including meteorology studies, aeronautical research and studies of the upper atmosphere. Even more striking the fact that Volterra had a key role in the organization of the Italian physicists, far beyond the role he played in the organization of Italian mathematicians. As mentioned above, in 1897 he was the main promoter of the the Società Italiana di Fisica, and he did so as a response to the founding, in 1896, of the Associazione Elettrotecnica Italiana (the society of electrical engineers promoted by Galileo Ferraris), as he feared that the AEI might become the only professional association available for the Italian physicists.

In Rome, Volterra taught at the School of Mathematics, in those years closely connected to the School of Engineering in San Pietro in Vincoli, but in 1902 he affiliated to the Institute of Physics, directed by Pietro Blaserna. Blaserna and Volterra wanted to promote in this Institute new research patterns, based on what was called, at that time, the "new physics": in order to strengthen this scientific approach they obtained the creation of a chair of "fisica complementare" on which they called the young and brilliant Alfonso Sella, whose untimely and sudden death in 1907 was at the origin of the coming in the physical institute of Rome of Orso Mario Corbino in 1908. Volterra sponsored the publication of important works of young Tullio Levi Civita in the *Nuovo Cimento*, prevailing on unwilling colleagues. As for the IEB, at the request of Trowbridge Volterra sponsored the scholarships of André Weil (proposed by Vessiot; Weil had just spent one year in Rome with Volterra on a scholarship from the Ecole Polytechnique), Robert Mazet (proposed by Vessiot; Mazet was to come to Rome to work with Levi Civita); at the request of Guido Castelnuovo he sponsored Bruno De Finetti. But the scholarships he himself proposed to the Rockefeller, with the exception of Szolem Mandelbrojt (to come to Rome to study with Volterra himself) were aimed at the development of studies in nuclear and theoretical physics: Enrico Fermi, Enrico Persico, and Franco Rasetti (who was, thanks to Volterra, exceptionally granted two scholarships, in 1928 and 1930).

Volterra had never liked Mussolini's government. In a letter of 1922 to Charles Guillaume he expressed his "concern" for the political situation in Italy, but, like many other members of the liberal establishment, he did not question, at first, the legitimacy of a government appointed by the King. He simply continued the activities connected with his important institutional position, and kept willingly to cooperate, when necessary, with governmental bodies. Things began to change in 1924, when the "Matteotti affair" demonstrated the true nature of Fascism and precipitated the gradual transformation of Mussolini's government in a dictatorship. Under these circumstances Volterra, who had been in 1923 an open opponent of the Educational Reform promoted by Giovanni Gentile, in October 1924 joined the Unione delle forze liberali e democratiche led by Giovanni Amendola, in 1925 signed the "Manifesto degli intellettuali antifascisti" proposed by Benedetto Croce, and soon after joined the group of anti-fascist Senators, the only legal group of opponents that Mussolini was forced to tolerate since he could not dissolve the Senate (composed of life-lasting members appointed by the King) as he had done with the Chamber of Deputies. At the beginning of 1926 the government started a, firstly non declared but gradually open, war against Volterra's influence on the scientific community: in June 1926 it prevented his re-election as president of the Lincei, then dramatically stopped funding the NRC until a new president (Marconi) was appointed in 1927, and in 1928, at the meeting of the International Research Council, the Italian delegates, at the general astonishment, declared that they would no longer recognize Volterra as vice-president.

In 1931 the university professors were ordered an oath of fidelity to the fascist government: by refusing it Volterra lost his academic position, and was forced to retire (it must be stressed that only 12 university professors on roughly 1.200 refused the oath); the story repeated in 1934, with a similar oath being imposed on the members of academies and science institutions: at this time, Volterra ceased to be a member of the Lincei. In 1938, being Jew, he was victim with his family of the racial laws, though, as a Senator, he was partially safeguarded against the worst aspects of anti-Semitic legislation. Volterra reacted with exceptional vitality to this situation: he kept going on with his scientific activity and kept alive his extraordinary network of relations, in Italy and abroad.[4] He died in 1940, at the age of 80, a few months after Italy had entered World War II.

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[4] Israel (2005).

Too Big or so Little? Nuclear Physics in the Thirties and Forties in USA and Japan *

Angelo Baracca[†]
baracca@fi.infn.it

ABSTRACT

The deep reasons of the birth of Big Science are still the object of a debate. The reconstruction of some aspects of the growth of nuclear physics with artificially accelerated particles in the 1930s and 1940s may help to throw light on the roots of large-scale research. On the basis of the original documents, we compare the attitudes and research styles of Ernest O. Lawrence and Merle A. Tuve, and their open clash. The first one was probably the most significant representative of the new approach, mainly interested in the construction of bigger accelerators. On the contrary, the latter – who gave fundamental contributions to nuclear physics, using a relatively small electrostatic accelerator – expressed the strongest and most explicit opposition towards large-scale research trends. Our argument is completed through the analysis of the effects of the introduction of particle accelerators in Britain and Japan, where they did not generate large-scale research before the Second World War.

1

Today we are so accustomed to large-scale research that we tend to consider it as an almost natural way of organizing and performing this activity. From an historical point of view, however, we cannot avoid questions such as: what was the genesis of Big Science? What were the causes and the conditions of its

* This article prosecutes a research presented in Baracca (1993). I wish to thank the Library of Congress, Washington D.C., the Bancroft Library of the University of Berkeley, California, and the Carnegie Institution of Washington for their hospitality and the permission to consult their archives during the completion of this research.

† Università degli Studi di Firenze, Italy.

birth and development? Which were the steps that prepared its advent?

In fact "Big Science" did not suddenly grow out of war-time emergence and of such enterprises as the "Manhattan Project". It was instead prepared and partly anticipated by a series of previous choices and changes that took place in the leading fields of scientific research. Such innovations developed in connection with the evolution of the role, the social position, stimuli and cultural horizon of the scientific community and of the role of science and technology and their mutual relationships.

In order to get a better understanding of these transformations and to place them in a historical perspective, I have chosen to investigate the contrasting attitudes that developed (explicitly or implicitly) against the early trends towards large-scale research and the alternatives that were proposed to them. Such an investigation should not give the impression of a nostalgic point of view, since our purpose is to contribute to the understanding of the objective historical trends. This approach does show in fact that the road to large-scale research was not a compulsory choice from a point of view of scientific investigation in itself: extremely valuable experimental and theoretical physics was being done by those scientists who did not accept this road; they sometimes got even more accurate or better results. But Big Science turned out to be the winning choice because it corresponded to the stream of historical and social development.

With this purpose in mind, I have studied the growth of nuclear physics with accelerated particles in the thirties, I have followed the war and postwar choices in research activity made by some of the leading scientists in this field and I have compared the developments in different countries, in order to distinguish and characterize conflicting or divergent roads or styles of research.

2

Let me start with the U.S. The outburst in this field of research took place here at the very beginning of the thirties and one is struck by its coincidence with the worst period of the economic recession: the growing difficulties in the funding and development of scientific research in general, strongly contrast with the relative easiness with which atom-smashers found financial support and started large-scale research. Behind this one recognizes the precocious interests of the leading industrial sectors toward the emergent fields and the new role that scientific and technological innovation had to play in the New Deal. New features appeared in scientific activity in such fields as particle accelerators and nuclear physics in the U.S. (in contrast, as we will see, with other countries): growing costs and dimensions of machines and labs, team research, competition and rush for the results, growing mean number of authors for each paper, management as part of scientific activity raising increasing funds.

Three groups developed early particles accelerators in the United States (Mc Millan 1979):

1) that of Lawrence in Berkeley;
2) Tuve at the Department of Terrestrial Magnetism (I will call it DTM) of the Carnegie Institution of Washington;
3) the group of Crane and Lauritsen in Pasadena.

Ernest O. Lawrence was probably the most significant representative of these new trends, while his friend Merle A. Tuve – another protagonist and leading scientist – expressed perhaps the strongest and most explicit opposition toward large-scale research trends.

Some striking features of Lawrence's character have already been analyzed (Davies 1968; Heilbron, Seidel, & Wheaton 1981; Seidel 1978): his competitive and managerial leadership, his constant trend towards larger machines and higher energies, his ability in collecting financial support everywhere, and in this connection his concern with showing the practical usefulness of his products.

Tuve, on the contrary, had a very different, and in many respects opposite, attitude. In fact it is striking that he was one of the scientists who made major fundamental contributions to the progress of nuclear physics during the thirties (and of other disciplines after the war) but his name and achievements are almost unknown to the great majority of today's physicists. Lawrence moreover was awarded the Nobel Prize in 1939, while Tuve missed out, even if he probably would have deserved it more than once. These facts greatly derived from Tuve's particular character and attitude, which led him to dislike the mechanisms and spirit that were increasingly pervading a research activity of ever growing dimensions. In this sense Tuve in the end ended up defeated by the changes taking place.

Remember that Lawrence and Tuve were born in the same town, were school-friends and constantly linked by deep friendship all through their lives. The bitter remarks Tuve had to make about Lawrence's research are even more significant.

Lawrence's group published the first results of experiments in nuclear physics with charged accelerated particles well before Tuve's group (Baracca, Livi, Piancastelli, & Ruffo 1985): unfortunately the lack of rigour in these experiments became evident in short time and was recognized and constantly remarked by Tuve himself. On the other hand it is well known that Lawrence, working at the cyclotron and disposing of it, really missed some of the main discoveries, namely artificial disintegration of the nucleus and artificial radioactivity.

The opposite attitude of Tuve's group is striking: a great accuracy in designing, the machines and the experimental techniques, in testing the apparatuses, before really entering nuclear physics research.

The first experimental results published by the DIM group (Tuve, Hafstad, & Dahl 1933) were in fact in clear disagreement with Lawrence's previous results, but Lawrence replied insisting on his own results, even if «there is always, of course the possibility that these alpha particles are due to impurities»[1] (and Tuve added a note to the letter: "Impurities?!").

In the same letter Lawrence reported the first results on the scattering by accelerated deutons, obtained in collaboration with the chemist Lewis. It is interesting to remark that, in spite of the growing divergences, the Lawrence-Tuve friendship was so deep that the first provided the latter with the heavy water necessary to perform the experiments with accelerated deuton beams.[2]

On the other hand these experiments became the major point of disagreement. In fact, Lawrence, proposed at that time the famous "deuton disintegration hypothesis" (Lawrence, Livingston, & Lewis 1933), that he reported at Solvay Conference raising the criticism of the European physicists (Heilbron *et al.* 1981; Baracca *et al.* 1985).

Tuve was already very sceptic on this hypothesis; he had warned Lawrence: «I am not able to follow your suggestion».[3] Lawrence had already replied that, if the initial evidence was effectively scarce, «I think we have now pretty conclusive evidence on that point».[4]

[1] *E. O. Lawrence to M. A. Tuve, May 3, 1933.* Tuve Papers, Manuscript Library, Library of Congress (Box 12, Special Letters 1933).

[2] A. Fleming to G. N. Lewis, May 9, 1933, Tuve Papers, loc. cit.

[3] M. A. Tuve to E.O. Lawrence, October 2, 1933, Tuve Papers, loc. cit.

[4] *E. O. Lawrence to M. A. Tuve, October 9, 1933*, Lawrence Collection, Bancroft Library, Berkeley.

After the Solvay Conference, Lawrence had to perform more accurate tests in order to exclude that his results derived from systematic contaminations (Lewis, Livingston, Henderson, & Lawrence 1934), as he wrote to Tuve on December 21, 1933.[5]

Tuve, significantly conscious of the relevance and the delicacy of the problem, had answered Lawrence's letter on January 6, 1934, specifying that he had no new result since the whole period was spent in a very rigorous test of the experimental techniques.[6]

But when careful experiments were performed by Tuve in the following weeks, the disagreement exploded. The «preliminary runs» already showed «a great deal of difficulty in correlating our observations with those you have published»[7] – with the whole set of observation, not only the deuton results! – and suggested: «that you check over your apparatus very carefully, since at present [...] there appear to be the basis for suspicion that at least part of your observations are due to some factor common to all your target, which may be contamination, slit edges, tar et mountings or some other factor».[8]

At that point Lawrence's reply[9] was lengthy but appeared very embarrassed, and outlined the first autocritical considerations, since in the meantime his deuton results had been contradicted also by the Pasadena group (Lauritsen and Crane 1934) and at the Cavendish Laboratory (Cockcroft & Walton 1934; Oliphant, Harteek, & Lord Rutherford 1934):

> You are quite right in surmising that in our preliminary measurements there have been some errors [...] Rather than continuing experiments we have decided to embark on a program of careful observations of things already brought to light and it is our intention to get as accurate measurement as we can.[10]

Lawrence finally admitted his mistake in the deuton disintegration hypothesis (Lewis *et al.* 1934). But Tuve criticism, as we have remarked, was

[5] E. O. Lawrence to M. A. Tuve, December 21, 1933, Lawrence Collection cit.; see also letter of January 12, 1934, ivi.

[6] *M. A. Tuve to E. O. Lawrence, January 6, 1934*, Lawrence Collection Bancroft Library, Berkeley.

[7] *M. A. Tuve to E. O. Lawrence, February 28, 1934,* Lawrence Collection, Bancroft Library, Berkeley.

[8] Ivi.

[9] *E. O. Lawrence to M. A. Tuve, March 14, 1934*, Lawrence Collection, Bancroft Library, Berkeley.

[10] E. O. Lawrence to M. A. Tuve, March 14, 1934, cit.

much deeper and concerned not a single result, but the whole set up and method of the experiments performed in Berkeley and the hurry and lack of caution with which they had been published. It is interesting to remark that on the contrary Tuve, up to the moment, had avoided making public the controversy, although he was already sure of his own results. At that moment, he sent on April 14, 1934 a letter to *The Physical Review* (Tuve & Hafstad 1934) contradicting practically all the results published from Berkeley and he sent a copy to Lawrence with some bitter notes:

> I wrote you at the end of February warning of the direction which our results were undoubtedly taking. After working up all of our results, we reached the astounding conclusion that we were unable to check a single one of the observations which you have reported so far [...] I must say that we were certainly not enjoyed the position in which we have been placed. Once in a lifetime is once too often.[11]

In Tuve's action one may recognize a mixture of real embarrassment and professional ethics, of a kind that probably has progressively disappeared in subsequent years. In this sense, on one side, evidently pressed by a growing debate on the issue, he personally pointed to Lauritsen that

> The question for many people as to whether we check Lawrence's work or not have became so insistent that there is no way of avoiding the issue and we decided that a bald statement was far preferable to any evasion of the question on our part. We have been very circumspect in what we have said even to close friends visiting the laboratory until the abstracts had to be written.[12]

On the other side, however, a harsh press release was emitted by the Carnegie Institution of Washington after the Meeting of the A.P.S. of April 26, with the ironic title "Atom-Smashers Reveal Atomic Masquerade", containing such statements as the following:

> Speaking before the American Physical Society meeting here today (April 26), Drs. Tuve and Hafstad of the DTM, Carnegie Institution of Washington, dramatically announced that they had succeeded in unmasking the outlaw atoms which have played havoc with the results of atom-splitting investigations currently iD progress in various laboratories. The renegade atoms which gave

[11] *M. A. Tuve to E. O. Lawrence, April 18, 1934*, Lawrence Collection, Bancroft Library, Berkeley.

[12] *M. A. Tuve to C. C. Lauritsen, April 18, 1934*, Tuve Papers, loc. cit. Box 16, Letters – Special 1934-5-6.

> rise to pseudo-transmutations of carbon, oxygen, and other targets when bombarded by high-speed atoms of heavy hydrogen, are the atoms of heavy hydrogen itself, sticking in the pores of the solid target after being driven there by the high-speed beam.[13]

On August 4, 1934 Tuve himself sent *Science* – through Fleming – an official rectification[14] since the Journal had reported in «erroneous and misleading» terms the results obtained at the DTM, had not explicitly referred of the «contamination effects» and had expressed the opinion that the experimental results from various laboratories were not in contradiction.

The whole story inspired Tuve with a sense of deep regret that he expressed to Lauritsen bitterly remarking that such an accident «must occur rarely, if at all» and, since Lauritsen replied that «that sort of things should never appear in print», he firmly added that rather «the sort of things that should never appear in print were what led to the necessity for such a statement by me».[15]

This course of events reveals not only the early emergence of different styles in performing research activity.

In the following years Lawrence concentrated on cyclotron building and insisted mainly on its use in medicine, while Tuve obtained from his rigorous and careful practice some of the most significant results in nuclear physics (Baracca *et al.* 1985), namely, in 1935, the first widths of nuclear resonances and, with his beautiful experiments on proton-proton scattering, the charge independence of nuclear forces.

I could note that the cyclotron was perhaps mainly the father of the post-war new generation of accelerators, while Tuve's "Atomic Observatory", built up at the Department of Terrestrial Magnetism, perfected electrostatic machines, but preserved the familiar atmosphere still existing today in this institution.

Lawrence's choices appear instead dictated more by the goal of rising funds for big enterprises, by a need of guiding or following the stream of advanced research, than by true scientific motivations. For instance, in 1935 he wrote Bohr:

> In addition to the nuclear investigations, we are carrying on investigations on the biological effects of the neutrons and various radioactive substances and are

[13] Carnegie Institution of Washington archives, folder "DTM-Miscellaneous 1934-35".

[14] A. Fleming to J. Mckeen Cattel, August 4, 1934, *Nuclear Physics Symposium: A Correction*, CIW archives, loc. cit.

[15] *M. A. Tuve to C. C. Lauritsen, September 26, 1934*, Tuve Papers, loc. cit. Box 16, Letters-Special 1934-5-6.

> finding interesting things in this direction. I must confess that one reason we have undertaken this biological work is that we thereby have been able to get financial support for all of the work in the laboratory. As you well know, it is so much easier to get funds for medical research.[16]

A different spirit was really born, anticipating the mechanism of Big Science.

3

A stronger confirmation of the new features that are appearing may be obtained following more thoroughly Tuve's uncommon choices during and after the war.

Note that Tuve had made important contributions in more than one field and that there were in principle many possible fields in which he could have given relevant contributions to war research. When he and G. Breit had tried as early as 1925 to determine the ionosphere height observing the echoes of short radio pulses, «they were troubled by echoes coming from airplanes, which interfered with the measurements»;[17] «this was the first recorded instance of distance measurements made by the pulse-radar method».[18]

Tuve made moreover leading contributions to the study of nuclear fission. With Roberts, Mayer and Hafstad he showed the first fission process at the DTM accelerator,[19] discovered the emission of the "delayed neutron"[20] and subsequently they contributed to show the possibility of a chain reaction:[21]

> We have been hard pressed to get some data on uranium fission, largely because Fermi, Rabi, Szilard, etc. have been afraid of chain reaction possibilities. Regular "war secr" with secret meetings etc.! Pres. Bush is anxious to see it settled. All indications now are that no chain can occur but it is pretty close.[22]

A confidential memorandum of June 1, 1939 to the Director of the DTM by

[16] *E. O. Lawrence to N. Bohr, November 27, 1935*, Lawrence Collection, Cartoon 3, Folder 3, Bancroft Library, Berkeley.

[17] Report of the President, 1952, Carnegie Institution of Washington.

[18] *Biography of M. A. Tuve* (anonymous), p. 4, CIW archives, Folder Tuve 1.

[19] *M. A. Tuve, Report to the Director of DIM for January 1939, 7.2.1939*, Library of Congress, Manuscript Library, Tuve Papers, Box 15, "Monthly reports"; Roberts, Meyer, & Hafstad (1939); Stuewer (1985).

[20] *M. A. Tuve, Report for February 1939, 9.3.1939*, loc. cit.; Roberts *et al.* (1939).

[21] CIW, *Year Book 1939* (July 1939 - June 1940), 87.

[22] *M. A. Tuve to G. Breit, 2.8.1939*, DTM Office Archive, File "Archive Uranium".

Gunn, Technical Adviser of the Naval Research Laboratory at Anacosta, explicitly mentions in this respects Tuve's availability «to carry on the final tests at his laboratory»;[23] on May 23, 1940 the Carnegie Institution of Washington appropriated $ 20.000 «for study on uranium fission».[24]

Tuve was a member of the Uranium Committee called by Roosevelt after Einstein's letter, but his attitude changed at the beginning of 1940. «It all started in February 1940 [...] At that time, Roberts, Hafstad, Heudemburg and I simply decided that we would do no more physics research if the likes of Hitler were to inherit our efforts. We undertook to find a way that we could contribute to the technology of modern war».[25] While «by May 1940, in talks with officers in the R and D division of BUORD, U.S. Navy, I had learned about the ridiculously low effectiveness of antiaircraft fire. I heard the term "influence fuze" (later "proximity fuze"), as wistful hope».[26]

The history of the "proximity fuze" has in part been written (Baldwin 1980). We are here interested in one specific aspect. In organizing and directing first the "Section-T" and then the Applied Physics Laboratory (APL), Tuve followed an attitude opposite to that then prevailing and growing in the other projects, of early Big Science. He started with the "four indians" and followed the concept of a «local and flexible group to test the feasibility of various ideas submitted to him».[27] In Tuve's words:

> One of the greatest "new developments" of the war [...] was the rediscovery [...] of the efficiency of the democratic principle of directing the effort of organized group of people [...] A boss using the democratic principle does not depend on just giving order from above [...] Asking people to help with the whole job was what I used in running the proximity fuze development [...] The democratic system is more effective, dollar for dollar ad hour for hour, than the autocratic system [...] The key to the effectiveness of the democratic system is simply that criticism flows both ways; criticism and ideas come up from workers as well as down the bosses.[28]

But, in spite of Tuve's subjective wishes and intentions, the Applied Physics

[23] R. Gunn, Memorandum for the Director, 1.6.1939, DTM Archive.

[24] Minutes of the Executive Committee, Meeting of May 23, 1940, CIW Archives.

[25] Tuve (1982). *APL News* (Feb.), 8 [questo riferimento manca in bibliografia!].

[26] Ibidem.

[27] F. R. Roberts, "Development of the Proximity Fuze", manuscript required quickly by Abelson on Oct. 20, 1977, CIW Archives, Folder DTM Misc., p. 65.

[28] Ivi., p. 5.

Laboratory evolved into a model of advanced large-scale research. This happened not only under the pressure of emergence in the war-period, but mainly because the force of things – in this case of the Big Science mechanism – was stronger than subjective intentions.

Tuve's post-war choices were an attempt to react concretely against Big Science and to follow a different path. In a research program he proposed in the spring of 1946[29] a preliminary choice was discussed in the initial

> General comments [...] It is pertinent to question whether the Institution should have any postwar program at all in nuclear physics, with large-scale government support assured in many countries and with this field of scientific effort sure to be tied up with political power-struggles, certainly for many years to come. The conclusion was reached, however, that work in this field should be continued at the Department.[30]

The end of war-time emergency thus no longer justified "large-scale government support". As a matter of fact, Tuve - coherently with his positions - had come back to the DTM (his pupil Hafstad had succeeded him as Director of the Applied Physics Laboratory and fully entered the Big Science mechanism). When Jewett submitted to Lawrence himself and other members of the Committee on Terrestrial Sciences of the Carnegie Institution on March 18, 1946 Bush's suggestion that Tuve be appointed to the Directorship of the DTM, he underlined Tuve's qualities, but raised doubts because «he has at times in the past shown a tendency to rub men the wrong way» (even adding that he «has matured very considerably in the last few years») and concluded that «both Bush and I are agreed that Tuve will be either a great success or a very great failure as Director».[31]

Tuve, on his part, presented the already mentioned suggestions,[32] and a

[29] "Suggestions for Postwar Laboratory program of the Department of Terrestrial Magnetism" prepared by M. A. Tuve; March 19, 1946; revised May 9, 1946, Lawrence Collection, cartoon 32, Folder 32. Bancroft Library, Berkeley.

[30] Ibidem.

[31] Frank B. Jewett to Dr. Homer L. Ferguson, Dr. Ernest O. Lawrence, Dr. Alfred L. Loomis, Dr. Frederic W. Walcott, March 18, 1946, Lawrence Collection, cartoon 3, Folder 32, Bancroft Library, Berkeley.

[32] "Suggestions for Postwar Laboratory program of the Department of Terrestrial Magnetism", cit.

subsequent more official statement[33] concerning the future research program of the DTM. In the official report the premise on «General objectives», an emphasis specifies the connection between the choice of continuing the research activity in a Department of limited possibilities and the kind of research that can be performed:

> Bearing in mind the special character of the opportunity presented by the Carnegie Institution of Washington, with its unusually great freedom of objectives, since there are no external groups whose interests limit the program, and viewing the corresponding obligations which go along with this freedom, it is agreed that we must make every possible effort to emphasize creative work, work with new potentialities, and work which lies on the front lines of knowledge. There are serious restrictions as to possible size of staff and annual expenditures, and accordingly our program must be chosen with regard to its effectiveness as a stimulus or catalyst to the work of all other groups concerned with a given field. These considerations lead naturally to a major emphasis on cooperative endeavours, in which the Institution and the Department can be of great influence and value if we are capable of vigorous leadership in fresh and significant directions.[34]

In this connection, Tuve proposed that work in nuclear physics should be continued anyway by a «recognized and well qualified group quietly working on private funds at an agency of high standing and very wide connections, such as the Carnegie Institution».[35] More precisely

> True research – creative research – is always done in very small groups, rarely exceeding five or seven individuals, and hence this separation of the Department's staff into very small discreet groups, with reasonable fluidity for shifts between groups, is regarded as both realistic and healthy; [...] creative research is never carried on by groups larger than seven members - usually four is a better size. Larger groups invariably concern themselves with engineering or development, not with the painful carving out of really new ideas or directions of progress. Several groups of three to seven members, each with one or two strong men (age difference is valuable), can be loosely associated but creative research is not carried out by large teams who are coordinated (that is,

[33] "Statement Concerning the Scientific Program of the Department of Terrestrial Magnetism for the Immediate Future", by M. A. Tuve, June 22, 1946, Lawrence Collection, Cartoon 3, Folder 32, Bancroft Library, Berkeley.

[34] Ibidem.

[35] "Suggestions for Postwar Laboratory program of the Department of Terrestrial Magnetism", cit.

ordered) or closely directed by a single head man. A leader can stimulate several groups to productive activity, but real creative research is not carried out toward goals which are defined in advance too specifically or in too limited a way. At best, its limitations can only amount to a positive encouragement or emphasis in a selected broad area of interest, and valuable offshoots are sure to occur in other related but rather unexpected directions. A single over-all leader, stimulating and guiding toward general goals, is, however, most valuable and even necessary, to insure cooperation and integration in place of fragmentation into separate compartments and unrelated interests.[36]

[...] It is our conviction that investigators can be stimulated and led to creative contributions, but they cannot be driven; hence we must evolve leaders in our small groups, but we cannot use authoritarian procedures. Individual professional responsibility, however, also means that individuals should be judged by their creative research contributions; steady or devoted work is almost irrelevant as a criterion of accomplishment or virtue. Since individuals differ in their capacity to contribute creatively, however, they will be expected to recognize this and to invest their energies willingly in directions which are pointed out by other members of the group working in their field of interest, after group consideration indicates that these suggested directions for effort give promise of creative fruitfulness.

One picture should always be kept in mind by the professional research staff: it must surely be evident to everyone that the Founder of the Institution had no thought whatever that his great free endowment should be used to keep 150 people simply busy six hours per day! In fact, he must have intended just the opposite; his endowment was intended to free a certain creative group of men from the necessity of having to be busy, and their success in measuring up to their opportunity can only be measured in term of their creative output.[37]

What kind of research did Tuve suggest in this context?

The chief aim of the suggested program as for any research program, appropriate to the Institution, may be stated as an effort to underwrite and support the vigorous personal activities of modest number of competent research men, associated in a congenial and cooperative group with a variety of different and related interests, who are pushing forward the front-line boundaries of knowledge. To be appropriate, their objectives should be to establish basic principles, or the materials on which such generalizations may be

[36] Ibidem.

[37] "Statement Concerning the Scientific Program of the Department of Terrestrial Magnetism for the Immediate Future", cit.

> expected to be formulated, the work should be directed toward major unknowns or big unanswered questions, and it should lie in areas of learning in which such new knowledge, if attained, would have importance, in the sense that it could be expected to have considerable significance to many human beings, other than the specialists directly concerned. The specialized laboratory work in nuclear physics at the Department before the war – resulting, for example, in the demonstration and measurement of the proton-proton and proton-neutron interactions – and the biophysical work with radioactive tracers during the war – concerned with fundamental physical processes in physiology – has met these criteria. Much more work of this fundamental kind remains invitingly open to immediate postwar attack. This is one appropriate goal for the laboratory program.[38]

But the research work «in government and private research institutes, contrasted with those of similar groups in various universities, also public and private» poses, in Tuve's opinion, a fundamental problem.

> The impact of young minds has long been recognized as a major factor in keeping university staff members productive and creative in fresh directions [...] In the course of ten years a (lively) professor will give half a dozen different courses, each of which requires him to work over a different area of his broad professional field. He will also be obliged many times to take charge of research students who select problems which lie more or less outside of his own special field of current interest and work; this, too, requires him to study, think, discuss, and even create new ideas in various different areas of his broad professional field. [...] Contrast this with staff members of specialized research institutes; in the same ten years, working all of his time in a narrow field, the specialist dries up many of the channels by which he should receive nutrition from his own broad professional field. [It follows that] the prewar program should go forward, but it should be modified to become something other than just a specialist group-activity in nuclear physics or biophysics. The dangers of over-specialisation in these fields may be a great as in many others. [Instead] a research specialist should actually work at least a fifth of his time *outside of his speciality* and in some other area of his broad professional field. [More precisely] it seems reasonable that an investigator might be required to "work" one-fifth of his time on problems which lie outside of his speciality, and that an actual output in this other area should be expected (that is, some arrangement is needed which requires him to face critical judgments of others) and furthermore that, although he may be a lifelong specialist in some one field, this second or

[38] "Suggestions for Postwar Laboratory program of the Department of Terrestrial Magnetism", cit.

minor area of his work should not remain the same subject for a number of years (this would just make him a bifurcated specialist).[39]

In the same context, «as before the war, the laboratory program in nuclear physics should again be concerned with "philosophical" problems relating to the primary particles of matter and the laws governing their interactions with each other and with radiation [...] (The Manhattan Project work was not directed toward these problems of nuclear physics; they were really concerned with nuclear "chemistry")».[40]

In the following years Tuve's positions explicitly clashed with many choices of scientific community. Allan Needell of the Smithsonian Institution has thoroughly reconstructed Tuve's struggle against Lloyd Berkner concerning the establishment and operation of a national radio astronomy facility in Green Bank (Needel 1987). Tube in fact had left nuclear physics since «it changed from a sport into a business». In the struggle with Berkner he expressed the conviction that the new, expensive tools of research were «subsidiary and peripheral» when compared with the support of individual researchers. He insisted that those tools, in his words «did not serve appreciably to produce or develop creative thinkers and productive investigators. [...] At best they serve them, often in a brief and incidental way, and at worse they devour them».

He repeatedly expressed himself against Big Science. In 1959 he published on the *Saturday Review* a long paper with the title: «Is Science too Big for the Scientist?» (Tuve 1959). He repeated this concept in a meeting in which President Eisenhower announced the appropriation of $ 100 million for the future Stanford linear accelerator: Tuve made such a bald statement that his colleagues publicly reprimanded him that «this was neither the time nor the place» for it (Lear 1959).

Since I started my analysis with a comparison between Tuve and Lawrence in their early research activities, I may just recall here the very different road followed by the latter, which remained most representative of the choices made by the scientific community and of the radication of Big Science. Lawrence collaborated with the National Defense Research Committee on microwave research and submarine detection, took part in the Manhattan Project, actively gave advice on the construction and the use of the bomb. After the war the Radiation Laboratory was financed with funds from the Manhattan District. In

[39] Ibidem.
[40] Ibidem.

1952, on request of the AEC, Lawrence founded a new laboratory at Livermore for military research, a prototype of large-scale specialized structure.

4

I have dwelt on Tuve's personality in order to single out, in contrast, the changes in American nuclear physics in the thirties that anticipated and led to Big Science.

But, instead of looking at specific personalities, one may study and compare the developments of nuclear physics in the same period in different national contexts. Such comparison shows the peculiarity of the conditions that led the U.S. to play an original role of absolute leadership in introducing and guiding the transformation of science and research.

It is not the task of this paper to perform a thorough analysis, but I would like to try to give some ideas.

The French, British and Italian physicists brought major contributions to nuclear physics in the thirties. Trends towards large scale research may undoubtedly be individuated also in these countries, but a careful analysis, which does not stop at superficial events, shows that these remained isolated examples and did not turn into a general and deep transformation of science involving its methods, structure, role and connection with technological change and with society in general.

In 1937 Hafstad, Tuve's most strict collaborator, visited Joliot's laboratory in Paris, where work was being done on a program of cyclotrons, high voltage and electrostatic accelerators. Hafstad noted that «no apparatus was in condition for the making of observations [...], in the U.S. this state of development was passed about three years ago [and] it was evident that Paris was far behind the United States».[41] A final judgement included also the Italian group in Rome:

> Nearly all European laboratories are at present engaged in a building program. This perhaps accounts for a rather surprising exchange of positions between American and European laboratories. A few years ago it was being said that, whereas much work on apparatus was being done in the U.S., practically all scientific results had been obtained in Europe using radium technique. The

[41] L. R. Hafstad, "Report on Laboratory Visits in Europe during the Summer of 1937", December 10, 1937, CIW Archives, Folder DTM-Miscellaneous 1930-37.

> situation is reversed as scientific results are being obtained from the perfected apparatus in the U.S., whereas the possibilities of the old radium technique in Europe are now practically exhausted. It is of the utmost significance that, for perhaps the first time, Europe is definitely behind the U.S. in experimental physics and that they now find it necessary to send men to this country to acquire techniques which can be carried back to Europe.[42]

It seems evident that large-scale apparatuses and new techniques in American nuclear physics were not in themselves a step towards Big Science; they were only the exterior events, induced by much deeper processes. The better confirmation is perhaps given by a comparison with the British situation, where accelerating machines had been built and used for the first time.

In 1930 British nuclear science had already a sound tradition. It however identified itself with Rutherford's personality, which had a very strong ascendancy on his pupils. The prevailing spirit was extremely different from that of the Americans. It was marked by the ethics of pure science as a disinterested academic activity. There was no interest in the possible technological value of the investigations (Cockcroft was in some sense an exception and a special figure: he was an electrical engineer; in 1935 he abandoned active research for some years and, after Rutherford's retirement, started the building of new machines). The figure of the British scientist seemed more eighteenth century-fashioned than similar to the American one. He had faith in the cognitive value of the experimental result in itself. The experimental groups hardly ever exceeded the number of a couple of scientists and had substantially distinct fields of interest, avoiding consequently competition. Direct interaction between experimenters and theoreticians was rare.

After 1935 there was a sensible decline in British nuclear physics, deeply contrasting with the growth of Americans physics. Chadwick had found in Rutherford opposition in following an advanced research program. It was not chance that, after Rutherford's retirement and death in 1937, only Chadwick and Cockcroft undertook a program of building new machines and they were among the British scientists most directly involved in war-time collaboration on the main projects with the Americans (Cockcroft on radar and Chadwick as the leader of the British team in the Manhattan Project).

[42] Ibidem.

5

There is however another national situation whose careful analysis would be extremely interesting and meaningful. I refer to nuclear physics in Japan. There is, in fact, a very interesting, peculiar feature of this situation: the Japanese nuclear physicists did build up ad use with a very short delay the new machines and instruments introduced by the Americans and the other western scientists but followed an original line of thought, linked to the Japanese philosophical tradition, that led to physical ideas different from and incompatible with the framework emerging from nuclear investigations of the western physicists.

In spite of the choice of machines and instruments and of their use in the laboratories, no large-scale style of research at all was induced in pre-war Japan, and the previous philosophical tradition had a much stronger influence on the programs and the results than the above mentioned material choices and the experimental results and programs.

A thorough analysis of this case-study would then throw light on the complex of factors that created the conditions for the birth of large-scale research and the premises of Big Science.

I will not actually develop in detail this suggestion and I refer to important contributions by Takabayashi (1983), Takeda & Yamagouchi (1982), Brown, Konuma & Maki (1980), Hayakawa (1981). I will limit myself to adding some brief comments.

Japanese physicists acquired the new quantum concepts between the end of the twenties and the beginning of the thirties. Some of them came back after stays in Western countries: Nishina in particular visited Bohr and Rutherford and played a very important role in orienting the activities in nuclear physics and cosmic-ray physics. These activities grew rapidly: the first cloud chamber was built in 1933 and coincidence methods and automatic operation were realized soon after Blackett and Occhialini and quite independently from them; in 1934 three Cockcroft-Walton accelerators started working (one of 200 KeV, and successively another of 600 KeV at Riken in Tokio; another of 600 KeV at Osaka); after 1935 Watase and Itoh started building a cyclotron. But the experimental activity, although intense, did not play a leading role, since the Japanese physicists were not so much interested in applied or technical aspects, as rather in elaborating a unifying scientific conception, having its roots in the Japanese philosophical tradition. Thus it was that they strictly linked together the problems of the nucleus and of cosmic rays and fundamental particles, which on the contrary were kept separated for a long period in Western physics.

They managed to build for this whole field a comprehensive, unifying conception very different from the set of theories and models that were elaborated by Western scientists.

In short, let's refer to Yukawa's meson theory. The meson was not only the agent of nuclear forces – as it was accepted in Western physics – but was a central element of a much more general and complex conception, that never was fully perceived in Western countries.

Apart from the easiness with which Japanese physicists introduced new particles (as contrasted to the early hesitations of Western physicists, for instance of Pauli for the neutrino hypothesis), Yukawa's meson was supposed to decay into an electron and to be consequently responsible for β-decay as for nuclear forces: contrary to Fermi's theory of β-decay, deriving from an interaction different from the nuclear interaction – the weak interaction – the conception proposed by the Japanese scientists had a unifying character. (We may recall that previously, in 1933, under the influence of Heisenberg's model of nuclear structure, and before Fermi's paper, Yukawa had proposed to attribute β-decay to a transmutation of the proton: at that time he considered the electron as a field mediating the nuclear force. In that occasion Nishina had suggested that the exchange of a boson between two nucleons would have preserved spin and statistic).

Starting from the previous comments, it could be very interesting to follow the further developments of the views of the Japanese scientists in the following years, in a condition of substantial isolation and independence from the evolution of the lines of thought of Western particle physics. It will suffice here to mention, apart from important contributions by Tomonaga and Yukawa himself, the evolution of meson theory with contributions of Taketani and Sakata. Their motivations were again not primarily experimental, but mainly ideological. The two scientists were working in the framework of Marxist philosophy.

A further development, stemming from the problems posed by the mean life of the meson, was the "two-meson theory". Only later this theory proved to be wrong when compared with the experimental data that were accumulating.

In 1952, finally, Sakata proposed a theory with tree fermions as the fundamental constituents of matter (the "sakatons") linked together by an unknown "B-matter". Sakata's theory anticipated in some sense the unitary approach, but was in fact quite independent from it and had moreover completely different origin and motivations. One may also perceive an analogy with actual gauge theories in terms of quarks and gluons, and probably such an analysis has become sounder, having a unifying proposal at its basis.

I hope to have given, from the perspective I have chosen, a modest contribution toward the individuation of the specific factors that created the conditions for the birth of large-scale research and of the features that really characterize a turning point in the development of science and research activity.

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Maintaining America's Competitive Technological Advantage: Cold War Leadership and the Transnational Co-production of Knowledge

*John Krige**
john.krige@hts.gatech.edu

ABSTRACT

This paper describes the mechanisms that shaped the transnational flow of knowledge about the gas centrifuge for uranium enrichment between British and American nuclear scientists and engineers in the 1960s. Through studying face-to-face encounters between researchers in laboratories on both sides of the Atlantic it places "coproduction" rather than "transfer" or "diffusion" at the epistemological core of the analysis of the circulation of knowledge across national borders. Coproduction, it is argued, takes place in an asymmetric field of force that was dominated by one of the poles, the United States. Washington could exploit London's historical dependence on it for nuclear materials and technology to gain access to advanced British research and development. American scientific and technological pre-eminence was not built upon an autarkic, self-contained research system. American global leadership was achieved by levering transnational collaboration with capable partners to enhance massive national investments in the production of knowledge, so pulling even further ahead of friend and foe alike.

It is widely accepted that the first two or three decades after World War II were marked by an asymmetry in economic, political and military power between the United States and the rest of the world. We should not forget, though, that there was also an asymmetry in scientific and technological knowledge between America and its allies and enemies. Maintaining that preeminence was already on the agenda before the war ended; it became a priority as the Cold war

* Georgia Tech Institute, GA, USA.

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gained momentum. The quest for competitive technological advantage became embedded in domestic policies, and their supporting ideologies, that coupled scientific and technological leadership with the construction and consolidation of the national security state. Qualitative technological superiority was initially justified as the only way to hold back Soviet "hordes" in Europe without the mass mobilization and militarization of the home front. The threat posed in the 1950s by successive Soviet scientific and technological achievements called forth a broader and deeper American response. The security of the West, a budget that balanced military and civilian needs, and the protection of domestic liberties and pluralistic institutions, demanded a program of permanent preparedness. This was underpinned by ceaseless scientific and technological innovation. Federal sponsorship became an essential complement to the industrial research laboratory. The Federal government's R and D budget increased dramatically after the Korean War broke out, more than doubling to $1.3billion in fiscal year (FY) 1951 and more than doubling again to $3.1 billion in FY1953. It was given another enormous boost by the launch of Sputnik in 1957: a decade later it had almost quadrupled to $15 billion (Kevles 1990 a, 1990b). As Friedberg puts it,

> From the onset of the Cold War, top American decision makers tended to believe both that it was necessary for their country to seek a technological edge over the Soviet Union and its allies, and that such an edge could be achieved and maintained. These beliefs helped to keep technology at the forefront of American strategy and to sustain a massive four-decade flow of resources into research and development. (Friedberg 2000, p. 297)

The pursuit of scientific and technological pre-eminence was driven, in the first instance, by the conviction that nothing less could protect America from an existential threat. But there was more to it than Friedberg says. The cold war was not simply a binary struggle for military superiority between superpowers. Scientific and technological leadership was not only sought after to defend the homeland from a Soviet attack. It was also needed to enhance America's global reach, to fulfill «a sacred mission thrust upon the American people by divine Providence and the laws of both history and nature» (Hogan 1998, p. 15). The U.S did not merely seek a competitive edge over its archrival: it also sought scientific and technological superiority over its allies. As Cristina Klein has noted, from an American perspective, the Cold War was «as much about creating an economically, militarily and politically integrated "free world", as it was about waging a war of attrition against the Soviet Union» (Klein 2003, p.

16). Washington sought to integrate Western Europe into its global agenda by encouraging it to play its part in the anti-communist struggle, while also striving to contain its ambitions within an American-led world system. The challenge faced by U.S. policy makers in the 1950s and 1960s was not simply to combat Soviet communism; it was also to help rebuild Europe's scientific and technological strength, without unleashing demands for independence that would undermine their hegemony.

The knowledge/power nexus that was crucial to the American global project after World War II helped put in place what Bright and Geyer call a "regime of world order". What made this regime so different from its predecessors, and above all from the imperial project of the European colonial powers, was that a transnational flow of knowledge enabled the United States to move «beyond the extension of power *over* others toward a direct and sustained organization *of* others, simultaneously, and in many parts of the world» (Bright & Geyer 2005, p. 205). American scientific, technical and intellectual leadership, and the massive investment in education after the war that made that possible, were «as important as its economic and military power in making world order cohere and, more important, in developing and organizing the consent of subordinate participants» (Bright & Geyer 2005, p. 228). The postwar pursuit of an American-led regime of order was not a top-down project of command and obedience. It was an ongoing negotiated process in which science and technology were shared or denied in an asymmetric field of force defined by a knowledge-deficit between its partners and the United States. If this was hegemony, it was consensual not coercive (Krige 2006; Lundestad 1999).

The construction of a national security ideology in the first decade of the cold war pitted the conservative defenders of an older, anti-statist political culture against the managers of an emerging, technocratic, proministrative state (Balogh 1991). Both tried to «frame a public policy that would protect the American way against the dangers of regimentation». Within that shared frame of reference, «both associated their critics with the un-American other, both spoke in a language of ideological opposites, such as democracy or totalitarianism, [...] loyalty or disloyalty, isolationism or internationalism» (Hogan 1998, p. 18). Many leaders of the American scientific community were engaged in this struggle (Wang 1999). Adopting a pragmatic approach to international collaboration, they insisted that tight restrictions on scientific and technological exchange would undermine, not secure, the nation's competitive edge. They were emphatic that to retain American leadership they had to collaborate, not retreat behind high walls, both to raise the level of scientific and technological capability abroad (so as to share the burden of defense of the West) and to be in a position to draw on the best that others had to offer. Admittedly, by sharing what they knew they could strengthen their competitors; what they learnt abroad, however, also stimulated innovation at home. As early as 1949, in a famous standoff between Senator Hickenlooper and David Lilienthal, J. Robert Oppenheimer defended an embattled "socialist" AEC from charges of mismanagement and lax security.[1] Vigorously encouraging closer collaboration with Europe against those who sought to stop the export of radio-isotopes for research, Oppenhemier pointed out to the Congressional enquiry how much the continent had to contribute to the American research effort.

> If discoveries are made in Europe, we are in a better position to profit by them than the Europeans, because of our advanced technology, our good organization. [...] History again and again shows that we have no monopoly on ideas, but we do better with them than most other countries. (Oppenheimer, quoted in The Great Enquiry, 1949, pp. 227–228)

Fifty years later, the leaders of America's four main weapons laboratories, laboring under the accusation that they were lax on security, protested violently that

> The world is awash in scientific discoveries and technological innovation. If the United States is to remain the world's technological leader, it must remain deeply engaged in international dialogue, despite the possibility of the illicit loss of information. (Committee on Balancing Scientific Openness and National Security1999, p. 11)

Transnational collaboration in science and technology was not a threat to the American hegemonic project. On the contrary, it was essential to it – or so it was argued time and again by the American scientific community, including its weapons researchers (Krige 2010).

Diplomatic historians are increasingly calling for a better integration of science and technology into studies of international affairs (LaFeber 2000; Westad 2000). Indeed we are rapidly gaining a better understanding of the

[1] Lilienthal was the Chairman of the Tennessee Valley Authority from 1941–1946, when he was appointed chairman of the AEC. In 1940 he was ardent advocate of the state-driven planning of large technological/social projects. See Hughes (1989, p. 378).

many modes of articulation between American knowledge and global power, to cite the title of a comprehensive review by Engerman (2007). Modernization theory, too, is providing invaluable insights into how Western expertise was "transferred" to other countries, supplanting local knowledge and practices (Cullather 2004; Engerman, Gilman, Haefele, & Latham 2003; Latham, 2000). Though immensely valuable, these studies remain mostly Americo-centric: knowledge is produced in the United States, and is "diffused" by American or American-trained intellectuals and experts who deploy it to advance transformative agendas that cohere with Washington's goals abroad. The vector of knowledge is unidirectional: there is transfer and diffusion but there is no *circulation,* no recognition that knowledge production is an ongoing process that is sustained through transnational contact and exchange. Correlatively, the notion of "American" knowledge itself is not problematized. If "American" knowledge is *co-produced through transnational circulation* does it make any sense to speak of American knowledge at all – at least as regards its content? If knowledge flows across national borders, and is transformed in the process, does it not lose its national identity, becoming a complex hybrid whose various "national" components become woven so tightly together as to be almost indistinguishable from one another?

My aim in this paper is to use a brief case study of the coproduction of knowledge between British and American nuclear scientists in the 1960s as a platform for further reflection on the knowledge/power nexus in the Cold War. In particular I want to show how American leadership was not simply built on the production of knowledge at home and its diffusion, transfer or imposition abroad. Instead I shall argue that U.S. leadership was sustained by its capacity to collaborate productively, and on its own terms, with others, exploiting the threat to withdraw support in some areas as a political weapon to gain access to sensitive information in others. I shall also suggest that our failure to "see" these processes of coproduction is due to the "blinkers" imposed by restricting studies of knowledge production to a national framework. The dominance of that framework, a framework that eclipses the kinds of transnational transactions that matter so much to U.S. scientists, reflects the inflated importance attributed to the bounded nation state as the only significant unit of analysis in the Cold War.

The Co-production of Knowledge: U.S.–U.K. Collaboration in Developing Gas Centrifuges for Uranium Enrichment in the 1960s[2]

Background. The idea of using gas centrifuges spinning at very high velocities to separate the fissile U^{235} (an isotope of uranium) from the far more abundant U^{238} had already been looked into during the war. The principle was simple: the concentration of the heavier isotope would increase from the center of the cylindrical vessel to the wall, and by extracting the slightly enriched mixture at an appropriate point on the radius and recycling it many times through centrifuges connected in series (a cascade) one could significantly improve the concentration of fissile material in the mix. (This is the technology currently used by Iran, of course). To implement this scheme in practice proved extremely difficult, however. In 1960 a report by Gerard Zippe, an Austrian who was released by the Soviet Union in 1956, and spent three years on centrifuge development at the University of Virginia, described a design of stunning simplicity (Scott Kemp 2010). This led to a brief burst of international publication in the open literature before all work on centrifuges was once again classified, reflecting its great potential.

Zippe's design promised to democratize the technology of uranium enrichment (and to facilitate nuclear proliferation). From an intelligent interpretation of his work it emerged that one could produce about 50kg/year of uranium enriched to a few percent with 10,000 centrifuges that occupied some 40,000 square feet.[3] These were relatively modest demands compared to the needs of the gas diffusion process that was developed in the Manhattan project and that was the dominant technique for uranium enrichment for the first two decades after the war. Gas diffusion was technologically complex and extremely costly, requiring a large scale effort beyond what most countries could afford so as to benefit from economies of scale, as well as access to cheap electricity. It was only implemented at enormous expense in countries with military programs like the U.S., Britain and France. For civil purposes most countries had to buy enriched uranium or use natural (unenriched) uranium for their reactors, this being less efficient and involving larger capital start up

[2] My interest in this was sparked by Twigge (2000) and Schrafstetter & Twigge (2002).

[3] Letter, Franklin, 11 March, 1968, FCO10/207, The National Archives, Kew, London (hereafter TNA).

costs.[4] Centrifuge technology promised to change all that. Its commercial exploitation did not depend as much on economies of scale, nor did it require huge amounts of cheap electricity.

In spring 1968 the Dutch publicly announced that they were moving ahead with a small prototype centrifuge plant. Officials from the Federal Republic of Germany reported that they too were actively looking into the technique, but that their efforts were still at the experimental stage. The British Atomic Energy Authority (UKAEA), which had invested about £2 million in research and development at the time, was so impressed with the prospects that it decided to abandon plans to extend its gas diffusion plant at Capenhurst in England, and to add a centrifuge separation facility instead.[5]

Sir Solly Zuckerman, the Chief Scientific Adviser to the British government was particularly taken with the prospects of centrifuge technology. It would not only provide enriched uranium for Britain's military and civilian needs considerably more cheaply than gas diffusion – as much as 15-20% on one estimate.[6] This revolutionary new development would also «help end our dependence on the United States in the key field of the supply of enriched uranium», all the more important, he felt, because «there is nothing they may not do to maintain their present monopoly» in enriched fuels for civil purposes.[7]

Zuckerman also emphasized that this was an ideal candidate for a tripartite venture in scientific and technical collaboration with the Dutch and the Germans. «We are all beginners» he wrote «and in effect we are all starting in on the basement». Sharing knowledge could only to be mutually advantageous, since «for all we know their design of the centrifuge is better than ours [...]».[8]

A joint venture had important foreign policy ramifications too. In a much publicized speech British Prime Minister Harold Wilson had suggested that

[4] "Aide Memoire on Centrifuge Classification", 18 October, 1968, PREM13/2555, TNA. See also Memo "Uranium Enrichment by the Gas Centrifuge Process," Foreign and Commonwealth Office to Sir Evelyn Stuckburgh, Rome, 12 June, 1968, FCO16/252, TNA.

[5] Memo Solly Zuckerman to the Prime Minister, "Centrifuge Collaboration," 27 November, 1968. The Germans had also developed a 'nozzle process' for enrichment but their research was in an early stage, Memo Anthony Wedgwood Benn to the Prime Minister, 9 April, 1068, both in PREM13/2555, TNA. Active efforts were being made in Europe, then, to develop alternative techniques to gas diffusion.

[6] This was the view of Sir John Hill, Chairman of the UKAEA since 1967, Memo I.T. Manley, "Centrifuge Collaboration," 3 July, 1969, HF19/25, TNA.

[7] Memo Solly Zuckerman to Prime Minister, 15 October, 1968, PREM13/2006, TNA.

[8] Memo Solly Zuckerman to Sir Burke Trend, 6 December, 1968, PREM13/2555, TNA.

Britain wanted to take the lead in «European-wide co-operation in producing advanced technological products for an international industrial market, on a commercial basis».[9] The European centrifuge project fit the bill perfectly. It could also count on the support of the State Department. The Johnson administration was deeply concerned about the technological gap that had (putatively) opened up between the two sides of the Atlantic in the 1960s. Reporting to the President in December 1967 an interdepartmental committee concluded that the gap was «a current manifestation of the historical differences between Europe and the U.S. in aggressiveness and dynamism, reflecting the American frontier past and its restless quest for progress and change». While it was essentially up to the Europeans to resolve this problem themselves, the U.S. could take specific measures to help, notably, «assist European initiatives toward intra-European technological cooperation in space science and technology, *in atomic energy*, and in the application of computers in research, industry, and government» (my emphasis).[10] The Anglo-Dutch-German centrifuge enrichment project seemingly fused British and American foreign policy considerations in Europe in a most attractive way.

The Co-Production of Knowledge: Who Contributed What? The success of the tripartite European venture depended crucially on American support. This was because, between late in 1960 and early in 1965 scientists and engineers in the two laboratories of the atomic energy agencies had worked together on developing the gas centrifuge, whereupon the U.S. partner unilaterally withdrew from the effort. The work done together was classified, and any decision to divulge it required U.S. approval according to Article IX(c) of the Anglo-American bilateral Agreement for Cooperation in the Civil Uses of Atomic Energy (1955). In terms of this Article «No material, equipment, device or restricted data», and «no equipment or device which would disclose any restricted data» could be passed by the U.K. to a third party without the permission of the party from which it was received.[11] The British now intended

[9] This is the way Wilson presented it to German Minister Stoltenberg, Extract from a meeting in Bonn, 12 February, 1969, PREM13/2555, TNA.

[10] "Memorandum From the Interdepartmental Committee on the Technological Gap to President Johnson", Washington, December 22, 1967, *FRUS, 1964-1968, Volume XXXIV, Energy, Diplomacy and Global Issues.*

[11] Summarized e.g. in "Anglo/U.S. Relations in the Nuclear Field", Paper prepared for Cabinet Ministerial Committee on Nuclear Policy, Centrifuge Collaboration, 19 May, 1969, CAB134/314, TNA.

sharing what they knew with their continental partners. Would they be passing on anything that they had learnt from the U.S. which was restricted under Article IX(c), and for which they therefore needed special permission?

It took nine months of sometimes acrimonious exchanges between Washington and London to resolve this issue. The immensely rich documentation that addresses itself to the core concern of the authorities (whether the British had in fact learnt significant new knowledge from the U.S., and what to do about it if they had) also reveals the multiple modes whereby information was co-produced between the partners. In other words, in what follows we will not only get a glimpse of high level policy making between government officials. We will also hear (indirectly) the voices of scientists and engineers who actually worked with each other on centrifuge science and technology. We will thus gain considerable insight into the messy process of knowledge-in-the making, we will see how knowledge is co-produced in an encounter between two partners who bring different experiences and skills to the table (Raj 2008).

Before the British made any move towards The Hague or Bonn, they tried to establish just what it was they had learnt from the Americans.[12] John Hill, the then-chairman of the Atomic Energy Authority (UKAEA), pointed out that both the U.K. and the U.S. had started from Zippe's published, unclassified design, but that it took a lot of additional work to turn that into a device that could be used for mass production on an economic scale. Many solutions to this problem had been explored together. One in particular was of interest: an American suggestion that a so-called "dished end cap" be used to compensate for the contraction in the length of the centrifuge's body when it spun at extremely high speeds. However – and the British were emphatic about this in December 1968 – this concept had been conveyed to them informally by their U.S. partners during a fifteen minute conversation. They were not granted access to American secret reports, they said, and they had to devise the theory for themselves. Thus, the design of the end cap in the prototype British machine (Mark I), in Hill's view, was entirely indigenous.[13]

On 5 and 6 December, 1968, senior delegations met in Washington DC.

[12] Draft memo by UKAEA, attached to PNO(C)(68)12, 19 December,1968, Cabinet Official Committee on Nuclear Policy. Sub-Committee on Gas Centrifuge, "Interpretation of Article IX(c) of the 1955 Civil Bilateral Agreement", CAB 134/3125, TNA.

[13] "Anglo/U.S. Relations in the Nuclear Field," Paper prepared for Cabinet Ministerial Committee on Nuclear Policy, Centrifuge Collaboration, 19 May, 1969, CAB134/314, TNA.

The British took a broad-brush approach to the question of sharing centrifuge technology, defining the problem as one of national security.[14] The Americans were not unsympathetic but were more concerned to know precisely what information Britain wanted to share with the Dutch and the Germans in their joint venture. In response, it was noted that while the exchange of information between the partners was bound to have affected the general thinking of both, this surely did not mean that the whole of the U.K.'s centrifuge technology was subject to restrictions in terms of Article IX(c). Instead, in the U.K.'s eyes, a reasonable interpretation of Article IX(c) was that «no *specific* information or reports which had been conveyed during the Anglo/American exchanges and no *specific* design features *directly* developed from these exchanges, should be transferred to the Dutch or the Germans without American consent» (my emphasis). Since they were emphatic that the exchange of information on the end cap had been no more than a general conversation that had lasted for 15 minutes, the British concluded that none of the data that they proposed to share with the Europeans was U.S. restricted data.[15]

The AEC officials who met with the British in December were pleased at U.K. efforts to proceed collectively with their continental partners. They did not have the authority, however, to decide whether or not the information that had been shared between scientists and engineers in the two countries in the early sixties was restricted or not. This was up to the (Congressional) Joint Committee on Atomic Energy, and there was no guarantee that they would interpret the British request in a favorable light. As a matter of fact the Committee was in an irritable and suspicious mood as regards U.S.–U.K. exchanges in the nuclear field.[16]

The British Ministry of Defense and the Embassy were extremely worried by the attitude of the Joint Committee. Above all they did not want to go ahead with the centrifuge programme in Europe against U.S. wishes for fear of

[14] The debate briefly described here took place under the shadow of the signature (1 July, 1968) and coming into force (5 March, 1970) of the Nuclear Non-Proliferation Treaty (NPT).

[15] PNO(C)68 2nd Meeting, Cabinet Official Committee on Nuclear Policy. Sub-Committee on Gas Centrifuge, 11 December, 1968. "Report of Washington Discussion". CAB 134/3125, TNA; PNO(C)(68)12, 19 December, 1968, Cabinet Official Committee on Nuclear Policy. Sub-Committee on Gas Centrifuge, "Interpretation of Article IX(c) of the 1955 Civil Bilateral Agreement", CAB 134/3125, TNA; Telegram Sir P. Dean, Washington D.C. to London, 6 December, 1968, "Gas Centrifuge: Anglo-U.S. Talks", PREM13/2555, TNA.

[16] Telegram British Embassy, Washington D.C. to Foreign and Commonwealth Office, 12 May, 1969, PREM13/2556, TNA.

retaliation in a range of more or less related issues. These included the provision of low-cost enriched fuel for nuclear submarine propulsion, as well as amendments to existing civil agreements for reprocessing irradiated fuel then being discussed.[17] America's "leadership" in all matters nuclear, and Britain's dependence on it, provided Congress with a political weapon that it could use to thwart independent initiatives by its closest ally if deemed to be contrary to U.S. interests.

March–June, 1968. The British Case Collapses. The next meeting of the British and American officials of the two atomic energy organizations occurred in March, 1969. This time the U.S. team was fully prepared to challenge the British position.[18] They were emphatic that the British had underestimated the extent of the help they had been given. The American contingent insisted that there had been a great deal of discussion on end cap design in the U.S.–U.K. exchanges, and that it was difficult to imagine that some of this discussion was not embodied in the ultimate UKAEA device. The British were not persuaded: they insisted that the design that they discussed together with their American colleagues was not unique. The British were also reminded that they had been given information by the U.S. on how to improve the design of the bottom bearing of the rotor supporting the centrifuge's cylinder. At the end of meeting the head of the U.S. delegation said that to better assess the American contribution to the British device it might be useful if they could see the United Kingdom centrifuge project at first hand.

The British were deeply distressed by the demand for visual access. For one thing, the design of their centrifuge was the centerpiece of their contribution to the proposed tripartite collaboration. Dutch and German partners would assume that it was of solely British provenance. They would feel betrayed, and would certainly not share their most important information, if this core data had already been given to the Americans. «A "dished end cap"», Zuckerman wrote, «should not be allowed to become a barrier to a major European political policy which the American government has not only endorsed but also

[17] "United States/United Kingdom Relations in the Nuclear Field", attached to memo Sykes to Killick, 1 April, 1969, FCO55/269, TNA.

[18] "Centrifuge Technology", Record of the United States/United Kingdom Talks Held in the Cabinet Office, London, on the 4 and 5 March, 1969, FCO55/265, TNA.

encouraged».[19]

Many in the Cabinet were also convinced that the main reason for the American demand for visual access was not the protection of national security – the terrain on which London had hoped to situate the debate – but commercial interest. As Zuckerman put it to Prime Minister Wilson, «the Americans are out to dominate the world market for nuclear fuel. Were we to allow them access [to our Mk I centrifuge] they might well pick up ideas from our production model which could make a real difference to their commercial exploitation of the centrifuge in third countries, if not in the USA».[20] For these reasons alone Sir Solly was determined not to yield to American pressure, even if that meant antagonizing the Joint Committee and perhaps jeopardizing the civil and military U.S.–U.K. agreements then under review.

The British case was dealt a lethal blow by their own, more systematic enquiries into just what information had passed between their scientists and engineers when they were collaborating under the restricted regime in the early sixties. A three-man panel reporting late in May concluded unequivocally that the British design of the end cap incorporated U.S. restricted data that could not be transmitted to the Dutch or the Germans.[21] After a further round of discussions, in July 1969 the British authorities, their case seriously weakened by the new revelations of their own internal investigation, and under assault from the AEC, finally agreed that an American team could have visual access to the Mark I production prototype of their gas centrifuge.

But the British conceded more. During a visit to the laboratories at Capenhurst, USAEC officials were also given a full and frank presentation not only of the centrifuge itself, but «also of the U.K.A.E.A.'s production plans, their machine trials and testing programs, their experimental workshops, and other associated facilities».[22] The British authorities also accepted to keep the USAEC informed about their programme for advanced research and

[19] Memo Zuckerman to Prime Minister, "Centrifuge Collaboration. Enquiry by Lord Penney, Sir Alfred Pugsley and Mr. T.C. Hetherington", 2 June, 1969, PREM13/2556, TNA.

[20] Memo Zuckerman to Prime Minister, "Centrifuge Collaboration. Anglo-United States Relations in the Nuclear Field", 21 May, 1969, PREM13/2556, TNA.

[21] "Report of Enquiry Relating to Restricted data on Centrifuge Design and Construction [...]", 30 May, 1969, FCO55/268, TNA; Letter Zuckerman to Prime Minister, 2 June, 1969, PREM13/2556, TNA.

[22] PN(69)14, Cabinet Ministerial Committee on Nuclear Policy, "Gas Centrifuge Collaboration. Extent of Agreement Reached. Note by the Minister of Technology", 17 November, 1969, CAB134/3121, TNA.

technology, and they invited a small U.S. team to come over and see which, if any of its aspects might be restricted under Article IX(c) of the joint U.S.–U.K. agreement. With these conditions met, in October the Joint Committee agreed that, exceptionally, the UK could share centrifuge end cap technology with its Dutch and German partners in a collaborative European programme.[23] Summarizing the dispute over centrifuge technology in his diaries twenty years later, Tony Benn, the Minister of Technology, wrote that what he had suspected «but had never been properly told, was that we have an arrangement with the Americans under which we are absolutely tied hand and foot to them, and we can't pass any of our nuclear technology over to anybody else without their permission.The harsh reality is that de Gaulle is right» (Benn 1998, p. 127).

Reflections on a Transnational History of Science and the Cold War

This case study has explored the processes whereby *knowledge was made* at the interface between qualified nuclear scientists and engineers in the United States and in Great Britain. The analysis provided an insight into the material practice of co-production, the dynamics of the process whereby two partners learnt from each other between 1960 and 1965. In also showed the strategies used by the USAEC to regain some control over the independent British effort pursued in the following three years. By combining legal arguments with veiled threats to withdraw support from important sectors of the British civil and military programmes, the U.S. used its vast lead in nuclear knowledge to extract major concessions from its ally. In fact American nuclear scientists re-inserted themselves into the European enrichment project as it began to take shape in 1969, if not directly, then by demanding access to both current and future developments. The ensuing transparency of the European project was intended to ensure that the U.S. maintained short-term control over its trajectory on the grounds that it *might* use classified American material. It was also intended to ensure that U.S. laboratories retained a broad understanding of subsequent European advances as centrifuge technology was improved. Thus even as British scientists, engineers and policymakers tried to break loose of their historical entanglement with the U.S., so they were obliged to yield critical knowledge to their rival, knowledge that could inject new ideas into the AEC's centrifuge program, perhaps accelerating its development and

[23] "American Aide Memoire", 1 October, 1969, CAB134/3121, TNA.

even its commercialization. American overall scientific technological leadership was deployed as a political weapon to browbeat the British into submission. Washington's capacity to influence the nuclear programmes of other states, and to enter new markets open to buying relatively simple and cheap centrifuge enrichment, was enhanced. Britain's hope of taking the lead in the development of a major new technology and of breaking the U.S. monopoly on the provision of enriched uranium was badly dented.

This story can be read exclusively from the perspective of U.S.–U.K. diplomatic relations. For Britain, it describes a re-equilibration in the balance of its relationships between America and Europe. This involved distancing itself from Washington in the interests of drawing closer to the continent, of undermining «the European contention that we are shackled to the American chariot» as Zuckerman put it.[24] It was also symptomatic of a dilution of the "special nuclear relationship" between Washington and London, that Zuckerman (though not Secretary of State for Defense, Denis Healey) regarded as illusory by the late 1960s.[25] For the U.S., the story highlights the maturing contradiction between its strong support for an integrated Europe and its urge to establish an American-led regime of world order. The Johnson administration actively encouraged the development of collaborative technological projects in strategic domains like the nuclear and space to close the "technological gap" between the two sides of the Atlantic. At the same time the very success of its policies threatened the dilution of the global influence that it was intent on preserving. As Ninkovich has put it, one of the abiding themes of American foreign policy in the 20thC has been the recognition that

> The very forces that made progress possible – technology, trade, a global division of labor, and interdependence, – also made possible the system's destruction if pushed in the wrong direction and not checked. The greater the degree of integration, the more explosive would be the disintegration produced by a runaway modernity. (Ninkovich 1999, p. 66)

The deep animosity between British Premier Wilson and U.S. President Johnson, along with the loss of legitimacy engendered by the debacle in Vietnam, only hastened that "disintegration". The maneuvers described in this paper to rein in the European centrifuge project in the late 1960s can be read as an attempt to "check" the pull of a world system moving "in the wrong

[24] Memo Zuckerman to the Home Secretary, 10 March, 1969, PREM13/2555, TNA.
[25] Memo Zuckerman to Prime Minister, 15 October, 1968, PREM13/2006, TNA

direction" as seen from Washington, as a struggle to reconcile a time-hallowed policy for postwar Europe with the need to curb "runaway modernity".

By focusing in detail on the procedures whereby scientific and technical knowledge of centrifuges for enriching uranium was co-produced, this paper seeks to move beyond a more traditional analysis of the exercise of American power in the Cold War by bringing non-state actors into the heart of the analysis. Scientists and engineers at laboratories in Capenhurst in the U.K. and at Oak Ridge in the U.S. are mostly faceless in my archival sources. But their expert opinions are constantly appealed to by high-level state officials, or the members of the USAEC who have to make national policy. It is only through the prism of their diverse forms of face-to-face interaction (discussing ideas, sharing blueprints and technical reports, visiting each other's laboratories, displaying prototypes) that we can see how together they made knowledge, and how that knowledge and its embodiment in a centrifuge and its end cap could be at the heart of a diplomatic squabble between the two countries.

The various channels – written and visual – through which knowledge flowed between the partners has emphasized the poverty of a "model" of knowledge "diffusion" or "transfer" that sharply distinguishes production from circulation, and that denies or at least restricts the agency of the "recipient". As we have seen, and as in fact the British insisted, the exchange at the scientific level was *mutual.* Knowledge was co-produced in a messy process that defies easy analysis. American hegemony does not spring only from «organizing the consent of subordinate participants». It is enhanced by the U.S.'s capacity to use their scientific and technological pre-eminence as a political weapon to extract the best from what others have to offer, and to make rapid and effective use of it by virtue of the «American frontier past and its restless quest for progress and change» (*sic*).

To recognize co-production is to acknowledge interdependence. The postwar dominance of American science and technology, and the determination to retain a global leadership that was underpinned by a dynamic research system, has led historians to think of American knowledge production as self-sustaining and autarkic. This is not the way scientists see it, at least not when challenged to defend their international linkages. Time and again, and notably when under threat from administration officials and Congressional members deeply concerned to protect national security, leading scientists and science administrators have insisted that, on the contrary, American scientific and technological prowess was enhanced by drawing on a global pool of knowledge.

Scientific and technological interdependence has also been eclipsed because the Cold War elevated scientific and technological achievement to a matter of national pride and international prestige: scientific and technological prowess became key markers of national power (Edgerton 2000; 2007). A French reporter was stunned by what he heard when covering the first sounding rocket campaign in the Sahara desert in 1959 being led by young space scientist Jacques Blamont. The rockets had been built by members of Wernher Von Braun's team who had settled in France immediately after the war and they were doing the countdown in German (Blamont 2001). When the State Department was scrambling to discredit Communist China's prestige after it had successfully tested its first nuclear weapon in 1964, leading Indian physicist and scientific statesman, Homi Bhabha suggested that U.S. press releases emphasize that the "Chinese" bomb could not have been built without Soviet help. In a postwar climate which emphasized inter-state rivalry, and in which scientific and technological achievement became markers of national prestige, nationally-based if not nationalistic narratives "inevitably" held center stage.

History as a professional discipline, Curthoys and Lake remind us «was constituted to serve the business of nation building, and has accordingly very often seen its task as providing an account of national experience, values and traditions, thus helping forge a national community» (Curthoys & Lake 2005, p. 5). Transnational history, by contrast, studies «the ways in which past lives and events have been shaped by processes and relationships that have transcended the borders of nation states». It is interested in understanding «ideas, things, people and practices which have crossed national boundaries» and its language reaches for «metaphors of fluidity, as in talk of circulation and flows (of people, discourses, commodities), alongside metaphors of connection and relationship» (Curthoys & Lake 2005, p. 6). In this paper these metaphors have been fused in the notion of co-production, here not in contradiction with the pursuit of American global leadership, but constitutive of it. To better grasp the place of America in the world, we need to understand the place of the world in America.

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The Carter Administration and its Non-Proliferation Policies: the Road to INFCE

Antonio Tiseo *
antonio.tiseo@gmail.com

ABSTRACT

The scope of this article is tracing back, with a keen chronological reconstruction, the path that President Carter undertook with his Non-Proliferation policy, outlining the difficulties he faced in managing the complex trade-off between curbing nuclear proliferation (trying to refurbish the Non-Proliferation Treaty) without damaging US image as a reliable supplier of nuclear fuel. The reconstruction will be organized around three chronological stages: a) the first phase (from the Presidential Campaign to the indefinite deferral of FBR), the second phase (managing Allies' complaints while trying to support alternative cycles and reactors to the LMFBR) and the third phase (the road to INFCE and its conclusion). It will include a specific part on the efforts that the Carter administration made to prevent, unsuccessfully, the spreading of sensible technologies (like plutonium fueled power plants) in Brazil and in Japan (Tokai-Mura complex). The debate over the safety of Tokai-Mura power plants proves to be extremely actual right after the emergency shutdown of the reactor and the structural damages to the cooling system of the plant caused by the terrible quake/tsunami that interested Northern Japan in March 2011.

Prologue: the role of FBRs before Carter's Non Proliferation Policy

> The Carter Administration began with a natural gas crisis and ended with the Iranian hostage crisis. From start to finish, energy issues crowded its agenda.
> (Walter A. Rosenbaum)

As the initial quote perfectly stresses, Carter's somewhat idealistic pursuit of a

* Università degli Studi Roma Tre, Italy.

national energy policy defined his presidency. As John C. Barrow would later note, on no other issue did Carter risk so much of his political capital, and on no other issue did Carter experience his greatest triumphs and most embarrassing defeats (Barrow, 1998). In Carter energy policy, it is possible to see the strengths of Carter's leadership, his enthusiasm to tackle inherently difficult national problems without regard to the political costs and his conception of the presidency as a leadership for the public good (Hargrove, 1988). Conversely energy policy also revealed the weaknesses of the president's management, his difficulty in building political coalitions, his inability to guide his party and inspiring confidence in his ability to lead the nation.

Hence, the aim of this essay is tracing back, with a keen chronological reconstruction, the path that President Carter undertook with his Non-Proliferation policy, outlining the difficulties he faced in managing the complex trade-off between curbing nuclear proliferation (trying to refurbish the Non-Proliferation Treaty) without damaging US image as a reliable supplier of nuclear fuel. The reconstruction will be organized around three chronological stages: a) the first phase (from the Presidential Campaign to the indefinite deferral of FBR), the second phase (managing Allies' complaints while trying to support alternative cycles and reactors to the LMFBR) and the third phase (the road to INFCE and its conclusion).

A serious enquiry could not start notwithstanding some preliminary remarks on the importance that FBR had on the whole US Energy Policy before Carter took the oath of office on January 20, 1977. From its inception in the fifties, the Fast Breeder Reactor technology has often been described as a means to provide self-fueling energy machines to a world that was quickly running out of uranium. It was seen as a holistic answer to all the energy needs of the forthcoming century: the United States started up the world's first breeder reactor in 1951 and followed with an operational pilot plant in 1963, the 20-megawatt-electric (MWe). The process reached its peak when in 1971 President Nixon established the *Liquid-Metal Fast Breeder Reactor* (LMFBR) as the nation's highest priority research and development effort. Meanwhile, the French, the British, the Germans and the Russians were proceeding with their own original plans of nuclear innovation: the 250-MWe Phenix, the 250-MWe Prototype Fast Reactor (PFR), the 21-MWe KNK II and the 350-MWe Bystrye Neitrony (BN-350) all came critical in the end of the seventies, showing the

US' scientists that their monopoly on nuclear enrichment technology sales was definitely broken.[1]

Because of grave international concern about proliferation (the so-called *Indian Syndrome* fed by the US intelligence reports about Pakistani secret enrichment plans) President Ford started taking preventive measures against the spread of the FBR techs introducing the "Conditional Contracts Formula". Then, during the electoral campaign, he announced that the US government would henceforth not regard reprocessing and plutonium recycle as a necessary part of the fuel cycle, adding that the commercialization of such activities in the US would be deferred until the government was satisfied that the proliferation hazards of the "plutonium economy" could be dealt with.[2] This reconstruction starts here.

1. The First Phase: From the Development of NPP to FBR Indefinite Deferral

The first occasion that candidate Jimmy Carter, a former nuclear engineer in the Navy, had to talk about nuclear issues was the UN sponsored Conference on Nuclear Energy and World Order, held in New York on May 13, 1976.[3]

Starting the conference, the presidential candidate showed soon the milestones of his Non-Proliferation Policy (NPP), portraying himself as no friend of nuke, claiming that there were good renewable alternatives to new reactors and that nuclear energy and weapons proliferation were "inherently twinned".[4] So the NPT was no more conceivable as a one-way street, as Nixon and Ford perceived it. A major undertaking of the nuclear weapon states would have been providing special nuclear power benefits to treaty members, particularly to the developing nations. According to Carter the advanced countries, indeed, had not done enough in this respect to convince Treaty signatories that they were better off inside than outside the Treaty. As a further part of the two ways street there was a clear obligation on weapon states to control and reduce the arms

[1] Report, *Alternative Breeding Cycles for Nuclear Power: an analysis*, prepared for the Subcommittee on Fossil and Nuclear Energy research, development and demonstration of the *Committee on Science and Technology*. Us House of Representatives, 95th Congress, Second Session, Volume VI, October 1978, p. 40.

[2] *Letter to the Honorable Gov. Jimmy Carter from Sen. J.O. Pastore,* June 16, 1976, Jimmy Carter Library Files, Subject Files, Atomic and Nuclear Energy, box 1976, six pages letter.

[3] Nuclear issues in the presidential campaign: three steps toward nuclear responsibility, in *Bulletin of Atomic Scientists*, October 1976, p. 8–14.

[4] *Ibidem.*

race. Progress toward SALT and a five year moratorium on all nuclear tests including the peaceful nuclear explosions would have rectified the situation.

However, on the domestic side, the fracture with the former presidents proved not to be so large: Carter admitted that to cope with the expansion of civilian nuclear industry the US should have strengthened the international safeguards system, bearing the costs of expanding IAEA. Since the safeguard system at that time did not provide adequate assurances against national enrichment possibilities being used for military purpose, Carter claimed the necessity to discourage the sale of reprocessing and enrichment facilities, even if safeguards were acceptable to recipients. Moreover the candidate from Plains announced his commitment to persuade other supplier nations to subordinate their commercial interest to non-proliferations concerns, assuring the developing countries at the same time, about the reliability of the US as a supplier of enriched uranium on January 1977, the nation was gripped by both a record cold wave and the most severe natural gas shortage in its history. Because of the economic chaos created by fuel shortages and the skyrocketing of energy prices, the period 1976-1977 should have been an ideal moment for the formulation of a new energy policy. Virtually all economists, experts, businessmen and politicians agreed that the nation had to change its energy consumption habits and reduce its dependence on petroleum. To develop the specific details of this new energy plan, Carter turned to James R. Schlesinger, a Ph.D economist who had originally made his name as a specialist on the economics of national security, who became Presidential Adviser for energy matters. Schlesinger, as proved by his activity in a past presidential cabinet, was a strong supporter of FBR: he was convinced that the new technology could play a crucial role in a new rationalized energy policy, helping the US to avoid the suicidal dependence on fossil fuels, and at the same time reducing the complaints of the environmentalist democrats building less power plants but with a bigger capacity (and the feature of producing plutonium at a greater rate than they consumed). However, the spread of FBRs evoked the ghost of proliferation of weapon grade plutonium, a very sensitive issue at the Department of Defense, headed by Brown.

In order to remove any incomprehension, the president called all his advisers in the *Situation Room* asking them a clear analysis of the nuclear perspectives of the US in the short term and an overall evaluation of them in the framework of the long term non-proliferation policy endorsed by the president. The results of the evaluation were included in the so-called *Presidential Review Memorandum, NSC-15 on Nuclear Proliferation*:

1. Assess the current status of US nuclear fuel assurance policies, reprocessing policies, including alternatives to reprocessing, and possibilities for the handling and disposal of nuclear wastes.
2. Review the decisions announced by President Ford in the statement of October 28, and identify the policy options required to implement those decisions.
3. Provide a review of the current status of major ongoing negotiations with and among foreign nations concerning proliferation.
4. Analyze the strengths and liabilities of bilateral negotiations, the London Suppliers group, and the IAEA as institutions for implementing US non-proliferation goals.
5. Identify current US nuclear export requirements and examine what new requirements might be applied to current and future export agreements, and what measures must be taken to insure US credibility as a nuclear supplier state.[5]

The meeting held the day before the oath was a crucial event for the development of the NPP: Schlesinger started working hard to ensure a good funding for the new institution created by the President, the DOE (the United States Department of Energy), a Cabinet-level department of the United States government concerned with the policies regarding energy and safety in handling nuclear material. Its responsibilities included the nation's nuclear weapons program, nuclear reactor production for the United States Navy, energy conservation, energy-related research, radioactive waste disposal, and domestic energy production. DOE's plans for improving the management of nuclear energy were really remarkable: fast breeder reactors were the most important element in the R&D budget of the new institution, and they were seen as the inescapable substitutes of the precedent generation of nuclear reactors.

The Department of Defense, afraid of the consequences of the spreading of weapon grade plutonium harshly criticized the new institution approach. The most divisive issue in the Presidential Cabinet was granting permission for the reprocessing of US-origin spent fuel, as Nye stressed out in an article on this

[5] *Presidential Review Memorandum, NSC-15, to the Vice President, the Secretary of State and Defense on Nuclear Proliferation*, 01/21/1977, in Presidential Review Memorandums (PRW) internet link http://www.fas.org/irp/offdocs/prm/prm15.pdf [visited 03/20/2009].

matter. Using the conclusions on FBR sent by blue ribbon panel of scientists headed by Philip Handler (later the document will be known as the Ford-Mitre Study on FBR), Harold Brown showed the President why an indefinite deferral of FBR was necessary at that time:

> Although nuclear power is an important energy source, the United States and the world are not critically dependent on it for future energy supplies or economic development, and it can contribute to the immediate energy problem.
> - Increased energy costs, with or without nuclear power will not have a fundamental effect on the growth of the economy or employment, and need not affect basic life style compared with that expected at constant energy costs.[...] Even viewed optimistically the cost advantages of nuclear power will have little significance on overall economy (small fraction of 1% of GNP) in this century.
> - Nuclear power new technologies can serious complicate proliferation problems if plutonium is introduced into the fuel cycle as a result of plutonium recycle in LWR, plutonium breeders, or reprocessing for waste management. [...] Plutonium, reprocessing and recycle has little, if any, economic significance and should be postponed indefinitely.
>
> The commercialization of the Liquid Metal Fast Breeder Reactor should therefore be deferred and the breeder program recast a long range insurance program against very high future energy costs.[6]

Harold Brown's initiative surely contributed to the Presidential decision that arrived on March 24. On that day Carter signed his Non Proliferation Policy, deferring indefinitely FBRs, authorizing R&D just on alternative designs of plutonium, and proposing an International Nuclear Fuel Cycle Evaluation Program. Here is part of the text of the *Presidential Directive/NSC-8 on Nuclear Proliferation*:

> It shall be a principal US security objective to prevent the spread of nuclear explosive, or near explosive, capabilities to countries which do not now possess them. To this end US non-proliferation policy shall be directed at preventing the development and use of sensitive nuclear power technologies which involve direct access to plutonium, highly enriched uranium or other weapons usable materials in non-nuclear weapons states, and at minimizing the global accumulation of these materials.

[6] Letter, *to the President from the Director of the National Academy of Sciences Philip Handler. Subject: Nuclear energy policy study group*, 02/24/1977, Jimmy Carter Library Files, Subject Files, Atomic and Nuclear Energy, box 1977/I.

1.Specifically the US will seek a pause among all nations in sensitive nuclear developments in order to initiate and actively participate in an intensive International Nuclear Fuel Cycle Re- Evaluation program (IFCEP later INFCE) whose technical aspects shall concern the development and promotion of alternative, non sensitive nuclear fuel-cycle.
2. For its part the United States Government will:
a) Indefinitely defer the commercial reprocessing and recycle of plutonium in the US.
b) Restructure the US breeder program so as to emphasize alternative design to the plutonium breeder, and to meet a later date for possible commercialization.
3. It shall also be US policy to strengthen the existing non-proliferation regime [...] Therefore the US will announce his intention to terminate nuclear cooperation with any non nuclear weapons state that [...] terminates or materially violates international safeguards or any guarantee it has given to the US.[7]

On April 7th Carter announced an indefinite postponement of the program for breeder reactors, including commercial reprocessing and plutonium recycling, promising that the United States would offer nuclear fuel supply contracts and guarantee the delivery of nuclear fuel (uranium) to other countries. The bill was heavily oriented toward a technological approach to non-proliferation. It assumed that reprocessing was the decisive problem and had to be solved primarily through technological alternatives. The redefinition of the only available technical process (Purex) as a “non- peaceful” process (because it was originally designed to produce plutonium for bombs) amounted to a dangerous unilateral reinterpretation that could have been a potential interference into existing arrangements.[8]

2. The Second Phase: External Consequences of NPP

Restraining the use of energy derived from nuclear power at home and discouragement of nuclear proliferation abroad emerged as the keynotes of Carter

[7] *Presidential Directive/NSC-8, to the Vice President, the Secretary of State and Defense and others. On Nuclear non-proliferation policy*, 03/24/1977, On the Carter Library website at the link: http://www.jimmycarterlibrary.org/documents/pddirectives/pd08.pdf [visitato il 20/03/2009].

[8] *Memorandum from Eizenstat/Schirmer to the President. Subject: Re: US attitude toward reprocessing abroad, and proliferation issues,* 04/19/1977, Jimmy Carter Library Donated Historical Material, White House Central File – Subject File, National Security – Defense – ND-18. Box ND-48: General ND 16/CO 172 1/20/77 through Executive.

nuclear policy. Milestone of Carter's domestic policy was sometimes a Conservative ethic: use less, pay more. His foreign policy follow up of the same, was contained in a special message to the Congress on 26 April 1977 calling for a swift action on a legislative package that would ban exports of nuclear reprocessing plants, ban new agreements to export weapon-grade uranium and plutonium and make necessary direct presidential approval of any sale of weapon-grade uranium greater than 15 kilograms.

The presidential hopes for exercising effective control over the world nuclear market were based upon the fact that most of the emerging suppliers of nuclear tech continued to be customers of US nuclear materials. One of the first acts of pressure exerted by the Carter administration was to block the shipment of enriched uranium to its pilot customers abroad. No supplies of US nuclear materials had reached Europe since July 1976 and 660 kg stockpiled for delivering to Europe were blocked pending Carter's policy initiative.

Abroad, Carter's decision came to be viewed as an independent attempt to legislate the results of issues which need to be negotiated with other countries, not unilaterally. In the short run eleven pilot plants were threatened by closure due to the lack of fuel just in Europe, while in the long run, countries like Japan, with little indigenous energetic resources were likely to suffer. Here is what Robert Fri (Acting Administrator of ERDA) wrote to Brzezinski:

> I believe that the President must be forthrightly alerted to the fact that several of the proposals in the Presidential Review Memorandum are likely to place the US in a adversary position with a number of other nations which simply do not believe that reprocessing can be deferred. (West Europeans and the Japanese remain strongly committed to the breeder).
>
> [...] Thus if one accepts the premise that a successful non-proliferation policy has to be broadly acceptable we will have to tailor our evolving non-proliferation strategy to deal with a variety of differing situations and foreign perceptions.
>
> [...] with regard to reprocessing, I believe it would be seriously damaging in terms of our relations with Japan, the UK and France for the U.S: to take a position in categorical opposition to the Tokai-Mura facility in Japan, the scale-up of the UK Windscale facility or the French operation at la Hague.[9]

[9] *Letter from Energy Research and Development Administration Director to Zbigniew Brzezinski, Special Assistant for National Security on Nuclear Proliferation*, 03/23/1977, obtained by FOIA, released 1/26/1998 under provisions of E.O. 12958 by R. Soubers, National Security Council, from the National Security Archive Foundation of Washington DC, Collection: nuclear non-proliferation, number 01424, 4 pages.

The most rapid challenge to Carter NPP came when the policy was still *in fieri.* After his inauguration, Carter sent Vice President Walter Mondale around the world to prove the uninterrupted American commitment to old friends and allies. In Bonn, however, a shadow was cast over an otherwise harmonious event by the vice president's urgent request that the Germans stop the planned selling of nuclear reactors and enrichment and re-processing technology to Brazil, in exchange for access to Brazilian uranium (the so-called *German-Brazilian Deal*). This agreement led to the most serious clash in U.S.-German relations since the war, because after all, the deal was concluded in what was traditionally regarded an American zone of influence (Potthoff & Miller 2006).

Both West Germany and Brazil, not surprisingly, insisted on implementing an agreement that was in conformity with international obligations and to which the previous American government had given its approval. Through an unfortunate coincidence, the last steps in the implementation of the German-Brazilian deal occurred just at the moment when the Carter administration was formulating its own nuclear policy. As a result, the two countries which had every interest, as partners, in the improvement of nonproliferation policy, were locked in an antagonistic quarrel. At the industrial level, leading US reactors salesman in Iran, Argentina and Yugoslavia spread rumors that the financial difficulties of the Kraftwork Union (KWU) the West German consortium responsible for the basic design of reactors, would prevent it from making promised deliveries; at the political level, the US Government proposed alternatives to Brazil that would answer its requirements for a full fuel cycle, providing US reactors at a lower price (Gugliamelli 1976; Gall 1976).

By March 1977, the situation changed, when the two delegations (US-FRG) came to realize that they agreed on goals even if they differed on views on proliferation. The American side became aware that pressing the Germans to renounce the sensitive technology part of the deal would have been counterproductive, damaging relations with a major ally and undermining the administration's attempt to reopen the proliferation debate through a cooperative international dialogue. The Germans, for their part, decided that if they did not go ahead with the Brazilian deal, their own credibility would be undermined; moreover, there was widespread feeling in Bonn that deferral could deliver a fatal blow to any effort to improve the nonproliferation system in cooperation with the major Third World countries.

It was probably this growing dialogue with the German side, as well as the critical reactions from other countries, that introduced a note of caution into Carter's own statement of April 7. The president insisted that "we are not trying to impose our will on those nations like Japan, France, Britain, and Germany which already have reprocessing plants in operation." Along with the announcement of the deferral of reprocessing and the breeder program in the United States, the American government proposed to open an international dialogue evaluating the fuel cycle from the point of view of energy and nonproliferation (INFCE) and the Germans were the first to accept the proposal.

Aside from the US allies, even the Third World countries reacted negatively to the April 1977 statement of the new American policy. A joint memorandum worked out by the participants in a conference at Persepolis in Iran-without U.S. governmental participation reflected this reaction:

> The essential point is that most countries look upon nuclear power as the only route to energy independence. For those countries which do not have large resources of uranium, this independence will come only with the breeder reactor . Any suggestion that reprocessing and recycling are unacceptable strikes at the very root of this motivation for adopting nuclear power, and naturally is viewed with alarm. The Carter statement is regarded by some as an implication of unilateral abrogation of international agreements. This perception, on the one hand, weakens the confidence of other nations in the U.S. promises of nuclear fuel supply, and on the other hand may weaken the effectiveness of the existing agreements and may even cause some NPT signatories to reconsider (Kaiser 1978).

But the hurdles on the road of INFCE were not finished. On May 6 1977, in front of delegates from 60 countries attending a IAEA meeting in Salzburg, André Giraud (General Administrator of the French Energy Agency) announced that France had devised a new way to enrich uranium that eliminate the risk of use for nuclear weapons. By claiming such an invention, the French disproved the US assumptions that nuclear technology would inevitably lead to weapons proliferation, and expressing the will to offer commercially this technology, they started presenting themselves as a more reliable supplier than the US. The announcement of the A-fuel breakthrough came with a general refusal of US position on fast breeders. While all the European delegations and the Japanese one were strongly in favor of the Liquid Metal Fast Breeder Reactors (LMFBR), the US delegation was isolated supporting the technological shift to

Light Water Breeder Reactors (LWFBR) or Gas-Cooled Fast Breeder Reactors (GCFBR) both based on a thorium/uranium-233 cycle.[10] The isolation position of the US delegation in Salzburg showed the President the difficulties on the road to INFCE, a meeting that could have been a political fiasco without a large consensus of the allies on a common platform on nuclear non-proliferation.

In order to circumvent such a result, between June and July Carter reverted to the single-bargaining strategy, trying to turn the Japanese opposition to his Non-Proliferation Policy in support of INFCE. Japanese sensitivity, at that time, stemmed from the fact that they had built a reprocessing pilot plant at Tokai-Mura under the assumption that previous American practices would continue. The Japanese sought assurances that the US would allow them to continue to operate the plant. Before leaving for a diplomatic mission in Tokyo, here is what Brzezinski wrote to the President:

> Tokai is bound to appear as an exception to our general standpoint against reprocessing. The key issue is thus how an exception can be made with as little damage as possible to our non-proliferation objectives.[...] Limiting damage to non-proliferation objectives will depend on what political measures accompany any technical solutions.[11]

As Brzezinski confides in his memoirs, in Tokyo the US delegation gave the required assurances to its Japanese counterpart, but asking in return their commitment to a productive participation in INFCE:

> I supported my staff's recommendation that the Japanese be given assurances with two conditions: that operation be geared to actual needs, which were quite small, and that no new initiatives be taken during the course of INFCE. Since we have made a dramatic change in non-proliferation policy, I felt we had to respect agreements made under the previous administration. (Brzezinski 1983)

[10] *Memorandum from Robert Fri to Brzezinski. Subject: ERDA Report: US nuclear nonproliferation policy reactions at IAEA Salzburg Conference, May 2-13 1977*, written by Office of International Affairs, US Energy Research and Development Administration, May 24, 1977, 06/02/1977, White House Central File – Subject File National Security – Defense – ND-18, Box ND-49: Executive ND 18 4/1/77 – 4/30/77 through Executive, ND 18 11/16/27 – 12-31-77.

[11] NLC-98-269, (July 12, 1977) p. 1, quoted in Costello, Ch. S. III (2003). Nuclear Nonproliferation: A Hidden but Contentious Issue in US-Japan Relations During the Carter Administration (1977-1981). *Asia Pacific: Perspectives 3*, (1), 1-7. San Francisco: University of San Francisco Center for the Pacific Rim.

However, After the German-Brazilian Deal, the Tokai issue became a dangerous second "exception" that raised a fundamental question about the motivations of US concerns about proliferation. Was Carter concerned about proliferation *per-se* or the President was just implementing a dangerous "selective proliferation" in light of US strategic interests?

3. The Third Phase: INFCE

The confrontational approach that was driven by events threatened to isolate the United States and promised further damages to a regime that Carter was trying to refurbish. So it became necessary to avoid the polarization of two hostile groups, one focused on London (LSG) and the other on Vienna (IAEA). In order to meet these various policies the president decided to speed up the efforts on the International Nuclear Fuel Cycle Evaluation Program. While officially INFCE was given a predominantly technical rationale, INFCE became a means of attracting broad participation into what was really part of a political process of stabilizing the basis for the international regime. The most important point was that INFCE could have focused other countries' attention on a U.S. question: non-proliferation.

At the organizing conference, held in Washington in October, it was agreed that INFCE was to be a technical and analytical study and not a negotiation, and that its results would not be binding on the participants. It was also agreed that all interested states and all the relevant international bodies might participate and that the evaluation would have been carried out in a spirit of objectivity, with mutual respect for each country's choices and decisions in this field.

The evaluation was based on three premises. The first was that:

> The participants were conscious of the urgent need to meet the world's energy requirements and that nuclear energy for peaceful purpose should be made widely available to that end.[12]

A second premise was that the participants:

> were convinced that effective measures can and should be taken at the national level and through international agreements to minimize the danger of prolifera-

[12] *International Nuclear Fuel Cycle Evaluation, Remarks at the first plenary session of the organizing conference*, 10/19/1977, Public Papers of the President of United States: Jimmy Carter 1977-1981, Published by the Office of the Federal Register, National Archives and Record Service General Service Administration, 1977 Book 2 – June 25 to December 31.

> tion of nuclear weapons without jeopardizing energy supplies or the development of nuclear energy for peaceful purposes.[13]

And the final one:

> The participants recognised that special consideration should be given to the specific needs and conditions in developing countries.[14]

Eight working groups were established, chaired by countries that volunteered to assume the responsibility: the activity of the working groups was coordinated by a Technical Coordinating Committee (TCC) which met nine times. Here is the list of the working groups:

- Working Group 1: Fuel and Heavy Water Availability (Co-Chairmen: Canada, Egypt, India);
- Working Group 2: Enrichment Availability (Co-Chairmen: France, Federal Republic of Germany, Iran);
- Working Group 3: Assurances of Long-Term Supply of Technology, Fuel and Heavy Water and Services in the Interest of National Needs Consistent with Non-Proliferation (Co-Chairmen: Australia, Philippines, Switzerland);
- Working Group 4: Reprocessing, Plutonium Handling, Recycle (Co-Chairmen: Japan, United Kingdom);
- Working Group 5: Fast Breeders (Co-Chairmen: Belgium, Italy, USSR);
- Working Group 6: Spent Fuel Management (Co-Chairmen: Argentina, Spain);
- Working Group 7: Waste Management and Disposal (Co-Chairmen: Finland, Netherlands, Sweden);
- Working Group 8: Advanced Fuel Cycle and Reactor Concepts (Co-Chairmen: Republic of Korea, Romania, USA).[15]

The working groups held 61 meetings in 174 days in which a total of 519 experts, representing 46 countries and 5 international organisations, participated and produced more than 20.000 pages of documents. 59 states and 6 international organisations took part in the final conference, and indeed, 66 states participated overall in the study in one way of another.[16]

[13] *Ibidem.*
[14] *Ibid.*
[15] *Final Communique of the Organizing Conference of INFCE*, Washington, October 21 1977.
[16] International Fuel Cycle Evaluation, *INFCE Summary volume,* IAEA,Vienna, 1980, pp. 3840.

It is impossible to discuss here all the results of INFCE. Politically, as Karl Kaiser pointed out (Kaiser 1978), the debate on a revision of the basic rules of non-proliferation and the access to nuclear energy technology was at last where it should have been much earlier: in an international forum that included all concerned parties. The evaluation represented a vast effort, bringing together a large amount of scientific, technical, political, and economic expertise, to evaluate the entire fuel cycle. So INFCE surely improved the climate of nuclear diplomacy, identifying, as suggested by Philip Gummett (1981), where on the relatively technical (as opposed to political) end of the non-proliferation spectrum it is worth expanding effort and where not. To confirm that vision it is worth recalling what Joseph Nye, Carter's Adviser on Nuclear Proliferation, said in one of his articles on the matter:

> INFCE provided a two-year period in which nations could reexamine assumptions and search for ways to reconcile their different assessments of the energy and nonproliferation risks involved in various aspects of the nuclear fuel cycle (Nye 1981).

However the limits of INFCE were quite evident from the final statements of the Third World Countries Delegations. They continued to feel discriminated, with a restricted access to nuclear technology and in permanent underdeveloped state. Even the near-nuclear nations expressed their doubts, underlining the discrepancy between the large amount of money spent for vertical proliferation, and the relatively small amount spent for reactors to satisfy the Third World energy needs. So INFCE became, for them, just an occasion for the US to present again what they called "the discriminatory rhetoric" proposed in the NPT. As a confirmation of this approach is possible to read the last lines of the Pakistani statement at the INFCE final meeting:

> The incentives towards a proliferation spring from insecurity and the political climate in which we live [...] We must go on to the heart of the matter which is security perception of nations. In order to strengthen the non-proliferation regime we must not forget that there is an urgent need for controlling unrestricted vertical proliferation which poses an ever present awesome threat to human survival.[17]

Similar grievances carried over into the NPT Revision Conference in August 1980, where was impossible even agree on a final common declaration. In

[17] *INFCE Final Meeting*, Pakistani statement.

conclusion what can be learned today from the mistakes of INFCE is that technology can lead to proliferation, but proliferation has important non-technological origins as well. These origins in INFCE were neglected. According to the motto "*there are no sensitive technology, only sensitive countries*" the Third World countries' delegations tried to shift the attention from the need to study the technological aspects of nuclear cycles on the necessity to analyze the uniqueness of each concrete case of potential proliferation. INFCE with his exasperated tension to multilateralism and his exaggerate focus on the technological dimension, failed in curbing nuclear proliferation. But as Philip Gummett later noted (Gummet 1981), paradoxically INFCE had the merit for suggesting to the US nuclear diplomacy of the future a more country-specific approach, an useful means to deal with the threats of proliferation in today's interdependent world.

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The European Search for Aeronautical Technologies, and Technological Survival by Co-operation in the 1960s–1970s... with or without the Americans? Steps, ways, and Hypothesis in International History

*David Burigana**
david.burigana@unipd.it

ABSTRACT

By a transnational approach interconnecting archives from different countries engaged in aircraft co-production in Europe, it is possible to investigate on the occasion for the Europeans "to overtake" the US "incumbents", the US leaders in aeronautical market. As Airbus's launch and survival would suggest, this was widely debated since the second half of the 1960s and up to the end of the 1970s. Particularly the project for a Common Aeronautical Policy in the EEC framework was sponsored by Altiero Spinelli, European Commissar for Industrial, Scientific and Technological affairs in 1971-74; and also in a series of international conferences organized by the Western European Union Assembly as well as inside an informal tripartite working group created by London with Paris, Bonn and La Haye to discuss EEC propositions. US leadership in the global market and their firms' penetration in European aeronautical industries by a financial and technological point of view was very relevant. Anyway, European firms held a strong position in the negotiations to obtain US technology for their development. They had money while US administration had no possibility to finance their aircraft industries. At the end, Europeans continued to cooperate with Washington, but outside the EEC, by intergovernmental cooperation.

* Università degli Studi di Padova, Italy.

> La domination de l'industrie américaine n'est pas liée à une domination de sa technologie en tant que telle, mais à la maîtrise du marché et par là même, du moment de l'introduction de l'innovation et du choix du domaine d'application de la technologie (Giget 1981, p. 43, quoted in Muller 1989, p. 171).[1]

Have European aircraft firms ever had the occasion to "overtake" the US "incumbents", that is, the US leaders in aeronautical market, and, if so, when? The launch and the survival of the "Airbus operation" suggest this may have happened between the 1960s and the 1970s (Burigana 2007). However, it is unclear whether EEC Members really wanted to challenge the USA by cooperating, and in particular by cooperating exclusively at European level.

In this article, we will suggest an answer by reconstructing the reflexions and the debate developed since the end of the 1960s and up to the first half of the 1970s around a sort of "European Air Space" to be organised by coordinating "communitarian" co-production of aircraft and engines. This analysis will be the occasion to present our research approach based on the interconnection of government and more rarely firm documents from French, British, US, Italian and Spanish Archives as well as from the EEC institutions. It also merges different histories from a transnational perspective, and particularly the history of International Relations, the history of European Integration and the history of technology.

In our researches, we have stressed the relevance of the different national decision making processes in their action at transnational level, and specifically the role of "experts", not only technical experts, and not only engineers (Schot 2007, 2010; Schot & Misa 2005).[2] We translate "experts" in the more large signification: firms' representatives, civil servants, diplomats, military personnel, high military authorities, industrial, scientific or technical advisers to chiefs of State or government, ministers or politicians engaged in scientific-technical policies or lobbying, Prime ministers and chiefs of State particularly interested in technological innovation (Burigana & Deloge 2010).

[1] Giget was Director of the Groupe d'Études Sociologies, Économiques et Stratégiques sur la Technologie.

[2] Experts' role is a strong point for Johan Schot, an historian of technology who tries to bring in the SHOT (Society of History of Technology) a transnational approach: http://www.tie-project.nl.

We would attempt to show the continuity between our researches and those works interconnecting, in general, the history of international relations and technology as a means of State power (Krige 2006; Krige & Barth 2006; Sebesta 1991; 2003), or more specifically, highlighting the role that technological innovation has as a diplomatic "tool" to confirm or to affirm political, commercial, economic as well as cultural leadership. Following those studies, technological and commercial control on an international scale shape alternative historiographical views, for example of the Cold War or of the European integration process (Burigana & Deloge 2010).

First of all, with respect to transatlantic relations, the use and the control of technology enlightens the birth of transatlantic competition, such as that one between the USA and United Kingdom, for the exportation of aircrafts not only in Western countries but also in communist States such as the Popular Republic of China (Engel 2007). Another case of transatlantic technological "attrition" could be represented by the US opposition to Concorde commercial viability (Owen 1997).[3] With respect to the European construction, historiography on European integration has paid little attention to technology, and particularly to technological cooperation between EEC members (Bossuat 2006; Guzzetti 1995, 2000; Guzzetti & Krige 1997; Kaiser & Varsori 2010; Lynch & Johnman 2002, 2006; Zimmermann 2000) as well as a possible common policy on R&D (Van Laer & Bussière 2007; Pigliacelli 2006) or on a particular technological field (as for instance aircraft production and air transport coordination: Heinrich-Franke 2007; Burigana 2008a) or program (only for aircraft co-production: Chadeau 1995). First of all, an analysis considering technology in Europe from a transnational point of view, or technological cooperation of European countries since the Second World War – and regarding some fields just before the end of the War – this analyze can enlighten how European construction process followed not exclusively the institutional way represented by the EEC evolution. Rather, there are several and durable examples of intergovernmental cooperation that concurred to manage and organize the relations among European countries (Bouneau, Burigana & Varsori 2010, p. 15–35).

In Euro–American relations an amazing example is the race at the end of the 1960s to engine cooperation agreements, which ended with the agreement

[3] I.e., the limitation of Concorde flights on US territory, and by this way of its commercial use by all countries interested in air connections with the USA.

signed in April 1974 between the French SNECMA (Société Nationale d'Études et Construction de Moteurs d'Avions) and the US General Electric. This marked the birth of CFM International, the present leader in the world for civil aircraft engines of 10 tons based on an agreement renewed the last year. The competitors GE, and US Pratt & Whitney (P&W), which was in between the 1960s and the 1970s the leader in civil engines, vainly tried to organize a similar agreement in 1972–76. Only in March 1983, Rolls-Royce arrived to gather P&W, MTU (Motoren und Turbinen Union), Fiat, Japanese Aero Engines and to establish the International AeroEngine, one of the present suppliers of Airbus, and challenging CFM International.

Therefore, the techno-industrial facets of the relationship with the USA searched since the end of the 1960s by all European western countries enlightens on the one hand the degree of the actual intra-Atlantic competition, and then the level of the Euro-American one, on the other hand the real will of the European countries to cooperate each other, namely without the American participation, and then to compete at techno-commercial level with the USA. Above all, it would be possible to enlighten the strategy followed by all the most relevant aircraft producers in western European countries. Actually, they wanted to obtain the best technology at the time, at the best price and with the best market perspectives in the world, i.e., the American technology. By this way, not only Italy and the Netherlands, but France, UK and FRG too tried to secure the survival of an aircraft industry at National level thanks to the innovative capabilities of US technology characterized by less R&D costs and major sharing of the world market.

In conclusion, in the first half of the 1970s, the most relevantly engaged in aircraft production among EEC members, i.e., Great-Britain, France and the Federal Republic of Germany decided that aircraft cooperation in Europe had to be firstly an intergovernmental cooperation and secondly an Euro-American cooperation. This meant that they had to cooperate outside EEC framework, i.e., completely free from EEC rules, but also completely free each other in concluding agreements with the USA besides an eventual European or "Communitarian solidarity", and last but not least completely free to sell and/or to propose a techno-industrial re-production or cooperation to "socialist" countries as well as to "communist" China (for instance the case of Spey engines by Rolls-Royce in 1971–73: Pugh 2001, pp. 59–63), and in this case by challenging US technological, commercial and diplomatic supremacy at global level.

1. Three “route points” for a transnational approach to aircraft cooperation, and its markets

Before suggesting our analysis, it may be useful to present three major points to deal with aircraft cooperation, The first one concerns the structure of the aircraft market over a long period starting from the Second World War; concerning this point, we follow an analysis proposed by some Italian economists in Scuola Superiore Sant’Anna of Pisa University (Bonaccorsi 1996; Prencipe 2000; Giuri 2003), connected to the Science and Technology Policy Research (SPRU) of the University of Sussex, after the European deregulation of the EU Air Space officially completed in 1997 (Button, Hayes & Stough 1998; Sinha 2001; Staniland 2003; Staniland 2008). Secondly, it’s not by chance that the European firms saw the possibility to enter and survive in the US market between 1967 and 1978. Finally, any research on aeronautical industry cannot ignore the several interconnections existing between civil and military production.

1.a. Aeronautical Industry, a “forced” oligopoly?

Among several factors characterising the civil aircraft market which the economists single out as fundamental variables for the US market and its technological leadership, it is interesting to remark the different dimension of the US market compared to the European one, that is, its country-specific factor. In the US market, uniformly regulated by Civil Aircraft Board, the airlines have the same law limits, and then they organize their network by a similar structure. Differently, in Europe during the 50s and the 60s a civil aircraft designed for the French air space could not immediately flight in the German sky, or in Italy and in Great Britain. Accordingly, penetrating in the US market, the largest market in the world, was fundamental for the European firms, as Aérospatiale sought by Caravelle but in a bad way allowing Douglas, while it negotiated the production license, to study performance and limits of French plane. Thanks to these studies, and to the contacts with Aérospatiale, the American firm was able to propose a similar product than Caravelle, that was DC-9 (Bonaccorsi 1996, p. 130).The correct, the sure way to cooperate with the USA without the risk to be copied had to be GE/SNECMA agreement for realizing CFM-56, the 10 tonnes engine, but anyway with dangerous perspectives of intra-European competition.

The launch of the Airbus was possible by discovering the "hole of the market" represented by the wide-body short haul which was not covered by the "incumbents", that is by Boeing, McDonnell-Douglas, and Lockheed. In the middle of the 1960s, they were occupied with the race for wide-body long haul, the future B747. Boeing wined, but Lockheed had to exit from civil market, and was saved from bankruptcy by the US Government, while McDonnell was saved only by merging with Douglas in 1967 which was sustained by military orders. Secondly, Airbus had to disserve many and different courses, it had to be a "flexible" plane. US Airlines' courses were very similar.

After the entry of Airbus, and with the commercial failures of the first European jet planes Comet and Caravelle[4], the European firms and governments learned the lessons of the 1950s and the 1960s. It was clear that they could not proceed alone in launching a new model with high costs of development . The solution was the constitution of a consortium based on a formula developed inside the NATO: the Armament development and production organizations, formed by a prime customer (the management Agency), and by a prime contractor (the joint industrial company). For Airbus cooperation, they experimented the French formula for a co-production multinational company that was the *Groupement d'Intérêt Économique* (GIE) (Picq 1990, p. 53–54; Martel 2000, p. 29). This formula was more efficient than the mixed committees used for Concorde (Zeigler 1976, p. 24–25; Picq 1990, p. 53–54).[5] In October 1967, a US aeronautical expert Robert Hotz remarked, and the French Embassy in Washington reported to Paris that

> La principale différence entre des entreprises américaines et les firmes européennes n'est pas leur taille, mais plutôt le fait que les premières savent organiser de vastes consortiums technologiques et économiques capables de répondre aux besoins commerciaux ou patronaux. L'incapacité des Européens dans ce

[4] Besides its relative success at European level because of the number of planes bought (282) by 86 airlines and 11 armed forces in the world, Caravelle was a commercial failure as concerned its successive development, in fact there was no Caravelle family.

[5] They will choose it in 1972 for Franco-German Euromissile. British administration in 1973 selected it as a good formula for the integration of European aircraft industry: National Archives, Kew Gardens [otherwise NA], AVIA 64 2349, Note by Department of Trade and Industry and Defence Ministry, 12th March 1973.

domaine est pour eux un handicap encore plus récent que l'écart technologique croissant entre l'Europe et l'Amérique.[6]

In March 1977, at the Western European Union Conference organized by the Technological, Scientific and Industrial Committee of Western European Union Assembly, on European armaments cooperation, Allen Greenwood, President of Panavia, Tonardo consortium, and British Aircraft Corporation representative, as first step towards «integration of European aerospace firms in one or two large groups» (Greenwood 1977, quoted in Battistelli 1980, p. 156), asked for the institutionalisation of the existing consortium, not only for Multi-role fighter Tornados but also for light fighters Jaguar and Alphajet, as well as for Airbus. Lord Greenwood was listened by industrialists such as Altiero Spinelli, member of the European Commission in charge of Technological, Industrial and Scientific Affaire since July 1970 to June 1976.

Concerning the industrialists, national and EEC officials, politicians knew to favour commercial aspects of launching a new plane rather than dreaming technological exploits. They were convinced that the commercial *fiasco* of the Concorde had not to recur. In August 1968, the three ministers in charge for Airbus in France, Federal Republic of Germany, and United Kingdom, decided to extend the period required in order to complete the design stage of the A300 aircraft, improving its technical performance, reducing its price and testing the market.[7] In his report, the British Minister of Technology Ben Wedgwood reported:

> The French and German Ministers were far more realistic than I had expected and agreed that the aircraft had got to be commercial and competitive. This was Included in the communiqué. The French colleague, Jean Chamant, made it clear that he did not see it as a prestige project.[8]

In December, Wedgwood reaffirmed it at the House of Commons.[9]

[6] Aviation Week & Space Technology, *Europe's Techno-Politics*, 2nd October 1967, translated in Archives du Ministère des Affaires Etrangères, Paris [otherwise AMAE], Série Amérique, Etats-Unis 681, D. 1708/AS, Lucet, Washington, 12th October 1967.

[7] NA, CAB 164 765, draft of Minister of Technology to the House of Commons, 12th December 1968, send by Private Secretary T. Manley to Prime Minister and Foreign and Commonwealth Office.

[8] NA, CAB 164 765, Note Wedgwood, Minister of Technology, to Prime Minister / Foreign Secretary / Chancellor / Secretary of State for Economic Affairs / President of the Board of Trade, 6th August 1968.

[9] Draft of Minister of Technology to the House of Commons, 12th December 1968, cit.

Last but not least, European firms "discovered" as a useful strategy for survival the "product family". They were able to exploit scope economies rather than scale economies, by the launching of new models in other aircraft classes, e.g. after the A300 wide-body short haul, the A320, a little short haul, or by "stretching" that is extending the plane, e.g. the A310 from A300, and the A321 from A320. Aircraft family allow increasing the commonality of firm planes compared to concurrent products. European airlines constituted two consortium in order to manage economically stocks of spare parts: KUSS[10] and ATLAS.[11]

1.b. 1967–1978: "L'espace d'un matin", or a timing hypothesis European for the European "sorpasso" of US incumbents

In 1967, Jean-Jacques Servan-Schreiber published *Le défi américain,* but above all the US Government cancelled the agreements on technical and military cooperation with Great Britain and FRG (respectively in September 1967 and February 1968) signed by the Secretary of Defence Robert McNamara with his colleagues, Uwe von Hassel (November 1964), and Denis Healey (March 1966), and provided for realizing V/STOL (Vertical/Short Take Off and Landing) (Burigana & Deloge 2006, p. 344–348). Probably Johnson Administration was convinced by necessities of balance and by the necessity to mantain the technological leadership (Sebesta 2003, p. 172–184; Zimmermann 2000, p. 98).

In June 1967, during the Air show in Paris, Dassault presented its Mirage G, a version characterized by Variable Geometry. The French government left the Anglo-French Variable Geometry Project. Denis Healey, Secretary of State for Defence, informed the House of Commons, on 5th July. Five days later, the British Aircraft Corporation (BAC) engineers were in Munich to propose a joint venture to Germans for their Variable Geometry, the NKF (Neue Kampftlugzeuge). They proposed it on 5th March to F-104 G consortium, to replace Lockheed fighter produced by license in Europe. London jointed Joint Working Group where Multi-Role Aircraft for 1975, the future Tornado was born (Burigana 2005). The US "lâchage" as well as the French "treason" had to push Germans and British to cooperate (Burigana & Deloge 2007). In 1967 there is a peak of air transport as well as in 1978, but differently that in

[10] Created in 1966 by SAS and SWISSAIR, and the (1967) KIM and (1970) UTA.
[11] In 1970 by Air France, Alitalia, Lufthansa, Sabena, and (1972) Iberia.

1978, in 1967 it was advanced by growing aircraft orders(Bonaccorsi 1996, p. 216).

On 14th July 1978, Boeing launched the B767 thanks to the United Airlines orders (Muller 1989, p. 117), a twin engines plane characterized by a new design facing the A300. Anyway,Airbus had its foot in the door, and in November the European GIE launched the A310. Airbus family was born. In October, an agreement was signed for the re-entry of UK with the new British national concentration, British Aerospace. After 1976, the «année terribilis», Airbus orders were 176 with 20 airlines, and by Eastern Airlines, Airbus had penetrated US market. Finally, on 24th October, the US Congress voted the *Airline deregulation Act* for opening American air space to all airlines in the world.

As for all timing hypothesis, we need a little flexibility: the need to cooperate as a necessary way to surve became evident in the middle of the 1960s as well as the perception of the technological gap dividing Europe and US (Sebesta 1999), and the consequent plans to overcome it, as Fanfani Plan (Guderzo 2000, p. 373–374; Sebesta 2003, p. 186–190; Burigana 2009, p. 92–93), or the European techno-scientific Community by Harold Wilson (Its ties with Franco-British aircraft cooperation and the possibility of UK entry in EEC: Lynch & Johnman 2006; and on the propositions of European technological communities and EEC framework: Van Laer & Bussière 2007). Last but not least, if the A310 used composite materials, Airbus will challenge Boeing on technological ground in 1984 by launching the A320 employing the fly-by-wire system used for the first time on Tornado and Concorde by British Aerospace (BAe).

1.c. Financial-Technological interconnections between military and civil production

There is an interconnection between civil and military production as regarding applied technology –e.g. the core engine in military GE F-101 passed to civil CFM-56 and fly-by-wire system on Tornado used by the A320 - as well as an economic-financial linkage. Founds gathered by military orders for bombers B-47 and B-52 allowed Boeing to cover R&D costs for B707, the first and the most famous US jet, which was developed from Boeing military tanker KC-135 ordered by the US Air Force. The European Commissioner for Technology, Science and Industry, Altiero Spinelli remarked the relevance of the military sector for the survival of the civil production in October 1974, during a conference in Brussels organized by the Association Européenne des Constructeurs de Matériel Aéronautique: civil aerospace for 38 percent compared to 62 percent for the military one in 1972.[12] During the *marché du siècle*, i.e., replacing about 800 EEC members' fighters with US F-16, in January 1975 Spinelli was defending the «European» Tornado versus the US fighter.[13] In March, a French mission sought to convince the German Fokker to buy Mirage «par la perspective d'une plus vaste coopération de construction aéronautique aussi dans le secteur civil».[14]

2. «...becoming a emasculated appendage»: industrialists, officials, politicians and the need of a European cooperation but... without loosing National leadership

In June 1973, in front of the possibility to structure the European aircraft industry inside EEC, the new President of the Society of British Aerospace Companies, E. R. Sisson, said to the British Minister of Transports, Michael Heseltine, that, by playing the role of «tail-end Charlie» in Franco-German programs, «and whilst this desirable collaboration has probably been very much in the interests of the great design of EEC, we have nevertheless in the process of collaboration passed on a considerable amount of know-how».[15] Time was to assume leadership in Europe, «because becoming a emasculated appendage» of French colleagues was not his project.

Since the Report of Lord Plowden, in December 1965, ordered by the Labour for drawing the future ways of British aircraft industry, political establishment aimed to the international collaboration in order improve relationship between sales and development and production costs, a cooperation with US was unrealistic – all European aircraft industries suffer from small home markets – so the main effort should be directed towards Europe.[16] Government had to continue contribution to civil development costs up to an excess of 50 percent. However, government should have played a bigger role in the civil

[12] Historical Archives of European Union, Firenze [otherwise HAEU], ASUE, 32.

[13] HAEU, AS 33, *Projet de déclaration de la commission concernant le remplacement d'avions de combat militaire dans certains Etats Membres*, 20th January 1975.

[14] AMAE, Europe 71-76, RFA 3027, Telegram 999/1000, Wormser, Bonn, 17th March 1975.

[15] *Financial Times*, 28th June1973, in NA, FCO 55 1268.

[16] National Archives and Records Administration, Washington [otherwise NARA], RG 59 CF 64-66 651, Telegram A-13295, Amembassy, Kaiser, London, 16th December 1965.

aircraft program, particularly in new requirements and market studies. HMG should have arranged conferences of the European aviation ministers in order to formulate a common policy for aircraft manufacture and procurement. There should have been a single government organization responsible for promoting aircraft exports.

After the cancellation of important national projects (Wood 1975), the Labour government acted in order to obtain the leadership in any European co-operation program devoted to restructure the aircraft industry at the European level. This was the sense of the visit in Bonn of Roy Jenkins, the Aviation Minister, to Hans-Christoph Seebohm, Minister of Transports in December 1965.[17] Describing the Airbus decision as a splendid deal for the UK, John Stonehouse, the Minister of State in the U.K., said that one of the Ministry of Technology's long term ambitions was to develop an integrated European aerospace industry that could meet competition expected from the big industries of the USA and the USSR in the 1970s. To reach this objective, the UK was promoting another tripartite meeting which had to consider a variety of the civil aviation projects. From the UK point of view, the specific agenda had to include consideration of the possible development of a scaled-down airbus; an aircraft to meet European requirements for a Boeing 727–200 type of aircraft in the 1970s and emphasis on promotion of a European integrated engine industry in order to take advantage of the presumed UK leadership in advanced aero-engine technology.[18]

In March 1967, in a meeting at the Ministry of Technology about the British participation in Airbus, Sir Arnold Hall, Deputy Chairman of Hawker Siddeley Aviation (HSA), said that «if design leadership went to France... HSA would prefer to see Sud Aviation put in full charge of the project with HSA acting as sub-contractor and taking the normal commercial risks of a sub-contractor by quoting fixed prices to Sud Aviation».[19] Otherwise the UK would have loosen British technological leadership.[20] Sud Aviation had invested more

[17] AMAE, Pactes 326, Telegram 7191/94, Seydoux, Bonn, 24th December 1965.

[18] NARA, RG 59 CF 67-69 524, Telegram A-1096, Amembassy, London, 29th September 1967.

[19] NA, AVIA 65 2008.

[20] NA, AVIA 65 2008, meeting of Air officials [Minister of State (Stonehouse), Secretary (Aviation), Dep. Sec. Aviation, Controller of Aircraft (Sir Christopher Hartley), Deputy Controller of Aircraft Research and Development, U.S./Air A (Leonard Williams), U.S./Air C, Assistant Secretary/Air B.3 (T. M. Crowley), Assistant Private Secretary/Minister of State] with Minister of State Stonehouse to decide about meetings in Paris, 8th and 9th April for AFVG and Airbus, 24th April 1967.

in engineers than HSA which «will be incapable of taking on leadership of a major civil project in the 1970's».[21] On the other hand, Sud Aviation had to flourish and «almost inevitably will emerge from the Airbus, should it have been successful, as the major European airframe manufacturer». The starting growth in Sud Aviation's technical manpower since 1962 contrasted with the fall in HSA's. It illustrated vividly the way in which the European collaboration was strengthening the French airframe industry at the expense of British industry, leading the Sud Aviation's domination in civil aircraft. That was the British Officials' idea. In August 1967, in the relevant meeting (Science and Technology (67) 4th Meeting) the Prime Minister Harold Wilson, in summing up, said «we should also seek to avoid having French leadership on the airframe side of the project. If we could not secure the leadership we should seek agreement to the formation of a joint company».[22]

3. Towards a new "PAC"... a "Common Aeronautical Policy"? Inside European Economic Community, and facing US technological and market leadership...

In the first half of the 1970s, inspired by the crises of the aeronautical sector, and by the technological competition with the USA, a debate was developing about a «European Air Space», a sort of «Aeronautical Common Market» (Burigana 2008a). The Assembly of the Western European Union, an interparliamentary forum, exercising responsibility for scrutiny of and debate on security and defence issues, organized two conferences with firms representatives, diplomats, military authorities and civil servants engaged in aeronautical production. The European Commission too, and particularly General Direction III in charge for Industrial, Technological and Scientific Affairs participated actively to theses debates. The British entry in EEC would push to launch a real Common Aeronautical Policy. This will be not. EEC members were manoeuvring to reinforce their intergovernmental cooperation. As the industrial counsellor of President Pompidou Bernard Esambert said speaking on Airbus operation, this had to be a «European not a Common cooperation» (Esambert 1995, p. 67–74). In this context, and in the perspective of a Common Market for Aircraft, as

[21] NA, AVIA 65 2008, Memorandum by Director General (Civil) to Director Civil Aviation (R&D), 21st July 1967.

[22] NA, CAB 164 96, Note by Rogers to Sir Burke Trend, Cabinet Office, 4th August 1967.

regarding a common tariff against US importations proposed by airframe firms, will it be a non-sense proposition?

3.a. An Aeronautical Common Market or «un espace aérien européen»

In October 1974, at the conference of the Association Européenne des Constructeurs de Matériel Aéronautique in Brussels, Spinelli remarked that,

> Seul à l'échelle européenne vous pouvez vous insérer dans le contexte international vue la dimension de vos investissements, le contexte international caractérisé par des facteurs d'inquiétude et d'incertitude profonde, la crise énergétique, les surcapacités des compagnies aériennes, les prix exprimés en dollars, l'inflation.[23]

Aeronautical EEC share in global transport was 14.7% (1970) and 17.9% (1974), of Western Europe 21.1% (1970) and 25.7% (1974), while European aircrafts in EEC market were 33% (1970) and 20.2% (1974), in the world 9.5% and 5.9% , with a decrease from 432.000 (1969) to 405.000 (1973) employments. What solution? A sort of «Schumann Plan for aircraft» illustrated by Spinelli in May 1975 at a conference organized by *Financial Times* about "World Aerospace and Air Defence Industries".[24] This new Schumann Plan had to inspire Spinelli's *Plan d'action pour l'Aéronautique européenne* published by the European Commission, on 1st October 1975 (Spinelli 1979, p. 67–78).[25] With its dual technology, aircraft industry was the master piece to re-launch the industrial development in Europe, as affirmed by the *Plan d'action,* but at the same time, a ground for fighting US technological leadership. The *Plan* observed sources dispersion, lack of a global program, fragmentation of decision making processes, growing US penetration in the European market. How facing these «politiques centrifuges qui auraient porté à la disparition d'une industrie aéronautique autonome»? For the civil production, the solution would be to create an «espace aérien européen», a sort of aeronautical Common Market, and to establish a common procurement Agency in front of the US counterparts.

[23] HAEU, AS 32, 28th October 1974.

[24] HAEU, AS 33, Spinelli's contribution, «Une véritable industrie aérospatiale européenne existera-t-elle jamais?», 27th May 1975.

[25] HAEU, AS 33. Signed even by Vice-President of Commission in charge of Transports, Carlo Scarascia Mugnozza; edited on 3rd October 1975 by *Bollettino delle Comunità europee*, supplemento 11/75, pp. 1–33.

European airframe firms supported the idea of a «marché européen coordonné» to organize themselves against the US policy of the Buy American Act.[26] We saw, in 1977, the proposition of Lord Greenwood about the institutionalization of existing European consortium. What did he mean? Panavia, the Tornado consortium, was not satisfactory.[27] First, there was no real merging of capital by the three companies involved, and for such a capital fusion there was an insufficient common interest to produce a genuinely united company. Although Panavia had its own share capital, BAC's stake was only £10,000. Because of taxation and transfer problems the profits attributed to Panavia (and opposed to the three component companies) were deliberately kept very small. A fully integrated company would incur smaller costs for collaboration, but conflicting national tax legislation was at once an obstacle and a disincentive to the formation of genuinely international companies.

Secondly, the present international agency that constituted an intermediary between purchasing governments and Panavia was unsatisfactory. Its members were simply representatives of their governments, and they had no autonomous authority, and above all no common funds at their disposal. There was also insufficient managerial experience and no habit of working together. A permanent agency handling a wide range of projects would be more efficient and economical than a succession of ad hoc appointments confined to particular projects.

Thirdly, improved arrangements were needed for agreeing operational requirements and pooling funds for research and development.

Greenwood believed that a more extensive collaboration and a greater commercial integration were needed to ensure the future competitiveness of the European aerospace industry. He thought that, in the absence of governmental encouragement and incentives, this would probably develop slowly and by stages. Once Marcel Dassault left the scene, there might be a merger in France and the emergence of a single French aerospace company. This might be viable on its own for another five to eight years. There might also be a merger between British and German firms, thus leaving Europe with two major aerospace companies. He did not think mere consortia would be enough: an

[26] Press Conference, in Brême, by Werner Knieper President of RFG Association of Aeronautical Industries; AMAE, Europe 71-76, RFA 3027, Dispatch Louis Hirn, General Consul in Bremen, 15th May 1975.

[27] Lunch with Lord Greenwood; NA, FCO 14 837, Note by J. E. Cable, FCO, 5th September 1971, Restricted.

actual exchange of capital shares was necessary to create a common financial interest.

This process could, however, be speeded up if industry believed that governments seriously intended to create a market for them, and to remove the fiscal obstacles to mergers created by national tax legislation. He expressed interest in the idea of a common market in defence equipment, protected by an external tariff, and coupled with specific financial incentives to European governments to buy collaborative European products, but he suggested that the Americans would probably be able to find come way around it. Lord Greenwood also attached great importance to the evolution of common operational requirements as a basis for building «standardised» aircraft and selling them to European governments. The greatest difficulty confronting the industry on military equipment was the tendency for projects to be abandoned or drastically modified. His company had never had a project, which developed according to plan, but this was just as applicable to purely national as to collaborative projects. He was rather vague on the costs of collaboration and his reasons for arriving at the estimate of 25-30% of production costs. He thought costs would probably increase with the number of participants,, though certainly not in geometrical proportion, but this was naturally much less important then the economies of scale derived from increasing production.

He judged important that governmental encouragement for collaboration should not be confined to military equipment. The industry needed a steady flow of civil orders to tide them over inevitable troughs in military purchasing and, without a civil market, European aerospace industry would not be viable in the 1908s.

In general terms, Greenwood welcomed the idea of governmental interest, encouragement and intervention. There was no suggestion that industry could, or should, go it alone. On the contrary, he emphasised that some existing trans-national consortia, particularly VFW (Vereinigte Flugtechnische Werke)[28]-Fokker, had run into difficulties precisely because they had gone ahead without their governments. From his point of view, the priority task for governments was to remove the legal and fiscal obstacles to the creation of genuinely trans-national companies. But above all, we suggest, and the History too, how EEC members would protect European aerospace industry not only

[28] WeserFlug merged with Focke Wulf and Heinkel for creating Vereinigte Flugtechnische Werke.

by structuring it thanks to a "Common Policy". They had to organise a real "Common Market" by establishing a "common tariff".

3.b. Defending Europe! "Paris asks for aircraft tariff" and the "Tripartite" projects for a European aircraft industry

Monday, 19th October 1971[29], Henri Ziegler, President of the French aerospace industry association, and Director of Airbus project, announced during a press conference that association has asked Government to seek a EEC agreement to establish customs barriers against large US aircraft. Ziegler said this action was necessary because of US surcharge which had further upset balance between the USA and European aerospace industries. The USA had already supplied 90 percent of world's air fleet and 80 percent of Europe's aerospace needs, and European firms had faced sales crisis for several years under these circumstances. EEC could not continue to permit duty-free entry of US aircraft. Ziegler said his firm Aerospatiale had lost an order from US of 20 helicopters because of surcharge. Anyway, on 11th October, Governments didn't ask for suspension, but defended airlines rather than airframe producers.[30]

About a month later on, further discussion with the European Commission and the Committee of Permanent Representatives to EEC (COREPER) indicated that the annual total suspension of external duty of 5 percent on aircraft expiring December 31 1971 is expected to be renewed for 1972.[31] On 15th November, with the duty-free act in favour of US aeronautical products expiring on 31st December 1971, some European airlines – SABENA, KLM, Lufthansa, Air France, Alitalia – send to the European Commission a Memorandum with a letter by Airbus Industries for the renewal of that act.[32] Also having no equity interest in the project, American firms had to receive orders totalling 18 percent by value of each Airbus.[33] As Airbus Advertising sounded in 1974: «Foreign? Read On. It's all the same to you… Proven systems and components:

[29] Dickes, A., Paris asks for aircraft tariff. *Financial times*, 20th October; NARA, RG 59 SNF 70-73 Ec 632, Telegram 7706, Amembassy, Paris, 20th October 1971.

[30] NA, FCO 14 837, UK Delegation to European Communities to J. Treble, DTI, 21st October 1971.

[31] NARA, RG 59 SNF 70-73 Ec 632, Telegram 3471, US Mission EC, Brussels, 22nd November1971.

[32] HAEU, BAC 28/1980 21.

[33] NARA, RG 59 SNF 70-73 634, Telegram A-1101, Amembassy, Cash, Bonn, 21st December 1972.

25 percent of the airplane is US built...»[34], and in fact General Electric had to produce at a facility near Paris about 65 percent of the power plant. SNEMCA and the German MTU had to produce the remainder of the engines, or roughly 25 percent and 10 percent respectively. And what's more, the Garrett Company's auxiliary power unit was in fact another significant US contribution, which, together with the engine and pod, raised the US input per aircraft to roughly US $3 million. The total aircraft should be sold for something on the order of US $15-17 million. An American company was collaborating in the formation of an aircrew-training centre to be used to train crew and engineering personnel of airlines operating the A-300B. Smaller components, including air conditioning, de-icing equipment and instrumentation, will be-supplied by such American firms as Sperry, Bendix, and Westinghouse.

In January 1973, President of SABENA, and State Minister Baron van Houtte wrote to President of the European Commission, François Xavier Ortoli[35], for the renewal of the duty-free act in favour of US aeronautical products. Two months later, although, in the name of the Union Syndicale de l'Industrie Aéronautique et Spatiale Française, and after consultation with his European colleagues, Ziegler wrote to Ortoli about the same duty-free act, and particularly about the US reinforced competition because of two devaluation of USD, a revaluation of Deutsch Mark, and the European inflation for 10 percent.[36]

> En outre, par un paradoxe qui la Commission a souligné à plusieurs reprises, le marché américain, dont l'industrie contrôle 90% du marché occidental, est protégé de la "concurrence" européenne par des droits de douane. En revanche, les avions américains sont importés en Europe en franchise. Ce traitement inéquitable est un contresens économique qui pénalise les produits européens de 5% supplémentaires.

What to do? Firstly, establishing a common commercial policy with a common preference, and then «restaurer l'égalité des régimes douaniers, c'est à dire, faute de pouvoir supprimer les droits de douane imposés à l'entrée aux Etats-Unis, d'en établir à l'entrée dans la CEE», because European aeronautical industrialists were «conscients de participer ainsi à une lutte capitale de

[34] British Aerospace Heritage, Farnborough, F1251, Note, Wilson, Haight & Welch, Airbus Industry Summary, 20 June 1974.
[35] HAEU, BBBAC 28/1980 21, Letter, 12th January 1973.
[36] HAEU, BAC 28/1980 21.

l'Europe pour son droit à exister en tant qu'entité économiquement indépendante».

A month before, a telegram from the Quai d'Orsay, the French Foreign Ministry, to all French Embassies explained that:

> La vente des avions européens ne connaisse, au cours des prochaines années, de sérieuses difficultés résultant notamment de l'attitude protectionniste des autorités américaines dans ce domaine, de l'arrêt des hostilités en Indochine, marché préférentiel pour l'industrie américaine, et de la dévaluation du dollar.[37]

Then, the Quai d'Orsay invited French Ambassadors for a diplomatic action in favour of selling Airbus in cooperation with their German and Dutch colleagues.

In May 1973,[38] a working group of the European Council met on aircraft industry with the European Commission representative Christopher Layton, the Chief of Cabinet of Commissar Spinelli.[39] They produced a project on "tariff war". Subject to French, German and Italian reservations, the effect of this text was to propose that one of the specific aims of the forthcoming GATT tariff negotiations had to be the reciprocal abolition of tariffs on aircraft, engines and their components. On 20th October, although, *Le Monde* reported 11 airframe firms urging the European Commission to protect their European market against US competition.[40] In 1965, planes used by European airlines were 30% assembled in Europe, they would have been in 1974 26% and in 1977 24%:[41]

> Political decisions must be taken very promptly if existence and independence of European aeronautical industry is to be assured, for otherwise only solution

[37] AMAE, Europe 71-76 RFA 3027, Telegram *circulaire* 105, Ministère des Affaires Etrangères-Direction des Affaires Économiques et Financières-AG, Paris, 15th February 1973.

[38] NA, CAB 168 163, Telegram 2903, UK Representative, Palliser, Brussels, 30th May 1973, Confidential.

[39] Engaged on technological development of EEC: author of (1967). *Investimenti attraverso l'Atlantico*. Bologna: Il Mulino, edited by Istituto Affari Internazionali linked to Spinelli; and of (1969). *European advanced technology: A programme for integration*. London: Political and Economic Planning.

[40] BAC, HSA, and Westland, Aeritalia, Fokker, Fokker-VFW, Dornier, Messerschmitt-Bölkow-Blohm, SNIAS, and Dassault-Bréguet.

[41] NARA, Access to Archival Databases (AAD), Wars/ International Relations: Central Foreign Policy Files en http://aad.archives.gov/aad/series-list.jsp?cat=WR43 [otherwise NARA, files online], Telegram Amembassy, Irwin, Paris, 24th October 1973.

will be to resign ourselves in more o less near future to modest and limited role of subcontractor for American industry.

At national level, or inside EEC institutions, nobody had to react… Why?

3.c. Anglo-French Concorde, Franco-German "French Aérobus", English-Nord European "Europlane"… a long history of inter-European projects, and… counter manoeuvres

With the negotiations for joining the EEC, Foreign and Commonwealth Office wanted to gain a sure position for the British industry. By the 1980's the Community expected to have developed a major aerospace industry whether or not UK's participation. The decisions taken in 1970's would have largely determined what part, and if any part had to be played, the UK would be able to play; and they had to be based on an assessment of its future. It could be difficult to predict the shape that such a European industry would take even over the next ten years but London had to avoid to prejudice its possible role in it by the decisions taken in the 1970's by western European countries. If the UK was to play a part, it needed not only to keep a viable British industry in being but also to avoid damaging the industry of their future partners, or jeopardising existing collaborative work, notably the future Tornado. They needed to consider the future shape of the UK aerospace Industry as a whole and not simply the individual projects in isolation.

The adverse effect on their negotiations could be reduced if they were able to demonstrate that by their decision they were building up a strong aerospace industry which could make a valuable contribution to the future enlarged EEC. In October 1971, at the House of Commons, Minister of Trade and Industry, John Davies said «the long-term future of this industry lies in ever closer cross-frontier partnerships, particularly with Europe».[42] In December 1970, a Working Group on British Aircraft Industry and Europe was constituted «to consider the future organisation of the British aircraft industry in terms of the most economic deployment of its resources in the context of the British approach to Europe».[43] The question was: «Do we in particular wish to promote a scheme analogous to the Common Agricultural Policy for the European provi-

[42] NA, FCO 14 837.

[43] NA, T 225 3890, Memorandum by Prime Minister to Minister of Aviation Supply, 23rd December 1970, Secret.

sion of aircraft?».[44] As in the case of the Common Agricultural Policy, there would have been an economic price to pay, but it could well be that the United Kingdom would not have to pay only a smaller part of it than they should be paying for the CAP, but could conceivably even be net gainers, given that their aero-space industry was amongst the largest – if not the largest – in Europe.

Anyway, after having left Airbus in 1969, as a sort of reassurance, versus the «French Aérobus», London launched in November1972 Europlane, a QSTOL (Quiet Short Take Off and Landing) with Germany, Sweden and Spain:

> It is extremely doubtful whether this project would be economically successful. It will be opposed by the French who have forward, a proposal for a smaller version of the European Airbus. But this is unattractive technically and economically and will provide little work for UK industry.[45]

It was abandoned. In January, French Ambassador in London, Jacques de Beaumarchais met Lord Carrington, Secretary of State for Defence, with a French proposition for multilateral armaments cooperation.[46] Lord Carrigton though «qu'il serait sage de fusionner les moyens existants en Europe si l'on voulait que les industries européennes deviennent compétitives vis-à-vis des américains». He suggested that «il serait nécessaire d'associer dès le début les Allemands à la coopération industrielle franco-britannique. Ceux-ci ne devraient pas avoir le sentiment d'être tenus à l'écart». But, as remarked Beaumarchais, the Secretary finished «par admettre que les Allemands ne devraient être approchés qu'après un échange de vues Franco-britanniques». The "others" would be arrived but only later on (Burigana 2008c, p. 190-191).

British objective was the rationalisation and integration of the UK's and the European aerospace industries,[47] but remained a choice between encouraging the creation of a national group in the UK (which would have probably promoted similar reorganisations in other European countries) and then bringing the national groups together, or encouraging links across national boundaries followed by a coming together of the new European groups. Meanwhile Paris

[44] NA, FCO 15 837, Memorandum by J. K. Wright to FCO-Western Organisations, Cable, 17th August 1971.

[45] NA, CAB 168 163.

[46] AMAE, Europe 71-76, Grande-Bretagne 319, Telegram Beaumarchais, 2nd January 1973, and NA, T 319 3891, Note by Department of Trade and Industry.

[47] NA, AVIA 65 2349, Note about *The rationalisation and integration of the UK and European aerospace industries*, [DTI and Ministry of Defence], 11th June 1973.

extended consortium strategy with Madrid in February 1973 for Héli-Europe, a Helicopters consortium[48] but, firstly, with British Westland (Seiffert 2004).

In 1971,[49] Didier Godechot, commercial director of Airbus Industries, spoke to the National Aviation Club in Washington. According to industry sources, the French government was considering the possibility of bringing about the amalgamation of several French avionics firms in an effort to improve their productivity and competitiveness in the world aerospace market. Such an amalgamation would have been a logical further step in the rationalization of the French aircraft industry, which had witnessed the integration in the past few years of two major airframe manufacturers Nord and Sud Aviation, and of several aircraft engine manufacturers into two major firms: SNIAS (Société Nationale Industrielle Aérospatiale) and SNECMA.

The French government's interest in rationalizing this industry stemmed not only from a need to assure French competitiveness in the world market, but also to assure the best possible use of government R&D funds. During the past five years, the French government provided about 270 million francs, directly or indirectly, to aircraft equipment manufacturers. In the government's view, the integration of these R&D activities would have had a better potential payoff than it was possible with the funds being scattered among a fairly large number of firms. In 1973, it was not clear that the recent mergers had to pose a serious threat to US firms in the foreseeable future. Much had to depend on the success of the current European aircraft projects, and on the extent to which the French government intervened to support its domestic equipment manufacturers.[50]

Finally, what was the policy of London, Paris, Bonn in front of the European Commission, or better facing "Communitarian" projects? «One thing on which the French and German Governments agreed is that collaboration on aerospace is essentially a matter for Governments and the role of the European Commission should be limited», wrote Treadgold of Department of Trade and Industry about Tripartite Group activities.[51] «Maintain close contact with the European Commission while recognizing that their influence in this field is

[48] NA, AVIA 65 2349, Dispatch Wing Commander A. R. Scott, Air Attaché, Madrid, 22nd February 1973, Confidential.

[49] NARA, RG 59 SNF 70-73 Ec 632, A-483, Amembassy, Culley, Paris, 4th May 1971.

[50] NARA, RG 59 SNF 70-73 Ec 632, A-153, Amembassy, Kubisch, Paris, 26th February 1973.

[51] NA, T 225 3890, draft on *Aircraft Industry – European Integration*, Air 3, S. W. Treadgold, 18th July 1972.

likely to be limited and that we do not want at this stage to encourage their pretensions». The way to develop was the transnational but intergovernmental and not "communitarian" cooperation.

In fact, Fritz Engelmann[52] and Lorenz Schomerus,[53] German representative in Tripartite Group emphasized that it was then even more necessary than before to make progress with a common European policy, and as a first step to be got tripartite talks by the French, German and British Governments. But they thought the best tactics for the moment would have been to exploit the submission of the European Commission document on aircraft policy to the European Council of Ministers. A working group of the ten would probably have been set up to study this document and inevitably the Germans, the French and the British would have been driven to consult *à trois* about the activities of this working group. Engelmann and Schomerus emphasized that «the exploitation of the Commission document would be a tactical ploy. Their general attitude that this was a matter between Governments rather than for the Commission had not changed» (Burigana 2008c).

In October 1972, during the first meeting of Working Group on the Commission project for a European Aircraft Industry, the British explained their aim:[54] «We will try to steer a careful course between our belief that progress on aircraft is best made between Governments and the UK's general policy of strengthening the Commission». As regarding the Commission, it would have been «a forum for discussion but little else of positive nature», because «they [European Commission] could well have a negative effect by setting up rigid procedures which would limit our freedom of action»[55]... freedom to export aeronautical materials to any country, and particularly to the communist ones, and including the Soviet Russia... and China[56]... freedom to cooperate not exclusively among EEC members but with the USA too, or better especially with the US "competitors" and "incumbents". This was not only the idea of British establishment for the "success" of aircraft cooperation in Europe.

[52] Eng. Engelmann (1907-2008) was *Ministerialdirigent* at Ministry for Economics, Direction «Air».

[53] *Ministerialrat* at Ministry for Economics.

[54] From a draft, October 1972, for Minister of Aerospace: NA, T 225 3890.

[55] NA, FCO 55 1268, Note by Department of Trade and Industry, 12th July 1973, Comment on draft of Report by Council Working Group (Aviation) for COREPER.

[56] We quoted the Rolls-Royce case, but we are concluding researches not only on British attempts but also on French initiatives towards European communist countries, Soviet Russia and China since the second half of the 1960s up to the 1980s thanks to French firms archives.

4. Conclusions: a three-lined route of the US penetration... or who decided for an intergovernmental Euro-American aircraft cooperation?

In his «Schumann Plan for Aeronautics», Spinelli approached the «inevitability» of the US participation to European cooperation. In August 1974, President of Ex-Im Tech. Inc. Thomas Callaghan jr published a paper on *US/European economic cooperation in military and civil technology* ordered by and in cooperation with the Politico-Military Affairs Service of State Department. Before finalizing his paper, Callaghan met in Europe some people engaged in high technology production, and for instance Hugues de l'Estoile,[57] a French high official of aircraft industrial cooperation and the future General Director of the Ministry of Industry with President Giscard d'Estaing, and one of the participants to the Tripartite Working Group on EEC aircraft cooperation. De l'Estoile's ideas were that contemporary organization of a market was fundamental for European industry as well as the cooperation with US firms keeping high technologies.[58] In this way, Callaghan proposed a «Transatlantic Common Defence Market», and he encouraged European firms to merge and to create a «European Procurement Agency» to coordinate their desires for products and their co-production.

4.a. The American way to a «Transatlantic Common Defence Market»: European re-production of US licences

In 1975-76, in front of Anglo-Franco-Italian Tornado, French Mirage and Swedish Saab 37 Viggen, the *marché du siècle* finished in the most "classic" way: with victory of the US General Dynamics fighter F-16 co-produced for Belgium and Denmark by Fairey/SABCA, for Nederland and Norway by Fokker/VFW, while Norwegian Konigsberg Co. would have to produce components for 400 engines by P&W (Burigana & Deloge 2007, p. 193-219). This was a way experimented since the fighter Lockheed F-104 reproduced by Atlantic allies since the 1958. Despite of several injuries caused to Luftwaffe personal caused partly by bad materials and assembly, but above all by instability to pilot because of too much hazardous aerodynamics (Sgarlato 1985, p. 26-30),

[57] AMAE, Europe 70-75, RFA 2963, Telegram 4317/19, Kosciusko-Morizet, Washington, 15th May 1975.

[58] NA, AVIA 65 2349, Report, DTI, Assistant Secretary/Air 3, DTI, S. W. Treadgold, 17th May 1973.

F-104 was a great business for... Lockheed, which could finance the development of the civil Tristar L-1011.

Lockheed sold only 153 F-104 to USAF, and they were passed to Air National Guard after only four service moths (Francilon 1988, p. 334). Total production was 2,578 of all types, of which 741 were directly produced by Lockheed, 444 by FIAT (245 the much improved F104Ss in 1966 only for Italian Air Force), 340 by Canadair (essential for its survival: Pickler & Milberry 1995, p. 180–194), 210 by Messerschmitt, and 50 by successor MBB (Messerschmitt-Bölkow-Blum),[59] 207 by Mitsubishi (F-104J special "Japan"), 188 by SABCA, and 48 by generic co-production (Francilon 1988, p. 332).. Altogether a total of 1,127 F-104Gs, representing 44% of the F-104 production, were built as follows: 139 by Lockheed for Luftwaffe, Greece, Norway and Turkey; 140 by Canadair for Denmark, Greece, Norway, Spain, Taiwan and Turkey; and the balance being built in Europe by four groups for Belgium, Germany, Italy and Netherlands, and in 1971-73 by MBB for FRG.

This American way to organize a «Transatlantic Common Defence Market» has arrived up today with the Joint Strike Fighter F-35 against its European competitor Eurofighter (Burigana 2010a), but it is not the only way to US penetration in Europe.

4.b. US Direct investment: control, technological exchange, and market leadership

In the middle of the 1960s, Lockheed shares 8 percent in MBB, Northrop 20 percent in Fokker, and United Aircraft Corporation 26.4 percent in VFW. In 1965, German government stopped the handover of Messerschmitt shares to Lockheed, but Bonn authorised a cross handover of Bölkow shares to Boeing and Nord Aviation[60] for reassuring Paris.

In May 1969, top officials of the VFW of Bremen revealed in a press conference the details of the merger between their firm and the Dutch H.V. Koninklijke Hederlandse Vliegtuigenfabriek Fokker of Amsterdam. Two U.S. aircraft manufacturers were involved in the merger, United Aircraft Corporation of East Hartford and the Northrop.[61]

[59] Created in 1969 by Blohm, Messerschmitt, and Bölkow which merged in June 1967.

[60] AMAE, Europe 1961–70, Allemagne 1666, Note Ministère des Armées-Cabinet militaire, Paris, to General [de Gaulle], 6th May 1965, about meeting of Messmer with von Hassel.

[61] NARA, RG 59 CF 67-69 527, T. A-79, Amconsul, Ellison, Bremen, 14th May 1969.

In March 1973, Cabinet Ministerial Committee on Economic Strategic[62] remarked that:

- MBB (Boeing 8.9%, SNIAS 8.9%, Blum family 24.85% Messerschmidt family 21.3% Ludwig Bolkow 13.42% Bavarian State 5.9% Siemens 8.35% Thyssen 8.35%)
- VFW-Fokker (Krupp 35.1%, Hanseatische Industries Focke-Wulf 26.4%, United Aircraft Corporation 26.4%, Heinkel 12.1%)
- Fokker-VFW [Northrop (USA) 20%, FN (Belgium) 15%, (Dutch Holdings) 65%]
- Fairey subsidiary of the Fairey Co. Ltd

The German VFW and Dutch Fokker companies merged in 1969 but they continued to operate independently as Fokker-VFW and VFW-Fokker. Each held 50% of the joint company Zentralgesellschaft VFW-Fokker.

As regarding Airbus, American Embassy in Bonn wrote, in December 1972,[63] the project's outcome had also to impact on US interests in a less immediate sense, as FRG views regarding the basis of its future participation in European aircraft production were strongly influenced by its experience with the Airbus. There was, however, no doubt that a more self-assertive FRG aircraft industry was inevitable and that interesting sales opportunities for those American firms with the agility to adapt to changing circumstances could accompany this development, and US incumbents could rely on their technological and commercial appeal as Italian case would demonstrate.

4.c. American Industry offering «infiniment plus garanties»... the Italo-American agreement on B7x7 (1964-75): Italy, lonely partner?

Italy was interested in European cooperation as Andreotti's steps towards France and his official visit in FGR in 1964 demonstrate (Burigana 2008c, p. 182). In France, the objective was to evaluate the actual possibilities to cooperate in civil and military aircraft. At Bonn, Italia mission had to verify German will to a common project on a V/STOL based probably on Italian FIAT G-91. Secondly, and particularly French observers trusted in the pro-European will for a cooperation affirmed by the Chief of Air Staff ~~as~~ General Duilio Fanali

[62] Note about *The rationalisation and integration of the UK and European aerospace industries*, [DTI and Ministry of Defence], 11th June 1973, cit..

[63] NARA, RG 59 SNF 70-73 634, T. A-1101, Amembassy, Cash, Bonn, 21st December 1972.

when his French colleagues had approached him several times in the 1960's to suggest a coordinated project for a new military transport aircraft produced by a multinational consortium characterized by a Franco-Italian leadership (Burigana 2007). In 1971, Paris proposed to Alitalia a co-production of Airbus, but, as remarked by the Minister for State shareholdings, Flaminio Piccoli, this was only a «re-production», and Italy wanted to be equal.[64] At the time, Rome was interested in Mercure, the competitor of Airbus by Dassault, but only for 10 percent. Influence by McDonnell-Douglas, Boeing and Lockheed was strong, and aiming to isolate Italian market, while technical and financial propositions to Italian industry remained very interesting.[65] The only way was to assure Italian establishment of «la certitude que l'association de nos deux industries aéronautiques sera toujours pour eux une source de progrès technologique et de satisfaction industrielle et économique», as theFrench Ambassador in Rome wrote, and «sur un pied de stricte égalité et de confiance réciproque». Italy made a choice in 1967 for no participation in Airbus during two ministerial meetings on 6th July and 23rd September.[66] Alitalia representative and Count Corrado Agusta representing the Italian little firms denied any utility of the European cooperation. Agusta said: «it do not need technology but quantity of orders to "mantenere il passo" [to keep up]». His firm reproduced helicopters... under Bell and Lockheed licenses, and just at the end of 1967 he was negotiating the reproduction of Vertol Chinook with Boeing, which had just acquired Vertol Aircraft in 1960. Chinook had to be one of the most relevant successes for Agustaup today. As regarding Alitalia, it had always been «réticente devant les projets de coopération sur un plan européen», because the US industry offering «infiniment plus garanties», as the French Ambassador wrote.[67]

[64] AMAE, Europe 71-75, RFA 3027, Telegram Burin de Rozières, Rome, 5th February 1971.

[65] AMAE, Europe 1966-70 Italie 367, Note Ambassade de France, Rome, for Minister Michel Debré, 9th April 1969.

[66] Spanish traduction of original Italian reports on meetings; Archivos de l'Esercjto de l'Ajre, Madrid [otherwise AEA], 13256, Dispatch Col. Emilio Garcia-Conde Ceñal, Air Attaché, Rome, 16th October 1967.

[67] (2005). *Documents Diplomatiques Français. 1965, Tome II.* Brussels: PIE-Peter Lang, doc. 272, 606-607, Telegram n. 1531-5, Armand Berard, Rome, 17th November 1965, Reserved, for joining Concorde.

In conclusion, for its 7x7 project, Boeing discovered its first partners in Japan,[68] and then in Europe. After attempting in the UK – it wanted to take possession of *Europlane* partners by sinking the British project[69] – Boeing succeeded in signing a cooperation agreement with Aeritalia[70] in 1974 inspired by anti-Airbus objectives.[71] As Spinelli wrote in his memoirs, Italy made the best choice: to stay with the best technology in the world in order to prepare a future Italian participation to European cooperation. But Italy was not lonely on the route to Washington.

4.d The EEC Indians, are they becoming cowboys? US technology for European money

Next to Spinelli, there were other reflections about a possible European procurement system for military and civil aeronautical products, as WEU Conferences in 1973 and 1976, Gladwyn Report (13th January 1975) approved by European Parliament in December 1975, the constitution of an Independent European Programme Group (IEPG), Forni Report to WEU assembly and Klepsch Report to European Parliament in November 1977. Technology was one of several items approached by these documents. In July 1976, Spanish Air Staff received a visit card by European Office of Aerospace Research and Development. Its mission:

> To support Air Force research and development laboratories, divisions, centres and system development divisions by providing liaison with members of the scientific and engineer community in Europe, the Near East, India, and Africa; by encouraging open communication between Air Force scientists and engineers and their counterparts within the EOARD area of responsibility; and by acquiring useful and important research, development and manufacturing technology of direct and potential usefulness to the Air Force.

Its objective:

[68] CTDC, Fuji, Kawasaki, Mitsubishi signed an agreement in November 1972: NA, FCO 14 1006 Development of Airbus by Japan.

[69] Conversation of Boeing with Secretary of State, William Pierce Rogers, on 27th March 1973; NARA, files online, Department of State, Rogers, to Amembassy, London, 11th April 1973.

[70] Conversation of Luigi Azais, Assistant to President of Aeritalia, with Raymond C. Ewing, First Secretary, on 20th March 1973; NARA, RG 59 SNF 70-73 Ec 643, T. A-177, Amembassy, Ambassador John A. Volpe, Rome, 28th March 1973.

[71] AMAE, Europe 71-75, RFA 3027, Dispatch, French Embasssy, Rome, 26th September 1975.

> To identify foreign technological capabilities and accomplishments which can be applied to the resolution of Air Force requirements with savings in Research facilities, funds, manpower, and time and to make then available to Air Force Research organizations.
>
> By what way?
>
> To make professional visits and personal contacts to identify and understand current and planned research and development programs, to arrange for exchanges of technology on a personal basis between US Air Force and foreign scientists and engineers, and to generate and monitor contracts and grants between the Air Force Systems Command research and development organizations and overseas scientists and engineers.[72]

This was a "philosophy", a policy applied by the USA to NATO countries too, and since 1952, by AGARD (Advisory Group for Aeronautical – then Aerospace – Research and Development) authorized by Standing Group in January 1952, but paid at starting by USAF[73] for which worked its inventor, the aeronautical genius Theodor von Karman (Burigana 2008b). The main objective of this «military Agency» AGARD was «to gather the most important personalities in aeronautical science from NATO countries in order to maximise research and scientists employ for the common Defence of NATO Community».[74] It had «to suggest real ways» for employing R&D capabilities, and to supply aerospace consultancy to Standing Group, to stimulate technological innovation, and to permit knowledge exchange[75]... then, the "original" turning point presented by Callaghan's project was the economic linkage that he suggested to establish by exchanging US technology for European founds. This was the sense of the SNECMA/GE agreement: SNECMA wanted GE technology to enter in the market of civil engines, and GE needed French money to overcome the lack of public funds and the economic crises. For both, this cooperation was the only possible way to survive and they were sustained by Nixon administration (Burigana 2010b), and by President Pompidou (Buri-

[72] AEA, c. 11057, Letter by American Embassy, Madrid, to Chief of Second Section in Air Staff, 19th October 1976.

[73] Financed by NATO, 11th September 1953; NATO Archives, Brussels [otherwise NATO], SGM-169-54, Memorandum by Military Committee-Standing Group to General Secretary, 5th February 1954. Object: activation and founding of AGARD.

[74] NATO, S.G. 110/4, Report by Research and Development Committee and Logistic and Materiel Planners to Standing Group, 5th February 1954.

[75] NATO, SGM-270-64, Memorandum, Military Committee-Standing Group to General Secretary, 3rd August 1964. Object: AGARD Statute.

gana 2010c). On 22nd June 1973, French press commented on announcement of GE/SNECMA agreement: it was the inability of European industrialists to agree among themselves and their divisions which most captured the attention, as well as the originality of such transatlantic agreements in a sector such as the aeronautical industry where, until that time, Europe sought to find its identity only by opposing the United States.[76] What Paris promised to Washington?

An undertaking: Paris would not have imposed the suspended tariff on aircraft and components on US imports into EEC during the life of the license... SNECMA officials had previously indicated that such an undertaking should not cause any difficulty for the French government[77]... On 17th-18th June 1974, the EEC Council of Ministers approved a regulation proposed by the Commission which suspended, for the period July 1st–December 31st, 1974, the duties on certain parts needed for maintenance or repair of Mercure and Airbus planes. The products included in the suspension list included air conditioning equipment, various electronic equipment, pressurization equipment, fire extinguishers, aircraft instruments.[78] All European engine firms, financed, saved, sustained by their own national Governments, were negotiating to buy US engine high technology by P&W and GE which the US Government was not able to finance the development of the next generation engines which had to assure leadership in the future global aeronautical market.

On 25th June 1974, EEC General Council, and Industrial Ministers too, approached aeronautical industry perspectives. As noted, the General Secretary of the Council, Emile Noël:

> Sur l'aéronautique on se limite à un résolution portant sur l'information et la consultation réciproque en matière de projets et d'intentions entre entreprises de coopérer sur le plan transnational. On est donc loin de la politique d'ensemble que la Commission avait proposée (intention de créer une industrie de dimensions européennes et politique active pour stimuler les projets communs ; discipline commune en matière d'aides, en favorisant les projets internationaux ; règlement crédits à l'exportation).[79]

As Noël wrote, «il s'agissait donc bien davantage d'amorcer la pompe et de faire confiance à l'avenir»... a future that is today confirmed: Paris, London,

[76] NARA, RG 59 SNF 70-73 Ec 632, T.17288, Amembassy, Irwin, Paris, 22nd June 1973.
[77] NARA, RG 59 SNF 70-73 Ec 632, Telegram 23072, Department of State, Rogers, to Amembassy, Paris, 27th June 1973.
[78] NARA, files on line, Telegram USMISSION EC, Greenwald, Brussels, 19th June 1974.
[79] HAEU, EN 474.

Bonn with the co-participation of La Haye and Rome have decided to cooperate in Europe but at an intergovernmental level and with the US participation. In this way, in the 1970s they stabilized the bases for the actual world success of Airbus and CFM International, for the relative success of ATR, BAe146, Eurofighter, and International AeroEngine... for theses «European but not Communitarian» aeronautical "successes" (Esambert 1995).

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Like a Stone Guest. European Space Cooperation and the Birth of the Community Research Policy (1960–1973)

Filippo Pigliacelli *
filippo.pigliacelli@unibo.it

ABSTRACT

The launch, in the Eighties, of the first nucleus of the European Research and Development Policy, mainly represented by the start of the First Framework Programmes for Research and Technological Development (1984–1988), is strictly connected both to the begin of the first experiences of European scientific cooperation and to the political and public debate on these themes arise in the Sixties. Nevertheless the scientific and also political relevance of this cooperation, European space collaboration remained at the margins of the political debate on European cooperation in scientific and technological fields. For these, European space cooperation – and its further development – becomes the test bench for the definition of new hypotheses and modalities of cooperation representing an acceptable compromises between the will to cooperate and the defence of national and/or partisan interests (industrial sector, military, academic and research). This article aims to analyse the different paths followed by European cooperation in space, in the Sixties and at the beginning of the Seventies, also with regards to the birth of a European or "Community" research policy, and to show how this asymmetrical development still affect, today, the cooperation of European countries in space.

Introduction

The history of space activities is a relatively recent affair. Its beginning dates back to 1957 when, for the first time, a human-made object crossed the Earth's

* Università degli Studi di Bologna, Italy.

atmosphere, thus paving the way for the cosmos. At the same time, literatures of all periods and places contain many evidence of the human's desire to overcome this boundary (Dupras 1999, pp. 1–5).

Tackling the issue from the perspective of the ideal and imaginative space conquest, rather than from the effective events,, there is no doubt that Europe has played a leading role, as shown by the long list of fathers – real or putative – from Lucian of Samosata, Johannes Kepler and Jules Verne, to the closest just to quote the most famous – Constantin Tsiolkowsky[1] and Wernher von Braun, a pioneer in modern astronautics who was responsible of the first practical application of modern – and military – space technologies: the V-2 rocket.[2]

The character and the biography itself of the later – who was also the father of the inauspicious Apollo programme – is a sharp demonstration of an important axiom related to space activities: a space technology can be considered "good" or "bad", all depends on the aim it is used for (Sebesta & Pigliacelli

[1] Constantin Tsiolkowsky (1857—1935) was a Russian rocket scientist and a pioneer of modern astronautics. Since the age of 16 he started to dream about the possibility of space travel and interplanetary flights. He published his first known scientific work "Astronomical Drawings" in 1879. Later, in Borovsk, Tsiolkovsky wrote "Free Space" (1883),where he considered the possibility of living in outer spaces and the effects of zero gravity. In 1903 he published an article "The Investigation of Space by Means of Reactive Devices". Here he first outlined his theory of spaceflight and published the basic equation for reaching space by rocket that is still known to students as the "Tsiolkowsky Equation". In 1929 Tsiolkovsky wrote and published his work "Rocket Space Trains". He suggested a method of reaching of escape velocity using a multistage booster, consisting of separate rockets joined together and launched simultaneously. These very last calculations about multistage boosters pushed Tsiolkowsky to the conclusion that the first space flights would take place within 20 to 30 years. He made this prediction during his last radio speech from Moscow on the 1st May 1932. He died in 1935.

[2] The A-4 rocket, better known as V-2 to underline its aim (V was the abbreviation of *Vergeltungswaffe* "retaliation weapon") was the world's first operational liquid fuel rocket. It was guided by an advanced gyroscopic system that sent signals to aerodynamic steering tabs on the fins and vanes in the exhaust. It was propelled by an alcohol (a mixture of 75% ethyl alcohol and 25% water) and liquid oxygen fuel. This system generated about 55,000 lbs (24,947 kg) of thrust at the start, which increased to 160,000 lbs (72,574 kg) when the maximum speed was reached. TIt rose to an altitude of 52 to 60 miles (83 to 93 km) and had a range of 200 to 225 miles (321 to 362 km). The V-2 carried an explosive warhead (amatol Fp60/40) weighing approximately 738 kg that was capable of flattening a city block. It was first fired operationally on the 8th September 1944 against Paris then London. The V-2 offensive would last from September of 1944 until March of 1945. Close to 2,500 rockets were launched in this time period. The London area was hit by over 500 rockets and several hundred more dropped in surrounding counties. At first London and Antwerp were the primary targets, but rockets also fell around Ipswich and Norwich, and many Allied held targets in France, Belgium and Holland, and even on Germany itself. For more information: http://www.v2rocket.com/

2008). The famous American comedian Mort Sahl had probably in mind this warning when he proposed to add to the title of von Braun's biographical documentary, *I Aim at the Stars* (1960),[3] the sentence *But Sometimes I Hit London.*[4]

During the Cold War period, which not surprisingly corresponds to the golden age of the space conquest (the so-called Space Age),[5] this intrinsic characteristic of space activities was further revealed by the already mentioned launch of Sputnik, with which the Soviet Union demonstrated to US government to master space technologies needed to put a man in orbit, and hit American soil, by the US response, represented by the Stars and Stripes planted on the moon in 1969, and by the most recent President Reagan's Star Wars programme. Taking into consideration these traits of the space context, the participation of Europe could be considered, for many reasons, a kind of anomaly. After being, during the Second World War, the arena of the first military use of space, Europe became during the Sixties the framework for the development of pacific cooperation programs.

For this reason space cooperation, as well as other fields of scientific and technical collaboration, became in the second half of the Twentieth Century another field of political integration among European States, intersecting the path of the European integration process, started in 1951, with the signing of the Treaty establishing the European Coal and Steel Community.

In both cases, cooperation was a hard aim to achieve. Extending this analysis to other areas of scientific cooperation in Europe, we realize that apart from sporadic examples the problematic nature of finding a successful formula for such collaboration has been a sort of lowest common denominator of almost all episodes of the History of the European scientific cooperation, which characterized the very definition of European research policy, as shown by its long gestation period. Actually, its hard development contrasts with the role played in Europe by science, in the same years, as privileged framework for intergovernmental cooperation, as result of the advent of the so-called "Big Science" (De Solla Price 1963).

This change is usually referred to the increasing need of resources, both in quantity (of dedicated funding or staff) and quality (knowledge) terms, to carry

[3] http://en.wikipedia.org/wiki/I_Aim_at_the_Stars

[4] On the controversial aspect of von Braun's biography see Biddle (2009).

[5] The best historical reconstruction of the Space Age period is still the 1986 Pulitzer History McDougall (1985).

out research activities, particularly in strategic fields like nuclear and space, but it regards also the meaning of "big" as a measure of national prestige.

"Big Science" produced in Europe very different outcomes. In some cases, it drove States to cooperation, in order to solve the resources dilemma. Sometimes this process drove to the institutionalization of the cooperation, as in the case of the *Centre Européen pour la Recherche Nucléaire* (CERN). Differently, in others cases, crucial political implications related to the development of these areas of research and new technologies, strongly limited international cooperation or its successful development.

The entire history of European scientific cooperation shows the clash between the stato-centric bias of post-war scientific research and its essential universal character. So much that, in many aspects, one of the final aims of EU research policy definition process is to find a solution to this dilemma.

The origin of European cooperation in space

The origin of European cooperation in space is closely related to two crucial episodes in the history of scientific cooperation: the International Geophysical Year (IGY) and the above-mentioned birth of CERN.

In October 1952, the Assembly of the International Council of Scientific Union, convened in Amsterdam, voted the setting up of an extensive program of scientific cooperation

> [...] to observe geophysical phenomena and to secure data from all parts of the world; to conduct this effort on a coordinated basis by fields, and in space and time, so that results could be collated in a meaningful manner. (United State National Committee for the International Geophysical Year 1956)

This idea sprang from a proposal submitted two years before by the Special Assistant to the Secretary of State for the Military Assistance Program, Loyd V. Berkner, who had theorized, since 1950, the use of scientific cooperation as an instrument of foreign policy.[6]

The IGY, undertaken between July 1957 and December 1958 by research teams representing 67 countries belonging to the two political blocks was the most important international scientific initiative since the end of World War

[6] International Science Policy Survey Group, *Science and Foreign Relations. International Flow of Scientific and Technological Information*, Department of State Publication 3860, USGPO, Washington DC, 1950, cit. Sebesta, L. (2003). p. 10.

II. Furthermore, it also represented the arena for the use, this time for peaceful purposes, of one of the most crucial technology developed in the last years: the rocket. During the planned activities of the IGY, on the 4th October 1957, the first human artificial satellite, the Sputnik 1, was launched.

The IGY provided to European countries a unique opportunity to take part to a project whose realization was well beyond the available resources at that time in Europe. Furthermore it showed the role of cooperation in overcoming such constraints.

It was therefore not a coincidence that during the years of IGY experience a group composed by scientists from different European countries began to work to a common aim: the building up of a European laboratory for nuclear research. It certainly wasn't a coincidence that the main promoter of this project was another eminent American scientist, the Nobel Prize for Physics Isidor I. Rabi.[7] As member of the US delegation at the Fifth General Conference of UNESCO, held in Florence in May 1950, he proposed a resolution for the establishment of a nuclear research laboratory in Europe Hermann (Hermann, Krige, Mersits & Pestre), which took shape in 1954 with the ratification of the Convention of CERN.

The relationship between the creation of the CERN and the beginning of the European space cooperation in space is very tight, a blood tie. It originated from the network of relations weaved during the inception of the CERN, and in particular within the group of scientists who had supported the project since its birth, the so-called "CERN lobby".[8] The successful end of this initiative brought this group to repeat the same experience in other scientific field, as space sciences.[9]

The two leading actors of this story were the Italian physicist Edoardo Amaldi (Rubbia 1991) and his French colleague Pierre Auger (De Maria 1993). According to the historical reconstruction given by the same protagonists, in 1959 Amaldi described to Auger his idea of launching, before 1965, a European scientific satellite, called *Euroluna*, during a walk in the *Jardins de Luxembourg* in Paris (De Maria 1993).

[7] Isidor Isaac Rabi (1898–1988) was awarded of the Nobel Prize for Physics in 1944 for his resonance method for recording the magnetic properties of atomic nuclei. For more information see http://nobelprize.org/nobel_prizes/physics/laureates/1944/rabi.html

[8] This phrase was coined by John Krige and Dominique Pestre to explain some features related to the establishment of the CERN and in particular to the role played by scientists and national science administrators against national interests involved (Krige & Pestre 1987 pp. 523-544; 532–534).

[9] On the relation between physics and space activities see (Krige 1992).

The proposal, made by Amaldi and immediately shared by Auger was explained in January 1960 at the meeting of the Committee on Space Research (COSPAR), held in Nice.[10] For this occasion Amaldi presented a brief report titled "Space Research in Europe", which also appeared in the French press with the title "Créons une organisation européenne pour la recherche spatiale"(Amaldi 1959, pp. 6-8), where the creation of a European space organization «autonome par rapport à toutes organisations militaires» (Amaldi 1959, p. 8) and governed by the same scientists, so similar to the CERN, was proposed.

The COSPAR Assembly received the Auger-Amaldi's proposal with great enthusiasm. Furthermore, the British representative, Sir Harry Massey, President of the British National Committee for Space Research (BNCSR), overcoming the traditional reluctance of his scientific community to these forms of cooperation, and so expressing his support to it, proposed to expand the initiative to the launch systems sector, through the provision of a British launcher (De Maria 1993, p. 22), but the plot thickened.

It was clear that the possibility to use satellites and space to conduct experiments in orbit depended on the availability of launchers. At the same time, opening the proposed cooperation to the launcher sector would have obliged to deal with political and military matters related to the use of these technologies. And this was exactly what the CERN lobby would have liked to avoid.

The main consequence of the situation created in January 1960 was the start of two parallel space cooperation initiatives. The first one aimed to establish a purely scientific organization to carry out space sciences experiments: the European Space Research Organization (ESRO). The second devoted to building up a European launcher: the European Launcher Development Organization (ELDO).

[10] The COSPAR was established in 1958 within the *International Council of Scientific Unions* (ICSU) in October 1958 to continue the co–operative programs of rocket and satellite research successfully undertaken during the International Geophysical Year of 1957–1958. The ICSU resolution creating COSPAR stated that the primary purpose of COSPAR was to "provide the world scientific community with the means whereby it may exploit the possibilities of satellites and space probes of all kinds for scientific purposes, and exchange the resulting data on a co-operative basis". For further information visit: http://cosparhq.cnes.fr/index.html.

Defining the cooperation framework: the birth of the ESRO and the ELDO

In June 1960 the inception of the ESRO started with the setting up of a scientific committee *Groupe d'Etude Européen de Recherches Spatiales* (GEERS), chaired by Prof. Auger and charged to draw up a program proposal for the future European organization. The working group sought to define the boundaries of such collaboration, keeping the spirit and principles of the promoters. In particular, during the conference held in London in October 1960, three decisions were taken, which in many ways led to the success of the cooperation. The first was related to the desire to avoid any possible future conflict between activities realized within the organization and those conducted within the framework of national programmes. The working group successfully proposed to limit the operational role of the future organization to the phase of integration in the satellites of scientific payloads conceived by national scientific teams. Secondly, it was proposed a principle of equitable distribution of the contracts (the so-called *juste retour*) based on their value in proportion to the States contribution to the ESRO budget. Finally, the development of a future European launcher was excluded from the cooperation finalities, in order to reaffirm its strictly scientific finalities (Krige & Russo 2000, pp. 34–35).

On the 1st December 1960, thanks to the consensus achieved by the program and by the proposed architecture of the future organization, an intergovernmental conference held in Meyrin, at the CERN headquarters, stated the setting up of the *Commission Préparatoire Européenne de Recherche Spatiale* (COPERS), whose aim was to gradually implement the ESRO Convention.[11]

The COPERS started its activities in March 1961. In June a final draft of the scientific program – the so-called Blue Book – was approved. It included the launch of: sounding rockets, already in use for scientific purposes[12], scien-

[11] *Ibidem*, pp. 35–39. The Meyrin Conference was attended by mixed delegations of scientists and government officials coming from: Belgium, Denmark, Federal Republic of Germany, France, Italy, the Netherlands, Norway, Spain, Sweden, Switzerland and United Kingdom.

[12] Sounding rocket is an instrument-carrying rocket designed to take measurements and perform scientific experiments during its sub-orbital flight in the highest Earth's atmosphere layer (between 50 e 2000 km). The origin of the term comes from nautical vocabulary, where to sound is to throw a weighted line from a ship into the water, to gauge the water's depth. Sounding. About the scientific use of the sounding rockets (Seibert 2006).

tific satellites and space probes by means of the launch systems available in the market.[13]

A few months later, the 14th June 1962, ten countries signed the ESRO Convention.[14] Despite some difficulties, mainly due of financial nature, which caused the downsizing of its ambitious programme, the ESRO experiment proved to be successful, thanks to the purely scientific nature of its program and its decision-making structure in which the role of member States was in practice limited to the financing, which size was calculated, as in the case of CERN, on the basis of the national GDP.[15] In addition to its conspicuous program of launching of sounding rockets, in May 1968 ESRO launched into orbit – by means of a US Scout launcher – its first satellite, ESRO 2B[16], which was followed until 1972, by six others.

Since its beginnings the cooperation in the field of launch system proved to be more difficult. In the months following the Nice meeting (January 1960), Sir Massey proposal took shape through the formulation of a specific offer: the conversion of the British military project Blue Streak[17] into a civil launcher, as a first stage of a European launcher. The real motivation behind the British proposal was revealed in April, when the British government announced the end of the Blue Streak project because of its obsolescence with regard to the new strategic environment and the consequent decision to acquire solid-propelled Submarine-Launched Ballistic Missiles (SLBM), which UK acquired from US – in particular the Polaris system[18] – in the aftermath of the Nassau Conference (December 1962).

[13] The Blue Book extended over eight years and included the launch of: 435 sounding rockets; 11 small satellites; 4 space probes; 2 astronomic satellites (Krige 1993).

[14] Belgium, Denmark, France, Germany, Italy, the Netherlands, Spain, Sweden, Switzerland and the United Kingdom.

The ESRO Convention entered into force on the 20th March 1964.

[15] Each member States had one vote in the Council, where it could be represented by not more than two delegates, one of whom was generally a scientist, the other an important national science administrator.

[16] The ESRO 2B satellite carried seven instruments to detect high energy cosmic-ray electrons, determine the total flux of solar X-rays, and measure trapped radiation, Van Allen belt protons, and cosmic ray protons. It was launched on the 17th May 1968, from the Vandenberg AFB (California).

[17] Blue Streak was the name of a British Intermediate Range Ballistic Missile Project started in 1955. It was designed and built by the de Havilland Aircraft Company, and Rolls Royce provided the rocket engines. See Hill, C.N. (2001).

[18] The UGM-27 Polaris was the first SLBM (Submarine-Launched Ballistic Missile) deployed by the U.S. Navy. Their inherent immunity to pre-emptive strikes has made SLBMs one of the most important assets of the U.S. nuclear armed forces ever since. The Polaris A-1 was powered by a two-stage

For this reason, the chance of a European civil conversion of Blue Streak appeared to UK government as providential, especially in the light of the considerable sum of 60 million pounds already spent for the programme in the previous years.

The approval of the British proposal by the French government – the most difficult opponent – proved to be difficult but still possible. In fact, the agreement on Blue Streak could be considered as a part of a political rapprochement process affecting Anglo-French relationships in the years 1960–1962, realized by the use of technological cooperation, also in rocketry (Sebesta 2003, p. 107–120; 127–142). For this reason the initial French reticence was finally overcome through the common decision to replace the second stage of the European launcher originally planned, the British rocket Black Knight, with a French one (Krige & Russo 2000, p. 37–90).

This fact eventually persuaded General de Gaulle to give, in January 1961, his consensus to the initiative which was officially formalized, at the Strasbourg Conference (30th January–2nd February 1961).

At the end of 1961, after the accession of the Federal Republic of Germany the agreement for the creation of a European organization for the development of a launcher was signed at the conference held in Lancaster House from 30th October 30 to 3rd November. Few months later, the 30th April 1962, the Convention of the ELDO was finally signed.[19]

A quick look at the decision-making process, the organization architecture and to the programme showed the differences existing with ESRO. Unlike the latter, the decision-making was entrusted to the ELDO Council where member States were represented, while the Secretary General had mere and vague coordination and supervision assignments.

The total estimated cost of the initial program was defined in the Convention in about 70 million pounds for the building up of three stages launcher, called ELDO A, then renamed in a more evocative way Europa, able to put in a almost circular low orbit a satellite of a ton. The British government would have to cover the 39% of the budget and to provide the first stage of the

solid-propellant rocket motor by Aerojet General, both stages using four nozzles with thrust-vectoring for flight control. Its inertial navigation system (designed by MIT, manufactured by General Electric and Hughes) guided the missile to an accuracy of about 900 m (3000 ft) at a maximum range of 2200 km (1200 nm).

[19] The ELDO Convention was signed in London by: Belgium, France, Germany, Italy, the Netherlands, the United Kingdom and Australia (associate member). It entered into force the 29th February 1969.

launcher (Blue Streak). Similarly, the French government would have to cover the 24% of the budget and build up the second stage (Coralie). The Federal Republic of Germany would have to assure the 19% of the budget and the production of the third stage (Astris),[20] while Italy was responsible for the implementation of a satellite test vehicle and it would have to contribute for a 10% to the budget (Pigliacelli 2008, p. 107–158). Belgium and the Netherlands would have to cover the remaining 6% and they would have to develop the telemetry and the launcher guidance ground system. Finally, Australia, as ELDO associate member, would have to made available the Woomera range to test the European launcher (Krige & Russo 2000, pp. 98–100).

The launcher development architecture and management reflected the rigid division of the costs. In particular, Article 16 of the ELDO Convention recognized to member States an absolute autonomy in the implementation of the programme assigned. In practice, the design and construction of the three stages were individually managed according to national guidelines (from the project design to the awarding of the contracts). Only in the final phase steps would be taken to integrate the three stages systems, putting in this manner, as it can easily understand, a serious claim on the program failure.

Matters related to the managements were only a part of the whole problem. In the early Sixties the United Kingdom was the only European country to have a fairly thorough knowledge of all the technologies related to rocketry, thanks to its special relationship with United States. France was the other European country to have ventured into an ambitious missile program, whose origins, which can be here only briefly summarized (Sebesta 2005), dates back to the end of World War II when a group of 40 German engineers provided the needed knowledge for developing the first French rocket French: Veronique.[21]

In 1959, the country started an ambitious rocket program – named *Pierres Précieuses* – as part of the plan conceived by the new French President de Gaulle to give the country a nuclear deterrent. The development of missile arsenal in France, as well as the building up of the French stage of the Europa launcher, was entrusted to the *Société d'Etudes et de Réalisation des EngIn-*

[20] For a technical description of Coralie and Astris see: http://www.capcomespace.net/dossiers/espace_europeen/ariane/espace_europeen/ELDO_europa_1960_65.htm.

[21] Veronique was a rocket project developed, since 1949, at the *Laboratoire de Recherches Balistiques et Aérodynamiques* of the *Délégation Générale pour l'Armement*, in Vernon. For a technical presentation of Veronique, see http://www.avas.free.fr/aventure/lrba/veronique/veronique%20vesta.htm.

Site Balistiques (SEREB).[22] However, France still missed some critical knowledge, mainly related to the guidance systems, so as to legitimate the hypothesis that French participation to the Europa programme would served the function of acquiring key rocketry technologies through the participation to a cooperation program with UK (Vaïsse 1998, pp. 103–107).

From 1964 to 1966, all the stages were tested. While Blue Streak performed very well,[23] the test of French and German stages were much less satisfactory. With regard to the former, two of the three launches of a test version of Coralie (Cora) were a failure.

The situation In Germany was even more critical. After a long and complex negotiation, in 1962, the construction of the third stage Astris was entrusted to the ASAT consortium (*Arbeitsgemeinschaft Satellitentränger*), formed by the two major national aerospace industries: the Bölkow and ERNO. But the lack of know-how forced ASAT to subcontract to other industries a significant part of the project, making it even more complex (Reinke 2007).

Finally, at the end of 1967, the German stage reached Woomera to be tested. In November 1968, Astris did its maiden flight in the first launch of the Europa launcher in its full configuration – that is, with all three stages activated – which ended in failure, precisely because of Astris.

Indeed, technical difficulties encountered in the implementation of the Europa project were a true reflection of the problems that had begun to affect the ELDO, on the political level. In both cases the main difficulty was undoubtedly related to the lack of an efficient collaboration. In fact, the lack of technical coordination among industries and the same individuals who had been entrusted with the management of the three stages building up was leading the project to failure; at the same time, on the political level, the initial consensus was vanishing and calling into question the same survival of the cooperation.

[22] SEREB was a public company established in September 1959, in charge of the developing of nuclear weapon systems included in the “Pierres Précieuses” programme. SEREB was responsible of the programme management fromn initial studying phase to the prototype realization and the contracts awarding to the National industries (Nord-Avion; Sud-Avion; SNECMA; SEPR e Matra).

[23] Blue Streak was tested three times (launches F1,F2 and F3) from the Woomera test range from June 1964 to March 1965. In the following launches (F4 and F5) the Blue Streak was surmounted by a model (non activated) of the other two stages. Tests were successfully carried out between May and November 1966.

Space, Technological Gap and integration process

While the European space cooperation was taking its first steps encountering, especially in the case of ELDO, many political and technical difficulties, the issue of cooperation in science and technology made again its appearance in the Community political agenda after having disappeared as a result of the abandonment of Community cooperation in the nuclear field and the subsequent crisis of the nuclear community: the Euratom.

Even if in the plan of its promoters it would have to revival, after the failure in 1954 of European Defence Community, the integration process even more than the sister community – the EEC –, the European Atomic Energy Community's sporting chance fell down due to political and technological matters raised at the end of the Fifties in this highly strategic sector, which made difficult to implement the plan for the Euratom envisaged, in 1957, by the "three wise men": Louis Armand, Franz Etzel and Francesco Giordani.[24]

This situation appeared in the aftermath of the 1957 Rome Treaty, as a consequence of an internal split, soon proved to be unbridgeable, among the members States. In fact, France and Germany tried immediately to reduce Community ties to ensure a greater freedom of action to develop their national and military programmes (Hecht 2004), and the other member States interested in compensating the lack of funds at national level with the launch of joint programme financed by the Euratom.

The reasons behind the position assumed by France and Germany were different but they had in common the determination to defend the role of their national programmes. For France this was a necessary guarantee to the further develop of its nuclear programme, also for military purposes. For Germany this shift was mainly the result of the national nuclear industries (Siemens and Krupp) request of an increased support of their position in European and international market (Nau 1974).

This situation suddenly made the Euratom one of the sacrificial lambs of the crisis that struck the integration process in the Sixties, and the consequent abandonment, in June 1969, of its flagship project -the reactor ORGEL[25] and, a few months later, the decision to postpone the approval of its third five-year program of research.

[24] The full text is available on www.ena.lu.

[25] ORGEL was a project, launched in 1958 and based at Ispra (near Varese) research centre, for the building up of a cooled water cooled new generation reactors (Guzzetti 1995, p. 23).

In the meantime, the development of key sectors such as IT, telecommunications and space activities had greatly increased the interest, within the European public opinion, towards technological development matters and its political and economic implications.

The Organization for Economic Cooperation and Development (OECD) was the main promoter of this debate. Actually, it had inherited the interest on these issues from the Organization for European Economic Cooperation (OEEC) which had promoted, in the second half of Fifties, many studies on the relation among science, politics and economic development.

The main results of the OECD action, with a more prescriptive content than the previous one, were, firstly, the publication in 1963 of the so-called "Frascati Manual", proposing a common methodology – still in use today – for the quantification of the R&D costs and investments (OECD 1963), and then, the publication, two years later, of a report drawn up by two researchers of the National Institute for the British Economic and Social Research, Christopher Freeman and Alison Young, on the comparison of R&D policies in the countries of Western Europe, the United States and the Soviet Union (Freeman & Young 1965).

Aside from the glaring differences between Europe and United States in the expenditure on R&D – also due to the difficulty of accurate figures – the report noted the total absence in Europe of a "research strategy" able to potentially reduce the existing gap and ensuring a more effective use of the limited resources through cooperation.

The task of clearing up to European public opinion and politicians, the risks coming from a complete lack of initiative in R&D was picked up by the French journalist Jean-Jacques Servan-Schreiber.

The challenge laid down by him, starting from his successful book title, *Le Défi américain*, which was published in Italy with a foreword by the famous Italian politician Ugo La Malfa (Servan-Schreiber 1968),[26]concerns the ability of European countries to develop effective – and possibly common – solutions to reduce this gap of whatever kind it was: technological, managerial or simply financial.

A first response to Servan-Schreiber's call to arms was advanced by the British economist Christopher Layton, one of the most passionate supporters of his country access to the Community, who proposed, two years after the

[26] On Ugo La Malfa see Soddu, P. (2008).

publication of "The American Challenge", the launch of a European research and technology policy.[27]

The development of a public debate on the technological gap phenomenon, thanks to Freeman & Young Report and Servan-Schreiber's book, finally intersected the path of the concurrent development of the European space activities.

This was mainly due to three reasons. The first concerned the same "structural" features of space activities, as the resources required – financial and technical knowledge – to carry out them. Secondly, among the most advanced technological fields, space, as well as information technology was the one that seemed to offer more development opportunities in economic terms related to the emerging communications satellite sector. Finally, among the fields affected by the technological gap debate, space was one of the fields in which the European public opinion – and sometimes governments themselves – was showing its greatest concern in relation to this situation.

The role played in the Sixties by technological issues in the development of transatlantic relations has been effectively explained in 1971 by the American political scientist Henry Nau:

> As long as a rather strong consensus of military and political goals tied the Atlantic Community together in the cold War period, the issue of control of key resources remained dormant. [...]
> When interests began to diverge, however, initially in the strategic controversy of the late 1950's, the issue of control, in this case with respect to nuclear weapons, became a topic of sharp debate.
> The technology controversy reflected the extension of this debate to the control of key industrial resources, which were important for military but also, and perhaps in the first instance, for economic and socio-technological purposes. Europeans discovered that just as control over their security resided in the decision-making centres of the Americans White House, control over their economic and industrial performance in leading sectors of advanced technology increasingly resided in the decision-making centers of American global corporations. (Nau 1971, p. 517)

However, it wouldn't be excessive to affirm that the great success of the technological gap debate derived more from its political instrumental use than

[27] Layton, C. (1969).

from its real usefulness to solve the problem. This situation was mainly due to the extreme flexibility of the concept of Technological gap which change into

Formidable political tool to legitimize:

- New dimensions of economic development in key sectors such as nuclear, aerospace and electronics, and state support to achieve these aims;
- Integration and/or European cooperation as a key to expanding markets and national industrial apparatus;
- Requests for financial and technological support to the United States (Sebesta 1999, pp. 13–14).

France was one of the European country where this instrumental was more evident. In that case, technological gap became the key topic in support of the new foreign policy approach conceived by general de Gaulle, in particular, with regard to: US-European relations, the European integration process and also cooperation among European Europe, even in space field.

At the first meeting of the ELDO Council (5th–6th May 1964), the French representative Gaston Palewski, Minister of State for Scientific Research concerning nuclear and space issues, proposed in his opening speech a dramatic revision of ELDO initial programme , through the sudden transition from the first project, Europa 1, to a new one, named Europa 2 (or ELDO B). In particular, the new project prefigured the replacement of the two upper stages with the introduction, in a first phase of a single stage (ELDO B/1), followed by two (version ELDO B/2), in both cases with cryogenic propulsion (oxygen and hydrogen at very low temperature), able to put a large satellite in a geostationary orbit.[28]

After several months of high pitched discussion, mainly due to British opposition, in July 1966 States finally agreed on the definition of a new project called ELDO PAS. Starting from the initial configuration of the Europa launcher, it would allow to put in geostationary orbit a satellite with a mass of

[28] A geostationary orbit is one in which the satellite is always in the same position with respect to the rotating Earth. The satellite orbits at an elevation of approximately 35,790 km because that produces an orbital period (time for one orbit) equal to the period of rotation of the Earth (23 hrs, 56 minutes 4.09 seconds). By orbiting at the same rate, in the same direction as Earth, the satellite appears stationary (synchronous with respect to the rotation of the Earth). For this reason the geostationary orbit is commonly used by telecommunication satellites to carry out effectively its task of "repeaters".

170 kg through the implementation of a Perigee Apogee System (PAS),[29] consisting of: a perigee solid-fuel stage (based on the French Diamant rocket), an experimental communications satellite and solid-fuel engine integrated into the satellite (Krige & Russo 2000, p. 269–270). Furthermore, the French government obtained the inclusion in the new program of the building up of an "equatorial" launch site in Kourou - French Guiana – that is the best suited to put satellite in geostationary orbit.

In order to understand the rationale at the basis of the French request one has to remind the evolution taking place in those years in the field of space activities, also affecting a US–Europe relations. In fact, since the early Sixties, the space race enriched of a new economic dimension, resulting from the emergence of the new communications satellite sector, opened by United States in July 1962 with the launch of the Telstar 1, which relayed the first transatlantic television signals.[30] In August 1962, the US Congress approval of the Communications Satellite Act marked the start, from the political point of view, of the US political offensive in the sector, through the creation of public-private company, called Comsat, with the role of majority shareholder (61% of shares) of Intelsat, the international organization created in 1964 to build up and manage future worldwide communications satellite systems.[31]

In the light of the above, it was quite plain to legitimate French government concerns about the US monopoly in the sector, especially after the first attempt of the government to coordinate national initiatives in Europe, through the creation, in May 1963, of the *Conférence Européenne des Télécommunications par Satellites* (CETS), had produced poor results and further weakening the European negotiating position during the most delicate phases of the Intelsat negotiations (Griset 1991, pp. 73–89; Sebesta 2003, pp. 230–246).

[29] The apogee and perigee are the points of Earth's orbit where the planet is located respectively at maximum and the minimum distance from the Sun. Similarly in a satellite orbit, the apogee and perigee are the points at which the satellite is closest and farthest away from the Earth. In this context the perigee and apogee refers to the procedure to put a satellite in a geostationary orbit. In this case a satellite is launched into an initial parking orbit. Then a two-phase manoeuvre, known as Hohmann Transfer, is needed. In the first one, at the perigee of the initial orbit a burn is made to increase the speed of the satellite and change the eccentricity of the orbit. Then, another burn must be made at the apogee of the transfer orbit. The second burn places the satellite in a higher, more circular orbit.

[30] The first images broadcasted using Telstar satellite can be seen at the following address: http://www.youtube.com/watch?v=FgplIWibv4Q&feature=related.

[31] Butrica, A.J. (Ed). (1997)

The fragile agreement reached in July 1966, while responding to the French government request, have laid the basis for the disengagement of the British and, in general terms, it had highlighted the structural limits of European space cooperation in the launcher sector. These were primarily related to the difficulty of keeping the essential unity in terms of cooperation finalities and the lack of political glue. Ultimately, the crisis had revealed the existence of a rift, very hard to settle, between two visions on the aims of the European space cooperation. The first one, supported by France and the Federal Republic of Germany, as a further proof of the rapprochement between the two countries sealed a few years earlier with the signing of the *Elysée* Treaty (22nd January 1963), was to provide Europe with an independent access to space. The second one, defended by the United Kingdom, was based on the will of keeping cooperation within the boundaries set in 1964.

As Servan-Schreiber noted, in his lucid analysis of the technological gap phenomenon, exactly in the space field European states had demonstrated their inability to overcome "the nationalist nostalgia" in favour of a more cooperative approach.[32] For this reason, since 1966, trying to find a political solution to the ELDO crisis, this task was delegated directly to governments, through regular meeting, at ministerial level of a European Space Conference, whose main task was to coordinate national space policies with European cooperation.

Facing the task it was created for, at the second session of the European Space Conference, held in Rome in July 1967, a reform process was started bringing in a few years to a complete review of the European space cooperation through the creation of a single organization: The European Space Agency.

The first act of this reform process was the creation of a *Comité consultatif des Programmes* (CCP), chaired by Jean-Pierre Causse, to draw up proposals on the future of European cooperation in space. The most revolutionary one, presented in December 1967, was to merge ELDO and ESRO, in addition to the developing a new generation of launch vehicles able to meet the new technical requirements imposed by the new satellite telecommunications sector (Madders 1997, pp. 124–130).

Unfortunately, the effectiveness of the Causse Rapport had to deal with the evolution taking place "inside" and "outside" the space cooperation context.

[32] Servan-Schreiber, J.J. (1968, pp. 99-107; 105).

Regarding the "inside", during the Report drafting ELDO program suffered, as already mentioned, new failures, which caused an expectable exceeding of the 626 MAU[33] budget ceiling, fixed in1966 as a peace term for the crisis.

The end of the *entente cordiale* between France and the United Kingdom, provoked, in November 1967, by General de Gaulle's second veto to British access to European Community, represented the main "outside" factor. In particular, de Gaulle denial marked the final failure of the British Prime Minister Wilson access strategy, based in large part on the cooperation in technology, of whom ELDO formed an integral part (Pigliacelli 2006, pp. 83–111).

For this reason, without waiting the following European Space Conference, which would have had to discuss the proposals of the Causse Report, British government announced its intention to take no part in any further discussion about the ELDO programs. In addition, the already awful consequences of this decision were further aggravated by Italian government's decision to join the British position because of the cuts imposed on the very part of the program whose implementation was entrusted to its national industries (Pigliacelli 2008, pp. 128-133).

The agreement finally reached in November 1968, while ensuring the ELDO program's survival, couldn't find a solution to the matter which have provoked this collision. In addition to this, the test launch executed in November 1968 and following two previous attempts(respectively the 31st July 1969 and the 11th July 1970) turned in a fiasco, in the first two cases – as above mentioned – for a malfunction of the German third stage Astris, while the third failure was because of the payload fairing failed to separate.

Finally, in April 1969, Italian and British governments decided to withdraw from the Europa programme, while France, West Germany, Belgium and the Netherlands decided to pursue it on the basis of the 1964 French government proposal.

The solution finally arrived from the ESRO. In December 1971, the Council decided to create a single organization with *à la carte* programme. Meanwhile, in the ELDO, after the failure of the first launch of Europe 2 (November 1971), the direction of the program was entrusted to a key figure of the French rocketry, General Robert Aubinière,[34] appointed on the 1st January

[33] MAU is the aacronym of Million Accounting Unit, namely the monetary unit used in ESRO/ELDO and ESA and based on the gold standard system. At the time, the value of a unit corresponded approximately to that of US Dollar.

[34] A brief Gen. Aubinière's biography is available on http://www.esa.int/SPECIALS/About_

1972, Secretary General of the ELDO. His first act as Secretary was the setting up of a commission to investigate the causes of the Europa 2 launch failure. The report presented by the board of inquiry was a perfect diagnosis of the disease that plagued the ELDO. In fact the cause of the accident were detected in the organization's *modus operandi*. The accident was caused by an inertial system malfunction, which was in turn caused by lack of coordination in the phase of stages systems integration.

The failure of Europe 2 marked the beginning of the final act in the history of the ELDO. The only viable path was that of a total revolution of the architecture of cooperation, based on what has already been decided in the ESRO and proposed in the Causse Report. In July 1973 the two organizations were finally dissolved to pave the way to a single European Space Agency, whose convention was signed in 1975. With the creation of the ESA it will open a new era in space cooperation. The main innovation, compared to the previous one was the distinction between a mandatory programme, including the large part of the scientific programme inherited from ESRO, to which all member countries should contribute, and an optional one which included, inter alia, the development of a European launcher.

The definition of a strategy: the role of Commissioners Spinelli and Dahrendorf

In the sixties, both the development of space cooperation and those of European technological issues shared the same inactiveness of the European Community.

The only exception was represented by the brief existence of the working group *Politique de la Recherche Scientifique et Technique* (PREST), better known as "*Groupe Maréchal*", created in 1964 within the existing *Comité economique de politique à moyen terme*, with the task of explore the feasibility of a coordination of national research policies. The group's activity ended in May 1967, following the decision of some countries, including Italy, to withdraw their representatives in response to the second French veto to the UK access to the Community (Bussiére & Van Laer 2007, pp. 513–517).

The framework hitherto described explains the reasons for this absence, especially in the space field. In particular, while the ESRO, thanks to the cau-

ESA/SEMYT13XTVF_0.html.

tion of its pioneers, was able to almost avoid the conflict among national and organizations program[35], the more ambitious ELDO programme was unable to overcome the inner matter of the weak States commitment and that of priority recognized to the so–called "national champions".

Christopher Layton gave, in 1969, a sharp description of this situation in his aforementioned book:

> A gardener who decides to plant a tree, leaves it lying about unplanted for three years, and when at last it is in the ground digs it up each year to shake it, prune it and generally knock it about, should not be surprised to find that the tree ails and shows little sign of comparing in health, let alone size, with the mighty oaks which tower beside it. Certainly he has no right to declare indignantly that this kind of tree won't grow. Yet this is an exact analogy with the treatment European politicians have given to the frail plant of a common European space endeavour (Layton 1969, p. 162).

But could be this situation different? Probably not, if one takes into account the difficulties encountered by the European integration process in those years, similarly to what it was happening for the European cooperation in space.

In fact, since the blowing of the first ELDO crisis, 1966, it had become clear the difficulties to assure the needed state support to the launcher program and countering centrifugal forces caused by the emergence of – political, economic and industrial – national interests. For this reason, the definition, even in the space sector, of a Franco-German axis was, in many ways, providential. The launch of the Symphonie telecommunications satellite project was, in the Sixties, the most important initiative undertaken under this umbrella. In particular the Symphonie program originated from the combination of two national projects – the French SAROS (*Satellite d'Application de Radiocommunication en Orbite géostationnaire*) and the German *Olympia* – in order to achieve a common aim: ensuring their access into the emerging telecommunications satellite market monopolized by United States.

The successful history of *Symphonie* is strictly linked to the less blessed ELDO story. In fact the lack of an independent access to space, due to the difficulties encountered during the implementation of ELDO program,

[35] In 1969 the EURODATA consortium -established to take part to an ESRO tender for the provision of electronics equipments- failed because of German opposition, due to the exclusion of the Siemens industry from the group (Peterson & Sharp 1998, p. 39)

obliged France and Germany to accept the harsh conditions imposed by US government for using its launching system. According to the agreement signed in 1974, *Symphonie* would be launched as an experimental non-commercial purposes satellite, in this way giving up to compete directly with Intelsat (Nguyen 2001, pp. 17–24).

The first resolute attempt to change this situation was made by Altiero Spinelli, who was appointed in July 1970 Commissioner for Industrial, technological and scientific affairs. This initiative was based on Spinelli's feeling that a Commission action in the technological field could make possible to it to regain its driving role in the European integration process hardly reduced due to French opposition and as a consequence of the so-called Luxembourg compromise, signed in January 1966 in Luxembourg, at the end of the "empty chair" crisis.[36]

The choice of technology was linked to the interest shown by European public opinion on the technological gap debate, but also to solve as soon as possible the crisis of the Euratom.

The first question Spinelli had to deal with was that of the legitimization. Spinelli found the solution recalling paragraph 9 of the Hague Declaration, signed in December 1969, which contained the prevision of a better coordination of national research policies. In Spinelli's design Community research policy had to become the catalyst to the technological and economic development in Europe, thank to its action in highly strategic industrial fields as electronics and information technology.

The main resistance to the implementation of Spinelli's action, would have come from States in defence of national strategic industries (nuclear, electronics and aerospace). For this reason Spinelli structured the implementation of his program as the combination of three strategic action. Firstly, he proposed a new management structure for the future policy (mainly through the definition of the Commission DG III as the solely responsible for the research activities services); secondly, the solution of the crisis of the Euratom and the defi-

[36] From 30th June 1965 to 29th January 1966, in opposition to a slew of Commission proposals addressing, among other things, the financing of the Common Agricultural Policy, France boycotted the meetings of the Council and insisted on a political agreement concerning the role of the Commission and majority voting if it were to participate again. This episode in European history is known as the "empty chair crisis". This crisis was resolved thanks to the Luxembourg compromise (in January 1966), which states that "when vital interests of one or more countries are at stake members of the Council will endeavour to reach solutions that can be adopted by all while respecting their mutual interests.

nition of a new mandate to the Community Joint Research Centre and, finally, the creation of two bodies: the *Comité Européen de la Recerche et Développement* (CERD) e l'*Agence Européenne pour la Recerche et le Développement* (AERD).

In Spinelli's idea Commission had to become the centre of gravity of all European actions in R&D field. This was clear looking at the membership and functions attributed to the two above-mentioned bodies. The CERD was a representative body composed by all the stakeholders in research field (universities, research centres, National research policy administrator and industries). Its main task was to help to define the scope and purpose of community initiatives in research field.

The AERD, an organ very similar to today's executive agencies, would be handled under the supervision of the Commission, the funding programs of research, addressed to public and private, creating a network of information, and research programs Community itself.[37]

Spinelli's action took place in a couple of years and it was based on a passionate "diplomatic" activity that the Commissioner and his *Chef de Cabinet* - the already-mentioned Christopher Layton – organised towards member States governments and also the European Parliament.

Unfortunately, the implementation of Spinelli's plan was stopped by an unexpected event. In January 1973, as a consequence of the enlargement of the Community to include the United Kingdom, Denmark and Ireland, the number of Commissioners was increased and Spinelli lost his portfolio for science and education which was assigned to Ralf Dahrendorf.

The changing of the guards between the two Commissioners, even if it didn't completely modify the ultimate aim of the Community action in R&D sector, led to a marked change of strategy. In fact, Lord Dahrendorf abandoned the hypothesis of a common policy, opting for a purely intergovernmental perspective, aimed to create the conditions for the harmonization of national research policies, the free circulation of knowledge in Europe and the creation of a "Single European Area of science".[38]

The more prudent Dahrendorf's action led to positive results in a short time. On 14th January 1974, just a months after the resignation of Dahrendorf to become the Dean of the London School of Economics, the European

[37] Commissione delle Comunità Europee (1970).
[38] European Commission (1974).

Council approved four resolutions establishing the European research policy, through: the establishment of the intergovernmental *Comité de la recherche scientifique et technique* (CREST), in charge of the coordination of national scientific policies; Community participation to the European Science Foundation; the agreement on the launch of a upcoming Community action plan for science and technology of the Community and, ultimately, and finally the launch of the *Europe +30* study, aimed to formulate hypotheses about the possible future trajectories of the new established policy.

Conclusions

The above-described events have tried to show the difficulties and constraints within which technological cooperation initiatives in Europe in the sixties and seventies had to cope with.

A very comprehensive explanation of this complexity was provided some years ago by John Krige in an article devoted to this subject (Krige 1997, pp. 897–918). In this paper, Krige argues that the governments inclination to scientific cooperation characterized, like space activities, both by a considerable outflow of resources and by results that can be achieved only in the medium-to long-term, depends on the competitive advantage that they can get from this collaboration.

This means that the acceptance of a reduction in terms of national sovereignty doesn't imply the abandonment of national targets. In other words, States try to achieve the same goal, but by other means.

Observing the case of European space cooperation, the main problem probably turned out to be the choice of these means, for both internal and external reasons. As regards the former, the cooperation architecture itself was based since its beginning on a weak agreement which originated from a series of fortunate events: UK government's will to "save" the Blue Streak investments; for France, it was an integral part of General de Gaulle's plan for a national foreign policy revision; as to German, space cooperation was an opportunity to strengthen its European ties and also to "rehabilitate" its research apparatus for its role in the last world war.

With regard to external reasons, the cooperation architecture itself was unable to adapt to changes affecting the context of space activities occurred in the decade. In fact, in the Sixties space increased its political and economic importance, mainly as consequence of the emerging market of communications satellites. But unfortunately, this wasn't the aim of neither: the ESRO was a strictly scientific organization, while the ELDO aimed, at least initially, to the development of a launcher not suitable for putting in orbit communication satellites.

For this reason, the only feasible solution, since the 1966 crisis, was a total renewal of European space cooperation. The establishment of the ESA marked the start of a second chapter in the history of the European space activities which ended, at least in the opinion of the writer, in 2004 when, with the first EU Council "Space" (25th November 2004) a new one has started, where space issue started to appear in the EU political agenda.

It is perhaps too early to understand the content and scope of this new phase, and if space cooperation, through this new development, has permanently abandoned its part as stone guest. Many signs would confirm it. Firstly, its presence in Article 172a of the EU Lisbon Treaty.[39]

Anyway, whatever the outcome will be, a first result has already been reached. In fact, more than fifty years after his birth, European space cooperation is today, not only as the synthesis between the legitimate aspiration of scientists to broaden their knowledge and an instrument of States foreign policy, but as the subject as the object of a common policy, with its own aims and tools.

What it's still not so clear is who will be the main character of this policy: the European Union? The European Space Agency? The Member States? And with regard to the latter, which of them? Considered that the two organizations have a different membership?

Finding an answer to these questions is essential to allow a further development of the European cooperation in space. Failing this, the "space policy" could remain just a meaningless concept and its space cooperation will find itself to play the even more tragic Donna Elvira's role.

[39] 1. To promote scientific and technical progress, industrial competitiveness and the implementation of its policies, the Union shall draw up a European space policy. To this end, it may promote joint initiatives, support research and technological development and coordinate the efforts needed for the exploration and exploitation of space.

2. To contribute to attaining the objectives referred to in paragraph 1, the European Parliament and the Council, acting in accordance with the ordinary legislative procedure, shall establish the necessary measures, which may take the form of a European space programme, excluding any harmonisation of the laws and regulations of the Member States.

3. The Union shall establish any appropriate relations with the European Space Agency.

http://europa.eu/lisbon_treaty/full_text/index_en.htm.

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Commentary

Storia della tecnologia

Luisa Dolza
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Lorenza Sebesta
lorenza.sebesta@unibo.it

In his *Apologie de l'historie* (Bloch 1974), Marc Bloch defines history as «l'étude des hommes dans le temps» associated with a «connaissance par traces».[1] Luisa Dolza undertakes the daunting enterprise of pursuing the history of western technology through a book populated by «hommes dans le temps» (with special reference to inventors and their times) and «traces» of their actions in the form of machines, representations and texts.

It is an old-style text where a pleasant literary style merges with frequent and wonderful quotes from primary sources. Despite the constraints imposed by the book series *Universale Paperback*, which demands conciseness, the Author manages to refrain from oversimplification. She does so through a very personal historical reconstruction «from the margins» – somehow akin to the spirit of the second generation of *Annales*[2] historians rather than to the encompassing (and somehow determinist) synopses of economic historians such as David Landes.

In the history of technology, like all other histories, moving to the borders helps one to spot the inner contradictions of the dynamics ruling the core centre. In this case, moving to the borders mainly means retargeting the attention from machines and their successes to the ideas, rules and practices

[1] As far as *traces* are concerned, Bloch reformulates a concept proposed by François Simiand, who referred to the « raisonnement construit sur les traces connues de [...] faits, appelés documents ». See François Simiand, « Introduction aux études historiques » (1898), in *Revue de Métaphysique et de Morale*, 1898, pp. 633–641, now in François Simiand, *Méthode historique et sciences sociales*, Paris, Éditions des archives contemporaines, 1987, pp. 99–108. The quotation is at p. 4 of the electronic version: http://classiques.uqac.ca/classiques/simiand_francois/methode/methode_09/intro_etudes_historiques.rtfp.

[2] I refer notably to Ruggiero Romano and his superb theoretical framework, which organises the content of sixteen volumes of *Enciclopedia Einaudi*.

that supported their invention and production, as well as to discourses and representations that give them a meaning.

This story starts from ancient times, when inventions were kept within the bounds of nature, which was 'unveiled' and 'interpreted' by the inventor. This is why mechanical arts were seen as 'servile' in opposition to the aristocratic ones, rooted on contemplation. Pursuant this approach, the organisation of knowledge during the middle age assigned practical activities (*factibilia*) at the lowest stage of the pyramid of disciplines, whilst the moral ones (*agibilia*) stayed in the middle and speculative activities (*scibilia*) at the summit.

The corollaries of such approach were the centrality of the concept of «limit» – a self-imposed boundary for the inventor, whose fundamental wisdom should consist in recognizing «the limits imposed by the gods» (p. 20) – and the use of memory, not intended as an individual attribute, but rather as a catalogue of models.

Surprise and wonder played an essential role in this vision, exemplified by the Author through the device of *deus ex machina* (p. 22), which ended many theatrical representations for a long time after the V century B.C., when it was first introduced. The intervention in the scene of extravagant machines was not only bound to offer a way out to the most intractable human affairs, but was also meant to engender bewilderment and wonder.

Indeed, it is wonder – along with the imagination supporting and activating it – one of the major threads of the book. Revealing quotations from original sources are there to remind us the crucial role it played. Such is the case of *Epistola* by Roger Bacon – a multi-faceted figure of monk, mystic, astrologer, and grammarian of the XIII century – writing that «it is possible to build machines by which the greatest ships, with just one man at the helm, will be able to proceed faster than if laden with oarsmen; it is possible to build carts that will move at unbelievable speed without draught-animals; it is possible to build flying machines in which a man [...] will be able to flap the air like a bird [...] machines that will allow to dive to the bottom of sees and rivers» (pp. 63-64).

In the same vein, according to Vasari, Brunelleschi «began penetrating the matters of time and motion, of weight and wheels by fancy [...]» (p. 80). The role played by imagination and utopias has indeed kept its centrality in furthering the frontiers of knowledge up to the XX century. Not few of the founding fathers of austronautics, for example, approached space, first of all, as a place of possible human regeneration. This was the case of Konstantin

Tsiolkovskij (1857–1935), father of the theory of rocket propulsion, an enthusiast of «cosmism» (or cosmic mysticism) which flourished in Russia at the turn of the century.

Along with the threads which make up the weft of the book (such as wonder and imagination), its warp seems to hinge mainly on two conceptual triads: the first includes man, nature and god, while the second embraces the inventor, machines and the state.

The first triad is most visible in the initial part of the story, when the Author outlines the Greek vision of the first invention, viz. fire, which was «ascribed [...] to the sign of rebellion» of Prometheus and the whole mankind with respect to the divinity (p. 46). In the middle age this approach was taken over by the idea of knowledge as «gift from God» (p. 47), as masterfully expressed by en excerpt of canon law quoted in the text: *scientia donum dei est, unde vendi non potest* (p. 47). According to Hugh of St. Victor and Thomas Aquinas, nature should be contemplated and respected without changing it (p.50). To Augustine, mechanical arts, aimed at taking « possession of the nature» (p. 48), defied God and were hence abominable. One should highlight how this vision still seem to influence, *mutatis mutandis*, Bolivian natives' arguments against the claim to patent genetic modifications practiced on Andes' traditional plants.

This approach did not compromise the tens of crucial inventions that took place in a middle age, a far from still era, characterized by a dynamic ferment that moulded the countryside and the urban landscape, as well as it prodded people to travel and trade.

The water mill (with its many different applications geared on the power produced by water wheels) and heavy plough began to be adopted widespreadly during a long span of time from the IX to the XI century. They allowed «a rise in harvests higher than the demand connected to mere subsistence», so marking the beginning of a trading economy based on surplus, geared to the medieval town (p. 55). This process would put monasteries intended as production centres to the sidelines and would lead to the assertion of «new forms of richness through manufacturing and trade» (pp. 56–57).

Between the XIII and XIV century, technical knowledge began acquiring self-standing with respect to the machines it produced (p. 79), while guilds consolidated by setting the rules of this «intangible knowledge» (p. 79) and regulating access to work and many crucial aspects of the social life of their affiliates. The mechanic clock – according to Lewis Mumford – was a crucial device in order to impress on labour the order and predictability that will be the keys to make it more fruitful and people more 'productive'.

A turning point in the transformation of the way to perceive technical progress and its authors was represented by the law on privileges enacted in Venice in 1474, whose provisions allowed putting one's name to a certain invention. This law – the first protecting the inventor and his invention for a renewable period of time (whose duration as a rule was inversely proportional to the invention's importance) – subordinated the concession to the «not modest usefulness and benefit for our state» (p. 86). Not only did the inventor become entrepreneur, but the state began keeping a watch, regulating, celebrating and taking avail of his work.

Among the elements which would constitute the stuff European state were made of, the capacity to benefit from the useful discoveries of the time became a crucial one. In this context, war machines were due to play a crucial role. Not by chance the first printed illustrated technical book ever published (by Roberto Valturio) came out right in the territories of the *Serenissima* and dealt with military machines and techniques (*De re militari*). The *Serenissima* asserted herself as a model of state *ante litteram* and military might was one of her pillars.

«The man is at the centre, but his world is at war» (p. 100), writes the author describing Leonardo's man inscribed in a circle. This is all the more true for the state, the new protagonist of European history and most important user of what Leonardo – in the letter to his future patron contained in the Atlantic Code – defined as «different and numberless things for offence and defence» (p. 100). Once the states were born, violence became their language and the inventor the latter's scrupulous interpreter.

The privileges and, later on, English patents (1552) are «the vantage point to interpret the dynamics of innovation» (pp. 130–131) in the XVI and XVII centuries during which, little by little, a new vision of labour arose, no more intended as punishment by God, rather as progress towards knowledge and grace. No wonder that many of the discoveries of this period focused on lessening people's fatigue and increasing labour productivity.

In order to get the inner meaning of the very concept of patent, Dolza puts it into the context of the innovative discourse about propriety that marked the development of western legal and political thought since 1690, date of publication of the first edition of John Locke's *Second Treatise on Civil Government*.

Property right – to which Dolza ascribes patents (and the defence of inventors against third parties) – became part and parcel with a restricted number of 'natural' rights upon which natural law doctrine was built, as well as the modern idea of citizenship as a group of rights that the sovereign was asked to 'recognise' – and not to 'bestow'. An essential stage of the transformation of bestowal into right was the law on *découvertes* passed during the Revolution, on the 7th of January 1791. Establishing a link between inventor and invention, the patents transformed human talent in one of the foundations of individual rights.

At the same time, however, the right to property of discovery privatised knowledge, perverting its original meaning into something heavily «monetized». Interested learning substituted disinterested contemplation as the core of the relationship entertained by the inventor with nature. Utilitarian rationale turned nature – and knowledge along it – from *province of mankind* to subject of exploitation and speculation, even of a financial kind. Dolza hints as the «frantic activity» and following huge losses (harbinger of later frenzies...) incurred by those ingenuous English citizens who, at the beginning of the XVIII century, acquired shares in societies that bought patents of illusory value (p. 167).

From now on patents became a microcosm reflecting the contradictions of a world where *cash nexus* (in the words of Thomas Carlyle) based on monetary exchange came to substitute the traditional social bonding, or 'connections', of older times. Patents would cease to be considered a 'natural right' of the inventor on the product of his work, but rather as a remuneration and protection of an investment. If it is fair to say that the dynamics of capitalism cannot be understood properly without looking at the enlargement of markets, the changes in the interactions between capital and labour and at the new technologies incorporated in the productive processes, Dolza enriches this vision by looking at how technological progress has come to be intermingled with modernization.

The role of institutionalized power pops out again and again in the chapters devoted to the XVI–XVII century. Let's take, for example, Francis Bacon, who not only did advocate the foremost relevance of experiments and control instruments for the progress of mechanical arts, but also «foreshadow[ed] a state policy for sciences and arts» (p. 137).

In this context, the transformation of patents into monopolies (*Statute of Monopolies*, 1624) gives rise to some fundamental questions. Which is the aim of the state and which are the interests it serves? Which kind of economic development is targeted by modern capitalism? The one that rises or, at least, safeguards employment or the one leading, in the words of the puritan Samuel Hartlib, to «the enrichment of the few» (p. 138)?

If it is true – as the author recalls – that in the XVII century the universe began to be represented as a huge machine (p. 140), it is in the same century that the state was thought and rendered as a «machine of machines». Among the practices leading to the consolidation of modern states (i.e. control, measure, war and production of richness), it is by no way difficult to discern the centrality of XVII century inventions regarding land surveying, topography, latitude measuring and ballistics (p. 144).

This is how a new triad linking inventors, machines and the state emerged as protagonist of modern technological development. This linkage became explicit, for example, in the public policies adopted by Jean Baptiste Colbert, who in 1663 consolidated in the Academy of Sciences those groups of scientists already operating in this sense (p. 154).

The Author refers to at least two paths of possible analysis to be followed in order to seize the complexities of the changes introduced by this new link. On the one hand, one could look at how technological progress contributed to the consolidation and economic development of the European national states, searching for the evidences – which become clearer from the XVIII century onwards – of a «politicisation of techniques» (*politicizzazione della tecnica*) (p. 159). On the other hand, one could look at the ambiguities of the impact of this link on inventors, as an impingement in their freedom of research and, at the same time, as an opportunity of social and economic rise.

Along with the institutional consolidation of the relationship between the state and inventions, there begins a power struggle opposing science and technique – almost a class struggle between aristocrats (the scientists) and plebeians (the technicians) – that will mark the following development of history of technology. For a while the Royal Academy of Sciences in Turin – established in 1773 – seemed to succeed in reconciling the opponents with a motto (*veritas et utilitas*) accompanied by an insigna where «a young woman, refined and proud, representing the *veritas* offers her hand delicately but condescendingly to a prosperous country-girl laden with cornucopias, *utilitas*» (p. 171).The Academy's involvement in the controversial question related with dyeing – notably the one with indigo, the colour of the Kingdom's uniforms – would clearly show the limits of this supposed reconciliation.

The book proceeds in a *crescendo*, merging in one chapter the XIX and XX centuries. Two centuries of history of technology proceed at fast pace and the different plots are once again hinged on the figures of the inventors. Somewhat a national hero in a XIX century eager of founding myths, the inventor – now owner of a patent and beneficiary of royalties, in the prototype figure of James Watt – enters in a complex relationship with the users of his most famous but not only invention, i.e. the steam machine, a relationship that often degenerated into open struggle. The users were especially the mine owners, interested in draining deep pits – a problem that had arisen several centuries earlier but acquired centrality during the golden age of iron and steel industry when coal demand rose consistently. Sale conditions of the patent would prejudice their profits and, on the other hand, would hinder «improvements and innovations in the steam technology» (p. 184). We assist here to the birth of what will evolve during the XX century into what was defined by Jospeh Schumpeter as the crucial difference between invention (the creative spark) and innovation (its fruitful application to manufacturing) (p. 216).

The dramatic images of the 'modern' exploitation of mines in Britain during the XIX century offer another interpretative thread for this history of technology getting closer and closer to contemporary times. Marx's *Gewalt der Gesellschaft* (a mix of force and violence characterizing societies based on trade and profit) offered revolutionary insights into the effects of the industrial revolution and the techniques it embodied on the unfortunate class of peasants-turned-city-dwellers that represented its backbone. The real mega-machine was no more the state (a simple administration for profiteers' interests, according to Marx), with its territorial and legal boundaries, but rather the XIX century capitalism, with the global reach of its markets, the exploiting nature of its productive system and its absence of accountability.

An enlightening quotation from Benjamin introduces what has been seen from many as an age of violence *par excellence*, the XX century, in whose endeavours science and technology played a crucial role. The many useful inventions exploited for the benefit of humanity during this time (from penicillin to the telephone, from the airplane to birth control techniques), do scarcely seem to compensate the insanity of the projects technology has been most deeply associated with, from the Shoa to the launching of the atomic bomb on Hiroshima and Nagasaki .

The XX century is indeed the period in history when the contradictions of technological development emerge most blatantly. Its spreading out does not necessarily mean progress and greater accessibility does not always stand for greater democracy. The first potential victim of these trends is the state. Once privileged guardian of knowledge and techniques (and responsible for public choices related to them), the state is more and more giving ground to private initiative in the field, but has still to cope with their most perverse effects. The second potential victim is our planet, or *Spaceship Earth*, whose inherent fragility has been so convincingly demonstrated with the help of satellite technology (one should only look at the images of Latin American mega mines to appreciate the magnitude of the destructions they imply). The third victim is the Man, intended as both individual and community, the potential beneficiary of a progress more and more intolerant of limits. While the roads opened by biogenetic practices challenge his very «human» essence, patents connected to eatable and medicinal plants, according to many observers, make traditional communities liable of being denied autosustentability.

Confronted with the vastness of these challenges, the author wisely chooses to conclude in a low key, at the same time lauding the endurance of the principle of public protection for the invention and pondering over the decline of the 'romantic' figure of the inventor –no more partaker of the Great Chain of Being but, more prosaically, of the global value chains.

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Report

Final ESF Inventing Europe Conference & 4^{th} Tensions of Europe Conference

Sofia, 17-20 June 2010

Mauro Elli
ellimauro@hotmail.com

The conference in Sofia has been one of those huge happening where the organisers – thanks to the practice of parallel panels – coalesce in a few days topics and initiatives of different nature. If this practice is useful in order to contain in a tolerable amount of time a number of contributions that would otherwise require at least some ten days, it compels the participant to choose among the panels. With such a provision it is clear the reason why I shall limit my considerations to some personal impressions on the strong and soft points of the conference.

Actually the conference included the final proceedings of different research networks: *Inventing Europe: Technology and the Making of Europe, 1985 to the Present*; EUWOL (*European Ways of Life in the American Century*); EUROCRIT (*Europe Goes Critical: The Emergence and Governance of Critical Transnational European Infrastructure*); SOFT-EU (*Software for Europe*); WRR (*The Development of European Waterways, Road and Rail Infrastructure, 1825–2005*); EUROCOMMONS (*Inventing and Governing Transnational Commons in Europe*); the conference as such on the role of technology in East-West relation during the Cold War.

Such an abundance of topics seemed to me rather excessive: if a restaurant displayed a 36-page menu, the clients would quite likely be embarrassed at the choice. In fact, it was most of all an occasion for coexistence of research groups already structured and connected by relations not merely of institutional kind. This configuration tended to strengthen the common leaning in people to take part in those activities with which they had already an 'organic' link – with those exceptions motivated by personal curiosity.

In spite of the exuberance in topics, it is possible to draw some general considerations on the conference with special regard to the use of interpretative categories. First of all, if it was far from being a novelty for Cold

War historiography that the Iron Curtain was somewhat a porous septum rather than a watertight partition, it was nonetheless interesting to deal with the issue from a standpoint and with topics unusual for a scholar in history of international relations. Indeed, the methodological choice of considering technology as a political and social process (i.e. not merely as production of artefacts, but both as formation and circulation of knowledge and as assertion of research and manufacturing practices) allows a wider application of the concept and encourages the scholar 'to translate' technical arguments into political data.

This effort put a premium basically on two kinds of approach. The first – the one of transnational history – emphasized the couple "alternative processes/parallel histories" in order to stress how the circulation of practices and knowledge, the assertion of organization models etc. point to a 'net' beyond the reach of international history (narrowly intended).

The other approach focussed on Europe intended as a 'laboratory', where one can consider how the diffusion of technology nurtures integrative dynamics beyond the political and economic purviews that are usual subjects of European studies. From this standpoint – a Europe made of infrastructural networks and transfer of know-how – new interesting data emerged regarding the Eastern Bloc, which appeared less sclerotic that one normally might suppose.

On the East/West dynamics in stricter sense, it was pointed out the urgency of studies that integrate the political, commercial, and technological purviews like in the case of COCOM activities, which can not only be used in order to analyse the lows and ebbs in the relations between the two blocs, but also their internal dynamics and the interrelations among countries pertaining to antagonist camps during détente. On the one hand one assists to Western European countries' wish to find new commercial outlets in front of American competition, devising new applications for technologies and processes that were becoming obsolete in advanced industrial economies; on the other hand, Eastern European countries seemed determined to acquire know-how not then available to them, especially – but not only – in the purview of consumer goods.

In my opinion the main flaw was out from the cultural formation of the participants to the conference. Indeed, often they took a degree in some of the 'hard sciences' and subsequently developed an interest for the history of their own discipline (i.e. history of physics, the most notable but not the only case).

In some other cases people came from social sciences or economics. Just a few among them had really a historical background. Sometimes the result was spine-chilling: I couldn't but feel a chill down to my back hearing of supposedly 'federalist' European aspirations on the part of de Gaulle's France in the attempt of giving the audience an overview of the European context between 1950s and 1960s. One must feel depressed seeing a rapporteur on the circulation of knowledge in the Hapsburg Empire being abruptly questioned why he was not using clear-cut East/West categories, as if the Iron Curtain had been an invariable feature of European history... Apparently smart and cultivated scholars had never had a chance – due to their specific formation – to read a companion of contemporary history.

On the other hand, just this limit seems to point to an effective collaboration between scholars in the history of technology in broader sense and historians of international relations. Since several years we have been assisting to the widening of the subjects of the latter discipline, which is no more limited to the legal and diplomatic purview. Still, if resorting to the help of economic and business history in analysing the political meaning of economic activity in international life does not apparently cause any inconvenience, not so self-evident does the necessity of technical knowledge seem in order to make the same operation in the purview of technology – though the latter does permeate the contemporary world.

I do not believe that the problem can be shortcut. Jumping directly –while addressing primary sources – to the résumé by some official for his/her minister or a government committee would be a mistake: first because it would blur the distinction between historical fact and interpretation (though the latter was originally aimed at being most objective); second, because one might be induced to forget that technical actors are by no means neutral, rather they articulate a 'political' discourse using different keys.

Thereupon international relations historians with a wish to pursue such subjects will be compelled to examine documents often redundant, never very enticing as narrative and whose meaning can be obscure – by necessity, one would say, in that they often require competences beyond the realm of humanities. This does not mean that either the historian should also become a physicist, a chemist or an I.T. expert, or the other way round; rather it would be useful a wider collaboration between the respective disciplines with many more occasions to meet and exchange knowledge.

Unfortunately, the idea of interdiciplinarity has been sacrificed to the altar of what is politically correct, so that mere juxtapositions – maybe out of chance, if not motivated by a sharing-out of available places – are smuggled as interdisciplinary occasions. Interdisciplinarity – intended as a 'new look' that is more effective and produces a deeper understanding compared to single disciplines – does not occur everywhere and every time. It seems to me that the conference in Sofia clearly showed the potential of the abovementioned themes (one just recall the contribution by the studies on infrastructural networks to the history of the European construction), so as the timeliness to coordinate at the highest possible degree the activities of the different subjects active in research

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History, Science and Technology

Edited by Matteo Gerlini

Recently, the history of science has extended its paradigms to cultural, social, economic and political elements, including their interaction with the development of science or scientific revolutions.

Prominent international academies and scientific institutions have now included in their activities studies and education in culture, science and society, as they already had done with studies in economics and technology. In this change of paradigm, recent work makes a particular focus on one aspect: specifically, the international dimension of science as a whole. This means, for example, scientific institutions, scientific communities, scientific experiments, and, moreover, the policy of fostering scientific research. Frequently, these researches in the history of science have overlapped with analogous researches in the history of technology. However, only infrequently do such researches coincide with those of international history.

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