Title

Studies of science before "Science Studies" : Cold War and the politics of science in the U.S., U.K., and U.S.S.R., 1950s-1970s

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Studies of Science Before "Science Studies": Cold War and the Politics of Science in the U.S., U.K., and U.S.S.R., 1950s-1970s.

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by

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ABSTRACT OF THE DISSERTATION

Studies of Science Before "Science Studies": Cold War and the Politics of Science in the U.S., U.K., and U.S.S.R., 1950s-1970s.

by

Elena Aronova

Doctor of Philosophy in History (Science Studies)

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Professor Naomi Oreskes, Co-Chair
Professor Cathy Gere, Co-Chair

This dissertation investigates the history of Science Studies (or Science and Technology Studies, STS) as it became a distinct area of expertise and academic inquiry during the Cold War. The dissertation pursues five distinct histories, each focused on a confined mode of analysis of science that articulated, evaluated, and rationalized Cold War sensibilities and concerns. The case studies in question are: (1) UNESCO and the framework of "scientific humanism" promoted by its two visionary founders, Julian Huxley and Joseph Needham, and implemented in UNESCO's major history of science project, History of Mankind, in the 1950s

and 1960s; (2) the Congress for Cultural Freedom and its quest, in the 1960s and 1970s, to promote "science studies" as part of its broader agenda to offer a renewed, "post-Marxist," framework for liberalism, (3) the Salk Institute for Biological Studies, which in the first ten years of its existence, 1962-1972, undertook the bold initiative of launching a sustained inquiry into social studies of modern biology; (4) the short-lived "philosophical phase" in medical ethics, marked by medical ethicists' interest in and appropriations from post-positivist philosophy of science, which I explore by analyzing the series of workshops organized under the auspices of the Hastings Center in the late 1970s and early 1980s; and (5) a particular mode of reflection on science and its intellectual foundations developed by Soviet philosophers in the 1960s – 1970s under the name of "naukovedenie."

All these modes of analyses of science represent roads not taken. The "vision" of science studies all these groups were promoting is different from science studies as we know it today. Yet these alternative visions, in which the issues of science politics were inseparable from those of science policy, science organization, and science governance, constitute an important "pre-history" of Science Studies. I argue that the promotion of the studies of science as a politically relevant area of expertise, undertaken within existing powerful institutional structures outside academia, helped to legitimize the disciplinary identity of science studies in the age of the Cold War.

Chapter 1

Introduction: The Cold War and its Legacy(ies)

The past decades have seen an explosion of works interpreting and reinterpreting the Cold War, explaining it as a cultural phenomenon as well as the political/diplomatic and military phenomenon. These works have begun to disentangle the broader contours of the Cold War processes and their complicated, contested legacy (or, rather, legacies), raising questions and provoking debates. These works illuminate the complex interactions within American and Soviet "empires" as well as smaller powers. They add to our understanding of such key Cold War events as the Marshall Plan, the Korean and Vietnam Wars, the Cuban missile crisis, the establishment of the CIA and the spread of McCarthyism in the U.S., the civil rights and countercultural movements culminating in the events of 1968 in the West, and the reform movements in the countries of Soviet bloc culminating in the Prague Spring of 1968, the détente and the changing purposes of the Cold War in the late 1960s and through the 1970s and 1980s (see Leffler and Westad, 2010). These works also reassess the role of ideology, and focus attention on the importance of culture in our understanding of the Cold War and its legacy (Engerman 2010a). The last several years have seen an explosion of books on virtually every aspect of culture (focusing mostly on America and Western Europe) and the ways that culture shaped and was in turned shaped by the Cold War, marking what was called a "cultural turn in Cold War studies" (see the review in Griffith 2001). Overall, even though the term "Cold War" still highlights mostly the military and political framework that motivated much Cold War historiography, there is increasing recognition that "the Cold War existed outside of military conflict, outside of diplomatic standoffs, and outside of superpower summits" (Gaddis 2005).[1]

This recognition of the larger contours of the Cold War is especially justified when it comes to the history of science in this period. During the Cold War, the pursuit of knowledge became a matter of states' concern as the instrument for ensuring national security, both with regard to the natural and to the social and the human sciences, on both sides of the Iron Curtain. At no time in human history had such abundant resources been poured into the development of scientific research in virtually every area of science and technology. Although these resources were largely devoted to the pursuit of military interests, the contours of the Cold War processes extended beyond defense contracts and overt Cold War operations, leaving a more subtle legacy than the one that could be easily "liquidated." Almost a decade after Boris Yeltsin took charge of the Russian Federation following the self-destruction of the Soviet Union in December 1991, large segments of the scientific and technical communities in the United States are still debating how to redefine programs and institutions created during the Cold War whose missions and purposes are no longer clear. U.S. congressional committees have been debating

[1] See also Hanhimaki and Westad (2003), Engerman (2004a).

whether to close national laboratories and reorient the federal science sector. The universities, which, in David Hounshell's words, "benefited enormously from the 'social contract' that emerged early in the Cold War," are still "scrambling to secure their viability" while "bracing for severe contractions in research funding" after the Cold War was over (Hounshell 1997, p. 238).

Cold War history yields many ironies. The RAND Corporation - a quintessential Cold War institution principally funded by the U.S. Air Force while being (or claiming to be) a free-standing, private and independent organization backed by an interest-free loan from the Ford Foundation - developed, as David Hounshell argued, "a culture that not only prized independence but valued openness in terms of publishing its research findings" and valued intellectual curiosity (Hounshell 1997, p. 242, 265).[2] RAND provided a model for what came to be known as a "think tank," and was emulated in many policy research organizations during the Cold War and beyond as a "method of organizing and financing research, development, and technical evaluation that would be done at the behest of government agencies, but carried out by privately run nonprofit research centers" (Smith 1991, p. xiv). Think tanks are now found all over the world. There are environmental think tanks, civil rights think tanks, feminist think tanks. Ideologically, they range from left, to right, to center. As a Cold War legacy *de facto*, they now play an important role in defining issues and ideas for both governments and for the public.[3]

The Cold War spawned a great variety of scientific activities and institutions, and inspired the creation of new ones, having impacted the entire

[2]On RAND's "culture" see also Ghamari-Tabrizi (2000).

[3]See interview with Christopher C. DeMuth, from 1986 to 2008 the president of a conservative think tank, the American Enterprise Institute, given in 2005 (DeMuth 2005). On the right-wing origin of "think tanks" in Cold War America and the discussion of their political agenda see Oreskes and Conway (2010), Hollinger (2000), and Dawson and Schueller (2009).

domains of the social sciences and many areas of the natural sciences.[4] In recent years the history of social and human sciences in the Cold War has become a burgeoning research unit in its own, with a proliferation of conferences and a flood of publications (see the reviews of recent literature in Isaac 2007, Engerman 2010b). These studies show that the Cold War context made every world problem a political and military problem, and that the military (especially in the U.S.) proved to be one of the most significant patrons, either directly or indirectly, for Cold War social science.

The impact of Cold War politics on social sciences in the aftermath of the WWII was so prominent that the formulation "Cold War social sciences" came to be used almost exclusively in a pejorative sense, taking it for granted that the service of social scientists to the cause of the state during the Cold War inherently skewed their scholarship, compromising their - otherwise supposedly objective, "true," politically neutral - results. This view was epitomized in the New Left radical critique of the relation between power and knowledge, and inspired many leftist critiques of social-scientific complicity in the Cold War written in the wake of the political unrest of the 1960s and 1970s.[5]

A classic expression of this view with regard to the history of Cold War physics is Paul Forman's impassioned article of 1987, "Behind quantum electronics: National security as basis for physical research in the United States, 1940-1960," which argues that massive funding of physics and electronics research by the U.S.

[4] See on the Cold War scientific institutions and universities: Hounshell (1997), Collins (2002), Heilbron and Seidel (1989), Gusterson (1996), Leslie (1993), Lowen (1997), Simpson (1998). On Cold War science see 2001 Special Issue of *Social Studies of Science* on "Science in the Cold War" summarized in Solovey (2001a) and Hounshell (2001). See also Doel and Harper (2006), Galison and Hevly (1992), and the reviews by Engerman (2003) and Mody (2008). Most recent works on Cold War science and its lasting legacies include Oreskes and Conway (2010), Krige (2008), Moore (2008), Vettel (2008), Turner (2008).

[5] To these critics, as Engerman had pointed out, "the anti-Communist crusade of the late 1940s and early 1950s was an era of political interference that vitiated the sanctity and security of the ivory tower" (Engerman 2003, p. 81).

federal government motivated by national security purposes not only influenced the institutions that pursued this research, but also explicitly altered the content and nature of quantum electronics research (Forman 1987). What came to be known as the "Forman thesis" epitomizes what historian David Hounshell called the "distortionist" critique of the Cold War science policy (Hounshell 1997). This view implies that the Cold War agenda that provided lavish resources for certain strategically important areas of expertise led to the deviation, or distortion, of scientific research from its otherwise different, or "natural," path. In complicating the "distortionist" critique of scientific complicity in the Cold War, many scholars demonstrated in different case studies that research for national security generously supplemented pre-existing trajectories, and that some components of what used to be perceived as the result of the Cold War had emerged well in advance, in similar studies initiated during World War II, often for reasons unrelated to the U.S.- Soviet conflict.[6] These works provide a good illustration of Science Studies' long-standing orientation towards the notion of "co-production" of science and the social order, rejecting the simplistic narrative of pure scholars corrupted by national-security imperatives.[7] The interpretation that emerges from these studies attributes more agency to the scientists themselves showing how they were able to use the opportunities of the Cold War with no less ingenuity and creativity than they exhibited in their scientific activities, turning the purposes of the Cold War into an entire array of scientific activities.[8] As Rebecca Lowen indicated, Cold

[6]See, for example, Lowen (1997) and discussion in Engerman (2003).

[7]See Jasanoff (2004).

[8]For example, Angela Creager in her paper presented at the workshop "How the Cold War Transformed Science" (May 2010, Pasadena, CA) had shown that the use of radioisotopes, which long predated the Cold War and the development of nuclear reactors, was greatly intensified during the Cold War when the commitment to produce radioisotopes and to promote their utilization became the most visible civilian "dividend" of the military development of the atomic energy (forthcoming in *Nation and Knowledge: Science and Technology in the Global Cold War*, eds. Naomi Oreskes and John Krige). Naomi Oreskes advanced a somewhat different argument, showing that in oceanography the techniques, knowledge, and technology developed during the

War science and Cold War Universities were both creatures of the Cold War state and energetic promoters of it — for their own benefit (Lowen 1997).

As with the "distortionist" critique of Cold War physics, the historiography of Cold War social sciences had been initially shaped by the New Left critique and accounts dominated by revelations about the CIA funding and the ties of national security to various areas in the postwar social sciences in America, which, in the words of Engerman, "have discovered (to use the title of one such critical account) 'who paid the piper,' but devote[d] little if any space to what the piper actually bought" (Engerman 2010b, p. 398). In the last decade, however, more and more scholars have began to reassess a rich vein of documentation in American and European archives, following "the sponsor's money not just into scholars' bank accounts but into their publications" (ibid). The emerging "new history" of the Cold War social sciences is problematizing and complicating the New Left interpretation of social scientists as servants of the military-industrial complex. These works are beginning to challenge both the simplistic reading of the real or supposed constrains imposed on social scientists in the service of the Cold War, as well as the totalizing view of the Cold War and its effects. As Engerman argues, although national security concerns were of primary relevance for the social sciences during the Cold War, their impact on intellectual life was varied and often unexpected, much more subtle and complex than "a deduction from the source of funding might suggest" (Engerman 2010b, p. 399).

The field of Russian or Soviet Studies gives a perfect example of the contingencies and ambiguities, as well as the ironies of the Cold War.

Cold War in pursuit of military power were turned toward peaceful purposes after the Cold War ended, encountering, however, a resentment of the public, distrustful of the Cold War legacy in physical oceanography firmly associated with its main patron, the U.S. Navy, which for decades prevented public scrutiny (forthcoming in *Nation and Knowledge: Science and Technology in the Global Cold War*, eds. Naomi Oreskes and John Krige).

Russian/Soviet Studies in the U.S. can be arguably identified as a "creature of Cold War": the field transformed from the one represented prior to WWII by just a handful of isolated scholars and enthusiasts devoted to the study of "all things Russian," to a booming area with more than two dozen universities establishing Soviet/East European area centers, thousands of Slavic or Soviet-oriented scholars organized into professional associations and publishing in numerous specialized journals, in an era of heightened American-Soviet tensions that made this scholarly expertise a matter of the state's concern (see Engerman 2004b, 2009). Yet, although critics placed this paradigmatic "Cold War enterprise" at the center of Cold War conformism, as historian David Engerman demonstrated in his fascinating recent study of the history of Sovietology in the U.S., its practitioners, the Cold War "sovietologists," more than often directly commissioned and sponsored by the national security organizations, left a legacy that often directly challenged their sponsors' claims about the aggressiveness and totalitarianism of the Soviet regime, bringing an array of different political views to their topic and producing reports and collecting empirical evidence demonstrating that the Soviet Union was a stable industrial society not so different from the United States (Engerman 2009).

Russian/Soviet Studies was just but one example of a field that received a major boost during the Cold War. There were many others, linguistics, psychology, and philosophy, as well as new different cross-disciplinary "clusters." Area and International Studies, Communication Studies and cognitive sciences were among those many "studies" and disciplinary "clusters" that became institutionalized during this period. Histories of these disciplines are now receiving ample attention from historians who are elucidating the ways in which the aims, contents, practices, and technologies of the Cold War affected leading social sciences during this period, and how these sciences contributed to the tense politics of the period.[9]

The image that emerges is that Cold War social science is a diverse and varied phenomenon defying generalization. At the same time, as many historians of Cold War social sciences state explicitly, or show implicitly, "it was all connected," as Noam Chomsky put it in his characterization of the intellectual ferment among MIT and Harvard-based social scientists in the aftermath of WWII (quoted in Janet Martin-Nielsen's essay on linguistics in the early cold war (Martin-Nielsen 2010)). These studies have started to challenge our perception of the Cold War, and the ways natural sciences, social sciences, and various cross-disciplinary "clusters" reflected but also contributed to the tense politics of that era.

Within the burgeoning area of research on the history of natural and social sciences in the Cold War, Science Studies (or STS, Science and Technology Studies), however, is virtually untouched territory with regard to historical accounts of how this field and its research community were affected by the Cold War politics. Historians of science and science studies scholars greatly contributed to our understanding of the ways in which the social, political, and conceptual developments of science and technology in the Cold War were shaped by the symbiotic relationships of science with the state and politics. As a professional community, science studies scholars themselves are, of course, no exception to the symbiotic relationship between professionals and the state. However, despite the vigorous work on the history of *other* disciplines, the history of STS itself is written mostly in the genre of the history of ideas or intellectual history, presenting the

[9]In his review of scholarship on history of the human sciences in the Cold War period Joel Isaac called the history of Cold War human sciences a "genuinely interdisciplinary research field" in its own, which has been booming since the 1990s (Isaac 2007). On the history of social and human sciences in the Cold War see, for example, Cohen-Cole (2009), Gerovitch (1996, 1998), Rohde (2010), Solovey (2001b, 2004), Martin-Nielsen (2010), Solovey and Cravens (2012), Lemov (2005).

development of that field almost exclusively as an intellectual, and decidedly not a political, project.

1.1 Cold War and the Politics of Science Studies: Some Remarks on the Existing Historiography

As philosopher of science Shane Glackin put it, reviewing John Zammito's recent historical account of theoretical developments in STS, "the study of science *studies* is no less fascinating or illuminating than the study of science *proper*" (Glackin 2006). Indeed, reflection on the intellectual origins and conceptual foundations of science studies was part of that discipline building, with the leading STS scholars engaged in discussions of the conceptual core of their field since its inauguration in the 1970s. In the last decade, several book-length accounts of the history of STS have made their appearance (Fuller 1993, 2000; Golinski 1998; Hacking 1999; Zammito 2004). Framed as a response to the "Science Wars" and the criticism of social constructivism - "the most interesting - and also the most troubling - theoretical development" in STS, in the words of a historian of philosophy Michael Friedman (Friedman 1998) - these works are focused exclusively on the theoretical and conceptual developments of the field. Repelling the attacks of the "science warriors," STS scholars presented a variety of convincing arguments showing that in the long run science studies appears as a harmonizing project, enabling us to see how science achieves its "universality" and what gives science its apparent stability and "self-vindicating totality" (Hacking 1992; see also Hacking 1996, Zammito 2004).

Historical accounts of STS usually highlight Kuhn's seminal book, *The Structure of Scientific Revolutions* (1962), as challenging the standard account of scientific development and greatly stimulating studies of science in the following decades. Indeed, Kuhn's account of scientific revolutions has been shown to have had a profound influence on science studies, fomenting a flood of Kuhn-inspired works that interpreted and reinterpreted Kuhn's "paradigms," blending Kuhn with Wittgenstein's philosophy (the Edinburgh school of social constructivism), with the lessons learned from anthropologists and ethnographers (Bruno Latour and laboratory studies), or reading Kuhn through the lens of Mertonian sociology of science (social networks studies) (see the book-length historical accounts of science studies: Fuller 2000; Golinski 1998; Hacking 1999; Pinch 1997; Zammito 2004).

As Kuhn's work is given prominent place in the large majority of publications in the history of STS, his figure in the intellectual milieu of STS is often greatly magnified whereas less visible figures and "events" recede into the background, including Kuhn's own less popular works and the "events" preceding *Structure*. Overall, the development of Science Studies as a field emerges as a somewhat "pure intellectual" crossover that melded ideas and approaches from different intellectual domains, such as cultural anthropology, critical theory, feminist theory, postcolonial studies, literary criticism and postmodernist theory, being part of what Marc Solovey called, after historian Peter Novick, "the epistemological revolution that began in the 1960s" (Solovey 2001b, p. 172). At the center of this revolution, according to Solovey, was "a multi-faceted scholarly challenge to the dominant post-WWII model of social science inquiry based upon an idealized positivist and empiricist image of the natural sciences - an image that posited an objective, value-neutral scholarly enterprise whose intellectual practices and products were well insulated from 'extra-scientific' or 'external' social influences" (ibid). Thomas Kuhn's work on scientific revolutions was among those in the history and philosophy of science that helped to undermine this commitment to look to the natural sciences for guidance: literary theorists, ethnographers and anthropologists, among many others, drew attention to problems requiring tools of analysis that the natural sciences could not provide.

This story is not wrong, but in describing events as if they fell into place neatly and without much conflict, as a somewhat straight path toward a conceptually and intellectually coherent body of approaches and methods, it yields a simplistic treatment of people, ideas and circumstances that led to the acceptance of the views that constituted "the epistemological revolution of the 1960s," obscuring many twists and turns that were taken along the way to eventual agreement. Although recent works on the history of STS have astutely demonstrated the intellectual roots of STS, their explicit focus on the theoretical and conceptual developments of the field presented the history of STS as a "pure" intellectual project, obscuring the political dimension of its history. Novick put it explicitly, arguing in 1988 that "although the highly charged political atmosphere of the period sometimes raised the stakes of controversies about objectivity in the social sciences, it was for the most part 'strictly academic' considerations which initiated debates, and contributed the categories in which heterodox views were advanced" (cit. in Solovey 2001b, p. 172).

Although not many scholars would endorse such a strong formulation now, the emphasis on the intellectual at the expense of the political aspect is still in place. Even though the importance of political thought and political philosophy for the STS project is thoroughly acknowledged (see for example, Hollinger 1995; Thorpe 2008; Zammito 2004), the current historiography of STS barely acknowledges the role of the radical science movement, New Left, scientists' movements, and organizations where scientists discussed the social, political, and ethical implication of their research - such as the Pugwash conferences, Science For The People, or The Union of Concerned Scientists – mentioning them only in passing (although historians working in a Marxist tradition have gone some way in this direction, see Martin 1993, Werskey 2007, Allen 2001).

This brief review should not suggest, however, that the issue of "the politics of science studies" was not on the table, but rather that the topic has moved through several phases. The discussion of the "politics of STS" goes back to the 1970s when the field was inaugurated, most notably with the Edinburgh School's Sociology of Scientific Knowledge (SSK) and its Strong Program (Bloor 1976). The methodological principles outlined in the Strong Program, especially the symmetry principle, promoted the methodological necessity of neutral social analysis, that is to say, a prescription not to take sides, to prevent any political interests. This "prescription to be non-prescriptive" and this stance of political neutrality elevated to an explicit methodological commitment almost immediately became a focus of discussion within STS. With different levels of intensity the debate on "whether the SSK/STS can be, should be, or inescapably is, "political" was within STS since its inception in the early 1970s (see Richards and Ashmore 1996). In the 1990s, with the political appropriations of the notions of "instability" or "uncertainty of scientific knowledge" by global warming deniers and other self-styled "skeptics," this debate about the "Politics of STS" involved almost all leading scholars in the field.

The issue of the "politics of STS" became particularly troubling when STS/SSK was under attack during the so-called "Science Wars." Thus, in May 1996 the leading STS journal, *Social Studies of Science*, devoted the entire issue to the "Politics of SSK" (see Singleton 1996, Richards 1996, Martin 1996, Collins 1996). The topic generated a vivid discussion, with numerous replies and responses to the replies, framed by the question "whether STS accounts should stick to descriptive (or explanatory) analyses of the politics of science and technology, or whether they should also engage in normative criticisms, assessments or recommendations?" - in other words, asking a methodological question, with the discussion articulated in term of the *should* discourse (Radder 1998).

The general trend within the field since then was to respond "yes" to the latter, breaking the self-imposed separation of "intellectual" and "political" sides of STS, which philosopher Steve Fuller aptly compared to the separation between the two "Churches," to capture two rather different ways of conceptualizing the trajectory of science studies: "High Church" in STS – focused more on the interpretation of science and technology and on developing sophisticated conceptual tools to analyze science and its artifacts; "Low Church" in STS – less concerned with understanding of the complexities of science and more with the social and political implications of science and technology, policy issues, and public understanding of science (Fuller, 1997). Questioning Fuller's suggestive analogy, however, Sergio Sismondo, in his historiographical review of the field of STS, points out that "these two projects have been better linked than Fuller's two Churches analogy might suggest" (Sismondo 2008). He refers in this connection to the achievements of "Science, Technology and Society," a group of STS scholars united by "the combination of progressive goals and orientation to science and technology as social institutions," and whose scholarship is an unquestionable part of the discipline of science studies standing apart from the constructivist project (Sismondo 2008). Overall, the field is now witnessing a revival of interest in science policy studies, and generally in the issues associated with what Steve Fuller called "the Low Church" of science studies (see Elzinga 1995).

It's probably not surprising that in parallel with "rehabilitation" of the "normative" approaches within STS there is growing interest among the science studies scholars to look anew at the historical origins of our field. In the last years, more and more historians of science started to turn to our own community seeking to understand our role, then and now, in current affairs. The discussion of the politics of STS is turning away from the "should" discourse, and is now focused on bringing the issue of "the politics of STS" into a historical perspective, asking not only what STS scholars *should do*, but also what they *do*, or, rather, did, and why.

In the last years there have been several groundbreaking works that placed the post-war developments in history and philosophy of science, and science studies at large, into a historical perspective. Though entering the debate on Cold War social sciences rather late, these works begin to disentangle the ways in which historians' own analytical tools and categories were informed, shaped and conditioned, in varied and often unexpected ways, by the particular concerns as well as the general climate of the Cold War. Among these studies are those by David Hollinger, George Reisch, Steve Fuller and Mary Jo Nye, who examined the ways in which the scientific and political philosophies of Thomas Kuhn, Michael Polanyi, and Karl Popper were shaped by and embedded in the political ideology and political culture of Cold War America (Hollinger 1995; Reisch 2005; Fuller 2000; Nye 2011). For example, George Reisch in his study of the development of the philosophy of science in the United States during the Cold War has demonstrated that a convergence of intellectual, cultural, and political forces in the 1950s turned logical empiricism, originally a philosophical program with decided political ambitions, into a strictly intellectual, apolitical project (Reisch, 2005). In his more recent work, Reisch shows how Thomas Kuhn's account of science fitted the demands of the time in successfully promoting a distinctive understanding of scientific theories as incommensurable and mind-controlling paradigms, mirroring the binary geopolitics of the Cold War era permeated by its ideology of two antithetical and incompatible political systems.[10]

While literature on the history of Science Studies as it became a distinct area of sustained academic inquiry is largely absent, there is a small but growing historiography of the formation of the history of science as a discipline, going back to Robert Merton's account of the discipline-building of history of science in America (Thackray and Merton 1972). Some of these works addressed the Cold War contexts in which the history of science was established as a profession, focusing on the role of the Marxist tradition in the history of science and its marginalization during the Cold War (Mayer 2000, 2004; Jacobsen 2008). The pioneering work of Anna Mayer on the formation of history of science as a discipline in Britain demonstrated the general turn to internalist history within the academic history of science profession with the advent of the Cold War (Mayer 2003),corresponding to a similar trend in the U.S. during the Cold War (see Dennis 2003).

All these works are beginning to assess the Cold War politics and contexts of science studies as a field largely formed during the Cold War. This dissertation seeks to contribute to this scholarship by exploring further the links between the "High Church" and "Low Church" sides of STS while treating the 'intellectual' and 'political' "Churches" of STS as parts of a single whole rather than as two separate projects. Without denying the importance of the conceptual developments that led to the "epistemological revolution of the 1960s" I seek to highlight and to

[10] Reisch's conference presentations at the workshop *How The Cold War Transformed Science*, California Institute of Technology, Pasadena, CA, 7-8 May 2010, and at the workshop *Politics and Contexts of Science Studies in the Cold War and Beyond*, Alfried Krupp Wissenschaftskolleg Greifswald, Germany, 22-24 March 2012.

disentangle some of the Cold War contexts of the history of Science Studies as a political, not merely intellectual, project.

Science Studies was among many other “studies” - or trans-disciplinary “clusters” – that were promoted and institutionalized during the Cold War. Under different names and different circumstances, the studies of science as an academic field and a distinct area of professional expertise in its own right became institutionalized in the Cold War years in a variety of politically disparate states on both sides of the Iron Curtain - thus the emergence of “science studies” in the U.S. and U.K., “naukovedenie” in the Soviet Union, “naukoznawstwo” in Poland, “naukoznanie” in Bulgaria, “natural dialectics” in China, and so on. How were these origins shaped by the political economy, cultural anxieties, and ideological dimensions of the post-WWII social and political order? How and in what ways did the studies of science meet the political challenges of the Cold War as manifested in different political systems, on both sides of the “Iron Curtain”? How was the conceptual core of the discipline affected by the political economy of the Cold War? How did Science Studies, as a community and an intellectual project, participate in defining the relationships between science, society, and the state? And what can all this reveal about the intellectual formation of the field that was obscured by the historical accounts focused mainly on the “theoretical” and politically neutral conceptual core of STS (the “High Church” STS, to use Steve Fuller’s (1993) expression)? That is, what are the legacies of the Cold War in Science Studies?

These questions are at the heart of this dissertation.

1.2 Theoretical Approach and Methodology: Cultural Production of Scientific Disciplines

To answer these questions, I draw on works in history of science and science studies that helped to illuminate the ways in which the social, political, and conceptual developments of science and technology in the Cold War were shaped by the symbiotic relationships of science with the state and politics. Historians of science and science studies scholars had long been interested in such prominent Cold War topics as the rise of the military-industrial-academic complex and Big Science, the nuclear arms race, the rise of science advisory apparatus and science policy, patronage and politics, and the evolution of science institutions and research programs during the Cold War, both in the natural and the social sciences. In these works the science studies scholars and historians of science have convincingly problematized the existence of a clear-cut boundary between politics, on the one hand, and scientific research, on the other, showing the deep interpenetration of the scientific and the political. I aim to build on and contribute to this line of work on the history of Cold War natural and social sciences. Along the way, I also hope to build, whenever possible, on the emerging integrationist perspective that bridges several long-standing lines of work: diplomatic, political, and cultural studies of the Cold War, on one side, and the history of Cold War natural and social sciences, on the other.

Theoretically, this dissertation endorses the approach that Timothy Lenoir called "the cultural production of scientific disciplines" in his collection of essays on the formation of different scientific, medical, and engineering disciplines (Lenoir 1997). Lenoir emphasizes that science, as cultural practice, is of a piece with culture and politics, and can be comprehended only by understanding the political, cultural, economic, ideological, and even artistic contexts of their creation. On this account, disciplines are spaces where the social and epistemic dimensions of science are intrinsically and complexly interwoven.

With this view in mind, throughout this book Lenoir developed a concept of disciplines defined not through such conventional markers as professional journals, university chairs and professional societies (dealing with the "regimes of the reproduction" of knowledge), but through what he calls "regimes of legitimation" of what constitutes authoritative new knowledge. From this perspective, disciplines are seen as both instruments of knowledge production as well as instruments for defining society, generated simultaneously within political and scientific/philosophical/cultural discourses. Disciplines, as Lenoir has put it, are "dynamic structures for assembling, channeling, and replicating the social and technical practices essential to the functioning of the political economy and the system of power relations that actualize it" (Lenoir 1997, p. 47).

Adopting this approach to disciplines in this dissertation, I explored the history of the emergence of science studies as a discipline (or, rather, a cross-disciplinary "cluster"). Shifting the focus to the "regimes of legitimation," this dissertation looks at how the studies of science were promoted as a distinct, and politically relevant, area of expertise in the Cold War era, and how this helped to legitimize the disciplinary identity of this field. In the coming pages, I will pursue five distinct histories, each focused on a confined mode of analysis of science that articulated, evaluated, and rationalized Cold War sensibilities and concerns, and set them in relation to each other. The case studies in question are: (1) UNESCO and the framework of "scientific humanism" promoted by its two visionary founders, Julian Huxley and Joseph Needham, and implemented in UNESCO's major history of science project, *History of Mankind*, in the 1950s and 1960s; (2) the Congress for Cultural Freedom and its quest, in the 1960s and 1970s, to promote “science studies” as part of its broader agenda to offer a renewed, “post-Marxist,” framework for liberalism, (3) the Salk Institute for Biological Studies, which in the first ten years of its existence, 1962-1972, undertook the bold initiative of launching a sustained inquiry into social studies of modern biology, developed before Bruno Latour famously landed at the Salk Institute in order to undertake an ethnographic study of scientific practices resulting in his and Steve Woolgar’s classic, *Laboratory Life*; (4) the short-lived “philosophical phase” in medical ethics, marked by medical ethicists’ interest in and appropriations from post-positivist philosophy of science, which I explore by analyzing the series of workshops organized under the auspices of the Hastings Center in the late 1970s and early 1980s; and (5) a particular mode of reflection on science and its intellectual foundations developed by Soviet philosophers in the 1960s - 1970s under the name of “naukovedenie.”

All these modes of analyses of science represent roads not taken. The “vision” of science studies all these groups were promoting is different from the science studies we know today. Yet, I argue, these alternative visions, in which the issues of science politics were inseparable from those of science policy, science organization, and science governance, constitute an important dimension of the “pre-history” of the field we have today. Focusing on discourses on science with explicit political and social agendas, I aim to give voice to those who may seem to be too idiosyncratic or too dissenting to be heard, and to readdress some of the issues that were at stake decades ago when the field of STS was taking shape, but were left behind or marginalized later. Historicizing the discourses that fell into obscurity, or were marginalized with the professionalization of science studies, allows me to tell a more inclusive story of the early dynamics of this field than presented in the existing historiographic accounts of Science Studies. It also assists in seeing what was left behind when STS became dominated, to borrow Robert Westman's expression, by a "fruitful but thoroughly intellectualist methodology" (Westman 1975).

1.3 Chapter-by-Chapter Preview

With this broad overview in mind, we can turn now to the exact contours of the historical narrative that follows. The next two chapters draw attention to the role of transnational organizations in the process of discipline-building, discussing, respectively, the United Nations Educational, Scientific and Cultural Organization (UNESCO) and the Congress for Cultural Freedom (CCF). I examine the ways these overtly political organizations sought to provide the conceptual framework and institutional niche(s) for history of science / science studies in the age of Cold War. Both organizations promoted specific frameworks for writing history of science and for studying science in its relation to the state and politics, responding to the anxieties and concerns of the Cold War.

As I discuss in *Chapter Two*, within UNESCO, the major prewar framework for the history of science - scientific humanism - was transformed to accommodate the scientific and the political concerns of the time. I discuss the particular visions of scientific humanism, promulgated within UNESCO by its two visionary founders, British scientists Joseph Needham and Julian Huxley. Since their formation as scientists in the interwar years, both men had endorsed the rhetoric of scientific humanism that emphasized the universal character of scientific knowledge. At the height of the Cold War, they modified this rhetoric, in order to promote a new cross-cultural synthesis, by means of documenting the history of humanity's scientific and cultural development. To this end, Needham was promoting a global, "oecumenical," approach to history of science. In highlighting Chinese scientific achievements he attempted to discern the characteristics of the "East" (China) as opposed to the "West", while promoting the ideals of the "universal" nature of "science" and "civilization." Huxley, in the wake of the Modern Synthesis in evolutionary biology seamlessly accommodated the notion of "Unity in Diversity," a slogan widely publicized by the architects of the evolutionary synthesis. Huxley deployed this notion as a powerful rhetoric for translating the notions of "internationalism" and "humanism" in terms that accommodated the new, multicultural agenda of the age of decolonization and the human rights movement.

Both visions were implemented in UNESCO's major history project, its *History of Mankind*, which sought to offer an account of civilization not written from the ethnocentric or Euro (or Western)-centric perspective. In order to achieve this goal *History of Mankind* placed history of science in the center of world history. The outcome, however, did not meet the aspirations behind the project. The very idea of "history for all," along with the universalist ideal of science and civilization it embedded, were thoroughly challenged in the process. I suggest that as an institutionalized transnational effort, it gave momentum to history of science on a global scale, and also to reflection on the framework and the methodology of history of science writing that would escape the pitfalls of universalist stories.

Chapter three turns to the Congress for Cultural Freedom (CCF) and the ways in which this powerful transnational organization sought to promote "science studies." This chapter places Science Studies as an emerging discipline at the very center of the cultural and political landscape of the Cold War. The CCF is remembered as a paramount example of the "cultural Cold Wars." The organization, as revealed in 1967, had been covertly funded by the CIA. The revelation of covert CIA funding placed the question "who paid the piper" at the center of its historiographical accounts dominated until recently by the stories of the CIA, phony foundations, and covert operations. With the "cultural turn" in Cold War studies, the history and legacy of the CCF is now being reassessed, as scholars begin to study systematically the Congress' publication activities, its periodicals, unearthing the set of subjects and big ideas this organization helped to promulgate in the age of Cold War - the big ideas that dominated the Cold War culture.

In view of these recent reassessments, this chapter offers a detailed account of the activities of the organization related to the promotion of the studies of science as a distinct - and politically relevant - area of expertise within the social and human sciences. I show that the quest to promote "science studies" was part of the CCF's broader agenda to offer a renewed framework for liberalism - one of the big ideas that dominated Cold War intellectual culture and in some crucial ways shaped the post-Cold War world. As a transnational organization, the CCF embodied the goals of negotiation and reconciliation across political divides, both in its "ideology," epitomized in two twin concepts the CCF had promoted – the "end of ideology" and the theory of "post-industrial society" – and in its transatlantic institutional structure. The "end of ideology" was as much a normative position as it was an attempt to secure, in Michael Polanyi's words, "a post-Marxian basis for liberalism" - an umbrella term for various reconciliations of the *free market* (a cherished ideal of capitalist system) and *centralized planning* (firmly associated with Soviet economic system), in the political economy of a post-WWII world shaped by the dramatically increased role of science and technology.

With its emphasis on "sober," sophisticated and dispassionate socio-economic analysis of modern industrial societies (or, rather, "post-industrial societies," according to the CCF-born conceptualization) and their political systems, the "end of ideology" turned the studies of science, its organization and its politics, into a topic of central concern. Science, or, more specifically, Big Science – a new mode of scientific research promulgated in the aftermath of WWII – and its changing relation to the state and politics, which apparently reconciled conflicting claims for *planning* and *laissez-faire*, needed to be assessed by social analysts, especially with regard of its implications for democracy, liberalism, individualism and freedom. As I show in this chapter, the CCF intellectuals sought to offer such an assessment. I argue that as a transnational organization with considerable structural power, the CCF, with its seminars, conferences and scholarly journals such as *Minerva*, provided a "semi-institutional" niche for the meta-studies of science broadly construed, helping to legitimate the disciplinary identity of science studies, and contributing to the construction of public space in which science was reconceptualized as a social activity, challenging the universalist ideal of science.

Having set these broad themes in place, in *Chapter Four* I turn to the particular history of how the early frameworks for the science studies discussed in chapters two and three, were institutionalized in the particular setting of the Salk Institute for Biological Studies in San Diego, California. British "scientific humanism" provided the initial intellectual agenda for what the Institute's founders conceived as the humanities component of the Salk Institute. Julian Huxley's close associate Jacob Bronowski was recruited by Salk to launch the "Department of Humane Studies." The aspiration was to provide a model for the reinsertion of philosophical-humanistic ideals, associated with Humboldt's idea of a humanistic university, within the contemporary American university setting. The Department, which ended up being a "one man show" of Bronowski, was expected to extend the description of nature offered by the "new biology" to a broader understanding of the world, connecting molecular biology to linguistics, philosophy, and the humanities in general, by inviting scholars in the humanities to the Institute. In 1968, the humanistic component of the Salk Institute was considerably extended and transformed. The Institute's new President, Joseph Slater, a long-term officer of the Ford Foundation International Affairs program, used his contacts and the Institute's scientific standing to involve distinguished scholars from diverse backgrounds in the activities of the Council for Biology in Human Affairs established under the auspices of the Salk Institute in 1969. The Council – a loosely connected web of the leading molecular biologists and legal scholars, social scientists, and public-policy makers on the East and West Coasts of the U.S. – listed among its members noted public intellectuals, such as Daniel Bell, Saul Bellow, and Herbert Passin, who had been previously associated with the Congress for Cultural Freedom. The new humanities programs at the Salk Institute amalgamated traditional "scientific humanism" with its emphasis on the integration of "two cultures" of science and the humanities, with the American concerns of the time: abortion, drug abuse, the threat of biological warfare, the effects of genetic manipulation upon human society, and the legal, ethical and social implications of the contemporary advances in molecular biology.

Despite the Council's initial success and impressive activity, the Council along with the humanities component of the Salk Institute were disbanded by the mid-1970s. Yet, I argue, despite the fact that Jonas Salk's aspirations to bridge "two cultures" and to develop a "unification" framework based on modern biology did not live up to initial expectations, the Salk Institute's initiatives were an important experiment in constructing a public space in which the relationship between science and the humanities could be debated, discussed, and reformed.

The programs that the Institute housed were diverse and varied, driven by the impulse to accommodate both the framework of "scientific humanism" with its "universalist" agenda, and the recent trends towards post-positivist epistemology, focusing on modern biology. I argue that the Salk Institute's forgotten initiatives in initiating a sustained inquiry into social studies of modern biology under the auspices of the Council for Biology in Human Affairs paved the way for the developments that led to Science Studies as we know it today.

The case of the Salk Institute and its visionary programs sheds light not only on the pre-history of Science Studies but also illuminates the discussions of the social, ethical and legal implications of molecular biology before these debates became an area of professional expertise and regulation. *Chapter Five* continues this line, looking at the beginnings of medical ethics as a field of professional expertise concerned with the ethical implications of modern biology and medicine. The chapter focuses on the short-lived "philosophical phase" of this new field, discussing the ways in which the discourse of medical ethics became a fertile ground for a dialogue between philosophically minded bioethicists and the philosophers of science who responded to Thomas Kuhn's challenge. In their discussion of the validity of Kuhn's work, these philosophically-minded bioethicists suggested a distinct interpretation of Kuhn, emphasizing the elements in his account that had been independently developed by Michael Polanyi, and advancing a view of science that retreated from idealizations of scientific method without sacrificing philosophical realism.

To elucidate this mode of philosophizing about science I focus on the deliberations of philosophers who in the 1970s and 1980s participated in the series of conferences organized under the auspices of The Hastings Center—the world's first institute of bioethics, founded in 1969 by the Roman Catholic philosopher

Daniel Callahan. These conferences attracted philosophers who had a parallel interest in ethics and in science and were versed in both moral philosophy and the philosophy of science. In their search for a philosophy of science relevant to the concerns of medical ethics, the philosophically-minded medical ethicists associated with the Hastings Center in the late 1970s and early 1980s raised questions rarely considered in traditional philosophy or history of science: What role does personal engagement play in science? How far can rationality serve as a "motive" for scientific work? What is the place of passion in science? The medical ethicists sought to reconcile ethics, medicine, and the philosophy of science in the hope of discovering the "soul in science," and to engage as well in "a bit of soul-searching" themselves, as they examined the practical policy implications of the epistemology they endorsed, and political appropriations of their work. The contribution of Karl Popper to the debate on the applicability of philosophy of science to the issues of medical ethics provides me with the opportunity to discuss the ways in which political agendas of different epistemologies of science intertwined with questions of concern to medical ethics.

I argue that this forgotten dialogue between philosophically minded bioethicists and the philosophers of science who responded to Thomas Kuhn's challenge in the 1970s - early 1980s offers important insights that sound prophetic now. Although largely absent from the subsequent developments of the post-Kuhnian studies of science, some of these early insights and deliberations seem strikingly pertinent to the today's "soul searching" within the discipline. They reveal that the ways in which STS proved vulnerable to political appropriation had already been identified as potential weaknesses when Kuhnian and post-Kuhnian philosophy of science was applied to the field of medical ethics.

In *Chapter Six* I turn to the other side of the Iron Curtain to examine the history of Soviet version of science studies – *Naukovedenie* (literarily meaning 'science studies'), in order to introduce a much needed comparative dimension and to look into the ways in which the Cold War politics shaped the positions and discourses on science on both sides of the Iron Curtain. *Naukovedenie* was first institutionalized in the Soviet Union in the 1920s, then resurfaced and was widely publicized in the 1960s through the 1980s, as a new mode of reflection on science, its history, its intellectual foundations, and its management. The simultaneous reconfiguration of the studies of science in the Anglo-Saxon world, associated with the proliferation of the sociology of knowledge, the extension of social anthropology into the history and philosophy of science, and the institutionalization of the field of STS in the U.K. and the U.S. was the backdrop against which the Soviet "science studies" – *naukovedenie* – was usually analyzed. Seen against this backdrop, the Soviet project of "science studies" appeared as a rather bleak version of its Anglo-American counterpart by championing an internalist vision of science, viewing science as a collection of neutral fact-gathering procedures, and clinging to a rather old-fashioned positivistic outlook. I seek to challenge this view, looking at *naukovedenie* from a different perspective. Rather than comparing it to what is called "science studies" in the Anglo-American cultural tradition I situate the Soviet *naukovedenie* project within the culture of late-socialism in the Soviet Union during the Cold War, asking what this discourse meant for its creators and practitioners, as well as for the high-ranking Soviet officials who provided the authoritative support for this field.

I show that in the 1960s-1980s the *naukovedenie* project was encouraged and supported by Soviet officials at the highest-level as part of a campaign to formulate more effective national policies and to mobilize support for the major decisions of the late 1960s to early 1970s: to pursue détente and substantially increase East-West trade, foreign credits, industrial cooperation agreements and the importation of Western technology. These efforts stimulated the theoretically significant discussions of the social and political consequences of Soviet borrowing Western scientific and technological know-how. These discussions were epitomized in the theory of Scientific-Technological Revolution (STR) - the Soviet counterpart of Big Science in the U.S. STR theory was one of the most evident and valuable developments in social theory in the Soviet Union in the 1970s and 1980s, a status which legitimized the disciplinary identity of studies of science as a distinct area of expertise within the social sciences and philosophy. I argue that, although Soviet *naukovedenie* took a path strikingly different from the version of science studies we are familiar with in the West, it was articulated in response to the local economic and political needs of Soviet state during that particular period of the Cold War.

In my account of *naukovedenie* I use as a case study the history of the Moscow Institute for the History of Science and Technology. Since its inception in the 1920s the Institute became a prototype for an entire country-wide "model" institution for revolutionary change in the historiography of science, conceived broadly as part of bigger project of meta-studies of science, called at different times "methodology of science," "general studies of science," and *naukovedenie*. The turbulent history of this Institute allows me to review all the major discussions and perceptions of history of science, in its relation to meta-studies of science and to the political situation in different periods of Soviet history, from its formation in the 1920s, to the 1970s and 1980s. The focus on the history of this Institute also allows me to tell not merely the story about the visions and theories but also the story of how these visions were implemented within this 'model' institution.

Finally, in my *Conclusion* I review some of the overarching themes of the dissertation, explaining their implications for our understanding of the processes that helped to legitimize the disciplinary identity of Science Studies in the age of the Cold War. One of the themes running through different chapters concerns the rise of Big Science and its role in legitimizing studies of science as a distinct area of expertise within the social and human sciences. On both sides of the Iron Curtain, Big Science, with its paradigmatic Manhattan Project, had been seen by social and political analysts as a cultural and political phenomenon, not merely as a mode of organization of scientific research, but as a complex phenomenon requiring assessment by social analysts. On the both sides of the Iron Curtain scientists as well as social scientists claimed that Big Science requires what might be called "Big Science Studies" - an independent expertise which would provide a systematic assessment and characterization of Big Science, and advise governments accordingly. In the U.S., the Congress for Cultural Freedom, by means of its Study Groups, international conferences, and its periodicals, such as *Minerva*, developed into an influential forum for examining the ways Big Science impacted the relations between science, society, and politics, becoming a semi-institutional niche for science studies before its professionalization within academia during the 1970s. On the other side of the Iron Curtain, the Soviet counterpart of the notion of Big Science - the theory of STR – likewise was used to legitimize the disciplinary identity of Soviet version of Science Studies - the *naukovedenie* project.

As I argue in my conclusion, and as I show throughout the dissertation, political developments and political concerns had a central role to play in legitimizing studies of science as a separate and politically relevant area of expertise, which greatly impacted public, political, and intellectual life within different political systems. The promotion of the studies of science as a politically relevant area of expertise occurred largely outside academia, within transnational organizations that represented both professional and political commitment (such as UNESCO and the CCF), within newly created institutes struggling to built their own reputation and authority (such as the Salk Institute and the Hastings Center), or was state-supported (as in the case of Soviet *naukovedenie*).

In their different ways, these groups contributed to the construction of a public space in which the relations between science and politics were debated, articulating the terms of the discourse later either appropriated or marginalized within the Science Studies in academia. This loosely connected network of intellectuals helped to invent a new subject, or set of subjects, in the 1950s and 1960s, reconceptualizing science as a social activity, promulgating the view that science is inseparable from politics, and in various ways exploring the science-society nexus and challenging the universalist ideal of science. In this way, I argue, rather than being a moment of rupture, Science Studies grew out of these early projects and intellectual programs driven by political developments and political concerns.

Chapter 2

UNESCO, "Scientific Humanism" and the Transformations of the Agenda for History of Science in the Aftermath of WWII and Beyond, 1940s-1960s

A scientific worker is necessarily the child of his time and the inheritor of the thought of many generations. But the study of his environment and its conditioning power may be carried on from more than one point of view.[1]

[1]Needham, Joseph. "Limiting Factors in the History of Science, observed in the History of Embryology" [1935], cit. in Young (1985).

As an academic discipline (or, rather, a trans-disciplinary "cluster"), science studies is a relatively recent specialty. Yet, the meta-studies of science and the reflection on science history, its method, its economy and its politics have been always an intrinsic part of scientific activity. Until the twentieth century, history of science was explicitly part of larger philosophical, intellectual and social-cultural programs. The elucidation of selective aspects of science's history, and the recognition of the societal and political significance of science and its world-historical importance was part of the justification for science that was promoted during the "Scientific Revolution" of the 17th century. Most notably, natural philosopher Francis Bacon articulated the view that science offered a progressive future based on scientific control over nature.

History of science was a brainchild of the Enlightenment project that not only positioned science as the exemplar of the intellectual activity, but also set the stage for history of science as an endeavor helping to uncover the principles of human reasoning and progressive scientific development that underlie and structure the course of human development. As part of the Enlightenment project, the historiography of science had a broad agenda, linking science to liberal notions of freedom, progress and individual creativity. The Enlightenment philosophers echoed many Baconian claims, promulgating a historically-based view of science's intellectual, political and social significance for humanity. In their view, alongside with general history, history of science illustrated the progress of enlightened humanity, by providing an account of the progress of the human mind (see Christie 1990).

As history of science moved into the 20th century, it moved into a new situation. While in the 18th and 19th centuries history of science was largely a domain of retired and/or practicing scientists and philosophers, in the 20th

century various intellectual, cultural, and political motivations of the fin-de-siècle and the following decades of the interwar years boosted a process of emancipation that transformed history of science into a separate area of academic inquiry and a field of professional expertise. During the 20th century, history of science became an increasingly institutionalized discipline, although remaining a field with a particularly open professional structure, with practitioners coming from the sciences, history, sociology and philosophy, or from professional occupations not involving teaching and research, such as museum workers (Christie 1990, p. 16). Nevertheless, it generated the kinds of academic forums, publications and networks, as well as professional associations and specialized journals that conventionally characterize academic professions.

The process of emancipation or professionalization of history of science in the 1920s and 1930s has received some attention in recent historiography. The impulse for the transformations that led to the formation of the discipline have been attributed to the philosophical "reformations" that came from physics, logic and mathematics, when scientists-philosophers in major European countries, most notably the logical positivists of the Vienna Circle, sought to extend the traditional concept of objectivity to accommodate new developments in science; as well as in the innovative approaches coming from sociologists (such as R.K. Merton), historians, and scientists (Christie 1990, Dennis 2003, Dear 2005). As the political situation lurched towards crisis at the end of the 1920s, the Marxist approach to the role of science in enhancing a nation's productive forces and the socio-economic explanations it offered increasingly appealed to the left-wing scientists. The 1930s were marked by systematic efforts to provide a dialectical interpretation of science's different branches.[2] The most influential Marxist account of science development

[2]Dialectical materialism was seen by Marxists and its sympathizers as a new "philosophy of science" (or even the scientific method *per se*), and a challenge and alternative to positivism.

was given by British scientist John Desmond Bernal in his widely read book, *The Social Function of Science* (1939), in which he sought to provide the basis for the new scientific discipline, "the science of science," grounding it in Engels' concept of nature (see Werskey 1978, Sheehan 1993).

The important first centers for history of science and, more generally, for the meta-studies of science, were established in the 1930s in the U.S., U.K., and Soviet Russia. In the U.S., the discipline was launched almost single-handedly by Belgian historian George Sarton, who moved to Harvard after WWI and undertook undergraduate teaching of history of science at this institution, ultimately succeeding in persuading Harvard to establish the department of history of science.[3] Simultaneously, history of science was institutionalized in Britain, with the formation of the History and Philosophy of Science Department at University College London in 1921 and the Cambridge History of Science Committee in 1936 (see Mayer 2003), and in the Soviet Union (see chapter six).

However diverse the approaches to history of science were in these three countries where it was first institutionalized in the first decades of the 20th century, stretching from Marxism to classical liberalism, most of the earlier and interwar frameworks for the "science of science" embedded the idea of unification of science as a way to harmony and a rational worldview, in a time of social crises. The "founding fathers" of history of science as a legitimate area of scholarly specialty promoted the view of history of science as a means to unify the world. Thus, Sarton saw science as a progressive, secular humanist pursuit with history of science as

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Thus, polymath J.B.S. Haldane in his widely read book, *The Marxist Philosophy and the Sciences* (1939) drew on the broad range of sciences - mathematics, physics, biochemistry, genetics, evolution and economics - not only interpreting existent science along the lines of dialectical materialism but also using dialectical materialism to speculate about the future development of science (Sarkar, 1992).

[3] See Pyenson and Verbruggen (2009) and other articles in a special Focus section of the March 2009 issue of *Isis* devoted to George Sarton: Focus: "100 Volumes of *Isis:* The Vision of George Sarton."

the only arena of human activity that demonstrated the progress of mankind. As he declared in his *Introduction to the History of Science* (1927-1948), “the history of science is the history of mankind’s unity, of its sublime purpose, of its gradual redemption” (cit. in Christie 1990, p. 17).

History of science as a scholarly specialty that Sarton sought to promote stood at the center of what he called “the new humanism” or “scientific humanism.” As a movement, scientific humanism – a commitment that expressed itself in the view of science as one of the constitutive domains of human experience, along with religion, art, and philosophy – gathered its strength in Britain in the interwar years as a potent rhetoric on behalf of science, reason and progress. Having secured its place in public discourse during the first decades of the twentieth century, scientific humanism extended its influence into the 1950s and 1960s (Blue 2001, Smith 2003).

Within the scientific humanist movement, history of science emerged as a legitimate – and politically relevant – scholarly specialty. Both “founding fathers” of the history of science, George Sarton in the U.S. and Charles Singer in England, saw history of science as part of their broader aspiration to “humanize” science, through “integrating [science] with the rest of our culture instead of allowing it to develop as an instrument foreign to it” (Sarton 1931, p. 118, 162).[4] Scientific humanism embedded the ideal of science as universal and, by that means, an international enterprise. For Sarton, the history of science had an essential role to play in coping with the spiritual crisis of the age, helping to expose science’s features as a universal and international kind of activity, cutting across national, cultural and political divides, counteracting the tendency of nationalism to isolate and separate people (Pyenson and Verbruggen 2009).

The notion that history of science was *international*, as historian Anna

[4]See discussion of Sarton’s views in Dear (2005).

Mayer pointed out, was crucial for the unprecedented growth and expansion of history of science as a discipline in the first half of the twentieth century in Europe and North America (Mayer 2003). Indeed, such institutional innovations as Sarton's *Isis* (founded in 1913), the History of Science Society (founded in the United States in 1924), the exclusive and prestigious Académie Internationale d'Historie des Sciences (founded in 1927) – all emphasized that history of science must be pursued collaboratively by international practitioners.[5] For all these developments, as Mayer had argued, the ideology that stood behind the notion of internationalism was of central importance.

These interwar contexts and "politics" of history of science were transformed in the aftermath of WWII, although sometimes in unexpected ways. In this chapter I will trace the transformations of "scientific humanism" as a framework for history of science in the 1940s and through the 1950s and early 1960s. In the late 1940s, the advocates of "scientific humanism," such as British scientists and visionary "scientific humanists" Julian Huxley and Joseph Needham, refurbished their visions of scientific humanism. They still presented scientific humanism as a remedy for the political conflicts of the time, which they interpreted as instances of a broader struggle between social degradation and evolutionary advance, and between a modern version of barbarism and a worldwide process of "humanization." Both Needham and Huxley played instrumental roles in setting up the United Nations' Educational, Scientific and Cultural Organization (UNESCO), and they used this transnational organization to promote history of science and as a vehicle for a renewed agenda for "scientific humanism" in the post-WWII world.

Prewar scientific humanism resonated with the notion of international

[5] On the history of the founding of *Isis* see Pyenson and Verbruggen (2009). See also Mayer (2003), especially the Introduction.

cooperation promoted under the protection of the League of Nations, the first permanent international organization that institutionalized "internationalism" as its *raison d'être*. The broader appeal and the resonance between the ideals of scientific humanism and internationalism became even stronger in the aftermath of the Second World War, when the League of Nations, with its International Committee for International Cooperation was replaced with a new international world organization, the United Nations, with UNESCO as the new locus for promoting transnational cooperation. In the wake of WWII and world-wide aspirations for a more internationally conscious and democratic world order, the old League of Nations emerged as an elitist and largely Euro-centered organization. UNESCO, created against this background, reflected postwar liberal optimism about the power of internationalism to prevent wars and build a better society on a global scale.

Scientific humanism was not in any fundamental sense responsible for the establishment of UNESCO, despite the crucial role Huxley and Needham played in founding this organization. Yet, their respective visions of scientific humanism were instrumental in shaping the early agenda of UNESCO that sought to broaden the scope of international intellectual cooperation in two major realms: first, to go beyond the predominantly Eurocentric mode of its predecessor under the League of Nations, and second, to broaden the notion of cultural exchange, bringing science into the picture. The organization's two visionaries promoted a framework for history of science that was informed and shaped by the denominations of humanism, which was liberal, international, scientific and evolutionary.

2.1 Joseph Needham's Scientific Humanism and his Vision of the Place of Science in the post-War World Organization

The creation of UNESCO in 1945 was the result of political decisions and negotiations started as early as in 1943, by the governments of the Allied powers, but primarily by the United States and Great Britain. In 1943, the Council of Allied Ministers of Education (CAME) began to consider plans for a new international organization, which was at first envisioned primarily as a vehicle for educational and cultural reconstruction of formerly Nazi-occupied territories. With this focus on education and culture, the initial acronym of the body was to be UNECO. The decision to include "science" within the new organization was partly the result of the vigorous lobbying undertaken by Cambridge biochemist Joseph Needham.

Early in the interwar years Joseph Needham (1900-1995) established himself as a leading advocate and a prominent scientific voice for social change in Britain during the Depression, as well as a well-known advocate of scientific internationalism. Along with other British left-leaning scientists such as John D. Bernal, Hyman Levy and J.B.S. Haldane, Needham promoted the idea that science should have a larger role in the determination of national policy. As a devout, but ecumenically minded Christian, Needham was consistently concerned with the social implications of different systems of religious thought, believing that religious beliefs needed to be aligned with modern scientific knowledge.[6] He also maintained that science should become an intrinsic part of a humanist outlook, in order to avoid a "dangerous dualism" between science and other dimensions of

[6]See on Needham: Winchester (2008), Habib and Raina (1999).

life.

Needham's major scientific treatise was his *Biochemistry and Morphogenesis*, published in 1942. In it he used recently developed biochemical methods to reconcile the experimental approach to studies of comparative embryological development with an evolutionary framework. This work immediately became a scientific landmark. For Needham it became a culmination of his career as a scientist, marking a dramatic reorientation of his career. From 1937 onwards, Needham's acquaintance with Chinese graduate students in Cambridge led him to his life-long interest in Chinese history, literature and the history of science and technology. By the mid-1930s, Needham was actively pursuing history of science, developing his own distinctive approach that incorporated a detailed examination of particular scientific innovation into a comparative historical framework which used Marxist categories along with biologically derived notions such as "facilitation" and "inhibition" to explain different forms and rates of historical change. Needham's views on the importance of history as a form of experience in its own right, along with his interest in history of science, were strong influenced by the scientific humanism of George Sarton (Blue 2001). By the early 1940s he gained enough sinological competence to engage in study of what he identified as "traditional Chinese forms of scientific humanism."[7]

Needham's interest in China was prompted by his participation in the China Campaign Committee, an organization created in order to mobilize support among British people in aid of occupied China following the Japanese attack on China in 1937. As a result of his activities in this organization and his sinological expertise, in 1943 he was appointed director of the Sino-British Scientific Cooperation Office. In that position, he dedicated himself to scientific liaison work, connecting scientists

[7]See Needham's chapter on "The Chinese Contribution to Scientific Humanism" [1942] in Needham (1969, pp. 89-97).

in the non-occupied parts of China with one another and with the wider world of international science abroad. Recognizing the importance of building bridges between scientists in the industrialized countries with those in poorer parts of the world, Needham launched a vigorous campaign for the organization of a system of offices that would foster scientific cooperation after the end of the war. As part of this campaign, Needham engaged actively in lobbying for the inclusion of science into a new international organization that would eventually become UNESCO.

UNESCO was established initially as a transnational organization of mostly Western Allies representatives. The Soviet Union withdrew from the planning of UNESCO in the early stages, because Stalin saw UNESCO as a tool of Anglo-American influence. Soviet withdrawal led Czechoslovakia, Hungary and Yugoslavia, which were among the UNESCO founding states, to stand aloof as voluntary absentees. Until 1954, the organization functioned without any representation of the communist countries. Internalizing this representational bias within the realms of UNESCO, the notion of scientific cooperation, with its central feature of the dissemination and exchange of scientific knowledge and information, emerged as the affirmation of *free market* ideology. The United Nations itself appeared as the embodiment of the application of liberal democratic doctrines to world-wide institutions of international cooperation. Science, with its “free flow of ideas,” fitted well with this image, resonating with the *laissez-faire* principle in economics and the affirmation of the free market as a foundation of liberalism and democracy (Blue 2001, Petitjean et al, 2006).

Needham opposed such a view. Writing from China, Needham circulated a forty-page memorandum entitled “The Place of Science and International Scientific Cooperation in Post-War World Organization” among scientists involved in setting up this new post-war international political organization.[8] The central point of the memorandum concerned the notion of scientific cooperation and scientific internationalism, and ways it could contribute to post-WWII reconstruction. Needham outlined his critique of what he called the "laissez-faire theory of international relations in science":

> The fundamental error of believers in "laissez-faire" is that they look at the scene too exclusively from the European-American point of view, that is to say, they think of oscillating between Paris, Brussels, London, New York, Washington, Montreal, and the like. They do not realize that the picture of world science looks very different when seen from Romania, Peru, Siam or China. For historical reasons, since modern science grew up in Western Europe, there is a "bright zone" covering Europe and North America, where all the sciences are advanced and industrialization is highly developed. It is particularly the scientists and technologists in the far larger regions of the world outside the "bright zone" who need the helping hand of international science. ... The parochial theory of the "laissez-faire" school is that in science everyone knows everyone else, and can therefore easily get in touch ... But this is simply not the case in the greater part of the world. A Venezuelan economic entomologist may have a problem very similar to that of a Chinese economic entomologist, but the difficulties of their coming into touch are enormous. ... According to theory, the actual publications (we need to say nothing of confidential matters) of the United States Department of Agriculture ... are supposed to reach automatically all those investigators in every country who need them. But this is palpably not the case. And yet no individual country could afford to maintain "selling machinery" which would ensure the efficient distribution of public knowledge, especially since most of the publications are [distributed] for free.[9]

The experience of "war science," Needham maintained, offered a different lesson to take home. Referring to his own experience in China during the war, Needham argued that the organization of science during the WWII set up an example of a more efficient mode of science's organization than the one

[8]Joseph Needham, "The Place of Science and International Scientific Cooperation in Post-War World Organization," April 1945. Joseph Needham Papers (Ref. NCUACS 54.3.95), Cambridge University Library (**Thereafter JNP**), folder D12.

[9]ibid.

modeled on free market capitalism and *laissez-faire* economics. For Needham, the war-time scientific agencies, which were either stimulated by the war or received the funding that they lacked before the war, showed that it was not autonomous, self-governing science, but "war science," with science explicitly put in the service of the state, stimulating the emergence of innovative institutional forms of scientific cooperation.

The war, Needham maintained, albeit somewhat counter-intuitively, opened new opportunities for these international initiatives, institutionalizing the exchange of scientific and technical information bearing on military affairs, in the form of scientific and technical Cooperation Offices, which included Needham' own Sino-British Science Cooperation Office. As Needham lamented,

> These science cooperation offices differ from pre-war international scientific cooperation mainly in that they have adequate funds, secretariat and mechanical aids; and are not confined to any one science, but have carte blanche to do anything which may assist in better scientific cooperation between the countries which they link. They are therefore rather a new departure, pointing the way to the future. ... What we need today is fundamentally an attempt to combine the methods which science has spontaneously worked out for itself in terms of peace, with those which the nations have had to work out under the stress of war.[10]

As Needham pointed out, the *free market* ideology was not helpful when the task was to enlist science into post-war reconstruction and to fight against ignorance, poverty and disease. Instead of the "free marketplace of ideas" principle, Needham suggested what he called the "periphery principle," which implied that the focus of the support should be outside the "bright zones" of the metropolis, and shifted, deliberately, to the peripheral countries, opting for more equal distribution of natural resources and manufactured goods as well as medical products across the world (ibid).

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[10] ibid.

Needham's memorandum was widely circulated among scientists involved in the planning of UNESCO, stimulating a vivid discussion. [11] Its main value, as one of participant in this discussion, Harvard law scholar Richard Field, pointed out, was not in the exact model it suggested but rather in its ethos (Elzinga 1996, p. 177). The issue at stake was the question of how to make fundamental concepts like freedom and democracy the basis for political unity rather than a source of tension, at the outset of the Cold War. The Cold War image of Western liberal conceptions implicitly equated liberal democracy with the normativity of the *free market*. In the Soviet Union and in Eastern Europe, after the annexation of East European countries within the Soviet bloc, the notions of freedom and democracy, as fundamental concepts, followed a different logic. On the level of rhetoric, free market fundamentalism was targeted as the major counterpart of the centralized planning that emphasized the role of the state in ensuring the principles of freedom and democracy. In the West, too, there was a growing acknowledgement that the free market and *laissez-faire* economics failed in the Great Depression, and it was taking the Marshall Plan to pull Europe out of the post-WWII economic meltdown. Against this background, science and the organization of science surfaced as an important resource for both the doctrinaire free-market fundamentalism on the Right, as well as the state-supported planned economy on the Left. Working the middle ground that allowed accommodation by both sides was the value of Needham's proposal that was generally welcomed by the UNESCO planners (Elzinga 1996).

One of these planners was another British scientist and "scientific humanist," Julian Huxley, an Executive Secretary of the UNESCO Preparatory Commission and UNESCO's first Director-General.

[11] See the discussion of Needham's proposal and the responses in Elzinga (1996).

2.2 The "Evolutionary Humanism" of Julian Huxley

Julian Huxley (1887-1975), a grandson of Thomas Henry Huxley, "Darwin's bulldog," and on his mother's side the great-nephew of Mathew Arnold, British poet and an iconic Victorian cultural critic, was embroiled in Britain's scientific and humanistic traditions. [12] An older contemporary of Needham, Huxley was likewise deeply concerned with the problem of harmonizing various facets of human experience. Since the 1920s, Huxley advocated the view that biology should be recognized as a subject whose implications concerned most of the activities of mankind.[13] During the 1920s Huxley established himself as a leading advocate of his own version of scientific, or "evolutionary," humanism.

Like Needham, Huxley framed his views against the background of traditional religious beliefs. Huxley defined himself as an atheist, in his *Religion Without Revelation* (1927), where he wrote about himself that he was "not merely agnostic on this subject [of religion]. ... I disbelieve in a personal God in any

[12]Huxley's family included, besides Julian's illustrious grandfather, his farther, Leonard, who was an editor and writer, his maternal grandfather, Tom Arnold, a renowned academic, and his brothers Aldous, the writer, and Andrew, a Nobel Prize winning biologist (Clark 1968).

[13]The notion that biology provided the framework within which humans could manage or mismanage the planet was the leading theme in Huxley's writings since the 1920s. Huxley's earliest published statement on societal implications of biology can be found in the collection of his essays *The Outlook in Biology* - a published series of Huxley's lectures given at Rice University in 1924. In the lecture on "Biology and Society," for example, Huxley proclaimed: "It is the only link, for instance, between the sciences of matter - that is, physics and chemistry - and the science of mind - that is, psychology. Both aspects are inextricably entangled in biology. Your animal from one point of view is a chemical machine, from another it is a being with a mind. Both views are in their degree true." (Huxley 1924). Although these early writings of Huxley were aimed at laymen, they provided the seeds for many ideas that Huxley developed in his *Evolution: The Modern Synthesis.* As the *Nature* reviewer put it, this book accomplished the very difficult feat of "placing before the non-specialists scientific reader concise understandable accounts of recent important researches which were often difficult to follow in the original publications." (Cit. in Clark (1968), p. 281.). As Huxley's biographer Ronald Clark put it, "Huxley was in this case doing for the scientist the synthetizing and explanatory task which he had so often done for the layman." (ibid).

sense in which the phrase is ordinarily used" (Huxley 1927). At the same time, however, he referred to his "faith" in Christian religion as a spiritual and emotional attitude of mind (Huxley 1927, pp. 137-138). Huxley called these attitudes of mind "value-frameworks" - different kinds of "valuable experiences" such as, for example, classical music could be instances of "valuable aesthetic experience" (Huxley 1927).[14] From this perspective, Huxley objected to any notion of truly 'independent', value-free knowledge, holding that no objects can be understood without a reference to human 'values' that frame scientists' perception of the world. Scientific knowledge, for Huxley, could not possibly produce neutral facts, because any knowledge always intimately tied up with the value-frameworks of its producers and practitioners. [15]

Like Needham, in his various writings Huxley promulgated the view that religious beliefs should be reinterpreted in light of modern science (for Huxley, especially evolutionary science), and like Needham Huxley sought to make his vision of scientific humanism relevant for the pressing political issues of the day. They both were leading voices in the most important academic attacks on racism in the 1930s. Huxley's most widely read anti-racist work of the 1930s, *We Europeans* (1935), co-authored with the anthropologist A.C. Haddon, offered a critique of Nazi racial theories from the perspective of scientific humanism. Huxley and Haddon argued that the notion of the "brotherhood of man" had found a new confirmation

[14] See discussion of Huxley's attitudes towards religion in Waters and Helden (1993).

[15] See discussion in Diball (1993). Huxley's views on religion attained even more significance during the Cold War. Writing in the heat of the Cold War in 1957, Huxley classified his evolutionary concept of progress as a religious system (or a secular substitute for religion), which he defined as "social organ whose function it is to adjust man to his destiny" (Huxley 1957, p. 288). Contrary to the old religions, Huxley wrote, religion today must "utilize all available knowledge in giving guidance and encouragement for the continuing adventure of human development" (ibid, on p. 287). And in this regard, Huxley argued, evolution science with its scientific, or evolutionary, concept of progress "is destined to replace not only the myth of progress, but all other myths of human earthly destiny. It will inevitably become one of the cornerstones of man's theology, and the most important external support for human ethics." (ibid, on p. 15).

in modern science of evolutionary genetics showing that claims of the existence of pure "racial types" and innate "racial characters" was without scientific validity (Huxley, Haddon and Carr-Saunders 1935).

In 1927, Huxley took the unprecedented step of resigning from his position of Professor of Biology at King's College, London, to devote himself to collaborating with Herbert G. Wells and his son on a project which was aimed at making modern biology available to a popular audience. The result was *The Science of Life* (1931), a 1500 page book authored primarily by Huxley. After completion of this book, Huxley was appointed a curator of the London Zoo, a position from which he promoted the idea that science has to play a larger role in national affairs.

After that, Huxley contributed to general biology and field studies in ethology, but he is best remembered for his book, *Evolution: The Modern Synthesis*. Published in September 1942, this book introduced the so-called Evolutionary Synthesis in biology - a framework that laid the basis for the next half-century of activity in the field of evolutionary theory. In *Evolution: The Modern Synthesis* Huxley brought together observations from a wide range of disciplines (largely from genetics, ecology, bio-geography, paleontology and taxonomy) and related them to a general neo-Darwinian framework.

As philosopher of biology John Beatty observed, Huxley's synthesis was more a *consensus* than a synthesis per se, in the sense that Huxley sought to bring together results from diverse fields rather than to suggest a unification of different theories. Indeed, Huxley emphasized that his unification framework gave ample room for a variety of theoretical mechanisms (Beatty 1993). Instead of stressing the multitude of evolutionary *theories* that emerged by the early 20$^{\text{th}}$ century as major rivals of the selectionist Darwinian framework (most notable being the alternatives of Lamarckian and orthogenetic evolution), Huxley placed the emphasis on the disunity of *disciplines* whose observations were not related to each other by a common framework.[16]

The synthesis, for Huxley, was the newly found capacity to relate results from different fields and to forge what Huxley called a "unification" of biology as a discipline:

> Biology in the last twenty years, after a period in which new disciplines were taken up in turn and worked out in comparative isolation, has become a more unified science. It has embarked upon a period of synthesis, until to-day it no longer presents the spectacle of a number of semi-independent and largely contradictory sub-sciences, but is coming to rival the unity of older sciences like physics, in which advance in any one branch leads almost at once to advance in all other fields.[17]

In the last chapter of his *Evolution: The Modern Synthesis*, entitled "Evolutionary Progress," Huxley outlined the larger, humanistic, implications of the evolutionary synthesis, presenting a vision of human progress as the culmination of the biological theory of evolution. The chief theme of this final chapter was the future of humankind and human species' control of its own evolution. The emergence of consciousness in humankind was the ultimate achievement of evolutionary progress, Huxley argued (Huxley 1942). In this sense, evolution is "a series of blind alleys," which all "terminated blindly," except for the evolutionary trend leading to humans. Man's future, therefore, is of central importance to the future of life as a whole.

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[16]As Huxley wrote, for example: "The facts of Mendelism appeared to contradict the facts of paleontology, the theories of mutationists would not square with the Weismannian views of adaptation, the discoveries of experimental embryology seemed to contradict the classical recapitulatory theories of development. Zoologists who clung to Darwinian views were looked down on by the devotees of the newer disciplines, whether cytology or genetics, *Entwicklungsmechanik* or comparative physiology, as old-fashioned theorizers; and the theological and philosophical antipathy to Darwin's great mechanistic generalization could once more raise its head without fearing too violent a knock." (Huxley 1942, p. 25).

[17]Huxley (1942), p. 26.

Huxley presented his view of evolutionary progress as an imperative for the future of "modern man" and a remedy for the ills of the modern world. Even without a purpose, Huxley declared, evolution could serve as a guide to ethics or give sense of meaning in human life:

> But if we cannot discover a purpose in evolution, we can discern a direction - the line of evolutionary progress. And this past direction can serve as a guide in formulating our purpose for the future. Increase of control, increase of independence, increase of internal co-ordination, increase of knowledge, of means for coordinating knowledge, of elaborateness and intensity of feeling - those are trends of the most general order. If we do not continue them in the future, we cannot hope that we are in the main line of evolutionary progress any more than could a sea-urchin or a tapeworm.[18]

Huxley's account of the role of natural selection challenged a popular misconception that Darwin's theory of natural selection justified unrestrained competition in the economic and political realms. Although Huxley argued for the efficacy of natural selection, he refuted a view of struggle in a nature "red in tooth and claw." Thus, intraspecific competition, which Darwin regarded as the major source of evolutionary change and improvement, seemed to Huxley of dubious value from the standpoint of evolutionary progress, especially with regard to the human species.[19] Hence, Huxley argued, "the notion, so assiduously rationalized by militarists and *laissez-faire* economists, that all man needs to do to achieve further progressive evolution is to adopt the most thoroughgoing competition," was unjustified from the standpoint of the Modern Evolutionary Synthesis.[20] Natural

[18]Huxley (1942), pp. 576-577

[19]In his 1936 essay entitled "Natural Selection and Evolutionary Progress," which formed the basis for Huxley's 1942 book, Huxley wrote that natural selection could arguably account for adaptation and for the long-range trends of speciation, however, as he specified, "All that natural selection can ensure is survival. It does not ensure progress, or maximum advantage, or any other ideal state of affairs. A type may survive be deceiving its enemies ... just as well as by some improvement in digestion and reproduction, by degenerate and destructive parasitism as much as by increased intelligence." (Huxley 1936, pp. 83-88).

[20]Cit Greene (1986).

selection, Huxley insisted, was a "blind," nonteleological principle, although giving direction to what appears to be a progressive evolution.[21]

Similarly to Needham's major scientific work, *Biochemistry and Morphogenesis* (1942), Huxley's major scientific achievement - his book *Evolution, The Modern Synthesis* - also published in 1942, marked the turning point in Huxley's career. With the beginning of the war, Huxley became involved into official organizations where his concerns and visions could take form of practical recommendations. During the war, Huxley was responsible for the investigation of the functioning of the British social service system and designing reforms in this realm (his reports on this subject were published in 1941 as a book, *Democracy Marches*). Also during the war he became involved in the BBC Brains Trust programme, which made him a house-hold name and one of the best-known scientists in Britain.

The creation of UNESCO, in the words of Huxley's biographer Ronald Clark, gave Huxley "the chance to plan" on an international scale. When UNESCO was established at the end of WWII, Huxley was involved in it from inception, finding in it the opportunity to materialize at least some of his broad aspirations with regard to the implications of the evolutionary science, or biology in general, for the contemporary intellectual and political visions of an anticipated new world order that inspired and rationalized the creation of this transnational organization.

[21]At the same time, this view of the evolutionary progress did not imply that evolution was purposive. As Huxley emphasized, "The purpose manifested in evolution, whether in adaptation, specialization, or biological progress, is only apparent purpose. It is just as much a product of blind forces as is the falling of a stone or the ebb and flow of tides. It is we who have read the purpose into evolution, as earlier men projected will and emotion into inorganic phenomena like storm or earthquake. If we wish to work towards a purpose for the future of man, we must formulate that purpose ourselves. Purposes in life are made, not found." (Huxley 1942, p. 576)

2.3 Two "Scientific Humanists" at the Beginning of UNESCO

As a spokesman for the sciences in immediate post-war Britain, Julian Huxley was recruited to participate in the planning of what became UNESCO by the British Ministry of Education. In 1945 he was appointed a secretary of the future organization's Executive of the Planning Commission. The person originally elected to become the first Director-General of UNESCO was Sir Alfred Zimmern, a leading British educationalist and a deputy director of UNESCO's predecessor, the old interwar League of Nations' Institute for Intellectual Cooperation. However, Zimmern was ruled out on account of his poor health. As permanent secretary of the executive, Huxley became the acting head. When the Truman administration proved unsuccessful in finding an American candidate, Huxley was appointed UNESCO's Director-General. [22] Simultaneously, he invited Joseph Needham to come back from China in order to help to form UNESCO's Science Section. At the time of the creation of UNESCO in 1945, Needham was in charge of British scientific assistance to China and Huxley, among many other scientists, successfully lobbied for Needham's candidacy to become the first head of UNESCO's Natural Sciences section.

In November 1946, the General Council formally ratified the constitution of UNESCO and officially established the new organization. At UNESCO's inaugural meeting in Paris both Needham and Huxley presented their addresses with their visions of the new international organization. Despite the fact that the

[22] Huxley himself presented his appointment as a pure chance: "It was only through the accident of Sir Alfred Zimmern's sudden illness early in 1945 (and, as Sir John Maud and Ellen Wilkinson at the Ministry of Education told me, because there was no other suitable Englishman free of commitments) that I took over the Secretarship of the Preparatory Commission in London." (Julian Huxley, "UNESCO notes" (1946), p. 1. Julian Sorell Huxley Papers, Fondren Library, Rice University, Houston, Texas (**Thereafter JSHP**), box 66, folder 2.

Soviet Union had not joined UNESCO, they expressed their hopes that the new organization would serve as a vehicle for easing tensions between the capitalist and socialist camps and for building bridges between them. In his address, Needham called for building a new cross-cultural synthesis, by documenting the history of humanity's scientific and cultural development. To this end, he highlighted Chinese scientific achievements and gave an overview of Chinese schools of philosophy, examining the Chinese attitudes to technology and nature.

Huxley's address was a sixty-page manifesto entitled *Unesco: Its Purpose and Its Philosophy,* in which he presented his vision of the organization's vocation in light of his vision of scientific humanism, which he recommended as the philosophical foundation for UNESCO's work and activities. "The UNESCO Philosophy," as it came to be known, was a carefully written argument for the "evolutionary humanist" framework as the foundation of UNESCO's activities. Huxley argued that UNESCO, as a transnational world-wide organization embodying the principle and practice of internationalism, laid the groundwork for a higher evolutionary synthesis and the most advanced expression of the evolution of mankind.

UNESCO's guiding principles, Huxley argued, should not be based on any specific politico-economic doctrine or an "other-worldly" outlook based on religion. Rather, as he stated,

> In order to carry out its work, an organization such as Unesco needs not only a set of general aims and objects for itself, but also a working philosophy ... concerning human existence and its aims and objects, which will dictate, or at least indicate, a definite line of approach to its problems. ... While fully recognizing the contribution made to thought by many of their thinkers, it cannot base its outlook on one of the competing theologies of the world as against the others, whether Islam, Roman Catholicism, Protestant Christianity, Buddhism, Unitarianism, Judaism, or Hinduism. Neither can it espouse one of the politico-economic doctrines competing in the

> world today to the exclusion of the others - the present versions of capitalistic free enterprise, Marxist communism, semi-socialist planning, and so on. It cannot do so partly because ... any such attempt would immediately incur the active hostility of large and influential groups, and the non-cooperation or even withdrawal of a number of nations."[23]

Huxley suggested that scientific knowledge about human evolution, in particular about progress in evolution, provided a common ideology to ensure social order and to guide social policy. He argued that the current chaotic international order was rooted in the absence of a "common theory of life." Just as biologists needed a consensus to built their discipline, a unified pool of scientific knowledge about evolution could provide a sanction for a unified common outlook and a common sense of purpose that would aid the process of unifying the fragmented world in search of a shared ground for political unity:

> Although political unification in some sort of world government will be required for the definitive attainment of this stage, unification in the things of the mind is not only necessary but can pave the way for other types of unification. Thus in the past the great religions unified the thoughts and attitudes of large regions of the earth's surface; and in recent times science, both directly through its ideas and indirectly through its applications in shrinking the globe, has been a powerful factor in directing men's thoughts to the possibilities of, and the need for, full world unity.[24]

Similarly to Huxley's view of the evolutionary synthesis - which implied that the synthesis did not intend to eliminate the alternatives to Darwinian theory, but on the contrary, emphasized that the unified framework provided room for a multiplicity of theoretical mechanisms - his view of UNESCO implied a pluralistic outlook forging consensus rather than homogeneity of opinions. Guided by what Huxley called"evolutionary humanism," UNESCO, in Huxley's view, had to

[23]Huxley (1948), p. 4.
[24]Huxley (1948), p. 18

transcend the Cold War contradictions and dichotomies embedded in stereotypical distinctions between "the American versus the Russian way of life; or capitalism versus communism; or Christianity versus Marxism; or in half dozen other ways," such as "individualism versus collectivism" (Huxley 1948, p. 72).

As part of evolutionary progress itself, UNESCO, Huxley contended, was to represent the highest form of civilization, standing above and beyond local ideologies, including the ideologies of nationalism. UNESCO, in Huxley's view, was to represent an innovative type of social organization, a vehicle to transcend the limits of nationalism and foster an internationalist spirit based on the ideas of equality grounded in the evolutionary common descent of humankind.

As Huxley explained, the evolutionary principle implied that nationalism must give way to internationalism:

> Unesco must constantly be testing its policies against the touch-stone of evolutionary progress. A central conflict of our times is that between nationalism and internationalism, between the concept of many national sovereignties and one world sovereignty. Here the evolutionary touchstone gives an unequivocal answer. The key to man's advance, the distinctive method which has made evolutionary progress in the human sector so much more rapid than in the biological and has given it higher and more satisfying goals, is the fact of cumulative tradition, the existence of a common pool of ideas which is self-perpetuating and itself capable of evolving. And this fact has had the immediate consequence of making the type of social organization the main factor in social progress or at least its limiting frame-work.[25]

At the outset of the Cold War, Huxley lamented, the United Nations had the responsibility for the stability of the post-war world, by forming a bridge linking East and West, and by making available to all nations the scientific and cultural resources that are necessary for social progress:

[25] Huxley (1948)

> At the moment two opposing philosophies of life confront each other from the West and from the East, and not only impede the achievement of unity but threaten to become the foci of actual conflict. You may categorize the two philosophies as two super-nationalisms; or as the American versus the Russian way of life; or as capitalism versus communism; or as Christianity versus Marxism; or in half a dozen other ways. ... Can the conflict be avoided, these opposite be reconciled; these antithesis be resolved in a higher synthesis? ... Since another war would be so appalling as to set back the march of human progress by centuries, I am convinced that the task of achieving this synthesis in time to forestall open conflict must be the overriding aim of Unesco. ... I believe that ...they can be reconciled along the lines of ... evolutionary humanism, in which ... the criterion of further evolutionary progress, the proper organization of society is recognized as the indispensable mechanism of that progress. ... Anything that Unesco can do to satisfy these needs through promoting education, science and culture, will be a step towards a unified way of life and of looking at life, a contribution to a foundation for the unified philosophy we require. [26]

Huxley called his philosophy of UNESCO "a scientific world humanism, global in extent and evolutionary in background."[27] As Huxley elaborated, UNESCO's philosophy

> ...must clearly be world humanism, both in the sense of seeking to bring in all the peoples of the world, and of treating all peoples of the world, and of treating all peoples and all individuals within each people as equals in terms of human dignity, mutual respect, and educational opportunity. It must also be a scientific humanism, in the sense that the application of science provides most of the material basis for human culture, and also that the practice and understanding of science need to be integrated with that of other human activities. It cannot, however, be materialistic, but must embrace the spiritual and mental as well as the material aspects of existence, and must attempt to do so in a truly monistic, unitary philosophic basis. Finally, ... it is essential for Unesco to adopt an evolutionary approach. ... In the last few decades it has been possible to develop an extended or general theory of evolution which can provide the necessary intellectual scaffolding for modern humanism. ... An

[26] Huxley (1948), pp. 72-74.
[27] Huxley (1948), p. 6

> evolutionary approach provides the link between natural science and human history; ... it not only shows us the origin and biological roots of our human values, but gives us some basis and external standards for them among the apparently neutral mass of natural phenomena; and it is indispensable in enabling us to pick out, among the chaotic welter of conflicting tendencies to-day, those trends and activities and methods which Unesco should emphasize and facilitate. Thus, the general philosophy of Unesco should, it seems, be scientific world humanism, global in extent and evolutionary in background.[28]

Huxley's proposal for a "UNESCO Philosophy" met a mixed reception. His vision of scientific (or evolutionary) humanism was never officially accepted as an official philosophy of UNESCO. During the preparatory meeting of the British national delegation for UNESCO, Cambridge Professor of Political Sciences Ernest Barker raised strong objections. As a Christian, Barker found Huxley's evolutionary approach to ethics unacceptable, and he made sure that the British delegation insisted that Huxley present his recommendation of scientific humanism as a his own personal point of view rather than publish it as UNESCO's official document, which Huxley obliged (Armytage 1989, p. 188). Barker's skepticism resonated with the generally conservative outlook of the British delegation. However, Huxley's self-assigned role as a mediator between political divides made him a target of denunciation from all sides of the political spectrum. A Yugoslav delegate, Vladimir Ribnikar, as the only representative of a Marxist perspective in UNESCO at the outset, dismissed Huxley's scientific humanism as a kind of "philosophical Esperanto" (Archibald 1993, p. 112). Speaking from the other side of the political divide, the American delegate William Benton, a U.S. senator and the publisher of *Encyclopedia Britannica* who emerged in the after-war years as a key architect of American post-war cultural diplomacy, denounced Huxley's proposal on the ground that UNESCO should not be tied to any single school

[28] Huxley (1948), p. 5-6

of thought (Ninkovich 1981, p. 101). Serious reservations about Huxley's views led to the strategic shortening of his term as UNESCO's first Director-General. In contravention to the newly adopted constitution, which stipulated that the Director-General's term of office was for a period of six years, the General Council appointed Huxley to that position for a term of two years only.

As Huxley commented himself on the opposition he met, "it turned out, the humanist attitude which I adopted led various delegates [at the first General Conference in Paris] (quite erroneously) to think that I was anti-religious, while my liberalism was taken by others as communism." [29] A journalist covering this story in *Life* magazine commented that the Soviets, at least, considered Huxley an enemy of theirs. In this regard, the journalist observed, "Huxley ... has been called a radical by a national American magazine and a stooge of the State Department and the Foreign Office by a Soviet magazine."[30] Having been put in a situation when he had to explicitly state his political views and affiliation, Huxley told the journalist: "I have never followed any [political] party at all. You might describe me as having voted Labor in the British elections. But I am not a Labor Party member in any organizational sense of the word. ... As a biologist I would assert that the higher synthesis in which two opposites can be reconciled must be the culture of the human species as a whole and considered in the perspective of its long biological future."[31]

In spite of the opposition, scientific humanism as advocated by Huxley and Needham did have a role to play in shaping several early initiatives undertaken by UNESCO during these two men's terms in office. The first initiative was the establishment of one of the defining components of UNESCO's Science Section, its

[29] Julian Huxley, "UNESCO notes" (1946), p. 1, JSHP, box 66, folder 2.

[30] "The Huxleys," (the answer to the attack to Julian Huxley in Life magazine, 1948), JSHP, box 117, folder 5.

[31] ibid

system of Scientific Field Offices which Needham organized on the model of the scientific liaison network he oversaw in China during the war. Continuous in its aims with Needham's war-time enterprise, the new system of Field Offices was to enhance the scientific capacities of non-industrialized societies, providing scientists in these societies with a greater say in scientific discussions internationally, and bringing their needs and achievements more fully to the attention of scientific organizations and communities in developed countries.

A second venture directly inspired by scientific humanism was UNESCO's monumental *History of Mankind: Cultural and Scientific Development*, the organization's major project in history of science. In his manifesto of the "UNESCO philosophy," Huxley envisioned a project focused on writing "a history of the development of the human mind, notably in his highest cultural achievements," which would transcend political and national divisions by making the development of scientific and technological achievements the analytical framework and the organizational principle of a historical narrative. Envisioned by Huxley, the project was eventually realized under the auspices of UNESCO. It was aimed at contributing to international cooperation and building world peace by publicizing humanity's shared heritage of scientific innovation and cross-cultural exchange. Needham and French historian Lucien Febvre joined Huxley to draw up the preliminary plan. This project placed history of science in the center of world history.

2.4 UNESCO's "History of the Scientific and Cultural Development of Mankind"

UNESCO's grand project on the history of mankind's scientific and cultural development was Huxley's major brainchild and the touted innovation he put forth under the auspices of UNESCO. The major innovation of the proposed project was the incentive to offer an account of civilization not from the ethnocentric or Euro (or Western)-centric perspective that distinguished world histories of the past. Rather, as Huxley insisted, "the development of culture in the various regions of the Orient must receive equal attention to that paid to its Western growth" (Huxley 1948).

The *History of Scientific and Cultural Development of Mankind* project's goal of writing a non-Western centered world history fitted well the political climate of the time. The international climate of the late 1940s and early 1950s, in which the *History of Mankind* project had been envisioned, was marked by diverse and increasingly strong Asian and Arab voices in intergovernmental agencies and organizations. Indonesia declared independence in 1945, the Philippines in 1946, India in 1947, Sri Lanka and Burma became independent in 1948, followed by other countries. Following American, Soviet and Chinese involvement in the war on the Korean peninsula between 1950 and 1953, it also became clear that Asian states in transition could easily become hot battlegrounds of the Cold War. Since the late 1940s and onwards, the Cold War animosity between Moscow and Washington fueled the predominant geopolitical connotations of "East" and "West." The rapid decolonization that ousted most European powers from South and Southeast Asia after WWII added to this tension, merging it with the older paradigm of "East-West" cultural opposition and further stirring feelings on all sides that

"East" and "West" were both culturally and politically opposed. Against this background, Huxley argued that the UNESCO's "philosophy" should work out a middle ground between "two opposing philosophies of life [that] confront each other from the West and from the East, and not only impede the achievement of unity but threaten to become the foci of actual conflict" (Huxley 1948, p. 61).

Huxley involved many people in the project. Not surprisingly, the first person whom Huxley invited was his old friend and fellow scientific humanist Joseph Needham. Both saw scientific knowledge and technology as the major unifier between people of various cultures over time, although the detailed schemes they proposed differed. Huxley saw the history of humanity as a continuation of the general process of biological evolution. History of science was important because science, Huxley contended, was the prime mover in the evolution of the human species.

Needham, on the other hand, with his deep interest in Chinese culture and history, was more inclined to emphasize that more attention should be paid to the history of the development of non-Western, Oriental cultures. In 1945 Needham had published a book on the history of Chinese science and technology, in which he demonstrated the enormous and underappreciated importance of Chinese inventions for developments in other parts of the world (Needham 1945). Drawing on his experiences in China, Needham suggested considering another unifying factor in the history of human civilization, pointing out that the major factor promoting social change was contact with strangers possessing new and unfamiliar skills. History thus could be better understood by focusing on mutual indebtedness and interdependence of the people and nations of the world. With this view in mind, Needham proposed to Huxley that the Commission should write the history of mankind stressing cultural interchange - as an antidote to history focused on military and political events and based on ethnocentric biases and preconceptions (see Duedahl 2011).

Needham also suggested to invite Lucien Febvre, a prominent French historian of the "Annales school" of history that emphasized social rather than political themes in history, to join the project. Needham knew Febvre due to his involvement with the French National Commission for UNESCO and insisted that "Lucien Febvre would be good [for the Expert Committee]. He founded and edited the *Annales d'Histoire Economique et Sociales*, which has paid much more attention than similar journals in other countries, to the history of technology."[32] As a result, Febvre became the *History of Mankind*'s third major visionary.

To Huxley's evolutionistic approach and Needham's emphasis on history of science showing that science was the primary mover in the evolution of human history, Febvre added a more explicit emphasis on the "exchanges" between all cultures, arguing that the *History of Mankind* should show the integration of all cultures within one world civilization. As Febvre emphasized, the project

> ...[had to] show that, since time immemorial, men have met peacefully with other men, that they have communicated by exchanges, and the borrowing of one another's particular wealth, be it tools, technology, domesticated animals or improved plant specimens; that a network of peaceful relations has never ceased, through the ages, to cover a world that we want to see as permanently self-damaging; finally, that there are no insignificant people, no poor or destitute civilizations that have not had their glorious moments of invention, that have not contributed in one way or another to the building of our great and overconfident civilizations that, in fact, survive by borrowing.[33]

[32]Joseph Needham to Julian Huxley, 30 October, 1948. JNP, folder D.161. Not only Febvre had the welcomed emphasis on the social and economic history with the emphasis on the material factors, technology including, he was politically acceptable. As Needham noted, "I should like to suggest J. D. Bernal, who, as you know, has an encyclopedic knowledge, but I am afraid it might not be politically wise" (ibid).

[33]Febvre's Report of the Beyrouth Conference to the French National Commission. *Notes et Etudes documentaries*, no. 1080, 26 February 1949, pp. 9-13, cit. in Petitjean (2006).

Febvre also suggested that, to ensure a global approach, the work should be written by a group of specialists representing all continents, and be a team work rather than a compendium of contributions by individual historians.

Huxley was fully content with these developments. He also suggested nominally separating the project from UNESCO, in order to give the future workers a sense of independence from UNESCO and its member states, but also to ensure that Huxley, the project's most dedicated leader, could continue to work on it when his term as Director-General was over. [34] With this view in mind, the project was entrusted to Portuguese historian Armando Cortesao, who at the time of the launch of the project led UNESCO's Natural Sciences Section's History of Science Division, under the guidance of Needham. Although nominally separated from UNESCO, the project was nurtured by the organization's Natural Sciences Section over the course of its implementation. With time, the representatives of the International Committee of Historical Sciences and the International Council of Scientific Unions also became involved in the project.

The *History of Mankind* project was debated and discussed from the inception of UNESCO, but only by the end of Huxley's term as a Director-General of the organization did it begin to take shape. During the first months of 1947, Huxley involved prominent scholars, mainly from France, into the discussion about science as the mover of history. By October 1948, Huxley could already inform Needham about considerable "progress with our project which you did so much to inspire for Scientific and Cultural History" and the concrete plans to establish a planning committee "of four or five men eminent in different disciplines," to which

[34]Huxley originally proposed forming the separate "International Foundation for the Scientific and Cultural History of Mankind" so that the execution of the project would be entirely delegated to it. "It is essential," - Huxley emphasized, – "for a worthy and satisfactory History could not be written if its details were subject to approval by an intergovernmental body such as the General Conference [of UNESCO]." (Julian Huxley, "Memorandum," Paris, January 17th, 1949. JHSP, box 118, folder 3).

Needham was cordially invited to join, "to discuss the general framework of the project and the general methods for realizing it."[35]

In 1948, when UNESCO's Commission for the *History of Mankind* project was being set up, Needham was in the beginning of his next big project on history of science in China, which became his monumental *Science and Civilization in China*, and which he offered for consideration as part of the UNESCO History project. As he wrote to Huxley in response to the suggestion to join the planning committee,

> I am very glad that some definite moves are now being made about this long thought-of project. ... I ...would send you a copy of the epitome of the book on which I am now engaged, 'Science and Civilization in China.' I suggest that this should be typed in a sufficient number of copies for the committee as a concrete example of the kind of basic studies which will be required if the book is to live up to its aims as covering the whole of mankind. ... Ascribing a leading role to the material factors - geographic, climatic, social and economic - [these] I feel to be quite indispensable. How interesting it will be to see what happens.[36]

Huxley saw the plans for the *History of Mankind* approved by the UNESCO General Conference just before he was replaced in early 1949 as UNESCO's Director-General by the Mexican diplomat and writer Jaime Torres Bodet. The Commission for *History of Mankind* was set up in 1949 as a body affiliated with UNESCO, but independent of it as well as of any other government body - exactly as Huxley wanted it to be - to give the future authors a sense that their writing of world history would be free from direct government pressures, and thus to be able to produce an outcome that would be an "objective and dispassionate" work, free from the influences of political commitments.[37]

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[35] Julian Huxley to Joseph Needham, 10 October, 1948. JNP, folder D.161.

[36] Joseph Needham to Julian Huxley, 13 October, 1948. JNP, folder D. 161.

[37] Huxley originally proposed forming the separate "International Foundation for the Scientific and Cultural History of Mankind" so that the execution of the project would be entirely delegated

The idea to write an "objective and dispassionate" history almost immediately became the subject of heated debate. Indeed, it was clear to all the project's visionaries that some of the ideas and assumptions that inevitably would be brought into UNESCO's historical account would derive not only from their own culture and place in society, but also from the ideas and beliefs to which they were committed. For Huxley, evolutionary humanism offered a remedy for this relativism. The Evolutionary Synthesis was articulated as a powerful discourse providing a major disciplinary realignment coupled with a liberal, humanistic and secular (i.e. "modern") worldview. Its slogan "The Unity in Diversity" became a call for the unification of biological disciplines, but also a political call for unity in the divided world of the Cold War. Belief in a rational scientific method, progress, liberalism, and humanism were embedded in the evolutionary "unity in diversity" narratives of the 1960s and 1970s.

In Huxley's view, an evolutionary principle of "Unity in Diversity," embedded in the evolutionary synthesis, should be implemented in UNESCO's attempt at writing a balanced world history. The "Unity in Diversity" slogan, widely publicized by the architects of the evolutionary synthesis, provided a powerful rhetoric for translating the notions of "internationalism" and "humanism" in terms that accommodated the new, multicultural, agenda of the age of decolonization and the human rights movements. Huxley argued that the evolutionary approach would arm historians, enabling them to rise above their conditions and inclinations and appreciate a diversity of world views and perspectives in their own writing, while not restraining them from "taking sides" on the issues. This approach, Huxley contended, would be more realistic one than

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to it. "It is essential," - Huxley emphasized, – "for a worthy and satisfactory History could not be written if its details were subject to approval by an intergovernmental body such as the General Conference [of UNESCO]." (Julian Huxley, "Memorandum," Paris, January 17th, 1949. JHSP, box 118, folder 3).

that of a "dispassionate" and politically neutral historical interpretation.

Insofar as the attainment of perfect historical objectivity might be considered to be impossible, Huxley felt that

> The *History of Mankind* ... will provide a tested body of facts and ideas which can serve as a guiding frame of reference, not only for the operations of Unesco and other United Nations specialized agencies and institutions, but for mankind, and better achievement of its destiny. It helps to clarify the right direction for man's immediate future - toward a world of variety and unity, in which the maximum degree of cultural and individual variety is kept and is maintained free from the threat of war and all major destructive conflict by an efficient unitary organization. It will help mankind to build up a new and truer image of his own nature, an image full of hope, but soberly aware of his own capacities for stupidity and beastliness. ... It will show man as he actually is, as the maker of history, a real phenomenon, tied in to the rest of the world by a web of relatedness. It will provide a foundation for the tempered optimism which is so necessary in this crucial and chaotic period of history. [38]

In Huxley's view, the title of the entire work should be "The Natural History of Civilization," to emphasize the "scientific" character of the work and the evolutionary approach embedded in it. As he put it in the memorandum outlining his vision of the project,

> It appears to me that the subject is really "The Natural History of Civilization," even though perhaps this might not be regarded as suitable for the title. ...The approach indicated by such a phrase ... would imply ... that the development of civilization can only be properly regarded as a continuation, on the human and social level, of the general evolutionary process which had previously led to the attainment of that level by evolving life. If such an approach were adopted, much of the author's difficulties about having an underlying 'doctrine' or 'philosophy' would disappear. The book would be written not in view of an *a priori* doctrine or dogma, but in the light of *theory*, in the proper scientific sense of the world, as a framework of ideas growing out of the facts and giving the most reasonable

[38]Julian Huxley, "Notes on the *History of Mankind: Cultural and Scientific Development*" (1961). JSHP, box 119, folder 5.

> interpretation of them. The facts in this case are, of course, human nature - the nature of the physiological and psychological nature of man, including his primitive social behavior and the actual facts of history and the conclusions to be drawn from pre-history and social anthropology. Without some such unifying theory, any such book will tend to become colourless, or just a catalogue of facts. ... I think that the scientific approach to the history of civilization would bring to light a body of general ideas ... [which] would bear on such vital problems as the relative importance and mutual interactions of the individual and the state, on the types of satisfaction and enjoyment which ought to be provided by society for individuals, the relations between the material basis of society ... and which would at least be in line with the fact of evolutionary reality.[39]

In accord with this emphasis on "scientific attitude," Huxley urged that the project should adopt a "team approach" modeled after the post-WWII Big Science mode of the organization of scientific research in the natural sciences. As he lamented, "...the 'team' might include a few part-time advisers of outstanding personalities, who could not be expected to give their whole time to the work." [40]

Huxley refined his idea of a UNESCO history of mankind in 1961, when the project was well underway:

> To begin with, History, in the technical and restricted sense of the brief history of civilization, is a continuation of the much longer history of mankind as a whole, with all the various species comprised under that designation, from its pre-human origins to our twentieth century civilization; and this in turn is a continuation of the many times longer history of life from its pre-cellular origins to the present. History is thus a continuation of the general process of evolution. It is the story and science of the evolution of life in its human and psychosocial phase. This phase of evolution has its own special characteristics, the most essential being that man possesses a second mechanism of heredity, in the shape of the cumulative transmission of tradition and culture, in addition to primary mechanism of non-cumulative transmission of self-reproducing genetic units or the

[39] "Memorandum by Julian Huxley on the Report of Professor Ozorio de Almeida on the Scientific and Cultural History of Mankind," July 1949. JSHP, box 118, folder 3.
[40] "Memorandum by Julian Huxley on the Report of Professor Ozorio de Almeida on the Scientific and Cultural History of Mankind," July 1949. JSHP, box 118, folder 3.

> genes. ... Evolution in the psychosocial phase is no longer essentially genetical in character, but primarily and preponderantly cultural. In spite of these basic differences, the actual course of cultural and biological evolution are in many ways similar. ... In organic or biological evolution, if we wish to discern significant regularities, trends, and directions, we must treat the subject on the largest possible scale, both special and temporal, and pay attention to the statistical results of long-term processes rather than to single species or isolated phenomena, however remarkable. In the same way, if we wish to discern significant regularities, trends and directions in psychological or cultural evolution, as observed or deduced in human history, we must again treat the subject on the largest possible scale, and pay attention to statistical and long-term phenomena rather than to single societies or isolated events.[41]

All three visionaries of the *History of Mankind* agreed that the work should not be an encyclopedic nor a mainly chronological account. It was to be constructed to become, in the words of Torres Bodet,

> ...a usable tool, presenting history from a new angle. The writing of encyclopedia of universal history is out of the question; it is practicable, however, to assemble and set out clearly established knowledge on an aspect of human history which is too often neglected. Thus, parallel with the traditional histories and the traditional teaching, this work will play its part in implanting in men's minds awareness of the world's oneness and interdependence, respect for cultural values, understanding of all people, and that love and peace which makes the growth of civilization possible.[42]

By 1950, the core team of experts included, in addition to the project's main visionaries Huxley, Needham and Febvre, Belgian biochemist Marcel Florkin, Swiss psychologist and philosopher Jean Piaget (who represented France in UNESCO), Swiss historian Carl Burckhard, and Egyptian writer Taha Hussein Bey.[43] With time, the project also engaged Pierre Auger, Raymond Aron, Claude Levi-Strauss,

[41] Julian Huxley, "Notes on the *History of Mankind: Cultural and Scientific Development*" (1961). JSHP, box 119, folder 5.
[42] "UNESCO/PHS/Conf., Paris, 24 January 1950." JSHP, box 118, folder 3.
[43] "UNESCO/PHS/Conf., Paris, 24 January 1950." JSHP, box 118, folder 3.

among other luminaries in the humanities, mostly from France, but also from Italy, the United States, Brazil, Mexico, India and Syria.

At the same time, for all its good intentions of overcoming Eurocentric and Western bias, the project's principal contributors were mostly from the United Kingdom, United States and, later, France, reflecting the fact that in the late 1940s significant portions of other continents were under colonial rule, while the Soviet Union and several other communist countries were not among UNESCO member states until after Stalin's death. As a form of recognition of the perspective of the non-Western countries, and in order to account for their un-proportional representation in the commission, the project leaders agreed to invite and consult the large number of correspondents from all parts of the world, to give voice to as much interest groups as possible and to provide specialist advice on the project.[44]

As a result, the scope of the project sparked people's imagination. At the outset it involved 500 scholars from around the globe, in order to "produce a historical record that would avoid national bias ... the definitive history of mankind [that] concentrates on the scientific and cultural progress of the human race rather than on military and political aspects" - what a *New York Times* reporter called "one of the most ambitious international ventures of modern scholarship" (Anon 1960). To deal with the enormous scale of the project, the Commission established an elaborate machinery of information gathering. Over the period of its operation the *History of Mankind*'s advisory services enlisted over 100 corresponding members in 43 countries – historians, philosophers, area studies specialists, economists, artists and writers, from whom the members of the Commission solicited manuscripts on particular topics.[45] By 1957, the Commission

[44] As proclaimed in the memorandum on "Geographical distribution of persons associated to the International Commission," 19 November 1952, cit. in Duedahl (2011), p. 124.

[45] "A History of Mankind, An Unparalleled International venture," UNESCO Press-Release, 12 June 1963. JSHP, box 119, folder 5.

had to process and to handle manuscripts received from all around the world and to arrange to send the received works to the appointed consultants, for review and annotation. As Huxley explained the procedure to *The New York Times* reporter, "each volume ... is the responsibility of one author-editor... These authors are assisted by 'participating scholars' who prepare research papers, 'corresponding scholars' who review and suggest, and other consultants" (Anon 1960).

As a 1963 press-release stressed, "The universal aspect [of UNESCO's history] was strengthened by the method used in writing texts. ... Viewpoints reflecting different interpretations of an event or a problem are covered in footnotes written by special consultants and published as supplements in the final texts."[46] This mechanism was expected to assure the operating principle of "Unity in Diversity," which Huxley advocated as the *modus operandi* of UNESCO's project.

In practice, however, the project soon ran onto problems. The joining of the Soviets to the *History of Mankind* project became the major test for the "Unity in Diversity" principle in action. The "team approach" was not that easy to implement when it came to history and the "Unity in Diversity" principle proved to be not easy to achieve when the diverse interpretations of world history across the Iron Curtain were involved.

The Soviet Union had been repeatedly invited to contribute to the *History of Mankind* project since its inception in 1948, even though until the death of Stalin in 1953 the Soviet Union was not a member state of UNESCO. The invitation was turned down by the Soviet Academy of Sciences, but in 1954, after Stalin's death and the following reappraisal of the USSR's foreign policy priorities under Krushchev, the Soviet Union joined UNESCO. Soon thereafter, the *History of Mankind* Commission received a note from the Soviet delegation announcing

[46] "A History of Mankind, An Unparalleled International venture," UNESCO Press-Release, 12 June 1963. JSHP box 119, folder 5.

that the Soviet scholars were prepared to take "an active part in this important, interesting and valuable undertaking of UNESCO" (cit. in Duedahl (2011), p. 124). As the Soviet representative Anatoly Alexeevich Zvorykin explained to the members of the Commission, he and his Soviet colleagues had already been working a series of detailed, in-depth comments on the tentative plan of the *History of Mankind*. Writing from Moscow, Zvorykin informed the Commission that he intended to present these comments as soon as possible, coming to Paris at his own expense if necessary.

Zvorykin was soon appointed a Soviet representative to the UNESCO *History of Mankind* Commission. An economist and historian of science and technology, at the time of the appointment Zvorykin was a vice editor-in-chief of the Soviet Grand Encyclopedia. Before WWII Zvorykin was a researcher at Bukharin's Institute for the History of Science which was disbanded after Bukharin's arrest and execution during the Stalinist purges of 1937-1938 (see chapter 6). By 1956 Zvorykin was appointed a vice president of the UNESCO *History of Mankind* Commission.

At this stage the project was well under way, and some members of the Commission, including its American member, Ralph E. Turner, a cultural historian from Yale University, objected to the idea that a Soviet delegate would become a vice president of the Commission, fearing that the Soviets would instill the Marxist philosophy of history along with their comments and thus would change the general approach of the entire project. Not able to derail Zvorykin's candidacy, Turner insisted that only minor modifications might be accepted at this stage of the project (Duedahl 2011).

Indeed, the first suggested modifications that came from the Soviet scholars commenting on the outline of the project turned out to be minor ones, only involving the inclusion of few Russian names and references. The situation had changed, however, when, starting in 1957, the full manuscripts of the volumes started to arrive on the desks of UNESCO editors. According to the agreed-upon procedure, the Commission circulated each manuscript of the volume among all members of the commission, then sent it to consultants all over the world and to all UNESCO National Commissions. The authors then received comments that were expected to be incorporated in the final outcome volume before it was prepared for publication.

By this time the Soviet comments had become voluminous. By the late 1950s, several East European countries had also been included in the work and they also sent their alterations. To be sure, the manuscripts of *History of Mankind* provoked comments and disagreements from the representatives of other nation-states, as well. Israel was outraged by the passages highlighting Arab objections to the state of Israel. A number of Muslim countries did not agree with the interpretation of the Christian crusades. The Catholic members did not like the representation of religion as dividing rather than uniting people. And so on and so forth (Duedahl 2011).

With regard to the first volumes, devoted to the prehistory of "mankind" and history up to the 18th century, the disagreements were resolved by either removing the most sensitive phrases or by inviting more non-Western scholars to take part in the editorial work. For everything that still presented points of disagreement the editors found an ingenious solution, complying with the "unity in diversity" principle: they suggested that all the revisions that were not accepted by the authors of these first volumes be appended in the form of notes at the end of each chapter so the dissenting views were represented (Anon 1960).

The last volume of *History of Mankind*, *Volume VI: The Twentieth Century*, not surprisingly, proved to be most troublesome. Here the Soviet objections reached their culmination point, substantiated by fundamental disagreements, to which the method of "consensus" history was hardly applicable. As Raymond Aron, the consultant to UNESCO's production of Volume VI on the 20th century, wrote with a note of despair,

> I have to admit (constater) that the revision of such an enormous manuscript exceeds the forces and possibilities of an individual... However, this practical motive is not the only [obstacle]. I am not sure that the corrections that I can make as an editor will improve the book considerably. ... The book seems to suffer already of the hidden preferences [of the commissioned authors] ... The authors tend to camouflage their prejudices and their opinions ... under the mask of apparent neutrality. ... The history of the 20th century presents particular difficulties. ... The interpretation of recent events, still loaded with passions, cannot go without being charged with values, inevitably tied up with more or less coherent system of explanation. En bref, the interpretation of the 20th century, taken as a period in the universal history, can only be personal and partial... This period, if told by one of our Soviet colleagues, will appear to me as a deformation, intentional or not-intended (malicieuse), of the reality. My own story, however, will not appear acceptable for our Soviet colleagues...[47]

The history of the twentieth century as it was presented in the first draft of Volume VI appeared, indeed, unacceptable for the Soviet members. Just a few days after the receipt of the manuscript of Volume VI, Zvorykin and his colleagues returned a comprehensive critical review amounting to some 500 pages. The objections concerned the treatment of communism, the historical account of technological development in the USSR, the Soviet economy and its political system. The review included a detailed guideline for the rewriting of the entire manuscript (Duedahl 2011, p. 127). All this was followed by an official note sent to UNESCO on behalf of the Soviet Academy of Sciences signed by the

[47]Raymon Aron, "Rapport sur le volume VI, Commission international pour une historie du development scientifique et culturel de l'humanité," 1960. JSHP, box 119, folder 4.

Academy's acting President K. V. Ostrovitianov. In this note Ostrovitianov informed UNESCO's History commission that Soviet scholars found the image presented in the *Volume VI: The Twentieth Century* completely inadequate.

Ostrovitianov started with extended commentary to the effect that UNESCO's history of the 20th century failed to achieve the balanced, not Euro-centered account announced as its major goal. As he argued, it misrepresented the history of socialist countries and downplayed their contribution to 20th century science: "By focusing their attention on the advanced capitalist countries and particularly the USA, the Authors-Editors completely disregarded the history of science and culture of other nations."[48] This misbalance of treatment, Ostrovitianov asserted, indirectly but obviously implied the endorsement of an "utterly fallacious concept based on the existence of two worlds: 'the worlds of democracies headed by the USA' and 'the world of totalitarian states"'. On behalf of the Soviet Academy of Sciences, Ostrovitianov insisted that the Authors-Editors should

> ...delete all formulations ...such as 'anti-liberal autocracy,' 'totalitarian regime,' 'authoritarian regime,' 'totalitarian state,' ... and also to delete all analogies between the state systems of the socialist countries and the fascist dictatorships of Italy and Germany. These analogies and cold-war invectives are clearly out of the place in a scientific publication, the purpose of which is to bring the peoples and scientists of all countries closer together." [49]

The issue of the usage of the term "totalitarian," as well as the notion of "totalitarianism" in general, almost killed the volume altogether. Several attempts at reaching a compromise failed. In responding to criticism raised by the Soviet reviewers, the Authors-Editors replaced "totalitarianism" with the term "welfare

[48] "Communication from K.V. Ostrovitianov on Volume VI, The Twentieth Century (Caroline Ware, K.M. Panikkar, J.M. Romein)" October 14, 1959. JSHP, box 119, folder 3.
[49] ibid.

state," explaining in the preface: "It will be noted that the Authors-Editors have deliberately avoided the use of the term 'totalitarian' and have not treated totalitarianism as such as one of the central development of this period."[50] Instead, as the authors explained, "We have used the term 'welfare state' to indicate a general concept of the role of the state as a positive instrument to promote the welfare of its citizens."[51]

However, on that score the manuscript was also severely criticized "by scholars in the West European liberal and Catholic traditions who [saw] 'totalitarianism' of both left and right as the key to the 'cultural crisis' and underlying conflicts of the times."[52] As a result of the attempts to achieve "unity in diversity," history became sanitized and "missing the point," as one of UNESCO's consultants, William Roepke from Switzerland, commented:

> It is significant that the word "totalitarianism" never occurs in this ambitious work, and that its possible connection both with the cultural crisis and with the change of our society into a "mass society" has not been mentioned, let alone analyzed. ... What strikes me in the first place is that obviously no effort has been made to interpret the history of the period in question in terms of one of the most profound 'cultural crises' which our - or any - civilization has ever seen. When reading these chapters one gets the impression that the authors - in spite of their impressive erudition - have never heard of authors such as Huizinga, Toynbee, Alfred Weber, de Corte, Rene Grousset, G. Ferrero, A. Rustwo, Guardini or myself. In the eyes of all those who take these authors seriously ... the whole work thus misses the real point. [53]

By the early 1960s, several years after the manuscript of *Volume VI: The Twentieth Century* was submitted, there was no sign of hope or compromise. The Commission extended the deadlines and submitted demands for additional

[50] "Preface to Volume VI," 1964. JSHP, box 119, folder 6.
[51] ibid
[52] ibid
[53] ibid

money to UNESCO's Director-General. In the meantime, several authors had died or resigned, which caused severe delays. Rumors were circulated in the correspondence between the members of the Commission that the early death of one of them had been provoked by the overwhelming amount of mainly Soviet objections and the prospect of repeatedly having to ask the authors to change and reshape their contributions. Indeed, the procedure seemed to be a nightmare. The Author-Editors felt obliged to incorporate into their texts "contra-notes" to their Soviet colleagues' notes, which, in turn, Soviet Authors-Editors tried to prevent. If they were not successful, they demanded space for notes to the author's notes.

The first volume of UNESCO's *History of Mankind* appeared in 1963, followed by the second volume released in 1965. While the first volume was reviewed rather kindly, the reviews of the second volume, *The Ancient World, 1200 B.C. to A.D. 500*, were critical. The *New York Times* published a review that was, in the words of one of the *History of Mankind*'s Editors, "one of the most savage reviews ever published in the *New York Times*" (cit. in Duedahl 2011, p. 128). The reviewer, Cambridge historian John H. Plumb, characterized the volume as a history "with no soul," an "encyclopedia gone berserk, or resorted by a deficient computer," as he commented on lots of distracting notes, finding the entire enterprise meaningless and ridiculous:

> I don't often wish I were as rich as Paul Getty. Today I do. I want to buy time on every commercial radio and TV from Patagonia to the North Cape, to hire sky-writing planes in all the world's capitals, to take pages of advertizing in all the world's press, just to say how awful, how idiotic is the second volume of UNESCO's projected six-volume "History of Mankind.[54]

In the following years, volume after volume started to appear, however, the criticism grew even louder. The sixth and final volume appeared in 1969,

[54] Plumb (1965).

which made it obsolete before it hit the street, since the events of 1968 were not adequately represented in the volume, which was written and revised largely during the 1950s and early 1960s.[55]

Although UNESCO's *History of Mankind* has never come to play the role that it set out to achieve, at the same time, as historians now argue, the project generated discussions that were sometimes more fruitful than the work itself (see Amrith and Sluga 2008). *History of Mankind*, as a distinct institutional and political project of writing global history of science on a large scale, under the auspices of the UN system, gave rise to new activities. Several recent studies, which readdressed many of UNESCO's initiatives in the humanities and social sciences, assessed them as a revealing examples of the "transnational turn" the within historical profession and the renewed interest in "world global history."[56] As historians of UNESCO argue, long before transnational or world history assumed a prominent role within the discipline as a framework for analyzing the past, UNESCO's grand project in the history of mankind's scientific and cultural development conceived the world's history as the history of global connections of cultures or "civilizations" constituted through interaction and exchange.

The work's biggest critics, both within and outside the UNESCO system, often not only criticized the work, but also insisted on improving it, especially in the 1970s and 1980s. In the process of their long efforts to reach a consensual account the very idea of "non-political world history" was thoroughly challenged. The very failure of UNESCO's *History of Mankind* challenged the universalist ideal of writing "one history for all." Not only those who were directly involved in project but those who read it (and given the amount of publicity around UNESCO

[55] On the publication history of UNESCO's *History of Mankind* see Petitjean (2006)

[56] See discussion in the Special Issue of the *Journal of World History* devoted to the "New Histories of the United Nations" summarized in Amrith and Sluga (2008)

projects it was hardly to be missed) began to search for perspectives that would escape the pitfalls of universalistic stories (Duedahl 2011). In some crucial ways the failure of the project, because of its high visibility, had contributed to shaping subsequent discussions on how to write history of science. As an institutionalized transnational effort, it gave momentum to history of science on a global scale, but also to the reflection on the framework and the methodology of history of science writing.

2.5 Conclusion to Chapter Two

The case of UNESCO illustrates well the complexity and contradictions of the Cold War. Being constrained by the Cold War at every turn, the organization at the same time challenged the dominant Cold War ideological agenda - the geopolitical dichotomies promulgated during the Cold War with its the Euro-West-North - centered bias. Functioning as a tool of nation-states and an instrument of legitimation of both late colonial and post-colonial state building, UNESCO was yet capable of maintaining, at least to some extent, its earlier, transnational and global agenda.

Despite the many distinctions the two scientists-humanists visionaries of UNESCO had received, scientific humanism as a framework for history of science declined, never mind the efforts of UNESCO's founders to revitalize it. With the beginning of the Cold War, growth of Big Science, and the increasing fragmentation of knowledge and culture, scientific humanism was gradually eclipsed, and then was displaced by other, competing viewpoints. The growth of public skepticism about claims to scientific objectivity and mounting public anxieties over dangers stemming from science's growing links with big politics and big business, epitomized in the Big Science mode of organization of scientific research in the aftermath of WWII, further reduced the reputation and appeal of the philosophical and ethical position of scientific humanism.

At the philosophical level, in the 1950s and 1960s this aversion was expressed in the critiques and repudiations of humanism formulated by the Frankfurt School, structuralist, poststructuralist and postmodernist theorists, and other various "anti-humanist" critiques targeting the notion of progress.[57] Of the latter, Karl Popper explicitly targeted Huxley's progressivist "evolutionary humanism" in his critique of organicist and evolutionary approaches to history (Popper 1954).[58] Popper's critique of Huxley's version of "evolutionary humanism" was part of a broad trend in the humanities and social sciences away from thinking in terms of continuities between natural phenomena and human institutions and behavior.

In the context of the Cold War, the critique of the idea of an identifiable direction in human evolution had often been accompanied by objections to the notion of economic and social planning. Both scientific humanist visionaries of UNESCO, a social liberal Huxley and a socialist Needham, advocated the benefits of planning as a remedy for "free market" fundamentalism. *Planning* was an idea that quickly started to run against the cultural narrative of the Cold War in the West, bent on discrediting communism and the Soviet regime with its planned economy promulgated within the ethos of Marxist ideologies, even as capitalist states and other institutions indulged for their own purposes in extensive planning. While humanism could be (and had been) interpreted in terms of a "free marketplace of ideas and information" through UNESCO instruments, it

[57]On the demise of liberal humanist cultural values and the ruling out of the liberal humanist rhetoric by Thatcher's government programs see Thorpe (2008).

[58]Developing his arguments initially presented in his *The Poverty of Historicism* ([1945]) in this later work Popper specified Huxley's "new synthesis" as the butt of the argument (Popper 1994, p. 66)

had always been a contentious issue, at least partially because of the hostility of American members towards what they saw a leftist sympathies within UNESCO.

The United States tried hard to push its own agenda on UNESCO. In the early years of the organization, United States officials saw UNESCO as potentially generating, in the words of UNESCO American delegate William Benton, a "world-wide Marshall Plan in the field of ideas."[59] William Benton was a U.S. senator from Connecticut and a former Assistant Secretary of State for Public Affairs, as well as the publisher of Encyclopedia Britannica, who was in charge of the Voice of America from 1945 through 1947. In 1950 Benton successfully advocated for his proposal for the "Marshall Plan of Ideas," presenting it to President Truman and to the Senate.[60] Benton's proposal recommended a six point program in the field of international information and exchange, including expansion of existing State Department activities and coordination with the noncommunist nations of the world. Such a counterpart of the Marshall plan, Benton argued, would block the expansion of "intolerant" communism and would "close the mental gap between the United States and the rest of the world" just as the economic reconstruction was doing in Europe (Ninkovich (1981), p. 154).

UNESCO was Benton's first candidate for the "Marshall Plan of Ideas." UNESCO, however, withstood the pressure to convert it into an instrument of American policy. The failure to use UNESCO to generate a "Marshall Plan of Ideas" was one of the rationales to establish another transnational organization: the Congress for Cultural Freedom. This alternative (in the eyes of American UNESCO delegates such as Benton) transnational organization supplied

[59]See Finnemore (1993); Preston, Herman, and Schiller (1989), p. 60.

[60]Brooks (1950). Discussed in Krugler, David F. "Will It Play in Peoria? The 1950 Campaign of Truth and the Reconstruction of Cold War Propaganda," Paper presented at the British Association of American Studies Annual Conference, University of Birmingham, Birmingham, UK, April 1997 (*http://www.uwplatt.edu/˜kruglerd/BAAS.htm* Accessed 10.04.2012)

an ideology more attuned to the demands of the Cold War. The new organization also promoted history of science, and, more generally, studies of science as a specialty and an area of expertise in its own right, but in quite different ways than UNESCO's founders envisioned.

Chapter 3

Instituting a "Post-Marxian Basis for Liberalism" in the Age of Cold War: The Congress for Cultural Freedom and its Quest for "Science Studies," 1950-1975

Once the war was over ... even the spat that divided Marxists and non-Marxists ... was momentarily suppressed, and soon a circle of figures, with commitments across the political spectrum, found itself united in promoting the history of science as a discipline encapsulating the goals of post-war reconstruction and *reconciliation.*[1]

The Congress for Cultural Freedom (CCF), an influential international association of elite American and European political and cultural intellectuals, is remembered as the spearhead of Cold War American cultural diplomacy. The CCF was established in 1950, with the aim to combat communist ideology and to secure the ideological and cultural-intellectual support for the Marshall Plan in the sphere of culture and ideas. The means would be a transnational network of anti-Stalinist scholars, with strong bonds between European and American intellectuals. Widely publicized conferences, cultural festivals and the CCF-affiliated periodicals, such as *Encounter*, *Preuves*, *Der Monat*, and, later, *Minerva* made the CCF an important centre for prominent American and Western European intellectuals with an anti-communist stand, but in other respects very different in outlook and opinion. The delegates to the Congress, convened in different Western European countries, included some of the most famous thinkers and public intellectuals of the period: Arthur Koestler, Michael Polanyi, Raymond Aron, Hannah Arendt, Daniel Bell, Sidney Hook, Arthur Schlesinger Jr., among many others.

No doubt, the financial generosity of the Congress contributed to its high profile status, and the source of its funding made it a paramount example of "the Cultural Cold Wars" (Coleman 1989; Saunders 1999). The organization, as it was revealed in 1967, had been covertly funded by the CIA, and its executive secretary, Michael Josselson – "the Diaghilev" of the Congress as he was perceived by the CCF intellectuals – admitted that he had for seventeen years been channeling CIA money into the organization.

The revelation of covert CIA funding placed the question "who paid the

[1] Fox (2006), p. 420.

piper" at the center of its historiographical accounts. In the words of historian David Engerman, these accounts boiled the CCF history more or less down to the CIA, the FBI, covert subsidies, false fronts, dummy foundations and so on, at the expense of a more in-depth look at the "tune" the "pipers" had produced – the big ideas, which dominated Cold War culture and in some crucial ways shaped the post-Cold War world (Engerman 2010). With this "cultural turn" in Cold War studies, the CCF is attracting renewed attention, as scholars are beginning to assess the intellectual and cultural legacy of the Congress, its publication activities, periodicals, and the set of new subjects it helped to promulgate in the age of Cold War. [2] This chapter offers a closer look at the activities of the organization related to the promotion of the studies of science as a distinct - and politically relevant - area of expertise within the social and human sciences. The quest to promote "science studies" was part of the CCF's broader agenda to offer a renewed framework for liberalism - one of the big ideas that dominated Cold War intellectual culture and in some crucial ways shaped the post-Cold War world.

As a transnational organization, the CCF embedded the goals of negotiation and reconciliation across political divides, both in its "ideology," epitomized in two twin concepts the CCF had promoted – the "end of ideology" and the theory of "post-industrial society" – and in its transatlantic institutional structure. The "end of ideology" was as much a normative position as it was an attempt to secure, in Michael Polanyi's words, "a post-Marxian basis for liberalism" - an umbrella term for various reconciliations of the *free market* (a cherished

[2]On the "cultural turn" in Cold War historiography see Scott-Smith and Krabbendam (2003). On the legacy of the CCF in literary studies see Berghahn (2001), Scott-Smith (2002). Many recent studies of the CCF are focused on the Congress' legacy in promoting certain artistic forms and visions, in music, dance, and abstract painting (see, for example, recent studies focused on music: Wellens (2002), Shreffler (2005). The intellectual legacy of the periodicals linked to the CCF is just now being assessed systematically, for example, Jason Harding is currently writing history of the journal *Encounter*, Maren Roth is writing a biography of Melvin Lasky, the editor of *Encounter*.

ideal of capitalist system) and *centralized planning* (firmly associated with Soviet economic system), in the political economy of the post-WWII world shaped by the dramatically increased role of science and technology. With its emphasis on "sober," sophisticated and dispassionate socio-economic analysis of modern industrial (or, in fact, "post-industrial") societies and their political systems, the "end of ideology" turned the studies of science, its organization and its politics, into a topic of central concern. Science, or, more specifically, Big Science – a new mode of scientific research promulgated in the aftermath of WWII – and its changing relation to the state and politics, which apparently reconciled conflicting claims for *planning* and *laissez-faire*, needed to be assessed by social analysts, especially with regard to its implications for democracy, liberalism, individualism and freedom.

The CCF intellectuals sought to offer such an assessment, and they did this on a large scale. As a transnational organization with considerable structural power, the CCF with its seminars, conferences and scholarly journals, such as *Minerva*, provided a "semi-institutional" niche for the meta-studies of science broadly construed and helped to legitimate the disciplinary identity of science studies as a distinct - and politically relevant – area of expertise. The vision of "science studies" that CCF-associated intellectuals were promoting was different from the science studies as currently practiced. Yet, this alternative vision, in which the issues of science politics were intrinsically linked to science policy, the organization of science and science governance, constituted a kind of pre-history of Science Studies as the field we know today.[3]

[3]Aant Elzinga has identified in his several publications that the STS began with "science policy studies" later marginalized within the science studies project (Elzinga 1995, 1997).

3.1 The CCF's "Ideology": the "End of Ideology" in the Age of Cold War

As an organization, the CCF started in June 1950 with inaugural conference held in West Berlin, an outpost of Western power in communist East Europe. The Congress was inaugurated with the *Freedom Manifesto*, written by Arthur Koestler, a Hungarian émigré whose influential anti-communist novels denounced totalitarianism and called to arms to defend freedom and oppose Stalinism. In his passionate speech at the opening ceremony of the Congress Koestler called called for a fight against communism through a worldwide network of magazines, national committees, and cultural programs. Koestler expressed these ideas in the combative, uncompromising style of his sharp prose. Addressing his "friends, fellow-sufferers, fellow-fighters" Koestler spoke with force and passion:

> Since the earliest days, the teachers of mankind have recommended two diametrically opposed methods of action. The first demands that we should refuse to see the world divided into black and white, heroes and villains, friends and foes; that we should distinguish nuances, and strive for synthesis or at least compromise; it tells us that in nearly all seemingly inescapable dilemmas there exists a third alternative which patient search may discover. In short, we should refuse the choice between Scylla and Charybdis and rather navigate like Odysseus of the nimble wits. ... The second, opposite advice was summed up two thousand years ago in one single phrase: "Let your communication be, Yea, yea, Nay, nay; for whatsoever is more than these, comes from evil."[4]

Koestler berated his fellow-intellectuals for refusing to see that "in vital emergencies like the present, when man stands at a crossroads which only leaves the choice of this way or that, the difference between the very clever and the simple in mind narrows almost to vanishing point or even turns to the latter's advantage" (Coleman 2005, p. 184).

[4]Cit. in Coleman (2005), p. 184

Koestler's words were met with applause but his messianic style provoked growing opposition within the CCF. The French Leftist Catholic *Esprit* warned the Congress soon after the Berlin Congress: *"Ne koestlerisons pas!"*(Coleman 2005). To "Koestlerize" meant to attack contemptuously and uncompromisingly the fellow-travelers, whom Koestler regarded as - literally - neurotics, "victims of professional disease," as he diagnosed the intellectuals' estrangement from reality and their adherence to the mental habits of looking for synthesis, compromise, and middle-ground. These "mental habits" made those among them who refused to join a rhetorical crusade against communism the "imbeciles," a word Koestler used twice in one speech at Berlin (see Koestler 1983). Koestler's "liberalism" was "militant," indeed.

By 1952, Koestler was marginalized within the Congress and lost his influence among its leaders, who, in the words of Nicolas Nabokov, a composer and the Secretary General of CCF, by that time were striving to establish "the Congress [for Cultural Freedom] in the minds of the European intellectuals as a positive, and not only as a political, organization" (Coleman 1989, p. 56). After Stalin's death in 1953 it became even more obvious that the Koestlerian "simple" message of "anti-communism" and uncompromising combat against totalitarianisim was not enough to sustain appeal among the European and American intellectual elite.[5]

The CCF lost no time in establishing its "positive program," which emerged in the mid-1950s from the combined work of the CCF network of intellectuals, most prominently Raymond Aron, Daniel Bell and Edward Shils. Their "positive program" was encapsulated in the "end of ideology" rhetoric, which found its sharpest articulation in Daniel Bell's classic *The End of Ideology*, first published in 1960. Since the 1955 Congress in Milan, attended by the most prominent

[5]See discussion of marginalization of Koestler's "hard-line" within the CCF in Berghahn (2001) and *Scott-Smith* (2002).

intellectuals of the time – Hannah Arendt, Milton Friedman, Raymond Aron, Michael Polanyi, Mircea Eliade, Ernst Nagel, Joseph Needham, Peter Winch, among many others, the "End of Ideology slogan" became, in the words of Michael Polanyi, "an expression of [the CCF] predominant aims [and] our official pronouncement."[6]

Michael Polanyi, a Hungarian émigré and a "philosophical chemist," who is now recognized as one of the most important thinkers on the nature of scientific knowledge and the social order of science, contributed to the fundamental revision of the image of science, its theory, method and practice in the 1960s and 1970s. Polanyi's philosophy of science was tightly connected to his thinking in economics and politics, and his life-long defense of the market model of the social order of science.[7] Polanyi became an active member of the CCF in 1953, persuaded by physicist Alexander Weissberg, who asked him to chair the CCF's Committee on Science and Freedom and help organize its meeting in Hamburg (Nye, 2011). Polanyi, in turn, persuaded Chicago sociologist Edward Shils to join the Congress in the same year (Scott-Smith, 2002). Polanyi was the source of Shils' perhaps deepest intellectual relationship, which, as Stephen Turner put it, "cannot be reduced to a model of 'influence.'... [Rather,] it amounted in a way to a kind of dialectical partnership that stimulated Shils' thought and Polanyi's as well" (Turner 1996).[8] Both had the greatest impact in making the "end of ideology" discourse the semi-official outlook of the Congress.

In Polanyi's words, the "end of ideology" was a "distinctive, passionately sober approach to culture and politics," the approach which encouraged the factual and calm examination of political systems and contemporary societal phenomena,

[6]Michael Polanyi to V. B. Karnik, 11 Jan 1961, Michael Polanyi Papers, Regenstein Library, University of Chicago (**Thereafter MPP**), box 6, folder 1.

[7]See discussion in Nye (2011), Thorpe (2009). See also discussion in Chapter V.

[8]See Swartz (1998).

rather than mere denunciation of totalitarian regimes and communism.[9] This rhetoric sought to shift the focus of the debate away from simplistic early Cold War dichotomies of East-West competition, thereby claiming greater scholarly merit and possibility to reach for a wider audience. The "end of ideology" rhetoric signified an important shift within the CCF's self-perception that occurred in the mid-1950s, "from an instrument of struggle against totalitarianism to an international forum for debate" (Scott-Smith 2002, p. 139).

All CCF-associated intellectuals had a long trajectory of political activism and engagement, and regarded their activities within the Congress as the forefront of political debate. These were the intellectuals looking for reconciliations, opting for a middle-ground position and offering a mix of political analysis, to ensure a broader public appeal. They prided themselves on their scholarly detachment and realism, and adopted a characteristic tone of ultra-sophistication that marked the CCF publications and activities in the 1960s. Their "end of ideology" position implied that politics should no longer be defined as a contest, a fight between rival ideological movements of the right or the left. Instead, the common forces resulting from the dramatic advances of science and technology were causing both democratic capitalist and authoritarian socialist and collectivist systems to adopt similar methods of socio-economic management. In the words of Konstantin Jelenski, a Polish émigré writer who led the Eastern European division of the CCF, it was the "growing realization that the realities of industrialism are perhaps a more important determining factor socially than political systems, whatever their ideological origin" (Jelenski 1962, p. 1).

The "end of ideology" was not only a normative position; it also sought to offer a substantial reformulation of the ideals and goals of classical liberalism. The

[9]Michael Polanyi, "CCF, Memo" 19 Nov 1961. MPP, box 6, folder 6.

post-war world saw the rise of welfare-state capitalism and the growing recognition of the role of the state in regulating and managing the economy in order to overcome the instabilities of the free market. These post-WWII realities had shaken the belief in an unregulated free market system and *laissez-faire*. The "end of ideology" sought to offer a renewed defense of capitalist "free society," with its central ideals of the "free market" and *laissez-faire* economics, to be attuned to the realities of the post-WWII politico-economic conditions, and to the subtleties and compromises of the Cold War with its post-war coalitions. In this way, as Polanyi contended, the "end of ideology" sought to "secure a post-Marxian basis for liberalism throughout the world."[10]

The "end of ideology" also turned the studies of science into the topic of central concern. With the emphasis on "sober," sophisticated and dispassionate socio-economic analysis of modern industrial societies and their political systems, the "end of ideology" promoted the view that dramatically increased role of science in the realm of public affairs and politics delimited a new phase in the development of Western liberal democracies. Science, its history and its politics needed to be assessed, especially in terms of their implications for democracy, liberalism, individuality and freedom. The CCF intellectuals sought to offer such an assessment, and they did this in a big way.

[10]Memo "Study Group of the Committee for Science and Freedom held in Paris 29 August to 1 September 1956." MPP, box 33, folder 10.

3.2 The CCF's Methodology: Study Groups and the Seminar Program

As Rebecca Lemov pointed out, during the Cold War era "methodological thought" was a preeminent concern (Lemov 2010). Indeed, within the CCF, with its "end of ideology" rhetoric calling for a non-ideological "sober" research based on facts, rational method and science, rather than ideological conceptions and "messianic claims," the concern for a "proper method" loomed large. The CCF encouraged the application of scientific methods to social problems. A model singled out in the organization's internal correspondence was the Committee for Economic Development (CED), as the organization that had an important impact on the Marshall Plan, and that had developed sophisticated methods of collecting, processing and analyzing vast amounts of social data.[11] The "Explanatory memorandum" of the CCF Seminar Program elaborated on this:

> The Congress would do well to adopt this CED technique or some appropriate variant of it whereby data which has been collected by specialized institutions on different topics of general importance would be submitted to a small study group of the Congress which would prepare on the basis of the detailed examination of the available data, recommendations for a special international policy statements, to be issued on the name of the Congress. Such statements ... could conceivably have a beneficial influence on the policies of other organizations, of foundations, and even of governments.[12]

The CED model was never implemented in practice at the CCF. The main form of the CCF activities was its Seminar Program, conceived as an innovative form of interdisciplinary contact, a method of "intellectual confrontation" in which

[11] "Memorandum," (n/d), International Association for Cultural Freedom Papers, University of Chicago Archives (**Thereafter IACFP**), Series III, box 99, folder 1

[12] "Memorandum," (n/d), IACFP, Series III, box 99, folder 1

critical discussion was methodically staged. As one of the CCF officers noted, "the seminar concept ... provides the most adequate form for the treatment of many issues ... stimulating thinking and discussion about ...new ways of organizing intellectual confrontation."[13] To achieve this healthy "intellectual confrontation," the CCF seminars and study groups adopted the practice of commissioning two or more position papers that would present opposing views "on a critical contemporary problem" (such as, for example, "an optimistic and pessimistic view of the quality of life in industrial society"), and submitting them for discussion in small groups.

The CCF Seminar Program comprised both international seminars and a large number of local meetings. The topics ranged from the issues of race to the role of university, mass culture, religion, and science in the changing world.[14] The unstated goal of the CCF seminars was to develop it into a clearing house of independent expertise on the pressing issues of the day, with the view in mind that eventually it will be used to advise government policy-makers. These discussions, fully transcribed and edited, ultimately were intended to be published in the special Congress series devoted to "major present preoccupations of the intellectual world."[15]

One of the chief designers of the CCF's seminar program was Edward Shils, who also helped to introduce another form of semi-institutionalized CCF activities – its "Study Groups." CCF study groups were modeled after the "Study Group on Science and Freedom," started by Michael Polanyi at the 1953 Hamburg Congress, as a continuation of the activities of the Society for Freedom in Science that he had

[13] "Memorandum," (n/d), IACFP, Series III, box 99, folder 1
[14] CCF Study Groups, IACFP.
[15] "Memorandum," (n/d), IACFP, Series III, box 99, folder 1

initiated in the 1940s.[16] Polanyi, as one of the main organizers of the 1953 Congress in Hamburg, presented the opening address, where he spoke about the system of the organization of science, as it had developed since the Scientific Revolution, as a resource for liberal capitalist democracy. An avid defender of the notion of "free enterprise" in science, Polanyi likened the functioning of the scientific world to that of the free market: "individual, independent research is ... coordinated by a common medium which fulfils in the realm of science the same function as does the market for a system of free enterprise."[17] Shils commented at length on Polanyi's "free market" analogy and shared many of Polanyi's ideas and concerns. At the same time, Shils noted in his comments that Polanyi's discussion did not particularly sharpen the picture of the proper relations of science and the state. In particular, as he pointed out, offering no satisfactory method of the governmental support of science and related policies.[18] The set of issues related to science policy would later become the focus of discussions, workshops and publications initiated by Shils under the auspices of CCF.

At the next Congress, held in Milan in 1955, Shils suggested to extend the Study Group into a "future Congress on Science and Freedom," as a permanent organization like the CCF itself but focused exclusively on the "impact on the academic community of the great historical changes of the past two decades, and particularly of the greatly increased need for finance from outside sources, and the invasion of practical and technical tasks in the academic sphere, with

[16] "Committee on Science and Freedom. Report on the First Year's Activities: July 1954-August 1955." Committee on Science and Freedom Study Group, Agenda (Paris, 1956). IACFP, Series III, Box 12, folder 4. See on Polanyi's Society for Freedom in Science: Nye (2011).

[17] Michael Polanyi, "Pure and Applied Science and their Appropriate Form of Organization," Science and Freedom Congress, Hamburg 1953, IACFP, box 5, folder 7.

[18] "Shils Comments on Polanyi," Science and Freedom Congress, Hamburg 1953, IACFP, box 5, folder 2.

all its resulting social, political and intellectual problems."[19] The envisioned "Congress on Science and Freedom" took place in 1959, in form of a CCF General Conference entirely devoted to the discussion of the dramatic changes in the role that technology and science came to play in the post-WWII world, profoundly altering the structure of Western society. The long-term goal was to produce a comprehensive account of the "Technical Age," into which both Western and non-Western societies had entered in the aftermath of WWII.[20] However, the topic was obviously too grandiose and too important to be limited to one meeting. As a solution, Shils suggested dividing it between several Study Groups, and making these a permanent form of CCF activities. The CCF adopted Shils' proposal, and six Study Groups were inaugurated, each having its own program within the general theme outlined in the conference proposal. With different levels of success and productivity the CCF "study groups" continued to function after the conference, organizing follow-up conferences and at least in some cases publishing their proceedings.

The first Study Group, headed by Shils himself, was framed explicitly in terms of the "end of ideology" discourse, focusing on the political aspects of the "Technical Age" dealing with the "decline of ideologies" in Western societies as the result of the rise of technical and scientific expertise and their growing role in the realm of public affairs. The second and third groups were concerned with the features, both positive and negative, of modern industrial and mass society, with one group focusing on the features of society while the other focused on the individual's relation to culture and society in the "Technical Age." The

[19]Committee on Science and Freedom Study Group, Agenda (Paris, 1956). IACFP, Series III, box 12, folder 4.

[20]Edward Shils, "Outline of the General Conference of the Congress for Cultural Freedom," Apr 1959, International Association for Cultural Freedom/Congress for Cultural Freedom/ International Council on the Future of the University Records, Regenstein Library, University of Chicago (**Thereafter IACF/ICFUR**), box 1.

fourth and fifth groups focused on the role of the "vast class of intellectuals" - artists, scientists, and academics of all kinds - in the contemporary world divided by the Cold War. Finally, the sixth group proclaimed as its aim the study of "the signs already visible of affirmation in the Communist world itself of those values which we hold dear. It will seek to pinpoint indications that such values do provide a point of convergence for various contrasting historical developments." [21] In subsequent years, the topics of Study Groups changed, although there always was some continuity in the original emphases: Shils, as a Study Group leader, was interested in science policy and science politics, Polanyi was concerned with the relationship of thought, mentalities, and contemporary politics, Raymond Aron focused his Group's discussions on the conditions of stability in democratic and authoritarian societies, while Nicolas Nabokov's group focused on the role of art in the post-WWII world.[22]

Science in relation to social and political order was the major focus of discussions in two groups, led by Shils and Polanyi. Polanyi's major philosophy of science book, *Personal Knowledge: Towards a Post-Critical Philosophy* (first published in 1958), was conceived before he joined the CCF, but the ideas in *Personal Knowledge* were elaborated within the CCF study groups. Polanyi is mostly remembered for his development of the notion of scientific community and his concept of "tacit knowledge," which articulated the view of science that appreciated the role of craftsmanship, apprenticeship, and authority in scientific research (Nye 2007, 2011). Thomas Kuhn acknowledged Polanyi's influence on his own ideas on scientific community. At the same time, Kuhn and other post-positivist philosophers of science read Polanyi selectively. The

[21]Edward Shils, "Outline of the General Conference of the Congress for Cultural Freedom," Apr 1959. IACF/ICFUR, box 1.

[22]CCF Study Groups, IACFP, Series III, box 27-29

major component of Polanyi's philosophy of science, his "theory of commitment," in which he sought to rehabilitate emotions as a legitimate object of sustained epistemological inquiry, did not evoke much interest on the part of philosophers and historians of science in the 1960s and 1970s (Nye 2011). [23] Polanyi's ideas on the epistemological and political relevance of emotions, passions, and motives - all these marginalized subjects both in traditional philosophy of science as well as within its post-positivist developments - were thoroughly discussed, however, at the CCF Study Groups meetings.

Shils' Study Group, on the other hand, was focused on "Scientific Policy - the Cooperation of Government, Economy and the Universities in the Development and Application of Scientific Research."[24] In 1960 and 1961, Shils invited leading academics and scientists to take part in discussions, or to contribute texts on the topic in question, for the study group discussions. As he explained to Robert Oppenheimer in 1960, "The kind of persons we wish for are scientists concerned with problems of scientific policy, 'scientific administrators,' civil servants and politicians especially concerned with the development of science and other aspects of the application of scientific knowledge."[25] Shils was involved in the atomic scientists' movement as one of the founders, editors and reporters of the journal *Bulletin of the Atomic Scientists* since its inception in 1945, and a long-term vice-director of the University of Chicago's Office of Enquiry into the Social Aspects of Atomic Energy, which he and Chicago anthropologist Robert Redfield established in 1945. As a "director" of the CCF Study Group, Shils now capitalized on this network trying to enlist atomic scientists in a more general and theoretical discussion of the science policy issues.

[23] See Chapter 5 for the discussion of how philosophers who turned to medical ethicists in the 1970s appropriated Polanyi's theory of emotions and passions in science.

[24] Edward Shils to A.K. Brohi, 11 Apr 1960, IACF/ICFUR, box 1.

[25] Edward Shils to Robert Oppenheimer, 12 Jul 1960, IACF/ICFUR, box 1.

The Study Groups were targeting the intellectual elite, the major "target" of all CCF activities. Shils, however, moved his Study Group's discussions in the public realm. In 1962, Shils established the journal *Minerva*. *Minerva* was conceived as a public forum for the discussion of the issues Shils started to discuss within his Study Group, intended to reach out not only the broader academic community, but also the government policy-makers, by developing independent expertise on the pressing issues of science policy in contemporary America and in major industrialized countries.

3.3 The "Minerva Debate": Social and Political Implications of Big Science

In his opening editorial address in the first issue of *Minerva*, Shils stated that Minerva will be dealing with the issues surrounding what he called "governmentalisation" of science:

> The governmentalisation of science and scholarship is, in part, a product of intellectual development and its changed relationship to technology, which entails costs which can only be borne by government, and returns, in which governments, even in capitalistic societies, have a great and appropriate interest. The governmentalisation of science in the past decade and a half is also, in part a result of the Cold War - as are also, to some extent, the political embarrassments and concerns of science. *Minerva* will be concerned with the indirect as well as the direct influence of the Cold War on the role of science and scholarship and on the performance of their true calling.[26]

The critical effect of the Cold War was that "scientists have become politicians" (Shils 1962, p. 9). Politics was always part of science, but now, Shils pointed out, the relationship between politics and science had become more

[26]Shils (1962), p. 16

explicit and prominent. The 'governmentalisation' of science, in Shils' opinion, delineated the start of a new era in the relations between American scientists and the federal government. *Minerva* sought to create a forum to discuss, describe, document, and examine these recent changes, and their impact on the relations between science and politics.

During the 1960s the journal established itself as such a forum, focusing particularly on national policy debates in the U.S. The first and most prominent topic of the newly founded journal, which became the continuing theme on the pages of *Minerva* from 1962 to 1967, was the discussion of the phenomenon of Big Science and its social and political implications. In the early 1960s the term "Big Science" was coined, and the discussion of this phenomenon was framed by two physicists – director of the Oak Ridge National Laboratory Alvin Weinberg and physicist-turned-historian Derek de Solla Price.[27] In his influential essay on the phenomenon that Weinberg called Big Science (always in capital letters), published in *Science* in 1961, Weinberg argued that the large-scale, centralized scientific operations exemplified by gigantic National Laboratories like Oak Ridge which required big facilities, big budgets, and big publicity had drastically changed the major features and societal relations of science in the post-WWII era. Despite the fact that Weinberg was the head of the paradigmatic Big Science facility he held an ambivalent view on Big Science, arguing that it should be "contained" within the walls of few National Laboratories, "to prevent the contagion of Big Science from spreading to the universities" (Weinberg 1961, p. 162). Pointing out that "Big Science is here to stay," Weinberg called for a systematic characterization of this phenomenon and its social and political implications. "These questions," Weinberg noted, ".... are so broad, and so difficult, that I cannot do more than

[27]See Weinberg (1961), Price (1961).

raise them here" (Weinberg 1961, p. 161).

The questions that Weinberg raised in his short 1961 *Science* essay became the primary concern of the newly founded journal *Minerva*. While planning the first issue of the journal in May of 1962, Shils contacted Weinberg inviting him to contribute a paper for the opening issue of *Minerva*, expanding on some of the themes Weinberg raised in his *Science* essay.[28] Weinberg apparently did not react immediately, but he responded to Shils' second call several months later, and sent Shils his manuscript that he wrote when he was a member of the President's Science Advisory Committee and that he presented at the meeting of the local engineering society of the University of Tennessee earlier that year.[29] Shils reacted enthusiastically and suggested to publish this essay with minimal changes in the second issue, since the opening issue was already in press.[30]

Weinberg's article, which Shils entitled "Criteria for Scientific Choice," was focused on the question of how funding for Big Science projects should be allocated in a democratic society. At the same time, it framed a set of more general themes. In Weinberg's view, when science achieved the size and complexity of Big Science operations, all of which were embroiled in institutional, bureaucratic, and national as well as international, politics, a qualitative change had occurred in the relation between science and the state, as well as in the ways scientists and the public understand the relationship between science and politics. The distinctive feature of Big Science was its explicit political character. Big Science implied czar-like control and promulgated hierarchies in scientific life. Moreover, it was the Cold War that stimulated the adoption of Big Science as the new agenda for post-WWII science, in order to maintain the nation's superiority in the high-technology post-war economy.

[28] Reference in the letter: Edward Shils to Alvin Weinberg, 2 Nov 1962, Minerva Records, Regenstein Library, University of Chicago (**Thereafter MR**), box 1, folder 12.

[29] Alvin Weinberg to Edward Shils, 15 Nov 1962, MR, box 1, folder 12.

[30] Edward Shils to Alvin Weinberg, 28 Nov 1962, MR, box 1, folder 12.

Weinberg argued that scientists and the general public have no choice other than to accept Big Science, but recommended an institutional pluralism, with the Big Science restricted to the National Laboratories, while traditional "Little Science" and middle-range science were preserved in self-governing universities.

Weinberg's essay opened the discussion on the social and political implications of Big Science on the pages of *Minerva.* Shils carefully planned the debate, transferring the approaches and formats of the CCF Study Groups to this forum. In line with the CCF's "method of intellectual confrontation," Shils commissioned two opposing "position papers" to frame the discussion of Big Science in the opening issue of *Minerva.* The opposite view on Big Science came from Michael Polanyi, whom Shils asked to write a paper in the new journal. Polanyi was an enthusiastic supporter of Shil's initiative, praising Shils for the "great achievement" of his "enterprise in bringing out the first issue of *Minerva*" and affirming Shils that he "subscribed to everything you say in the editorial introduction."[31] As Shils later admitted, Polanyi became less interested in the journal when it became focused on "science-policy" (Shils 1976). Nevertheless, Polanyi wrote an essay for the opening issue of *Minerva*, the famous "The Republic of Science," which contributed, in Shils' words, to the "fundamental theme of *Minerva*," while presenting a point of view quite different from the one advocated by Weinberg (Shils 1976).

In this essay Polanyi restated his long-held views on science and its governance, arguing that the social order of science, as self-perpetuating and self-governing community of scholars-citizens, with no central authority, and internally coordinated "by mutual adjustment of independent initiatives," is, at the same time, a system that "works according to economic principles similar to

[31]Michael Polanyi to Edward Shils, 15 Nov 1962, MR, box 1, folder 4.

those by which the production of material goods is regulated" (Polanyi 1962).[32] As he put it, "in the free cooperation of independent scientists we shall find a highly simplified model of a free society" (Polanyi 1962, p. 54). Polanyi contended that governmental support of science, though welcomed, should not imply any control of scientific enterprise, either in the form of planning or regulation, since this would undermine the ideal of autonomy of the self-governing scientific community. Referring to the movement of left-wing British scientists, most prominently to Marxist John D. Bernal's plea in support of planning science in Britain, and mentioning in passing the "reports from Soviet Russia," Polanyi stated:

> We may sum up by saying that the movements for guiding science towards a more direct service of the public interest, as well as for coordinating the pursuit of science more effectively from the centre, have all petered out. Science continues to be conducted in British universities as was done before the movement for the social guidance of science ever started. And I believe that all scientific progress achieved in the Soviet Union was also due - as everywhere else - to the initiative of original minds, choosing their own problems and carrying out their investigation, according to their own lights.[33]

Science, according to Polanyi, should maintain its traditional - "Little Science" - mode of organization and governance, functioning as a decentralized network of independent self-coordinated initiatives. This mode of organization, he argued, provided science with its crucial strength and should be maintained even when scientific enterprises take advantage of government subsidies and support.[34]

The views expressed in the two 'position papers' written by Polanyi and Weinberg framed the discussion of these issues in the subsequent issues of *Minerva* - "the *Minerva* debate on scientific choice," as one of the participants, philosopher

[32]On Polanyi's political philosophy see Nye (2011).

[33]Polanyi 1962, p. 66.

[34]As Mary Jo Nye had argued, these views were rooted in Polanyi's career and experiences he had in the 1920s and early 1930s as a physical chemist in the Kaiser-Wilhelm-Gesellschaft Institutes in Berlin-Dahlem- the institutions that enjoyed the government support while at the same time not limiting the scientists' autonomy (Nye 2007).

Stephen Toulmin, called it (Toulmin 1964). Shils commissioned papers from scientists and social theorists suggesting that they respond to and comment upon the views expressed by Weinberg and Polanyi.

One of the first contributors to "the *Minerva* debate" was John Maddox, British physicist-turned-science writer and soon-to-become the editor of *Nature*. Maddox argued for moderate planning of science, citing as an example British Biological Research Committee of the Royal Society established in 1961 in order to plan more generous support for biological sciences in Britain (Maddox 1964, p. 142). While Polanyi referred to British Leftist scientists' movement of the 1930s, Maddox related his discussion to the plea for the accountability of science voiced largely by conservative British scientists, who, similarly to British Marxist scientists of the 1930s, supported control and regulation of science from the outside. Thus, the organized planning and control of science, Maddox argued, might go well enough without transforming the scientists into socialist sympathizers.

The recognition of the fact that the political realities of post-WWII years have increasingly turned science into a cultural weapon and, as Maddox put it, into "the harbinger of success in the Cold War," became another concurrent theme in the discussion of Big Science and its implications on the pages of *Minerva*. As Toulmin argued, science, simultaneously with becoming "Big," was also becoming a "political constituency": such exemplary Big Science operations as NASA, for example, "instead of being a purely administrative agency, ... is ... a political agency taking political decisions: 'a state within a state,' to which Congress has delegated some of its powers under the Constitution" (Toulmin 1964, p. 355).

Big Science clearly distorted the Polanyian image of science as spontaneous pursuit of knowledge by scientists-citizens living in a self-governed republic of science free of control and regulation. It was obvious that the long-cherished ideal of the self-governing autonomy of science was in conflict with the Big Science realities of the day. In his review summarizing the "*Minerva* debate," Toulmin concluded that Polanyi's image of the "republic of science" was perhaps out-of-date:

> In real life, the republic of science cannot stand apart from the general commonwealth. Back in the 1930s, Polanyi's campaign to defend the autonomy of science against ... state centralism had a real point. By the 1960s, the need for academic science to be self-governing seems to be being conceded even in Russia and Polanyi's protestations are - surely - more insistent than they need be. As the social sciences too approach their coming-of-age, his distinction between the republic of science and the rest of the community becomes excessively disjunctive. The urgent question today is, rather, how the self-governing republic of science is to be integrated, not only into the broader academic confederation, but into the whole community of citizens.[35]

Weinberg added another twist to the "*Minerva* debate." Big Science, in Weinberg's view, required what might be called "Big Science Studies" - an independent and decentralized expertise which would provide a systematic study of Big Science mode of research and advise the government accordingly (Weinberg 1963, p. 160). The choices rationalizing decisions for the allocation of funds for Big Science projects, Weinberg argued, should be made not by politicians but by "some well-informed observers" - experts not in sciences *per se* but rather in the *studies of science* as a social and political institution. "For this reason alone philosophic debate on the problems of scientific choice should lead to a more rational allocation of funds" (Weinberg 1963, p. 160). As Weinberg jokingly remarked in retrospective, his papers in *Minerva* launched his own "career as a moonlight philosopher of scientific administration," while the "*Minerva* debate" contributed to the recognition of "the importance of philosophic examination of

[35] Toulmin (1964), p. 354.

the sanctions for public support of science" and stimulated "something of a cottage industry in the philosophy of science policy" in the 1960s (Weinberg 1996, p. 42).

Overall, during the 1960s the CCF sponsored a network of influential magazines, organized large and small international conferences and seminars on a wide range of topics, including those on science and its role in broader culture, society and contemporary politics. By the mid-1960s, the CCF was regarded as a big success. The moment of rupture came in 1967, when a series of publications in the U.S. media exposed the close linkage between the CCF and the CIA. The CIA connection presented an ultimate - and very sensible - test for the theoretical discussion of the societal effects of the "governmentalisation of science" and the consequences of governmental - statist or political - interference with science's affairs. During the 1960s, criticism of the CIA's covert apparatus and coercive measures accelerated to the extent unknown during the 1950s.[36] The CIA's infamous involvement in the operations in Iran and Guatemala in the 1950s, and Cuba and Vietnam in the 1960s, made the question of "governmentalisation of science" and the issue of "the indirect as well as the direct influence of the Cold War on the role of science and scholarship" (to quote Shils' editorial) as much a moral as an epistemological one for the CCF-associated intellectuals.

[36]See the discussion of the CIA and its broad albeit clandestine involvement in funding scientific research in various fields, and scientists' general willingness to aid the CIA in providing scientific intelligence on atomic, biological, chemical weapons as well as basic science fields in the 1950s in Doel and Needell (1997). Discussing the ways different scientists involved in intelligence gathering had tried to negotiate, reconcile and/or in various ways struggled with fundamental differences between the "ideals and values" of intelligence gathering and those of science, Doel and Needell argued that these experiences "have profoundly altered the image and practice of science in postwar America."

3.4 "Who Paid the Piper": the CIA Connection and the Moral Crusade of the CCF in 1967

The news erupted in 1967, when the *New York Times* published a series of articles on CIA covert activities, which included references to the CIA's close links to the intellectuals' organizations, including the CCF.[37] The journalists' investigations were seconded by the public revelations of the man who supervised the cultural activities of the CIA, Thomas Braden.[38] As Braden revealed, the CIA "placed one agent in a Europe-based organization of intellectuals called the Congress for Cultural Freedom ... and another agent became an editor of *Encounter*" (Braden 1967).

The revelation caused public outrage. Not only has the role of the CIA been to finance the CCF, its agents were incorporated within the cultural organization with the explicit aim to promote "anti-communist programs" within the Congress. What can a "free thinker" say about "freedom," asked the *Sunday Times* of London in May 1967, "when he finds out that his free thought has been subsidized by a ruthlessly aggressive intelligence agency as part of the international cold war?"[39] Another journalist similarly pointed out that the fact that the intellectuals employed by the CIA with or without their knowledge were "being used for

[37]As the *New York Times* reported in May 1969, the CIA funded a bunch of "anti-Communist, but liberal organizations of intellectuals such as the Congress for Cultural Freedom, and some of their newspapers and magazines. *Encounter* magazine, a well-known anti-Communist intellectual monthly with editions in Spanish and German, as well as in English, was for a long time - though it is not now – one of the indirect beneficiaries of CIA funds through the arrangements that have never been publicly explained" (Lasch 1969).

[38]Braden represented a new type of bureaucrat, equally at home in government and in academic circles. Before joining the CIA in 1950, he was an executive secretary of the Museum of Modern Art (MOMA); later he became president of the California Board of Education, where he defended a liberal view of academic freedom against those who wished to ban J.D.Salinger's *The Catcher in the Rye* from schools' libraries (See on Branden and his revelations Lasch (1969)).

[39]Cit. in Lasch 1969.

concealed government propaganda," made a "mockery" of intellectual freedom.[40] As Christopher Lasch, the author of the first comprehensive account of CCF history, put it, "the whole wretched business seemed inescapably to point to the conclusion that cultural freedom had been consistently confused with American propaganda, and that 'cultural freedom,' as defined by its leading defenders, was - to put it bluntly - a hoax" (Lasch 1969, p. 104-5).

For most of the CCF associates the revelation about the Congress' direct link to the CIA did not come as a surprise. As Sidney Hook admitted in his autobiography, "I have heard, like almost everyone else, that in some way the CIA was involved in funding the Congress. Everyone mentioned it, even though no one had any hard evidence. ... In my own mind I had no doubt that the CIA was making some contribution to the financing of the Congress. ... Everyone involved in the activities of the Congress had heard rumors of covert CIA support" (Hook 1987, p. 451). Most of the Congress members did not discontinue their membership when they became aware or suspicious of the CIA funding of the CCF activities, assuming that as long as they are not dictated or controlled in their intellectual activity they can claim their intellectual independence and integrity.

The general line of defense taken by the CCF intellectuals was expressed in a response of Arthur Schlesinger, Jr., to the CCF crusaders. As he argued in his open letter published in *Book Week* in September 1966, the attack on the CCF was ill-conceived, based on the "apparent inability [of accusers] to conceive any reason for opposition to communism except bribery by the CIA" (Lasch 1969, p. 106). Within few days following the revelations publicized by *New York Times*, several distinguished CCF associates – John K. Galbraith, George Kennan, Robert Oppenheimer and Arthur Schlesinger, Jr. – sent the letter to the editors of the *New*

[40] *The New York Times*, 27 Mar 1967 (cit. in Lasch 1969).

York Times, stating that "on the basis of our own experiences with the Congress over the past 16 years - with its seminars, its artistic festivals, its magazines, its staff - we can say categorically that we have no question regarding the independence of its policy, the integrity of its officials, or the value of its contribution. In our experience the Congress ... has been an entirely free body, responsive only to the wishes of its members and collaborators..."[41]

Neither *Minerva* nor its editor was directly attacked in the press, but Edwards Shils felt as disturbed as others by seeing that their valuable enterprise was being discredited. Like other CCF associated intellectuals, he insisted that at no point of its history his journal's editorial independence was corrupted and that the CCF associates had loyalty only to the "commitment to cultural freedom." Shils went so far as claiming that the CCF was not "political." Writing to Crawford Goodwin, professor of economics at Duke University and program officer in charge of European and International Affairs at the Ford Foundation, Shils stated: "it might be reasonably claimed that the Congress ... was not political. It sought to promote the understanding and solution of fundamental problems which concern serious intellectuals ... cutting across the boundaries of nationality, party, intellectual field and discipline. ... It created and fostered a sense of affinity among these intellectuals in a way which is, I think, unique in the history of the present century."[42]

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[41] "Copy of the text of the letter sent on May 4th to the Editor of the New York Times by John Kenneth Galbraith, George Kennan, Arthur Schlesinger, Jr." [1967], IACF/ICFUR, box 1.

[42] Edward Shils to Crawford Goodwin, n/d. IACF/ICFUR, box 1, folder 12. As a counter-measure against the accusations thrown by the press Shils suggested to launch a project aiming at producing a well-documented history of the CCF, which would imply the organization of the archive of the CCF records documenting its activities and the oral history interviews with the "intellectual figures who played leading roles in the history of the Congress," such as Michael Polanyi, Raymond Aron, Arthur Koestler, Willy Brandt, George Kennan, Daniel Bell, as well as with the members of the CCF executive staff - Michael Josselson, Nicolas Nabokov, Francois Bondy, Konstantin (Cot) Jelenski, Pierre Emmanuel, and Melvin Lasky (IACF/ICFUR, box 1, folder 12.)

This line of defense failed, however, in view of Braden's revelations. The independence and autonomy of an organization was the provision of CIA involvement, Braden explained, because real independence, not just the semblance of it, was expected to give the greatest credence to the organizations "co-opted" by the CIA. As Braden described the rules that guided the international organization of the CIA: "Use legitimate, existing organizations; disguise the extent of American interest; protect the integrity of the organization by not requiring it to support every aspect of official American policy..." (Braden 1967, p. 14). As Lasch aptly put it, no matter how the intellectuals perceived their sense of freedom and control over their scholarship, the CIA regarded them as instruments of its own purpose (Lasch 1969).

The CCF leaders were stuck between a rock and a hard place. The complexities of the interrelation between politics and science that they had been discussing at length in the seminars and study groups, now appeared to be part and parcel of their personal experience with the CCF. In some ways, the CCF intellectuals were reasoning according to the very logic of "Big Science" that they were disentangling, accepting the fact, with Weinberg, that Big Science had not only changed science, but also the way the relation between science and the state is understood. If Big Science was political, then "Big Scientists" - those who live in the world of Big Science – could only be relative in their perception of their freedom.

The positions taken in the "*Minerva* debate" continued to resurface, now on the "moral plane." The "intellectual confrontation" staged by Shils on the pages of *Minerva* resurfaced as a "moral confrontation" among the CCF associates now facing the moral conundrum. Polanyi, once again, presented a "dissenting" view. Reasoning consistently with his theoretical defense of "Little Science," Polanyi's major concern was with a "little person" - Michael Josselson, the dedicated driving force behind the Congress' activities who, after he had been revealed to be a CIA officer, was ostracized by other CCF leaders. Polanyi called the Assembly's decision to "eliminate" Josselson "another darkness at noon" alluding to Arthur Koestler's novel and the ex-communists' disillusionment with the communist cause as, first of all, morally wrong. Moreover, it was not "rational" either: the "elimination" of Josselson did not resolve the moral conundrum; it was only a way to claim the ignorance of the CCF connection to the CIA, a position that Polanyi found neither moral nor rational to hold. In his letter to Raymond Aron, Polanyi asked: "What kind of figure are we going to cut? Are men like you or me ... going to declare that in 15 years we did not notice that we were being manipulated to serve sinister purposes? Are we going to proclaim our awakening, a new version of The God that Failed?" [43]

The fate of their "valuable enterprise," in Polanyi's view, was inseparable from the fate of its "little" leader, even though it would imply "los[ing] face" and admitting to knowing about the connection between the CCF and CIA. Appealing to the Congress' support "for saving Mike [Josselson], the Congress, and our honour," Polanyi took his defense to the end, stating:

> I would have served the C.I.A. (had I known of its existence) in the years following the war, with pleasure. We were faced with an ubiquitous madness, supported by an empire and organized on conspiratorial lines. ... In the years after 1950 we battled against a phalanx of Stalinist or Stalinisant intellectuals throughout Europe, for the vindication of free thought, which was despised and ridiculed by those who are now forcing us to dismiss Mike Josselson, because he had accepted the support of like-minded American officials, who appreciated the ideals he was fighting for.[44]

In the end, it was the forced resignation of Josselson rather than the

[43]Michael Polanyi to Pierre Emmanuel, 9 Apr 1968. MPP, box 6, folder 13.
[44]Michael Polanyi to Raymond Aron, 9 May 1967, MPP, box 6, folder 10.

revelation of the CIA funding that compelled Polanyi to resign from the CCF.[45]

The revelation of the CIA connection, although it caused a public outrage, did not lead to the demolition of the organization. In the same year the CCF was quietly resurrected under a different name – the International Association for Cultural Freedom (IACF) – with the funding secured by a grant from the Ford Foundation.[46] Pierre Emmanuel, French poet and essayist and a long-time CCF associate, was elected the Director of the Association, while Shepard Stone, the Ford Foundation President and Chief Executive, became the reorganized Congress' President. The resolution of the last CCF conference in September 1967 announcing the reorganization of the organization, stated: "the Association ... emphasizes the non-partisan, critical spirit and rational approaches to problems."[47] With the exception of a few resignations, the reorganized Congress continued to rely on old networks. IACF also "inherited" from the Congress its magazines and continued its practice of the international seminars and workshops.

A more subtle change involved refurbishing the Congress' conceptual framework. The revelation of the CIA connection and the heated public debate it unleashed did not resonate with the "end of ideology" rhetoric with its resentment and explicit avoidance of "emotion in politics, and of politics of passions and hatreds," as Daniel Bell put it in his classic *The End of Ideology* (Bell [1960]/1988, p. 415). The "end of ideology" as the official slogan of the CCF did not continue into the late 1960s and 1970s. Instead, a similar concept had advanced within the reorganized Congress: the "theory of post-industrial society." It was mostly a change in the rhetoric rather than in the content, as the two concepts were as

[45] As Polanyi wrote to Pierre Emmanuel announcing his decision, "I expressed the feeling that I could not remain connected with our organization, if we decided to eliminate Michael Josselson from it. ... I beg you, therefore, to accept my resignation from the community to which I have so long adhered." Michael Polanyi to Pierre Emmanuel, 9 Apr 1968. MPP, box 6, folder 13.

[46] "Press Release. Monday, 2 Oct 1967." MPP, box 6, folder 10.

[47] "Some facts about IACF," 1970. IACFP, Series III, box 80, folder 6.

closely interconnected as the individuals who framed them.

3.5 A Refurbished Intellectual Framework of the CCF in the 1970s: the Theory of Post-Industrial Society

The discussion of the social and political consequences of Big Science articulated awareness that the growth of science, for better or for worse, had significant implications for modern society. By the 1970s it was a widely held view, expressed by social theorists such as Jacques Barzun, Spencer Klaw, Edward Shils, and John Galbraith, who argued in different way that access to the power of the atom, the computer revolution, the exploration of the cosmos, and the great cultural, social, economic, and political significance that science had come to hold in Western societies, were opening a new phase in the history of mankind. The theory of post-industrial society was conceived as the descriptor of this new post-WWII social structure, which attributed the major role to advanced science and technology, and offered new visions of social change and social structure.

The theory of post-industrial society had achieved wide circulation in the 1970s, especially following Daniel Bell's renowned 1973 book, *The Coming of Post-Industrial Society: A Venture in Social Forecasting,* the standard reference for this concept (see Brick 1992). Like its predecessor, the "end of ideology," the post-industrial society theory was conceived within the CCF network of intellectuals. It was first presented by Bell in 1967 in his "Notes on the Post-Industrial Society" published in the journal *The Public Interest.* Within the CCF network, the concept was formulated and circulated since the mid-1960s, within the CCF Study Group led by Bell.[48]

In the 1970s, the IACF organized a series of seminars, devoted to "post-industrial society." The first in this series was organized by Bell in Zurich in June 1970.[49] Invited participants at the Zurich meeting included old-time CCF associates: Shils, Lipset, Aron,Galbraith and Jelenski, along with few Soviet and Eastern European specialists (Cyril E. Black and Martin Malia), as well as Polish sociologists Zygmunt Bauman and Leszek Kołakowski. Bell also invited British moral and political philosopher Alasdair MacIntyre, known for his influential critique of both Marxism and right-wing liberalism, to whom Bell described the agenda of the meeting: "The group we are bringing together is composed primarily of sociologists and political scientists who have been working on the common theme of the relationship of social structure to culture. We all know that in recent years there have been some questions about the convergence of advanced industrial societies. ... We feel that an exploration of the questions will provide some answers to some of the most interesting theoretical questions in contemporary sociology."[50]

Indeed, the post-industrial society implied a new ("post-Marxist," in Polanyi's words) vision of social change and social transformation, which presupposed not conflict and a revolutionary reorganization of society, but an evolutionary drift towards modernity in a scientifically and technologically manipulated world. Not only "interesting" theoretically, this framework turned the discussion of social change into a civilized, "non-ideological" discussion of economic development - a gratifying experience for the theorists of the "end of ideology."

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[48]At the CCF Study Group meeting in 1965, Bell already was using the wording "post-industrial society" ("Memorandum," n/d, IACFP, Series III, box 99, folder 1). For the discussion of the origin of the "post-industrial society" concept see Brick (1992), especially p. 351.

[49]"Draft of Press Release," IACFP, Series III, box 79, folder 6.

[50]Daniel Bell to Alasdair MacIntyre,26 Jan 1970. IACFP, Series III, box 79, folder 7.

The "post-industrial society" rhetoric fitted well in the changing context of the Cold War politics. By the late 1960s a partial *détente* with the Soviet Union and the "communist" countries in Europe made the anticommunist rhetoric of the 1950s obsolete, although it, of course, did not make anticommunism obsolete. The anticommunism of the late 1960s - 1970s required a new rhetoric, and the "post-industrial society theory" supplied exactly what was needed, presenting the images of the emergence of a unitary "post-industrial society" in both mature capitalist and socialist "techno-structures" - the so-called "convergence theories," widely promulgated throughout the 1970s. The major idea behind the "convergence" theory was the agreement that scientific and technological performance and, by extension, economic performance were the defining elements of modern life and a measure of success in the Cold War. As Bell affirmed, behind the argument about convergence lay the recognition of the fact that, in Bell's words, "market was rediscovered in the Communist socialist world, and the market was losing its importance in the Western economies."[51]

The theory of "post-industrial society" preserved the zeal of the "end of ideology" normative stance. Encouraging and promoting a sophisticated discussion of the politics of science, the "post-industrial society" framework turned science, its history and its politics into a central topic, indeed *the* topic. The rhetoric of post-industrial society emphasized the linkage of knowledge and power in a society where the intellectual played the central role. This linkage did not imply that the intellectuals became the new ruling class within the post-industrial society, but rather suggested that the expanded social functions of science, the development of computer-based technologies and the growth of both public and private funding for science made intellectuals themselves and the intellectual institutions (most

[51] Transcript of Proceedings of the conference "Post-Industrial Society and Cultural Diversity," 12-14 Jun 1970. IACFP, Series III, box 80, folder 3.

notably the universities) socially central in contemporary society, in ways that tied academic institutions and centers of political power far more intimately than they had been in earlier academic institutions.

Within this framework such themes as the relation of science and society, political role of science and technology in modern society, and, more generally, the relation between knowledge and power, moved to the very center of the IACF agenda. During the 1970s, the issues on the nexus of science-society-politics became main topics of the conferences organized by IACF: "structure of knowledge," "social goals of technology," "history and its politics," "knowledge and beliefs" were the concurrent themes in the IACF seminars' outlines and proposals. The network of participants of the meetings sponsored by the IACF also expanded, to include or to seek the participation of scholars who later became associated with the conceptual core of the field of science studies: Thomas Kuhn, Everett Mendelsohn, Joseph Ben-David, Stephen Toulmin, Gerald Holton, Yehuda Elkana, Mary Hesse, Jürgen Habermas, and Jacques Ellul were on the top of IACF's lists of "proposed participants."

3.6 The IACF's Conferences on Science and Politics: an Institutional Rival with Science Studies.

1970 marked the twentieth anniversary of the CCF. The anniversary was not emphasized as such but was quietly 'celebrated' by a series of international meetings organized under the auspices of the reorganized Congress in the anniversary year: the meeting on "post-industrial society" was held in Zurich in

June, the conference on "technology and its social role" took place in Aspen, Colorado, in late August - early September, and the smaller conference on "Creative Imagination" was held in Poigny la Foret (France) in October.[52]

The conferences organized by the IACF in the 1970s expressed a founding set of anxieties for what soon became the academic field of Science Studies. Jelenski outlined this set of framing issues in his 1969 proposal for the "major Congress seminar" in Aspen, Colorado, in 1970.[53] His proposal, aptly entitled "IACF and the Crisis of Advanced Industrial Societies," encapsulated all the major preoccupations of the relativist turn. As Jenenski stressed, the transition to post-industrial society was occurring in the period of "crisis," which encompassed an environmental crisis resulting from the realization of the widespread 'side-effects' of technological developments; a cultural crisis, as a result of the "counter culture" movement; and a crisis in philosophy, the result of the failures of reductionism and, more generally, of positivist philosophy. The symptomatic examples of the "crisis in philosophy" included Karl Popper's critique of positivism, Michael Polanyi's conception of "Personal Knowledge" as a reaction against reductionism, Arthur Koestler's "fight against reductionism," as well as the revisionist trends within Marxism coming along with the rediscovery of young "Hegelian" Marx, both in the West and in the Communist countries.[54] As Jelenski concluded, "the justification for the IACF

[52]IACFP, Series III.43, box 79-84. The "legitimation crisis" of 1967 did not stop the Congress' colloquia and conferences, which continued almost without any interruption. As Jelenski noted in the 1969 memo, "The IACF Seminar Program has been concerned, ever since late 1967, with problem of man and his environment in advanced industrialized societies: the IACF conference on Mass-media and Cultural Creation (Venice, 1967), The United States: Its role and its Impact in the World (Princeton, 1968), The Student Rebellion and the Future of Advanced Industrial Societies (Alghero, 1969), Pacifism and Violence: Their Uses and Limitations as Instruments of Reform (Bergneustadt, 1969) ... In June 1969, the IACF will hold a conference in Konstanz on "Post-Industrial Society and National Variations" (K.A. Jelenski, "IACF and the Crisis of Advanced Industrial Societies," 16 Nov 1969. IACFP, Series III, box 80, folder 6, on p. 3)

[53]"Technological Change and Cultural Options," outline of the conference, n/d. IACFP, Series III, box 80, folder 6.

[54]K.A. Jelenski, "IACF and the Crisis of Advanced Industrial Societies," 16 Nov 1969. IACFP, Series III, box 80, folder 6

choosing this theme for its major seminar in 1970 is that [the post-industrial society] does have these philosophical and cultural implications and that, to a certain extent, it is at the heart of the new ideological divisions in Western industrial societies."[55]

In view of the organizers of the Aspen meeting, the IACF was "in a particularly favorable position to provide a bridge and a platform" to bring these diverse themes together, focusing on the problem "of the implications of technological development for politics."[56] Not only did the IACF, as the CCF's successor, help to establish a network of intellectuals and leading scientists from Western Europe and the United States across the disciplinary and national frontiers, it institutionalized this network in the form of regular seminars and study groups. In this way, Jelenski pointed out, it helped to articulate the issues and to create a public space for the discussion of how the scientific rationality and decision-making of modern industrial society is intertwined with significant transformations in the realm of politics and political theory. The relation between these two areas of concern was almost nonexistent outside the CCF network, conference organizers emphasized: with the exception of the Frankfurt School and, more specifically, Jurgen Habermas with his influential text "Technology and Science as Ideology" (1970) and Herbert Marcuse's *One-Dimentional Man*, liberal political philosophy of the late 20th century was marked by the striking absence of any discussion of the role of science and technology, or turning technology "into a scape-goat, perceived as having become an end in itself":

> At a risk of simplification, it could be said that research stressing the technological and environmental aspects [of the crisis] tends to

[55] "Technological Change and Cultural Options," outline of the conference, n/d. IACFP, Series III, box 80, folder 6

[56] K.A. Jelenski, "IACF and the Crisis of Advanced Industrial Societies," 16 Nov 1969. IACFP, Series III, box 80, folder 6

> be future-oriented, optimistic, more interested in the 'progress' of civilization than in its 'discontents.' The cultural analysts, on the other hand, tend to be pessimistic and nostalgic, fascinated by the past. We do not wish to imply that everything is polarized between the euphoric extrapolation of GNP curves, and the pessimism of Marcusian prophecy, but we can safely say there is little continuous effort of bridging the technological and the cultural aspects of contemporary crisis.[57]

Building on the previous CCF experience of "bridge-building" the Aspen meeting would contribute to the reconciliations among these "camps," Jelenski concluded.

IACF not only relied on the old CCF network of intellectuals, but also sought to expand it. Thomas Kuhn was on the top of the list of invited participants of the 1970 Aspen seminar.[58] Thomas Kuhn's *Structure of Scientific Revolutions* (1962) appeared in the midst of the discussion of the social and political consequences of Big Science on the pages of *Minerva*. The connotations of Big Science were only implicit in Kuhn's work, although his commentary on Big Science can be read between the lines of his book. As a physicist-turned-historian, Kuhn depicted the world he knew best - the practices and the political economy of physical science that went through revolutionary changes in the wake of WWII. Kuhn's scientists were team workers, "organization men" who followed instructions and defended their "paradigms" - these were the scientists of Big Science, not "little science." Not surprisingly, Shils considered publishing a review of Kuhn's *Structure* in *Minerva*, commissioning it from Toulmin. Shils wanted the review to do some "translation work" for Kuhn's *Structure*, relating it to the discussion of science policy and science politics: "...the main point about Kuhn is that it should be

[57] K.A. Jelenski, "IACF and the Crisis of Advanced Industrial Societies," 16 Nov 1969. IACFP, Series III, box 80, folder 6

[58] "Material and possible participants for the Social Control of Technology Seminar," n/d. IACFP, Series III, box 81, folder 1.

centered around the implication of Kuhn's conception of scientific development for the planning and administration of science - not an easy task!"[59] The review of Kuhn's *Structure* never appeared in *Minerva*: the *Structure* appeared too "apolitical" to contribute to the ongoing discussion of Big Science.[60]

Few years since "the *Minerva* debate" on Big Science, Kuhn's *Structure* was seen by the Aspen conference organizers as an articulation of the belief that fundamental science is not amenable to forecasting and planning: "Kuhn's basic position is that fundamental science can only deteriorate by contact with society and social needs. ...Any attempt to introduce stimulation or focus from outside can be only harmful according to the author." [61] In view of the organizers, Alvin Weinberg, on the other hand, suggested a view on the role of science in society that was more concrete, less sociological and more political than the one presented in *Structure*. No wonder that Weinberg, as the author of the essays in *Minerva* on social and political implications of post-industrial society and its major feature, Big Science, was also among welcomed candidates for the Aspen conference.[62]

Neither Kuhn nor Weinberg made it to the Aspen meeting. Overall, although the meeting generated much interest at the preparatory stages, it was not realized as planned. From the point of view of at least some of the participants, the meeting failed to live up to the original expectations. John Maddox in his

[59] Edward Shils to Stephen Toulmin, 19 Feb 1964, MR, box 3, folder 10.

[60] Somewhat ironically, while Kuhn was found too "apolitical" to be reviewed on the pages of *Minerva*, Kuhn himself returned the same argument in his review of Toulmin's work, writing about Toulmin's and June Goodfield's co-authored account of the history of the concept of time from classical antiquity to the 20th century, *The Discovery of Time:* "The development of mining and geological mapping, of plant and animal breeding, all contributed significantly to the evolution of the ideas with which this volume deals. And so, I presume, did the political and institutional development of the countries within which the ideas evolved. Though the authors have brilliantly described the main stages in the development of man's sense of historical change, they have not always seen with clarity the processes that connect those stages" (Kuhn 1967).

[61] "Material and possible participants for the Social Control of Technology Seminar," n/d. IACFP, Series III, box 81, folder 1.

[62] ibid

report on the Aspen conference noted that in the end it appeared impossible for the participants to come up with any "unified view ... about the relationship between technology and the rest of society" (Maddox 1970, p. 1003). Maddox's report was aptly entitled "Intellectuals of the World Disunite" and presented the picture of the conference that "seemed to begin well enough, but in the end the hundred or so participants had little to say except that they have no single view about the place of technology in the modern world" (Maddox 1970, p. 1003). Likewise, another participant, science journalist Maurice Goldsmith, described the conference in similar terms: "...we were bound to find difficulty in securing a common denominator. There was little agreement, for example, on the nature of 'the crisis' that faces us ... there was ignorance about what was meant by 'technology' ... there were too many participants, with ill-formulated terms of reference, to secure a unanimously approved statement...." (Goldsmith 1970, p. 28).

During the 1970s, the IACF agenda was more and more overlapping with the new professional community - the science studies scholars and professional historians and philosophers of science. The illustrative example is the IACF seminar "The Basic Structure of Knowledge or What we May no Longer Take for Granted: The Critique of Science," held at the Aspen Institute in Berlin from September 4-9, 1975. The aspiration behind this meeting was, as Jelenski put it, to assemble "some of the most authoritative voices in this field. ... This alone allows us to expect a hearing which could not be expected otherwise." [63] The towering "authoritative voice" was Daniel Bell. However, this time, as IACF officers Jelenski and Adam Watson suggested, the IACF would organize it "with one difference: while the participants of the Zurich seminar were chosen among

[63]K.A. Jelenski, "Culture and its Discontents in Post-Industrial Society (An IACF project)," 6 May 1974. IACFP, Series III. 49a, box 93a, folder 2.

scholars who largely shared Daniel Bell's conception of post-industrial society, I thought we should, this time, invite intellectuals representing different points of view."[64]

"Different points of view" that the organizers sought to have represented at the meeting included Thomas Kuhn, Gerald Holton, Karl Popper, Richard Lewontin, Robert Young, William Provine, Mary Hesse, Hilary Putnam, Marjorie Green, Paul Feyerabend, and Willard Quine, among others luminaries who were invited to participate. Under the general title "Reexaminations: A Critical Review of Contemporary Conditions in Science, Philosophy, and Culture," and with the matching grant from the Fritz Thyssen Stiftung, the IACF proposed to organize three consecutive seminars, focused respectively on critique of science, critique of philosophy, and critique of culture.[65] Three seminars were designed to bring together twenty to twenty-two participants chosen among leading scholars in each fields, with a small "core group" of participants, which originally included Daniel Bell, Pierre Emmanuel, Jurgen Habermas, Leszek Kolakowski, Jacques Ellul, Edward Shils and Stephen Toulmin, who were expected to be present at all three seminars, thus ensuring the continuity of the project. [66]

As the organizers admitted, the topic of the conference was "the 'tarte a la crème' of contemporary intellectual journalism," in the sense that it has been discussed "at dozens of conferences organized by the United Nations, the Council of Europe, the Nobel Symposium, and innumerable American and European foundations and private organizations, including our own conference on *Technology, Social Goals and Cultural Options* at Aspen in 1970."[67] What made the organizers

[64]ibid

[65]"Reexaminations. A Critical Review of Contemporary Conditions in Science, Philosophy, and Culture." IACFP, Series III. 49a, box 93a, folder 2.

[66]ibid

[67]K.A. Jelenski, "Culture and its Discontents in Post-Industrial Society (An IACF project)," 6 May 1974. IACFP, Series III. 49a, box 93a, folder 2.

think they could make a distinct contribution was the fact, as Jelenski stressed, that "the CCF played the role of a forerunner in this discussion" with its engagement in the topic going back to the CCF Conference on Progress in Freedom in Berlin in 1960.[68] Like Colorado's Aspen meeting of 1970, however, the conference in Berlin's Aspen Institute in 1975 was not realized as planned. Some of those who initially expressed interest finally did not come. Both Thomas Kuhn and Stephen Toulmin, who were regarded as key participants by the organizers, cited professional meeting of philosophers of science as their major reason of their decline of the invitation to take part in the conference.[69]

This conference marked the general decline of the Congress activities, by this time largely focused on science in its relation to societal issues. One of the obvious reasons of the decline of the IACF, which never recovered the level and the success the Congress enjoyed throughout the 1960s, was the public revelation of the CIA connection and the "moral crusade" of the Congress in 1967. At the same time, the IACF was by no means ostracized in the 1970s. The "death blow" of the Congress was not the CIA connection, but a much more prosaic force: the professionalization of science studies within academia during the 1970s, which successfully marginalized the early, semi-institutional niches for science studies. By the mid-1970s, the Congress had a rival in the emerging field that Roy MacLeod characterized as a "new field, fashionably called 'liberal studies of science"' (MacLeod 2003). In some sense, the Congress became a victim of its own success. While in the 1960s the Congress' seminars and its network of

[68]K.A. Jelenski, "Culture and its Discontents in Post-Industrial Society (An IACF project)," 6 May 1974. IACFP, Series III. 49a, box 93a, folder 2.

[69]As Thomas Kuhn explained to Shepard Stone, "the seminar to be held by Aspen Berlin ... unfortunately ...overlaps ...with the forthcoming meeting [of] the International Congress of Logic, Methodology and Philosophy of Science, to which I and probably others of interest to you are already committed. Though your topic attracts me very much, there is no way in which I can arrange to be with you." Thomas Kuhn to Shepard Stone, 14 May 1975, IACFP, Series III.49a, box 93a, folder 6.

scholars and scientists helped to create a semi-institutionalized (or networked) niche for what later became "science studies," in the 1970s it became marginalized because of the institutionalization and professionalization of that very field they aspired to establish. The self-dissolution of IACF was a question of time under the circumstances, and indeed in 1979 the organization quietly dissolved itself.

3.7 Conclusion to Chapter Three

Oxford historian of science Robert Fox, commenting on the current generation of Oxford postgraduate students many of whom "see the academic history of science of today as born with Kuhn," noted that "their perception seems to overstate the influence that *Structure* has had on the practices of today's historians ... [carrying] with it the risk of undervaluing currents that not only throw light on Kuhn's questions, but also pose new ones" (Fox 2006, p. 410, 427).[70] Against this background, Fox argues that the roots of history of science and science studies as an academic discipline are rather to be found in the post-WWII "political context of great complexity," within which "even the spat that divided Marxists and non-Marxists ... was momentarily suppressed, and soon a circle of figures, with commitments across the political spectrum, found itself united in promoting the history of science *as a discipline encapsulating the goals of post-war reconstruction and reconciliation*."[71] The case of the Congress for Cultural Freedom extends and

[70]Resonating with Fox, historian of science Nathan Reingold, reflecting on the history of history of science as a discipline in America, noted that much of the developments that were decisive for forming the history of science community in the United States "occurred outside of history of science programs; [with] key works and trends antedated the writings of T.S. Kuhn which are often credited with producing the development of more socially attuned history of science. ... Individual historians like myself and others ... bridled at the implication that our mind-sets derived from Kuhn. Our concerns antedated his writings and had other roots" (Reingold 1996, p. 115, 117)

[71]Fox 2006l, p. 420 (emphasis added - EA). See also Robert Fox Oral History interview, transcript, British Society for the History of Science Oral History Project "The history of science

substantiates Fox's observation, illustrating the distinct way(s) the CCF-associated intellectuals understood, articulated and rationalized what Fox called "the political context of great complexity," and showing that the studies of science emerged in this context not merely as part of this articulation and rationalization but at the very center of it.

As the discussion on the pages of *Minerva* in the early 1960s demonstrates, the CCF-initiated discussion of the social and political consequences of Big Science and, more generally, of the relation between science and politics had been part of what cultural historian David Hollinger characterized as "a watershed in the history of discourse about science," and what another historian, Peter Novick, called "the epistemological revolution" of the 1960s (Hollinger 1995, Novick 1988). With the changes in the political economy of science that occurred during WWII, especially in the physical sciences in the wake of the Manhattan Project, associated with science's dramatically raised level of economic dependence on public resources and military patronage, the conventions for representing the scientific enterprise have also changed. Beginning in the early 1960s, scientists and social analysts responded to the political challenges of the time by promulgating the image of science as a concrete, historical, and interacting community of scientists-citizens rather than timeless and universal "science" with no national or political affiliations (Hollinger 1996, p. 101). As I argue in this chapter, the CCF greatly contributed to this "watershed" change in the perception of science. The loosely connected network of intellectuals associated with the CCF helped to invent a new subject, or set of subjects in the 1950s and 1960s, reconceptualizing science as a social activity, promulgating the view that science is inseparable from politics, and in various ways exploring the science-society-politics nexus.

in Britain, 1945-1965," Brotherton Library, University of Leeds (**Thereafter BSHSOHP**), BSHS 10/8/9.

The history of the CCF activities analyzed in this chapter also points to the roots of "science studies" outside academia and, initially, within the set of issues dealing with science policy, the organization of science and planning of science, at least during the 1960s. By the 1970s, however, the Congress was covering a broad set of issues on the nexus of science-society-politics, which was also the domain of a new professional community - science studies scholars and professional historians and philosophers of science. The establishment of the journal *Science Studies* in 1970 became a blow in the face of the CCF-associated "science studiers." Perhaps the founders of *Science Studies* did not fully realize the extent to which the launch of this journal put an end to the earlier enterprises. Roy MacLeod and David Edge, who founded the journal *Science Studies* (later renamed *Social Studies of Science*) in 1970, both recorded the moment of the creation of the journal in their recollections. As David Edge recollected in an oral history interview, while John Maddox, then the head of the Journals Division of Macmillan publishing house, was enthusiastic about the idea to establish a new quarterly journal called *Science Studies* (even hoping that it would become a weekly), Edward Shils, in Edge's words, "wanted to sabotage us, [saying] that we were committing academic suicide ... He wanted to stop us. ... He did not stop us, but he kept treating us like we were ... I don't know ... just nuts."[72] MacLeod gave a more polite version of the same story in his published memoir: "Edward Shils cordially discouraged me from doing anything of the sort. There was simply not enough good material, he said, with the implication that anything 'good' he would publish in *Minerva* himself" (MacLeod 2003). Shils, however, gave his own account of this moment in his letter to Shepard Stone immediately after the journal *Science Studies* was launched in 1970: "He [John Maddox] undertakes to establish another periodical

[72]David Edge Oral History interview, transcript, BSHSOHP, BSHS 10/8/7, on p. 52

on more or less the same subject as *Minerva* ... and he had one of his handy-girls prepare a promotional letter for this journal (it is called, I think, *Science Studies*) in which it is alleged that there is no competition between the magazines because *Minerva* deals only with relations between science and government. This is, of course, a caricature of the wide range of subjects treated by *Minerva*."[73]

Overall, the case of the CCF and its quest for "science studies," as part of their broader agenda to offer a renewed framework for liberalism, is suggestive of a more complex picture of the Cold War legacy (or legacies) in our profession than it is usually argued. The histories that trace the developments in history of science during the Cold War are usually the stories about the turn to internalist history (or intellectual history), coupled with the concern for disengagement from politics and ideology. Many historiands argue that one of the prominent symptoms of the Cold War university was the depoliticization of the social and human sciences (Engerman 2003).[74] Outside academia, however, the picture was different. This transnational and overtly political organization promulgated the view that science is inseparable from politics, and in various ways promoted the studies of political and social-economic dimension of science, through its conferences, workshops and seminars devoted to the discussion of science as a social and political

[73]Edward Shils to Shepard Stone, 6 Oct 1970, IACF/ICFUR, box 1, folder 16.

[74]As Anna Mayer, for example, showed in her work on the history of history of science profession in Cambridge, anti-Marxism became the defining feature of the professional identity that formed the intellectual agenda of the Department of History of Philosophy of Science in Cambridge in the early Cold War years, which promoted the image of scientific work as a disinterested journey of the mind, and institutionalized this image through its appointments policy in this period (see Mayer 2000). Many other historical accounts of the development of history of science in the Cold War has demonstrated, likewise, that the politics of representing science as an impartial neutral and apolitical affair was part of the cultural narrative of the Cold War, coupled with historians of science concern for constructing the disciplinary identity for themselves (see, for example, Enebakk 2009, Porter, 1990). At the same time, however, in might be argued that politics was always at the forefront of the concerns of many if not all scholars in anthropology, feminist studies, and sociology in the 1950s, 1960s, and 1970s. The creation of such new fields as Feminist Studies and other interdisciplinary programs in the wake of the civil rights movement, women's movement and decolonization can be seen as a reaction to both the disciplinary constraints in traditional departments as well as to the "depoliticization" of these departments.

institution. I would argue that the promotion of studies of science as a politically relevant area of expertise helped to legitimize the disciplinary identity of Science Studies and provided it with a semi-institutional niche before Science Studies were institutionalized within academia in the 1970s.

The case of the CCF may also illustrate the complexities and ambiguities of what has to be known as "cultural cold wars" and its legacy(ies). The outcomes of discussions on science initiated under the auspices of the CCF were *shaped* by the organization's explicit political agenda but not *determined* by the political demands. Although the studies of science during the Cold War encapsulated the political concerns and anxieties of the time, there was no single Cold War "party line." In this regard, the CCF and its engagement with the studies of science presents a story akin to other social sciences that have received ample support during the Cold War (area studies, behavioral science, human relations, development studies, American studies, and a host of other disciplinary and interdisciplinary fields), and that served national interest but not necessarily in predictable ways (Engerman 2010). The claims of the CCF intellectuals of being "independent" and "free" in their thinking would not survive the 1960s, as the revelation of the CIA sponsorship of the Congress shattered comfortable assumptions of scholars in the service of the state. Yet, their quest for "middle ground," reconciliation, and compromise, as part and parcel of their conceptions of scholarship and service, was effectively shifting the debate away from simplistic Cold War narratives of East-West competition.

This chapter, in part, is a reprint of the material as it will appear in the forthcoming issue of *Minerva* 2012. Aronova, Elena. 2012. The Congress for Cultural Freedom, *Minerva*, and the Quest for Instituting "Science Studies" in the

Age of Cold War, *Minerva* 50/3. The dissertation author was the only investigator and author of this paper.

Chapter 4

From “Scientific Humanism” to Asilomar: The Salk Institute for Biological Studies and its Programs in Biology in Human Affairs, 1960-1975

I often think of the Institute as a sculpture. This building, designed by Louis Kahn of Philadelphia, developed as a result of our thinking together. We felt it should provide an environment and an atmosphere in which both science and art could find a meeting place. The object was to bring together people who have concerns not only for their own

> *disciplines, or their own particular art, but who have a common interest in man. ... A further hope or desire on our part is to use the Institute as a social mechanism for bringing about communication, understanding, thinking together by seemingly diverse individuals... – the artist, the scientist, the manager of society, the philosopher, and the teacher .... The purpose of the Institute is to make this possible, to facilitate this.*[1]

Established in 1960 in La Jolla, California, the Salk Institute for Biological Studies, a small private research institution bearing the name of its founder, the inventor of the polio vaccine Jonas Salk, sparked the imagination of anyone who visited it in the 1960s. Housed in two stark Louis Kahn buildings, one populated with brilliant scientists and the other empty until the late 1960s, for more than six years since its official inauguration in 1960 the Institute lived largely in the future, and in the minds of the men who have envisioned and shaped it: Jonas Salk, the late Leo Szilard, Warren Weaver, Basil O'Connor, Jacob Bronowski, and C. P. Snow. The Institute defined its credo in terms of "the advancement and unification of knowledge relevant to the health and well-being of man."[2] Over the course of the first ten years of its existence, roughly from 1962 until the early 1970s, the Institute repeatedly attempted to launch visionary programs aimed at relating the new science of molecular biology to a wide range of social sciences and

[1] Jonas Salk, notes. Feb 1967. Jonas Salk Papers, University of California at San Diego, Mandeville Special Collections Library **(Thereafter JSP)**, box 369, folder 3

[2] "The Salk Institute for Biological Studies," Statement (1960). JSP, box 345, folder 4

the humanities, in order to give a concrete form to this credo, and to bridge the "two cultures" of sciences and the humanities.

The history of the attempts to develop the humanities and social sciences component of the Salk Institute provides a unique opportunity to explore the ways in which the early frameworks for science studies discussed in chapters 2 and 3, were institutionalized in this particular setting. British "scientific humanism" provided the initial intellectual agenda for the humanities component of the Salk Institute. Julian Huxley's close associate and protégé Jacob Bronowski was recruited by Salk to launch the "Department of Humane Studies," with the aspiration to provide a model for the reinsertion of philosophical-humanistic ideals, associated with Humboldt's idea of a humanistic university, but lost within the contemporary American university setting. The Department, which ended up being a "one man show" of Bronowski, was expected to explore the possibilities of extending the description of nature offered by the "new biology" to a broader understanding of the world, connecting molecular biology to linguistics, philosophy, and the humanities in general, by inviting scholars in the humanities to the Institute.

In 1968, the humanistic component of the Salk Institute was considerably extended and transformed. The Institute's new President, Joseph Slater, a long-term officer of the Ford Foundation International Affairs program, used his contacts and the Institute's scientific standing to involve distinguished scholars from diverse backgrounds into the activities of the Council for Biology in Human Affairs established under the auspices of the Salk Institute in 1969. The Council – a loosely connected web of the leading molecular biologists and legal scholars, social scientists, and public-policy makers on the East and West Coasts of the US – listed among its members noted public intellectuals, such as Daniel Bell, Saul Bellow, and Herbert Passin, who had been previously associated with the Congress for Cultural

Freedom. The new humanities programs at the Salk Institute amalgamated the traditional "scientific humanism" with its emphasis on the integration of "two cultures" of sciences and the humanities, with the American concerns of the time: abortion, drug abuse, the threat of biological warfare, the effects of genetic manipulation upon human society, and the legal, ethical and social implications of the contemporary advances in molecular biology.

Despite the Council's initial success and intense activity (more than twenty meetings in the period 1968-1973, with leading American intellectuals, politicians, and national leaders involved), following Slater's resignation in 1972 and Bronowski's death in 1974 the Council and the humanities component of the Salk Institute were disbanded and since then only the continuation of linguistics studies at this otherwise highly specialized biological institute recalls the broader aspirations of its founders. Nonetheless, despite the fact that Jonas Salk's aspiration to bridge "two cultures" and to develop a "unification" framework based on modern biology did not live up to initial expectations, the Salk Institute's initiatives were an important experiment in constructing a public space in which the relationship between science and the humanities could be debated, discussed and reformed.

4.1 Jonas Salk and his Dream Institute

On April 2, 1955, at a special meeting at the University of Michigan, it was announced that Jonas Salk, an epidemiologist and a research physician from the University of Pittsburgh's School of Medicine, had created a vaccine for the prevention of poliomyelitis. In the public imagination and for millions of American families finding the prevention for polio, the feared disease that targeted mostly children, ranked as a near miracle. For Salk himself, the invention of the polio vaccine changed his life drastically. Almost overnight, as Salk's biographer Jane Smith has put it, "Jonas Salk stopped being a promising young scientist and took the first steps toward becoming an international hero" (Smith 1990, p. 340). Prestigious awards almost immediately started to pour in: Salk was awarded a Congressional gold medal on behalf of President Eisenhower and received the Albert Lasker Award, one of the nation's top medical prizes. The Pennsylvania Medical Society presented him with its first Distinguished Service Award, and, unprecedentedly, the Pennsylvania legislature established the Jonas Salk Commonwealth Professorship, making Salk himself the first recipient (Paull 1986, p. 202). At the same time, Salk started to face growing skepticism within the scientific community. The disastrous "Cutter Incident" in April 1955, which caused 204 cases of vaccine-related poliomyelitis when it turned out that some lots of the vaccine produced by Cutter Laboratories in Berkley, California, contained live polio virus in what was supposed to be an inactivated-virus vaccine, led many people to distrust Salk's killed-virus technique. Some scientists became increasingly critical of the sensationalism that greeted the Salk vaccine, while others accused Salk of an ungenerous attitude toward his colleagues who helped him in his research, suggesting, for example, that Salk failed to credit Harvard's John Enders for growing polio virus cultures in non-nervous tissues, a technique Salk relied upon in his own procedure. [3]

The years following the invention of the polio vaccine proved to be years of change also for the National Foundation for Infantile Paralysis, the major supporter of Jonas Salk's research on polio. The National Foundation, or the March of Dimes, was established in 1938 by Franklin D. Roosevelt, himself a polio victim, with the

[3] See discussion of the tensions surrounding Salk's meteoric rise to a celebrity status in Oshinsky (2005) and Smith (1990).

aim to provide funds for polio research, education about polio, and aid to polio victims. The National Foundation was supported by private donations through annual fund-raising drives – the March of Dimes. After the Salk vaccine was approved, many people expected the National Foundation to divert its resources to fight other causes, such as arthritis, mental illness, cancer and birth defects (Smith 1990). The task of refocusing the foundation's activities, however, was not so simple, given the fact that March of Dimes fund-raising mechanism consisted of grassroots campaigns run primarily by volunteers. The marching mothers and crippled children gave the March of Dimes fund-raising drives its momentum. By the same token, once this momentum was lost following the success of Salk's vaccine, the donations were lost or at least went down, while, at the same time, expenses remained as high as ever: the polio victims' care had always been the largest expense in the National Foundation's budget. In the words of historian Jane Smith, "[after 1955] it seemed that the public expected the past victims of polio to rise up and walk away, cured by the miracle of federal licensing" (Smith 1990, p. 371). Concern about moving the National Foundation "beyond polio" and how to do so became the pressing issues for the Foundation in the years following the testing of the vaccine.

For Jonas Salk, who just turned forty at the time of the announcement of the success of the polio vaccine, it was hard to accept this as the greatest triumph of his life. Salk continued to work on a host of problems related to his vaccine, defending his kill-virus principle, at the same time increasingly feeling tired of the technical aspects of vaccine research which no longer gave the satisfaction and the excitement of working towards a big and noble goal. His thoughts turned to the "larger mysteries of biological research" beyond polio, as he would recall later (Oshinsky 2005, p. 257). Almost as soon as the polio vaccine was licensed Salk started to develop plans for establishing his own institute within the University of Pittsburgh's School of Medicine. Facing reluctance on the part of the scientific community to recognize the development of the polio vaccine as an advance in fundamental science rather than an accomplishment of an applied nature, he dreamed his institute would blur the "dichotomy between what is referred to as fundamental and as applied research."[4]

It would be a dream institute, an "experimental institute," and a new kind of institution, in which Salk himself could invest the other half of his life and which would give the National Foundation, searching for the new cause, a fitting new goal. Basil O'Connor, the founder and president of the March of Dimes Foundation who had masterminded the war on polio, pledged the National Foundation's support to help realize Salk's vision, eager to move the National Foundation "beyond polio" and to bring Salk the honor and research conditions he could not find at the University of Pittsburgh.

Salk involved leading American scientists in discussion of the envisioned institute. His model was the renowned Institute for Advanced Study in Princeton. Founded in 1933, the Institute for Advanced Study was for many years the only institute of its kind, conceived as a new kind of research center, with no students, no teachers, no classes, where the world's greatest scientists would gather to do research, in the setting of a modern university, but not constrained by formal connection to a university, being financed entirely through private grants and donations. J. Robert Oppenheimer, the controversial head of the Manhattan project during WWII and the Institute for Advanced Study's director for more than twenty years, used to describe it as an "intellectual hotel" – a refuge to which scholars could repair and focus entirely on their research. Comprising four

[4]Jonas Salk, "Notes on establishing an Institute for Experimental Medicine in Pittsburgh" (1957). JSP, box 344, folder 1

schools – Mathematics, Natural Sciences, Historical Studies and Social Science – the institute was mostly focused on mathematics and physics, but nevertheless the humanities were seen as the intrinsic part of it.[5]

Salk thoroughly discussed his plans with Oppenheimer, who in turn wholeheartedly encouraged Salk to establish an independent intellectual center, a place without the strictures of academic departments and disciplines, free of the rivalries and limitations of university life. In contrast to the Institute for Advanced Study, Salk, however, expected the University of Pittsburgh to provide most of the Institute's funding, with the National Foundation giving a large private donation. Salk discussed his plans with other scientists, as well. In 1956 he met Leo Szilard, another visionary nuclear physicist whose work was instrumental for the development of the atomic bomb, against which he actively campaigned for the rest of his life. In Szilard Salk found an enthusiastic supporter of his plans to establish an innovative institute.

Szilard, like many other atomic physicists, moved from physics to biology in the aftermath of the WWII. A close associate of Max Delbrück and a member of Delbrück's "Phage group," Szilard was among those physicists who contributed to the establishment of the new science of molecular biology, as a trans-disciplinary field attracting scientists from diverse disciplinary backgrounds. Simultaneously, as a spiritual leader at such forums as the Pugwash conferences and the Council for a Livable World, Szilard strived tirelessly to bring the rationality of science into the resolution of political and social problems, lobbying for arms-control and disarmament proposals (Lanouette 1992). In the late 1940s and during the 1950s, finding no established institution to accommodate his blend of science and social policy, Szilard tried consistently to invent one for himself, coming up with

[5] For the history of the Institute for Advanced Study in Princeton see Regis (1987).

extravagant ideas, for example submitting an application to NSF in 1956 to fund an inter-institutional appointment for himself that he described as a position of "a Roving Professor."[6] Szilard quickly became the major supporter of Salk's plans. Indeed, Salk's ideas sounded like a great opportunity to realize an institution which would naturally accommodate many of Szilard's own interests.

Within days after their first meeting in late 1956, Szilard, along with Columbia chemist William Doering, wrote an outline of ideas on the new type of institute, which would provide an institutional home for a community of scientists reflecting on the social and political consequences of their work. Referring to the example of physicists' triumph in both nuclear physics and biology, and most probably thinking about himself, Szilard envisioned creating an institute that would "provoke the invasion of outsiders" into the field of medicine, in order to attack fundamental problems of human health.[7] Szilard's conception of the prospective institution was a system of two interactive units: the Research Institute for Fundamental Biology and Public Health (with staff and affiliate members), and the Institute for Problem Studies (with no permanent staff but rather visiting groups of scientists from different disciplines to collaborate on a given project in a field outside their own). The scientific program of the proposed institution was to include areas as diverse as public health, mammalian reproduction and population control, fat metabolism and coronary disease, the effects of cigarette smoking, sleep problems, and mental health.[8] All these problems were to be attacked by applying the new methods of molecular biology. The list of scientists whom Szilard had proposed as the members of the envisioned institutes included scientists

[6] Leo Szilard, "Research in Theoretical Biology: Proposal," 30 Aug 1956. Leo Szilard Papers, University of California at San Diego, Mandeville Special Collections Library (**Thereafter LSP**), box 32, folder 11 (the proposal was not funded - EA)

[7] William Doering and Leo Szilard, "Memorandum: A Proposal to create two independent research institutes operating in the area of public health," 4 Jan 1957. JSP, box 400, folder 1

[8] ibid

coming from physics, genetics, chemistry, and geology. In addition, historians, economists, and other social scientists were to hold short-term appointments in order to incorporate social science perspectives into the study of human health. Together these two institutes would integrate science and the social studies of science into a unified "science of Man."[9]

Szilard, anxious to move Salk's envisioned institute to the next stage of its implementation, put Salk in contact with scientists he thought would be useful for the realization of Salk's plans: the physicists who were involved, like Szilard himself, in the Pugwash movement, such as Roger Revelle, Jerome Wiesner, Alvin Weinberg, Victor Weiskopf, and molecular biologists with whom Szilard was in close contact, including James Watson, Jacques Monod, and Francis Crick. In 1959, when Salk was still determined to establish his institute in Pittsburgh, Szilard was negotiating to locate the Institute in La Jolla, California. As he warned Salk, "Frankly, I see no possibility of getting many first-class people to move to Pittsburgh."[10] Referring to a "conversation with Jim Watson ... and with Roger Revelle ... regarding the hypothetical possibility of convincing Jonas Salk to set up a research institute for basic and applied biology at La Jolla in loose affiliation with the UC at La Jolla" he reported to Salk:

> I happened to meet Jim Watson by accident. I had previously heard from Francis Crick that Jim Watson has been thinking a lot about the cancer problem and so I asked him about this. ... it turned out that he was actively exploring the possibility of setting up a research laboratory staffed with people who are interested in the basic problems of biology, but who are sufficiently interested in applied problems to enjoy cooperating with each other on the problem of cancer. ... Watson and I agreed that in order to make such an operation successful one would have to create conditions which would make to get practically everyone who is desirable to

[9]ibid

[10]Leo Szilard to Jonas Salk, May 1959, cit. in Lanouette (1992), p. 400

> join. One necessary condition for this would be to set up the institute in California in the Berkeley, Palo Alto or La Jolla area.[11]

The circle of people Salk contacted to discuss the prospects for the future institute grew, but gradually he confined it to scientists whom he considered desirable potential fellows of his institute: Hermann Muller ("...just a note to appreciate how very much I appreciate the opportunity to talk with you"[12]), Linus Pauling ("I expect to be visiting Dulbecco ... If by any chance you will be in Pasadena and could see me for a short visit I would very much appreciate such a opportunity"[13]), George Beadle ("Just a note to thank you for your comments and encouragement"[14]), among other biologists whose work was at the cutting edge of contemporary research in molecular biology and genetics.

By the end of 1959 Salk settled on the location of the Institute. The opposition he encountered at the University of Pittsburgh from its new chancellor, Edward H. Litchfield, made it clear to him that his idea to establish the Institute of Experimental Medicine in Pittsburgh would not be realized: Litchfield maintained that as long as Salk would be spending Pitt dollars and relying on Pitt resources he would have to share the control (Oshinsky 2005, pp. 258-261). While Salk was trying to negotiate the terms that would ensure autonomy for his institute in Pittsburg, there came the offer of land - and the prospect of affiliation with the newly planned campus of the University of California at San Diego - should he decide to base the Institute in La Jolla. Oppenheimer, who tried, but did not succeed, to help to push Salk's case in Pittsburgh, now strongly encouraged Salk to seek possibilities on the West Coast. As Salk recollected retrospectively, Oppenheimer "was the one who said to me, 'Did it ever occur to you to go to

[11] Leo Szilard to Jonas Salk, 7 May 1959. JSP, box 349, folder 7.
[12] Jonas Salk to Hermann Muller, 30 Oct 1959, JSP, box 350, folder 1
[13] Jonas Salk to Linus Pauling, 30 Oct 1959, JSP, box 349, folder 7
[14] Jonas Salk to George Beadle, 10 Nov 1959, JSP, box 350, folder 1

California, where you can do more unusual things than on the East Coast?"'[15] The plans solidified further when the UC President Clark Kerr, who was interested to attract Salk in order to strengthen the newly planned UC campus in La Jolla, offered his support. Roger Revelle, director of the Scripps Institution of Oceanography in San Diego and the major promoter of the idea to establish UC campus in La Jolla, was equally enthusiastic about the prospect.[16]

In 1960, the voters of San Diego County approved a ballot referendum giving Salk a parcel of land on the Torrey Pines mesa overlooking the Pacific Ocean – a beautiful undeveloped stretch of coastal land in La Jolla, a small, exclusive enclave north of San Diego. The plans for the establishment of the Institute were publicly announced and on paper the institute was inaugurated in 1960. The funding for the institute was provided by the National Foundation. Basil O'Connor, the Foundation's President, insisted putting Salk's name on the institute, "The Institute of Biology in Torrey Pines" thus becoming The Salk Institute, in the hope this would help with fund-raising.

By this time, Salk had developed his own strong vision of the institute. During the years following the successful testing of the polio vaccine he began to think about himself as a scientist-philosopher, seeking ways to ally himself with likeminded individuals in a setting, "the purpose and orientation of which would be the support of individuals who possess a feeling for humanity, in addition to their special skills developed deeply and broadly enough to permit them to function at both levels," as he put it in the proposal for the Institute for Experimental Medicine.[17] With time he started to emphasize more and more this other, "humanistic," component of his dream institute, envisioning its goal as combining

[15] Cit. in Oshinsky (2005), p. 269

[16] Jonas Salk to Massoud Simnad, 25 Sep 1959. JSP, box 349, folder 7

[17] Jonas Salk, "Notes on establishing an Institute for Experimental Medicine in Pittsburgh" (1957). JSP, box 344, folder 1

"hard science" and the humanities in pursuit of "biology with a conscience."[18] Warren Weaver, whom Salk met for the first time in 1960 at the Sloan Foundation that Weaver then headed, left a vivid testimony of their first meeting:

> Jonas's basic concept [was to] have an institute that would be devoted to research on basic biological problems, but all within the settings of a concern for man and his needs: his artistic needs, his ethical needs, his philosophical needs, and his moral needs. ... This, I thought, was a noble concept and I was very deeply impressed by Jonas on this first meeting. I thought this is a perfectly wonderful young man. He is a very gentle person, he has a beautiful artistic mind. I was a little surprised [that] his mind was not, in a sharp sense, the scientific or analytical mind I had expected to find in him. It was much more of an artistic mind. And, he spoke almost in parables, using similes and metaphors that a poet might have use. ... Indeed, I remember the culmination of these two hours to explain why he was doing this the way he was going to do it, [when] he said: "Warren, I have to be true to the music that I hear. I can't pay any attention to the music I don't hear, it's the music I hear that dominates my life."[19]

The contact grew into a long-term commitment of Weaver to the cause of the Institute. Weaver, who designed the Rockefeller Foundation's programs in 'molecular biology' (the phrase that he coined in 1938), saw the new discipline, from its inception around 1930, as a mission-oriented program. The Rockefeller Foundation, the principal patron of molecular biology from the 1930s to the 1950s, envisioned it within a larger "Science of Man" agenda, a cooperative venture between the natural, medical, and social sciences. Weaver had immediately recognized and appreciated this aspect in Salk's idea of the institute:

> This [Salk's concept] seemed to me perfectly wonderful and in a way ... to me [this was] a perfect combination of what I had been struggling to do in the Rockefeller Foundation since 1932, because this was going to be just exactly the kind of modern quantitative biology in the closest possible relationship with physical sciences,

[18]Cit. in Oshinsky (2005), p. 269

[19]Warren Weaver, oral history interview, 1969. Jacob Bronowski Papers, University of Toronto, Thomas Fisher Rare Book Library (**Thereafter JBP**), box 116

> but with added advantage of having this orientation towards a deep concern for Man.[20]

Salk sought to express the artistic dimension of his aspirations for the institute in the institute's building. As he wrote in his diary, "it seemed quite appropriate to use art and architecture to help build and enhance such an environment that it might have an effect on people who are creative, who think deeply not only about their own subjects, but about man and his future."[21] The design of the building was entrusted to a renowned Philadelphia architect Louis Kahn. As Salk used to say, he asked Kahn to create "a facility worthy of a visit by Picasso."[22] Kahn, with whom Salk shared a somewhat mystical vision of the Institute, gave form to some of Salk's ideas. Scientists at the institute were supposed to get to fundamental problems - the buildings are stark concrete and teak, with the lines of the buildings straight and simple, yet with many angles allowing for many viewpoints and "framings." Scientists should have room to meditate - Kahn designed thirty six "meditation towers" each with a view of the shoreline and the Pacific. Scientists should be able to focus on their work yet to reach out to the world with equal facility - Kahn integrated blackboards in the public walkways in the vicinity of the buildings.

The institute was initially planned as a small campus consisting of three main groups of buildings: two flexible laboratory buildings, village-like housings for fellows, and a Meeting Center. The Meeting Center (never realized) represented the spiritual purpose of the Institute. While the laboratories were the institute's working mechanism, the Meeting Center was its humanistic soul, a place "that will symbolize the spirit of the Institute and its relationship to the world at large."[23]

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[20] ibid

[21] Jonas Salk, notes (February 1967). JSP, box 369, folder 3

[22] Cit. in Oshinsky (2005), p. 270

[23] J. Salk "Origin of the idea." Annual report of the Salk Institute, 1962.

The Meeting Center was conceived as a place particularly conducive to interaction - hence, Kahn equipped it with guest rooms for short term visitors, seminar rooms and auditoriums, and, most importantly, the Library of the Arts and Sciences, which "would express the interest of the Institute in the human problem, and would represent the opposite pole of interest of the Fellows of the Institute." [24]The Meeting House with its Library Tower was to "symbolize the purposes of the Institute."[25]

With generous stipends offered to the prospective Fellows of the Institute Salk was able to recruit a stellar group of leading practitioners in molecular biology as the Institute's the first Resident Fellows appointed in 1962: Seymour Benzer, Melvin Cohn, Renato Dulbecco and Edwin Lennox. Francis Crick, Jacques Monod, Leo Szilard, and Warren Weaver joined the Institute as its Non-Resident Fellows.[26]

Simultaneously, Salk was looking for in-house "humanists" - people who would share his new interests in art, philosophy and the sources of creativity, at the same time relating their activities to scientific research going on at the institute. Julian Huxley was Salk's first candidate. At the early stage of planning the institute, when he was still undecided about its right location, "considering establishing [it] at Stanford where several basic areas are so very well represented," Salk explained to Basil O'Connor that besides first-rate scientists, he was looking for the kind of people, "such as Huxley, whom it would be good to have come as visitors for as long as they would deem profitable, ... persons who are regarded as humanists, ... and who are fundamentally concerned with the problems of man."[27]

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[24]ibid

[25]Louis Kahn, "Abstract of architectural program for the Salk Institute for Biological Studies," JBP, box 89. See also Larson (2000).

[26]The Institute's operations have not started, however, until 1964. By that time Benzer had resigned and L. Orgel was appointed a Resident Fellow. Leo Szilard became Resident Fellow in 1964, a few months before his death. Salvador Luria and Jerome Wiesner joined as Non-Resident Fellows in 1965.

[27]Jonas Salk to Basil O'Connor, u/d. JSP, box 350, folder 5

Salk found such an in-house "humanist" in Huxley's close associate British mathematician Jacob Bronowski, who translated Salk's "artistic" (to use Warren Weaver's definition) ideas about the humanitarian aspects of biological studies and the humanistic component of his institute into a program of research. Bronowski, who apparently was recommended to Salk by Warren Weaver, joined the institute as the Resident Fellow in the humanities, to explore the broader social, philosophical and humanistic implications of the new revolutionary discoveries in molecular biology. The humanistic component of the Salk Institute was further solidified by the invitation of C.P. Snow to join the Institute as a member of the Board of Trustees. As Weaver explained the purpose of the newly announced institute to Chauncey Leake, a pharmacologist with the interests in medical history and philosophy,

> The initial emphasis [of the institute] will be heavily on modern molecular biology (genetics, viruses, enzymology, immunology), with, from the outset, concern for the interrelation between science (particularly biology) and the humanities. This latter interrelation explains Bronowski, and (I suppose) myself, as well as the presence of Sir C.P. Snow on the Board of Trustees.[28]

4.2 Jacob Bronowski: a "Scientific Humanist" Par Excellence

Jacob (Bruno for everyone who knew him) Bronowski, a mathematician, an inventor, an administrator, a poet and a "scientific humanist," remembered by many for his monumental television series on the BBC, *The Ascent of Man*, first broadcast in 1973, was born in 1908 in Łódz, Poland.[29] The eldest son of an orthodox Jewish father and an atheist communist mother, Bronowski

[28] Warren Weaver to Chauncey Leake, 12 Jun 1962. JSP, box 350, folder 7

[29] See biographical details in Emmitt (1982), Cattanach (1983)

fled Poland with his family during WWI moving to Germany and, in 1920, to England, where, swiftly learning English, he made his way to Cambridge. While reading mathematics at Jesus College he developed interests in the arts and poetry. Following completion of his doctorate in geometry in 1934 Bronowski was appointed a lecturer in mathematics at the newly formed University College, Hull (then part of London University). Simultaneously he started to publish his literary works: his first book, *The Poet's Defense*, sought a literary criticism which was to be "as reasoned as geometry" (Bronowski 1966, p. 8). This was followed by a collection of poems about the Spanish Civil War (1939), and a biography of William Blake (1944).

WWII fundamentally changed his life and career. In April 1943 Bronowski was recruited into the Military research unit of the Ministry of Home Security, where he was put in charge of developing statistics methods for establishing the most effective bombing raids against German and Japanese cities. Moving away from operational research, Bronowski received his next appointment as the Scientific Deputy to the British Chiefs of Staff Mission to Japan in 1945, to survey the effects of the atomic bomb at Nagasaki. What he saw there horrified him. After writing the British Mission's report on the destructions for the Home Office, Bronowski decided not to return to his university job as a mathematics lecturer. Over the next few years he wrote occasional papers in mathematics and statistics, but his major interests moved to the problems of ethics and the moral responsibility of scientists. As his widow, Rita Bronowski, recollected this period, he became "a philosopher, humanist, and a sort of evangelist" (Bronowski 1985).

In the aftermath of WWII Bronowski also gradually built an informal but important career as a broadcaster. His first contact with the British Broadcasting Corporation (BBC) was almost accidental: he was asked to stand by while BBC was covering the night of the Atomic Bomb test in the Bikini Islands. The radio interference from the explosion made the live broadcast impossible and what was broadcast instead was Bronowski's speech "Mankind at the Crossroads." This speech instantly made him a popular media figure.[30] Around the same time he became involved in the radio program "The Brains Trust" where he met Julian Huxley and British philosopher Alfred Ayer, along with other distinguished panelists (Ayer 1984). Through this program, which was transferred to BBC television in the 1950s he became a household name in Britain, making a number of TV specials on modern scientific breakthroughs. As Ayer noted in retrospect, "Bronowski... could lay claim to being the programme's star performer. He appeared more often on it than anyone else, with the possible exception of Julian Huxley, and while Huxley may have had the greater range of knowledge, Bronowski excelled him in his powers of exposition" (Ayer 1984).

Addressing a general audience, speaking in his eloquent prose, Bronowski emphasized that human values were the driving force behind the scientific enterprise, arguing that such values as honesty, independence, tolerance and originality, which form the basis of Western democracy, were reinforced with the advent of science during the Scientific Revolution of the 17th century.[31] In 1953, on a Carnegie Foundation grant, Bronowski was a visiting scholar at MIT, where he gave a series of lectures on the connection between science and art, between science and human values, and on the moral dimension of science. These lectures, published as *Science and Human Values* in 1956, became one of his best known books. It followed and expanded his earlier *The Common Sense of Science*, first published in 1951. Together, these expositions sought to "humanize" the

[30]See discussion in Emmitt (1982)

[31]See discussion of Bronowski's works in Emmitt (1982), Holton (1985), Vice (1989), Sandefur (2002), Topper (1979).

contemporary view of science, emphasizing the creative side of science, and its affinity with art and arguing that the sciences constituted an intrinsic part of culture along with the arts and humanities.

Television and writing, however, was merely a weekend activity. Bronowski's official appointment in the 1950s was at the British National Coal Board, which he helped to start in 1950, and then as the head of an affiliated research laboratory. In this function, Bronowski was praised for the invention of a new type of smokeless fuel that became known as "Bronowski's Bricks." Simultaneously, throughout the 1950s, he wrote poetry, continued to publish (mostly on William Blake) and engaged in writing radio plays (Emmitt 1982, Cattanach 1983).

Jonas Salk, who met Bronowski in 1960, was instantly impressed. He wrote to Bronowski soon after their first meeting:

> After reading your book, *The Common Sense of Science*, ... I was impressed by the extent to which we seem to have converged in our paths of thought from different beginnings, and through different experiences. Your book adds to my convictions that the institute idea is sound and timely. ... I did not realize, when we met in London, how far you had already gone in pointing the way to a course of action for science today, for which the institute would be so suitable a vehicle.[32]

Bronowski was impressed by Salk, too, and especially by the image that Salk painted of the Institute. Bronowski, who had spent the past ten years in industrial research, felt excited by the opportunity to return to academic science. As Salk described their first meeting to Basil O'Connor, "I told him the story of the Institute, without any idea on my part that he would be interested, he asked to see me again and expressed the feeling that the contribution he looks forward to being able to make in the reminder of his life could best be done under circumstances

[32] Jonas Salk to Jacob Bronowski, 29 Nov 1960. JSP, box 396, folder 8

such as we contemplate creating."[33] To Salk, Bronowski seemed to be an ideal candidate for a job: not only could he represent the humanistic "soul" of the new Institute, but Salk also expected Bronowski to assist him in administrative matters concerning public communication, the task for which Bronowski, with his extensive experiences both in administration and in public communication of science, seemed to be exceptionally well qualified.[34]

Bronowski, in addition, was a close associate of Julian Huxley, Salk's first choice for a "humanist." The friendship with Julian Huxley was essential in shaping Bronowski's philosophy. They had much in common. Like Bronowski, Huxley was less eminent as a scientist, but was much better-known for his skill in communicating science to the public. When Huxley became the first Director-General of UNESCO, Bronowski was also involved in UNESCO activities, serving as the Director of UNESCO Mass Communication Department in 1948-1949. After leaving UNESCO in the late 1940s, Huxley assembled a group of friends and colleagues in various fields, including his fellow panelists at the Brains Trust, Ayer and Bronowski, in an informal "Group on systems of belief and ideas." Huxley attempted to institutionalize the group's activities in the 1950s as an "Institute of Human Study," or the "New Humanist Institute." Huxley's unrealized Institute became the prototype of Bronowski's "Department of Humane Studies."[35]

[33] "Jacob Bronowski," statement by Jonas Salk, u/d [1960]. JSP, box 396, folder 8
[34] Jonas Salk to Jacob Bronowski, 19Jun 1961. JSP, box 396, folder 8
[35] Sometimes called "human study," sometimes "humanistic studies" in the documents - EA

4.3 Julian Huxley and his "Ideas-System" Group in Search of an Institutional Home

In 1950, the year Huxley's term of as a UNESCO's Director-General ended (see chapter 2), he started lobbying for an alternative institutional structure to implement the ideas he outlined in the proposal "The UNESCO Philosophy."[36] Huxley envisioned "the creation of an 'Institute of Human Studies,' analogous to the Institute for Advanced Study at Princeton, but focused on human possibilities and the techniques for their better realization, or an 'Institute for Evolutionary Study' focused on evolution in its different aspects."[37] The end goal would be the formulation of a new unifying order - "idea-system" – that would provide a new understanding of science in a modern society, taking "the place of a materialism that dominated the science of the 19$^{\text{th}}$ century."[38]

Initially Huxley intended to relate such an Institute to UNESCO's "Scientific and Cultural History of Mankind and its associated projects."[39] Another possibility that Huxley considered was to affiliate it with "various ethical

[36]As a development of the ideas he outlined in his manifesto for UNESCO, Huxley envisioned to undertake a systematic enquiry into "socially effective ideas - i.e. broad general ideas with an emotional charge and general appeal and capable of symbolizing and unifying wide ranges of human activity" - what Huxley called, simply, "idea-systems." (Julian Huxley, "Modern systems of ideas and their adaptation to a changing society," [1950], JSHP, box 113, folder 4). Such "idea-systems" included widespread systems of beliefs like Roman Catholicism, Marxist communism, as well as more limited ones, such as Humanism, "broad idea-systems like *laissez-faire* individualism, though this survives only in a much modified form and is now less relevant to economic reality," and "incompletely formulated systems like the concept of the Welfare State" (ibid). More importantly, such an inquiry into the established "old key ideas" should include, Huxley argued, the "field of new knowledge" stimulated by the discoveries and developments in natural sciences, most prominently, evolutionary biology, genetics, "modern anthropology with its stress on cultural relativity," and cosmology, in order to formulate new "idea-systems" appropriate for the modern, post-WWII world (ibid). The urgency of the project was substantiated, in Huxley's view, by the "need to recover a unity in European thought" (ibid).

[37]ibid

[38]The Rockefeller Archival Center (**Thereafter RAC**), RF, 1.1., series 401 D, box 35, folder 445

[39]"New Humanist Institute, note by Julian Huxley after talks with Mr. Besse," 29 Aug 1950. JSHP, box 68, folder 1.

and humanist societies," preferably in the U.S.[40] By 1950, Huxley had surrounded himself by a number of dedicated followers and institutionalized his idea on a small scale, animating an informal gathering called "Idea-Systems Group." The group listed among its members Huxley's friends and colleagues of diverse background: mathematician Jacob Bronowski, philosopher A. J. Ayer, politician E. M. Nicholson, English poet and the editor of the CCF-affiliated journal *Encounter* Stephen Spender, journalist Francis Williams, and sociologist Barbara Wootton formed the core group.[41] As the group presented itself, it was "a small group of people of fairly wide experience in the natural and social sciences, the arts and public affairs [that] after systematic discussion during the past two years have come to the firm conclusion that the successful establishment of one or more small, high-level, advanced Institutes for Advanced Studies is both practicable and urgently necessary."[42] The urgency, as the proposal stated, was related to "those problems of integration between the social sciences and the biological sciences and their broad applications which our discussions have confirmed to be among the outstanding intellectual challenges of the present time." [43]

The group members met regularly in London since 1950, having its most active period from 1950 through 1955. The topics discussed included Huxley's "evolutionary humanism;" the issue of the reconciliation of science and religion in past and present, especially as concerned evolutionary theory; a unifying impulse of a "new evolutionary approach" in the wake of the Modern Synthesis; research in animal communication, and other topics, mostly related to evolutionary studies and their broader social implications. In the end, it was hoped that the group

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[40] Julian Huxley, "Suggested future work on systems of ideas and possible new type of organization," 9 Apr 1951. JSHP, box 113, folder 4

[41] "Minutes of the meeting of the Idea-Systems Group," 13 Dec 1951. JSHP, box 113, folder 2.

[42] "An Institute for Human Studies," 5 Jan 1952. JSHP, box 113, folder 5.

[43] ibid

would identify the "idea-system" (or the set thereof) that would be congruent with the contemporary state of knowledge in modern society.[44] The group grew slowly but steadily and by 1955 it included Oxford historian Alan Bullock, renowned biologist and experienced university administrator Eric Ashby, psychiatrist John Bowlby, art historian Kenneth Clark, influential French social anthropologist Claude Levi-Strauss, and novelist Aldous Huxley, Julian's brother.[45]

Huxley actively sought new possibilities to institutionalize the group's activities, looking for a sponsor and an institutional home in the U.S. Applications were sent to the Rockefeller, Ford and Carnegie Foundations. Bronowski, at that time in the U.S. as a visiting scholar at MIT, met with a number of people from the foundations and university's centers developing similar interests, to discuss the proposal and explore the possibilities *in situ*.[46] The responsible persons in both Rockefeller and Carnegie Foundations made it clear that they were unlikely to finance the Institute although might support individual projects if they were formulated more modestly and more specifically.[47]

Overall, among the three foundations approached, the Rockefeller Foundation, with which Huxley had been in contact since 1950, looked as

[44] "Minutes of the meeting of the Idea-Systems Group," 3 Dec 1952. JSHP, box 113, folder 2.

[45] "Group for the study of Idea Systems" Memo, 8 Nov 1955. RAC, RF, record group 1.1., series 401 D, box 35, folder 445

[46] Thus, as the minutes of the group meeting in January 1953 recorded, Bronowski was asked to "get in touch with Warren Weaver and John Marshall (friend of Julian Huxley at one time at Unesco) at Rockefeller Foundation (Huxley will send Bronowski note of introduction), Bernard Berelson, Director of Behavioral Science Division, Ford Foundation, [as well as]... the Harvard Group on 'Social Action', Talcott Parsons and Edward Shils" ("Minutes of the meeting of the Idea-Systems Group," 15 Jan 1953. JSHP, box 113, folder 2.). Bronowski soon reported back to Huxley: "I have seen Berelson, who behaved pleasantly and fairly non-committally. He had on the whole ... a favorable account of what I had said ... and was likely to take us with a better grace. I learnt incidentally that a) our schemes had been thought vague and slightly superhuman by Ford; [and] b) our list of persons likely to be associated with us was also thought vague, and a clear statement of who will really work full time for how long always help. ... As for the future, I told Berelson that you had it in mind to propose a more modest, specific set of ... projects" (Jacob Bronowski to Julian Huxley, 10 Jun 1953. JSHP, box 113, folder 5).

[47] "Minutes of the meeting of the Idea-Systems Group," 13 Jul 1953. JSHP, box 113, folder 2.

"the most hopeful source of finance." [48] Warren Weaver was "very interested" in the proposal, Bronowski reported, although also "more in a collection of projects than a continuing Institute." [49] The negotiations with the Rockefeller Foundation, however, had reached a stalemate not moving beyond correspondence that abounded over the course of few more years, until gradually Huxley gave up the idea of establishing the institute, opting for individual projects, as reviewers suggested. Quite naturally, he started with himself. Referring to the group's decision that "has authorized [Huxley] to apply on their behalf for a grant to [Huxley] personally," [50] he submitted a proposal for

> ...a book on Evolution, which is hoped would be one of a series of volumes devoted to the general subject of the idea of human destiny and its social implications. This particular volume would be devoted to the evolutionary background of human destiny: the time-scale and mechanisms of biological evolution; the main trends of biological evolution, including evolutionary ecology, adaptation, specialization, stabilization, extinction, the succession of dominant types, biological advance and evolutionary progress; the improvement of awareness and communication; and man's place in the evolutionary process – ... new possibilities of evolution by cultural transformation.[51]

This study, by "outlining the new picture of human destiny" which emerged out of recent developments in evolutionary theory and genetics, would, in Huxley's

[48]Ibid.

[49]Ibid. Initially the reaction within the Rockefeller Foundation was rather cautious. It was suggested that "three RF officers would give H[uxley] a full chance to explain his present interests in studies of idea systems" in person, during the meeting with Huxley arranged in September 1953 ("Julian Huxley visit," 3 Sep 1953, RAC, RF, record group 1.1., series 401D, box 35, folder 445). After the meeting, most reviewers felt that although the proposal had some merit, "at present [the proposal] lacks substance" (ibid). The proposal was not killed on the spot, though, and the decision was left open-ended. As the conclusion for the report on the interview of Huxley with three Rockefeller Foundation's officers stated, "While the foregoing undoubtedly sounds as if JHW[illits], GRP[arsons] and JM[arshall] were taking this proposal seriously, it must be read in light of their tacit agreement to give H[uxley] the reception at the RF that his long history of relations with the RF seems to merit. Somewhere toward the end of this conversation, the phrase came up, 'supposing that anything came of this' - and JM would say that that was in a way the text of this memorandum" (ibid).

[50]Julian Huxley to the Rockefeller Foundation, 1 Nov 1955. JSHP, box 113, folder 3

[51]ibid

mind, "build up an ideology, or general system of ideas about human destiny," which would be "based on or congruent with modern science ... relevant to present conditions" and "so could provide an intellectual and moral dynamic for the modern world," divided and disunified by the Cold War tensions.[52] Warren Weaver was quite satisfied with Huxley's new proposal, seeing it as a "a pretty reasonable compromise on the request which had been laid before the RF ... over the last few years ... towards producing a large work ... which would sum up the findings and philosophies of this London Huxley-dominated group."[53] Encouraged by favorable response from the Rockefeller Foundation, Huxley sought to arrange similar "compromise" solutions for other members of his group, with the hope of eventually implementing the idea-system proposal "piece-meal." Bronowski was his right hand, and Huxley negotiated travel grants for Bronowski arranging with Henry Kissinger, then Director of the Rockefeller Brothers Fund, "for a grant for Bruno's transatlantic and other extra travel."[54]

Over the course of their communication in relation to Huxley's proposal, Weaver developed a high respect and interest in Bronowski's work, considering him "one of the ablest individuals thinking about and writing about the general nature of science."[55] As a result, Weaver offered to Bronowski "a special fellowship of the

[52]As Huxley explained in the proposal, "Now that the Cold War has been shifted on to the ideological plane, it is more than ever urgent that the western world should set about to building up an ideology based on the results of free progressive inquiry rather than on any rigid, a priori, or dogmatic system, whether traditional or modern. ... In the task of formulating such a background or framework of ideas, it is clear that evolutionary biology provides ... the only valid picture of man's place in nature" (ibid).

[53]Warren Weaver, RF Diary, 1 Feb 1956. RAC, RF, record group 1.1., series 401 D, box 35, folder 445.

[54]Draft of a letter, u/d. JSHP, box 113, folder 6

[55]W. Weaver's diaries, 1957-1959. Record of 10 Oct 1957. RAC, RF, 12.1. In his diary Weaver recorded his impressions of Bronowski's personality after their first meeting: "Bronowski ... is very attractive, both personally and intellectually. He is exceedingly interested in the relations between science and the humanities. He is a very good friend of Julian Huxley, and is a member of a group which has been meeting about once a month for the last year or two, presumably largely under Huxley's inspiration, ... trying to discover what have been the great central ideas which have dominated important periods of history. ... We talked a little bit about the importance of

RF," which would allow him to take a year off and to devote all of his time to his scholarly work.[56]

Bronowski's acquaintance with Jonas Salk, however, changed these plans. The opportunities opened by the newly formed institute looked more exciting than the grant from the Rockefeller Foundation: the positions at the Salk Institute were appointments for life, thus providing an institutional structure to implement a grand visionary program. Bronowski certainly was interested in Salk's institutional plans. Whether or not it was Warren Weaver who connected Jonas Salk and Bronowski, Weaver also became more supportive of Salk's plans once Bronowski was identified as the prospective member of the Institute responsible for its humanities component, believing that Bronowski would give substance to Salk's plans. As Salk wrote to Basil O'Connor informing about his negotiations with Weaver, "I talked with Weaver of the Sloan Foundation [again]. I believe his interest will have been rekindled because of Bronowski's interest. I think they [the Sloan Foundation] can be approached for money; perhaps even softened by Bronowski himself."[57] Indeed, Weaver agreed, after initial hesitation, to assume the chairmanship of the Board of Trustees of the new institute, with the condition that later he would assume a non-administrative position: "I understand that for the present I will be designated as Consultant to the Director... I further understand that at some later time (as early as July 1964) I will become an active

having a better understanding of science in a modern democratic society, and it turns out that B[ronowski] completely shares WW[eaver]'s ideas in this particular area also." (W. Weaver's diaries, 1952-1956. Record of 10Jun 1953. RAC, RF, 12.1.) Not only Bronoswki appeared to be "one of the most gifted writers in this general area," Weaver stressed, "he is also one of the very few who has real competence both in science and in the humanities" (W. Weaver's diaries, 1957-1959. Record of 10 Oct 1957. RAC, RF, 12.1.).

[56]Weaver offered a similar scheme to Julian Huxley, however, Weaver considered Bronowski "a much better risk, and one which the RF ought to be very happy to take." (W. Weaver's diaries, 1957-1959. Record of 10 Oct 1957. RAC, RF, 12.1.)

[57]Jonas Salk to Basil O'Connor, 2 Jun 1961. JSP, box 371, folder 8

Non-Resident Fellow."[58]

To solidify the plans for the development of the humanistic component of the Institute, Salk invited another "humanist" and an advocate for bridging the "two cultures" of science and humanities, the very author of the influential formulation – C.P. Snow.[59] Writing to "Sir Charles" and inviting him to join the Institute's Board of Trustees, Salk wrote:

> [It is my] desire to have a number of people act as Trustees who ... are interested in the purposes for which the Institute is being created. It is my further desire that there be on the Board of Trustees citizens of the world, whatever other loyalties they may have. Since this will be a self-perpetuating body, it is especially meaningful to me that you will have consented to serve as a Trustee.[60]

By 1962, the "embryo" of the future humanistic component of Salk's institute was formed, smoothly incorporating British "scientific humanism" into the agenda of the newly formed Institute.

4.4 Jacob Bronowski and the "Department of Humane Studies" at the Salk Institute, 1962-1968

Bronowski presented his program to Salk in the following way:

> ...The section of the Institute which you asked me to plan has as its object, we have agreed, the study of human aspirations and values, seen as a natural expression of the biological nature of man. In my

[58]Warren Weaver to Jonas Salk, 7 May 1962. JSP, box 371, folder 8

[59]On C.P. Snow and the history and impact of the formulation of a "two cultures" dichotomy - the threat of the increasing bifurcation of different forms of knowledge that divides and polarizes the world of science and the world outside of science - see Collini (1993), De La Mothe (1992), Graham (1999).

[60]Jonas Salk to C.P. Snow, 17 Oct 1961. JSP, box 395, folder 2

> mind, I have called this the Department of Human Studies. In the long run, what the Department would do (what I have been trying to do in recent years) is to understand the potential of man, both as an individual and as a social being, fulfilling this exquisite biological potential. I have stressed the word "biological" in this description, because it expresses what I find most stimulating in the plans for your Institute. ...
>
> I say that a Department of Humane Study in your institute should have three main academic preoccupations. Obviously most important, it should occupy itself with the philosophy of biological science: this is the crux of the matter, academically ... Second, to support this, the Department should occupy itself with the methods of biology, and particularly the methods of reasoning in biology. And third, it will be useful to have some work done in the history of biological discovery, and particularly of biological concepts, in order to place these abstract themes in their perspective.
>
> These three sub-divisions are intended to serve the over-riding aim which I have already stated, which is to draw a composite picture of nature in which men will recognize the wealth of their own potential....[61]

The "humanist" aspirations of Jonas Salk would be met by implementing "the study of human aspirations and values, seen as a natural expression of the biological nature of man," - Bronowski explained to Salk.[62] More specifically, Bronowski's "Department of Human Study" would explore the possibility of extending the description of nature offered by the "new biology" - molecular biology – to a broader understanding of the world.

Bronowski had to play a special role at the Institute, being not only a Fellow but also as a spokesman for the Institute and a spokesman of the Institute's scientific programs to the public. Explaining Bronowski's role at the Institute to Basil O'Connor Salk emphasized that Bronowski promised be "one of the most effective expositors for... present place of biology... as well as contribute to the

[61] "Jacob Bronowski," statement by Jonas Salk, quoting from Bronowski's letter as of 7 Oct 1960. JSP, box 396, folder 8

[62] Jacob Bronowski to Jonas Salk, 7 Oct 1960. JSP, box 396, folder 8

development of a philosophy that can emerge from biology."[63]

In the meantime, the plans for the "Department for Humane Studies" had to wait: it was to be located in the Meeting Center - one of the three architectural groups designed by Louis Kahn to give the architectural form to Salk's vision of the institute. The construction of the Meeting House, however, was still far away in the future. The laboratory buildings were anticipated to be completed, equipped and furnished by the summer of 1963. However, the implementation of Kahn's design turned out to be much more expensive than foreseen in the beginning, and consumed most of the National Foundation seed money pledged to the institute. Under the circumstances, the construction of the Meeting Center was postponed, along with the plans for fully developed programs in the humanities.

In 1964 Bronowski finally moved to California, urged, in part, by Weaver, who strongly advised Salk and Bronowski to move to Southern California as early as possible, to start the Institute's activities.[64] After two years of extensive planning, the buildings and office facilities were not yet available and the laboratories were housed in temporary buildings. Bronowski confessed to a journalist that he "arrived ... here to live but had no very clear idea of what work I wanted to do..." (Anon 1965). A special position of a Deputy Director was created for Bronowski to entrust to him the daily operations of the Institute and the assistantship to Jonas Salk in his role of the Director-President. [65]

Salk referred to his institute as "a self-governing and self-perpetuating community of scholars." This principle of "a self-governing and self-perpetuating community," although never defined, was implemented in the administrative

[63] "Jacob Bronowski," statement by Jonas Salk, u/d [1960]. JSP, box 396, folder 8

[64] Warren Weaver to Jonas Salk, 26 Sep 1962. JSP, box 351, folder 2

[65] Salk asked Bronowski to assist him "in matters that concern public as well as professional education and problems of relations with those concerned with public communication generally" (Jonas Salk to Jacob Bronowski, 19 Jun 1961. JSP, box 396, folder 8)

structure of the institute in the first years of its functioning: the Fellows had simply run their institute by themselves, assuming various administrative positions.[66] While Salk, as the Director-President, was responsible for long-term planning of the institute's activities, the Fellows assumed responsibilities for specific areas of day-to-day operations: Melvin Cohn was responsible for animal quarters for the whole Institute, Renato Dulbecco ran the seminars, Edwin Lennox assumed the responsibility for managing the work of the "kitchen" and biochemical preparations and so on. Since Salk proved to be a poor administrator, Bronowski's experience in scientific administration in industry made his role as a Deputy Director essential for the functioning of the institute on the daily basis. Bronowski himself was responsible for the library and for the day-to-day management of the Institute, supervising the staff of the office of Deputy Director, administering grants and budgeted funds, publications and relations with the press.

With Bronowski preoccupied with his role of the Deputy Director the humanities programs were largely limited to his own research. The limited scale of the humanities programs was felt to be justified, however. During the planning period it was decided that the envisioned humanities "Department" should not start up right away. The Institute's "basic policy" - "to select outstanding persons and then give them the freedom"[67] to pursue their own intellectual interests - was felt to be hardly applicable to the humanities program. Rather, the humanities component was sought to be provided though short-term visiting appointments, as Snow had suggested. As Salk reported to the prospective Fellows in 1962, "Snow emphasized that he felt it would be wise to go very slowly before appointing additional humanists as Resident Fellows. He felt that calculable harm could be caused by the wrong appointments and urged that humanists could be better

[66]Melvin Cohn, interview with the author. Nov 2004.
[67]Warren Weaver to Chauncey Leake, 12 Jun 1962. JSP, box 350, folder 7

judged after a spell as visitors at the Institute."[68]

By May 1965 the laboratory buildings were finally constructed, with a year's delay largely due to negotiations for funds necessary to complete the structures. A delay of still another year occurred before occupancy, which finally took place in the summer of 1966, when the scientific staff moved to the permanent quarters in its first, North laboratory building. As Jonas Salk noted in his speech summarizing the year 1966, "The move was accomplished with little fanfare; there was an almost prosaic quality to the orderly transfer which gave little hint of the exhilaration that was part of the event."[69]

The changes at the Institute had been also marked by the appointment, in the end of 1965, of Augustus Kinzel, a former President of the Union Carbide Corporation, as the Salk Institute's first full-time President and CEO, thus freeing Salk from administrative responsibilities. Kinzel's appointment jeopardized Bronowski's position as a Deputy Director. Bronowski tried, with support of other Fellows, to keep his position, arguing that "the Office of the Deputy Director has established the working framework for a self-governing community of Fellows," which was "working to the general satisfaction of everyone involved."[70]Despite these protestations the Board of Trustees advised Bronowski to resign from the position of Deputy Director and transfer the administrative functions to the new President.

With these changes, the time had come to move on with the humanities. The memorandum circulated among the Fellows in 1965 for the first time addressed the "humanistic studies" at the Institute at length, admitting that

> ...we have never spelled out a detailed humanities program for the

[68]Memo from J. Salk on the meeting in New York, 21 Jun 1962. JSP, box 350, folder 7

[69]*The Salk Institute Newsletter*, vol. 1, no. 4 (Dec 1966), p. 1

[70]Memorandum from The Resident and Non-Resident Fellows to the Board of Trustee. 23 Sep 1965. JBP, box 45

> Institute, although this is one of the truly unique and fascinating aspects of the place. Salk, Bronowski and Weaver have each addressed themselves to this feature of the Institute's purposes ...but there is no single document - and no agreed upon Institute policy or program - to which anyone can turn to precise information on this matter.[71]

The memorandum announced that Bronowski, stepping down from his position of the Deputy Director, will now devote himself entirely to his research and the development of the "humanistic" component of the Institute. To this effect, Bronowski suggested three areas of activity through which he believed this part of the Institute's life could begin to find an expression:

> The first division will be concerned with the special nature and identity of man ... which science has uncovered in this century, and which on the whole we have failed to communicate ... In general, this division will be much concerned with man's own view of himself, in biology, in the study of mind, in philosophy and in social studies. ...
>
> The second division would provide the historical background for our confidence that a good society can be inspired by man's belief in himself, in all field of knowledge. In particular, we want to assemble a Library of Man on a plan which will demonstrate the evolution of human thought. But I am not content here to regard history as a printed record of the past. At the heart of the library, there is planned a collection of records and films in which great scientists, thinkers, writers and artists will, as it were, bring man's view of himself to life. We want to have them speak (and be seen speaking, on film) about their lives, their work, their reflections on what they did and on the world that they helped to change.
>
> Finally, we must plainly have a division in which men of science, of the arts, and educators and laymen meet to discuss and disseminate the philosophical and the historical findings. In part, the meetings and conferences will be intended ... to familiarize men in the different disciplines with one another's outlook at first hand. .... But it is not enough to elucidate the unity of modern thought; it is essential to demonstrate that unity to everyone who wants to take an intelligent interest in the modern world. This is why our schemes included a Journal, a Theater and other modern media.[72]

[71] William Glazier, "Humanistic Studies," 19 Nov 1965. JBP, box 45

[72] ibid

This broad program sought to fulfill the original aspiration of the founders of the Salk Institute to develop a broad unifying agenda of the new institute, bridging together the cultures of sciences, the humanities, and the arts. Unification projects always have many cross-disciplinary connections. From his program Bronowski selected two major areas, in which biology and humanities could be integrated uniquely in the setting of the Salk Institute. One area was concerned with the interconnection of linguistics and biology. To stimulate the studies on the nexus of biology and linguistics at the Salk Institute Bronowski invited Roman Jakobson, a noted linguist with a parallel interest in biology, to come to the Institute as a Visiting Fellow. In addition, Bronowski suggested an appointment of a "junior person to be on Bronowski's staff at the Institute and to work in the general field of human and animal communication."[73] As he emphasized, he saw "this appointment, and Jakobson's periodic visits here, as the beginning of a study of the fundamental constituencies (genetic and social) which human and animal cultures share, and those in which they differ."[74]

Simultaneously with inviting Jakobson, Bronowski arranged for the visit of another noted scholar, Karl Popper, a philosopher of science with an interest in evolutionary biology. He came soon after Jakobson left as the next Visiting Fellow in the humanities, to stimulate interdisciplinary studies on "the philosophical implications of new scientific discoveries, ideas and methods, particularly as they are being traced out in the life sciences."[75]

These two areas, linguistics and philosophy of science with a special focus on the life sciences, seemed to have a potential to build real, not metaphorical, connecting links between the two cultures of science and the humanities, bound by

[73]ibid
[74]ibid
[75]"The Salk Institute for Biological Studies." The Institute's fund-raising brochure circa 1965.

shared scientific and programmatic concerns. In this way, as the founders of the Salk Institute hoped, it would become not merely an experimental institution for probing new disciplinary connections, but an agent for change and innovation in the disciplines themselves.

4.4.1 Integrating Science and the Humanities through Linguistics: Roman Jakobson and the "Language Studies" at the Salk Institute

Announcing the appointment of a Russian-born Harvard linguist Roman Jakobson as the first Salk Institute's Visiting Resident Fellow in the humanities, the Salk Institute's *Newsletter* underscored, in advance to his visit in June-July 1966, that Jakobson, whose "theories have become accepted as the basis for much of present-day research on the nature of language, ... has been a pioneer in the development of linguistics as one of the bridges between the sciences and the humanities."[76] Indeed, Jakobson was one of the best possible choices for a role of bridge-builder between the "two cultures." A key mediator between various disciplines and between different communities of scientists and scholars in different countries, Jakobson literally and physically bridged multiple academic and national traditions over his long career in linguistics and especially in the 1960s (Kay 2000, Gerovitch 2002).

One of the founding members of the Russian Formalist movement in linguistics in the 1910s, Jakobson was the co-founder of Prague Linguistic Circle in Czechoslovakia where he emigrated in the 1920. As a Jew, Jakobson had to move again in 1939, when, along with other prominent Jewish scholars he fled

[76]The Salk Institute's *News Abstracts*, Jul 1966, p. 2

Prague and finally found a refuge in the United States in 1941. After few interim appointments Jakobson started to work at Columbia University and established himself at Harvard, where he became professor of Slavic Languages, Literatures and General Linguistics in 1949.

Since his early involvement in the Formalist movement in the 1920s Jakobson developed a Saussurian view of language as a formal system of signs. This view implied emphasis on "precise terminology" and scientific methods of literary and linguistic analysis, seen as an alternative to the traditional rhetorical and narrative studies in the humanities. For Jakobson, as for many other Russian linguists, the alliance of linguistics with hard sciences was not only a way to turn linguistics into "true science" but also a panacea against "ideology." Jakobson's structuralist approach to linguistics was centered on the notion of phoneme - the elementary units of speech - devoid of intrinsic meaning, and deriving its value from its interactions with other phonemes.[77] This structuralist approach thus placed the emphasis on the processes of communication, implying that the message has significance only in relation to other messages. With such views, Jakobson was clearly riding the crest of the cybernetic wave and enthusiastically perceived communication theory as opening a new conceptual framework for all sciences, including linguistics.

In the late 1940s, in the intellectual milieu of Cambridge, Jakobson quickly established contacts with Norbert Wiener, soon after the publication of Wiener's *Cybernetics*, as well as with Claude Shannon and Warren Weaver, the authors of influential book *The Mathematical Theory of Communication* (1949). The latter was the combination of Claude Shannon's mathematical model for a general theory of communication that he developed as his wartime work, and

[77]See detailed discussion in Kay (2000) and Gerovitch (2002).

Weaver's "popularization" of Shannon's model. Weaver's popular account of Shannon's wartime work refashioned it as a fundamental "information theory" with potentially unlimited applications. Jakobson was instantly excited by the prospects of the information theory for linguistics. In his letter to Weaver, then the head of the Rockefeller Foundation's Science and Agriculture Division, Jakobson emphasized the relevance of the information theory to scholars in the human sciences:

> May I tell you as I continue to work on the problems of sound and meaning I realize still more the decisive influence of your and Shannon's book and I had the opportunity to discuss this matter with two outstanding French scholars both of whom would be happy to possess the book: C. Levi-Strauss and A. Koyré.[78]

The conversations with Weaver quickly developed into a proposal to explore the implications of information theory for various fields, to be pursued collaboratively at Harvard and MIT.[79] Jakobson outlined the program of study of different systems of communication, which included speech communication, with its "neurological and physiological aspects of the emission of reception," the engineering aspect of transmission, "the linguistic and mathematical aspects of the coding, encoding, decoding and recoding processes, logical and aesthetic analysis of the message," the psychological "inquiry into the behavior of the sender and of the

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[78]Roman Jakobson to Warren Weaver, 14 Feb 1950. Roman Jakobson Papers (MC72), MIT Institute Archives and Special Collections (**Thereafter RJP**), box 6, folder 37

[79]Jakobson hoped that the Rockefeller Foundation will help to fund such a trans-disciplinary initiative, explaining his proposal to the officers of the Rockefeller Foundation's Humanities Division, John Marshall and Edward d'Arms: "For several reasons, it is hard to find another center so appropriate for the creation of such a research group as Harvard. (1) There is a great understanding and adaptation for interdepartmental work and specifically for problem of bridging sciences and humanities in the field of communication ... (2) There is a high number of scholars and scientists specialized in the multifarious aspects of this field ... (3) There are fully equipped laboratories for the experimental studies which this work implies. ... (4) Harvardian linguists have consistently opposed any bias which reduces the frame of their science in its excluding a semantic and cultural approach. Such isolationist trends in linguistics are responsible for undermining a possibility of a cooperative solution of the intricate questions of communication" (Roman Jakobson to John Marshall and Edward d'Arms, 15 Jun 1951, RJP, box 6, folder 37)

receiver, and the sociological account of their interaction," as well as "the historical view of communication in relation to the time factor, and the geographical (areal) view with reference to the space factor."[80]

The proposal was not funded. However, a few years later, in 1957, the proposal resurfaced and was given fullest consideration. The Center for Communication Sciences, envisioned by Jakobson, was promptly established in 1958, although not at Harvard, but at the MIT-affiliated Research Laboratory of Electronics. The Soviet launching of *Sputnik* was a direct and immediate reason for this sudden reappraisal of the proposal. As historian Lily Kay pointed out, although it was a center for basic research, its military relevance was implicit. The linguistics analysis was central to cryptanalysis, which was the focus of Shannon's work, as well as for the national project for the development of automatic mechanical translation (mainly from Russian to English), and the Center received lavish funding being supported by major governmental agencies, including the CIA (Kay 2000, p. 301).[81]

Besides Jakobson, the Center for Communication Sciences' linguistic section included the MIT and Harvard linguists Roger Brown, Noam Chomsky, Morris Halle, Eric Lenneberg, William Locke, and K. N. Stevens.[82] The main task of this group was the "development of a general theory of language to be tested and elaborated through application to a wide variety of 'natural' languages."[83] Jakobson envisioned major expansion of the Center, with the addition of "smaller groups - at the University of Chicago, University of Michigan, Ford Institute for

[80]ibid

[81]For the discussion of the history of linguistics in the Cold War, especially in America, see Martin-Nielsen (2010).

[82]Draft of the introductory chapter to the "Survey of the Communication Sciences" [1959]. RJP, box 3, folder 64

[83]ibid

Behavioral Sciences, Stanford and the University of London."[84] As Jakobson noted, he hoped that the Center would eventually operate like the Institute for Advanced Study, with different departments - "Mathematics, Biology, Economics, Psychology" - contributing more or less equally to the study of language and communication.[85]

The Center for Communication Sciences failed to live up to this interdisciplinary vision. As Lily Kay put it, "it disappeared into the annals of history" (Kay 2000, p. 303). Kay has shown that this failure was the result of the "discrepancies between an expansive scientific imagination and the technical limits of information theory" (Kay 2000, p. 303). Jakobson himself saw the failure in the institutional constraints of the university. The military relevance of the Center's production narrowed the scope of research at the Center making Jakobson's vision increasingly irrelevant. By the early 1960s Jakobson was actively looking for an alternative institutional home.

The opportunities came from the West Coast. The new and growing campus of the University of California at San Diego emerged as an attractive possibility to Jakobson, to the extent that he suspended his arrangements with MIT and Harvard, planning to move to UCSD permanently. The negotiations started in 1962, when UCSD, established in 1960, began to shape its humanities departments. With the university's explicit commitment to "hard" sciences, the recruitment of the first faculty in the humanities at UCSD did not go without some animosity. Scientists who were recruited to become first UCSD faculty in the natural sciences complained that "the people who were being touted to us in the Humanities and Social Sciences were, by and large, just not of the same caliber as our scientists" (cit. Anderson 1993, p. 209). At the same time, the understanding was that, as

[84] "A Center for the Information Sciences. Report." RJP, box 3, folder 65
[85] ibid

Richard Popkin, the founding Chair of the Philosophy Department put it, UCSD's founders did not have the "intention of subordinating the humanities in the ways MIT or Caltech had done," and that the two traditionally polarized "camps" of sciences and the humanities might blend on UCSD campus (cit. in Anderson 1993, p. 210).[86] The shared sentiment was that the humanities should not be restricted at the new campus to function as mere "services" to the university, supplying "general education" courses and writing programs to the colleges. The university Chancellor John K. Galbraith encouraged the attempts of the humanities and social sciences departments' chairs to lure the best scholars offering attractive conditions and opening possibilities (see Anderson 1993).

Literature was first humanities department established at UCSD in 1962, followed by a separate Department of Linguistics. The linguistics department was to be headed by Leonard Newmark, then a thirty-five year old linguist regarded as one of the best in the country (cit. in Anderson 1993, p. 211). The separation of literature and linguistics at UCSD was an innovative move that gave linguistics a social science identity instead of being construed around the "humanities" courses as was usually the case in the 1960s. Newmark used the opportunity to full extent, setting up an ambitious plan to move the entire linguistics "branch" of the Center for Communication Studies from the MIT to UCSD. By 1964 the negotiations looked like being close to realization. Newmark updated Jakobson on the recent developments:

> It is imperative that we learn as soon as possible whether the plan to move Cambridge to California - you, Chomsky, Halle, and Miller - is feasible. ... George Mandler, who will probably be the chairman of the department of psychology here ... talked with George Miller in Cambridge and came away with the impression that Miller might well move to La Jolla if Noam did. ... Things seem to be going well here:

[86]See Richard Popkin's own account on the beginnings of the Philosophy Department at UCSD: Popkin (2003).

> Galbraith has taken an increasingly stronger role in the planning and recruiting functions of the University ... We have $25,000 that we can spend right away. I propose that we think about ... a conference next year to bring together the relevant people ... [and you can] go ahead with your plans to have xeroxes made of materials you would want in the Center here.[87]

After long negotiations the plan, however, did not work out, collapsing largely because of the impossibility of meeting one of the conditions set by Jakobson, who insisted that his wife, scholar in Slavic studies Krystyna Pomorska, would be appointed to the Literature Department on the conditions equivalent to those she had at MIT.[88] Pomorska felt that the position she was offered by the Literature Department was not fair and Jakobson quickly resumed his arrangements with Harvard and MIT. While UCSD's arrangements with Jakobson had collapsed, along with the plan "to move Cambridge to California," the Salk Institute emerged instead as an attractive opportunity.

Jakobson met Bronowski and Jonas Salk while still pursuing negotiations with UCSD, having visited the Salk Institute during one of his visits in La Jolla between 1962 and 1965. The Salk Institute charmed Jakobson by its physical beauty and intellectual intensity. Moreover, by this time Jakobson had developed an intense interest in molecular biology. In the 1960s, the parallels between the genetic and verbal codes provided a vivid imagery for a variety of scientists who tried to "crack" the genetic code.[89] As Kay put it, Jakobson was "swept up by the euphoria around breaking the genetic code" (Kay 2000, p. 304). With his interdisciplinary vision of linguistics as the science of signs, Jakobson was enthusiastic to extend this vision to include "possible relation to molecular

[87]Leonard Newmark to Roman Jakobson, 8 Sep 1964, RJP, box 3, folder 108

[88]Roman Jakobson to John Galbraith, 5 Apr 1965, RJP, box 3, folder 108; Leonard Newmark to Roman Jakobson, 6 Nov 1964, RJP, box 3, folder 108

[89]See discussion in Kay (2000)

information code," exploring the parallels between phonemes, the elementary units of speech, and genes; and between "verbal heredity" and genetic heredity.[90]

The Salk Institute, with its stellar staff consisting of several luminaries in molecular biology research, at the same time committed to "bridge" the two cultures of science and the humanities, presented an excellent opportunity to develop the kind of interdisciplinary study Jakobson had aspired to launch at Harvard, as well as to accommodate Jakobson's own interests in the connection between linguistics and biology. As general manager of the Salk Institute William Glazier reported to Bronowski about his conversation with Jakobson, "He spoke to me at length about how he saw linguistics as the first bridge between the sciences and the humanities. He explained that he was more and more drawn to biology in projecting his future work and to the Salk Institute because of the kind of people who work there."[91]

Jakobson's interest in the interrelation between biology and linguistics was not limited to the parallels between the verbal code and the molecular code. Within evolutionary thought, linguistics and biology were closely interconnected. Jakobson was introduced to evolutionary biology in his twenties, when, as he recollected in 1968, he became

> ...interested in the problems of [the lines of] similarities between biology and linguistics. I was very much impressed, while an adolescent, by the work of a great Russian biologist, Berg, whose book on *Nomogenesis* appeared in the early 1920s in Russian and was immediately translated in English and published in Great Britain. This book introduced me to the questions of evolution and evolutionary theory ... and biological analogies ... in linguistics.[92]

He found that the analogies between the development of human languages

[90]On Jakobson's engagement in biology see Kay (2000), especially chapter 7.

[91]William Glazier, "Arrangements for Dr. Roman Jakobson, Memo to J. Bronowski," 7 Dec 1965. JBP, box 43

[92]Transcript of the debate "Vivre et parler" 1968 (in French). RJP, box 18, folder 49

and the evolution of species from a common ancestor – the view widely promulgated in the 19th and early 20th century – were "premature," but these analogies sparked his interest in evolutionary biology. As he wrote in his notes in the 1930s,

> Causal explanation [of the similarity between the evolution of species and evolution of languages] is unconvincing. However, it is possible that there is [a foundation for] an analogy between biological laws of evolution and the laws of the evolution of language systems. It's necessary to acquaint oneself with biology.[93]

Jakobson's first book on the "phonological evolution," in which he pursued the ideas about the biological foundation of the differences between languages (the ideas he later came to consider "premature"), was published as early as 1929.[94] While pursuing his other interests, Jakobson cultivated his interest in evolution and the biological foundations of language. In the 1940s he embarked on the studies of aphasia - a linguistic disorder which manifests itself as a set of diverse syndromes of the disintegration of language - finding it a "very productive" way to study the evolution of human language (see Jakobson, 1968).[95]

The Salk Institute, as a biological institution welcoming the possibility to erect bridges connecting sciences and the humanities, seemed to offer the possibility, for the first time in his academic career, to pursue a broad program that would focus on the interconnections between linguistics and biology. As he later attested,

> In the 1960s, reading in molecular biology and especially the conversations with biologists and my work at the [Salk] Institute of Biological Studies, led me to the conviction that it's possible to find not only isomorphisms but connections much more profound

[93] Unfinished draft, 4 Aug 1932 (in Russian). RJP, box 35, folder 6

[94] Transcript of the debate "Vivre et parler" 1968 (in French). RJP, box 18, folder 49

[95] In his paper "On Aphasic Disorder from a Linguistic Angle" [1973] Jakobson emphasized "the vital necessity of inquiring into child language and aphasia for linguistic theory and phonetics" (Jakobson (1980), p. 94).

> and important for linguistics, and, judging by what I have heard from biologists, for biology as well.[96]

Jakobson suggested a broad program of research, writing to Bronowski:

> The program of research which I planned with respect to our cooperation and which seems to me the most productive area of my work, both for the Institute and for myself, encompasses ... [a] complex of problems closely linked with the biological background of language. ... Questions which at present particularly interest me in this connection [are]: The symbolic, iconic and indexical components of language; the structure of the verbal code on its phonemic and semantic ... level; the problem of verbal creativity and the diverse functions of language (cognitive, emotive, conative, phatic, metalingual and poetic); language as a convertible code, its stability and mutability in time and space; radius of communication and internal language; information and redundancy; acquisition of language and glottogenetic hypotheses; language pathology (aphasia and mental illnesses); synesthetic problems; sound features and visual associations; the relations between language and other semiotic systems; the stratified structure of language; the typology of languages and universal laws. Some of these problems still need interdisciplinary discussions and experiments. ... I am eager to learn what you think about this program.[97]

The program was broad and forward-looking, and Bronowski thought highly of it. However, by this time Jakobson had already re-established his old arrangement with MIT and Harvard, following the collapse of the scheme with UCSD, and had committed himself to continue at MIT as a research scholar and at Harvard as a consultant at the Center for Cognitive Studies for three more years. The solution was found in an arrangement with the Salk Institute according to which Jakobson was appointed a Visiting Fellow in 1966, with the possibility of appointing him a Non-Resident Fellow in the following year.[98]

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[96] Transcript of the debate "Vivre et parler" 1968 (in French). RJP, box 18, folder 49

[97] Roman Jakobson to Jacob Bronowski, 21 Jul 1965. JSP, box 397, folder 3

[98] William Glazier, "Arrangements for Dr. Roman Jakobson, Memo to J. Bronowski," 7 Dec 1965. JBP, box 43, folder 1

In the summer of 1966 Jakobson spent two months at the Salk Institute as a Visiting Fellow, participating in the seminars and giving lectures. Although the plan to affiliate Jakobson with the Salk Institute as a Non-Resident Fellow never materialized, Bronowski suggested to go ahead with additional appointments in his "department" envisioning a long-term program initiated by a young researcher who had been trained by Jakobson and who could move to the Salk Institute permanently. "I have in mind a young research worker with a special interest in the problems of human and animal communication which we share,"—Bronowski explained to Jakobson asking him to recommend a suitable candidate.[99]

Ursula Bellugi, a recent graduate from Harvard with a doctorate in psychology and linguistics, and a research associate of Roger Brown at the Center of Communication Studies, was the researcher Bronowski appointed to work on linguistics studies. Bellugi moved to La Jolla with her husband, Edward Klima, Jakobson's student and his close associate and colleague at the MIT, where Klima taught from 1957 to 1967. While Klima assumed a professorship position at the Department of Linguistics at UCSD, Bellugi was appointed in 1968 as a research associate of Bronowski. Working with Klima and using her background in language acquisition in children as a starting point, Bellugi soon developed a research program of her own, focusing on sign language. As she described the program in 1973 in the Institute's Annual Report,

> As a way of examining the biological foundations of language, we are investigating language as it develops in another modality. This is making use of an "experiment of nature" in which hearing is not present from birth to examine the development of language which uses the hands and body ... instead of vocal apparatus, and which relied on the eye for analysis instead of the ear.[100]

[99] Jacob Bronowski to Roman Jakobson, 27 Jul 1965. JSP, box 397, folder 3

[100] U. Bellugi, "Annual Report of the Laboratory for Language Studies," The Salk Institute Annual Reports, 1973.

The studies initiated by Bellugi developed into an extremely productive research program. In 1970, Bellugi founded her own "Laboratory of Language Studies" at the Salk Institute, supported by the grants from the NIH and NSF. It was decidedly not part of Bronowski's "humanistic studies," though. Rather, Bellugi positioned her research as a scientific program in the neurosciences and cognitive sciences. The name of Bellugi's laboratory changed twice, reflecting its leaning towards the "scientific" pole of the cultural divide. In 1978, following the dissolution of the humanistic component of the Institute, the name of the laboratory had been changed to become "Language and Cognitive Studies," and in 1991 it became "Laboratory for Cognitive Neurosciences."[101] While this certainly proved to be a productive line of research, the integration of sciences and the humanities through the studies of language was accomplished at the expenses of the humanities' component.

4.4.2 Integrating Science and the Humanities through Philosophy of Science: Karl Popper and the "Logic of Biology"

In his statement describing his program of proposed research to Jonas Salk, Bronowski pointed out that the new science of molecular biology needed to be assessed with regard to its implications for philosophy, with its explanatory structures, conceptual problems, and evidentiary relations to be taken as an object of philosophical analysis in its own right. At Cambridge Bronowski was exposed to the philosophy of science of Bertrand Russell and Ludwig Wittgenstein, attending their lectures as a student. Referring to the situation in the physical sciences in the

[101] The Salk Institute Annual Reports, 1978 and 1991.

1930s, Bronowski argued that developments within the biological sciences, most spectacularly associated with the rise of molecular biology, presented philosophy of science with a similar challenge: just as the discoveries in atomic physics at the beginning of the century challenged the picture of the structure of the physical world that philosophers had long agreed upon and this challenge gave rise to philosophy of physics as a centerpiece of philosophy of science, in a similar way, in the second half of the century, biology had emerged as a central science for philosophy of science:

> The first half of this century has been a time of wonderful advance in the physical sciences. ... Now it is clear that the second half of the century is going to be as heady a time of advance in the biological sciences. ... These are the discoveries which fire me now. I am attracted by the methods (particularly the methods of reasoning) which underlie the new biology. Hitherto, the philosophy of science had been preoccupied with physics; I want to turn it to study the logic of biology.[102]

In 1965, Bronowski invited Karl Popper to come at the Salk Institute as a Visiting Fellow to collaborate on the study of the "structure of scientific laws" in physics and biology and the "philosophical issues which underlie physical laws and biological laws."[103] Popper was intensely interested in the proposal: while Popper's classic, *The Logic of Scientific Discovery,*[104] was based entirely on the physical sciences, in the 1960s Popper turned his attention to biology, in particular, focusing his philosophical analysis on evolutionary theory and the philosophical problems arising from it (see Aronova 2007).

Popper's interests in theoretical biology were long-standing: he participated in the informal "Biotheoretical Club" in London in the 1930s, and became a permanent member of it after WWII when he settled in London and established

[102] "Bronowski - personal statement." Jul 1962. JSP, box 397, folder 1
[103] "Humane Studies," The Salk Institute Annual Scientific Report, 1968-1969
[104] First published in German in 1935 and translated in English in 1959

himself at the London School of Economics (see Abir-Am 1987, Aronova 2007). In the 1960s, biology became Popper's major interest replacing physics as an object of philosophical analysis. Popper's first public discussion of evolutionary theory was his 'Herbert Spencer Lecture' given at Oxford in 1961. In his Spencer Lecture Popper argued that although the Neo-Darwinian theory is unfalsifiable and has a tautological character, it might be "improved" as a theory. Popper suggested several schemes along the lines of what he called the "enrichment" of Darwinism, arguing that more attention should be given to the role of behavior and suggesting that in evolutionary developments the genes controlling behavior prepare the way for changes in the genes controlling anatomical traits (his so-called "Spearhead model").[105]

The invitation from the Salk Institute pleased Popper very much. Intellectually, it sounded like a great opportunity to refine his views and to garner support from practicing molecular biologists. Popper's deliberations on scientific status of evolutionary theory and his "spearhead model" had met a chilly reception from evolutionary biologists. Even his friends, Ernst Mayr and Peter Medawar, urged him not to publish his Spencer Lecture. As Popper wrote to Bronowski, accepting the invitation to visit the Salk Institute,

> All I have obtained almost without help from professional biologists (with the exception of some very valuable criticism from Medawar - of a stage of my researches which has since been superseded). You will understand how much I should welcome some kind of co-operation with Biologists; and that for this reason alone, I should very much like to visit your Institute.[106]

[105]The "spearhead model," first introduced by Popper in his Spencer lecture in 1961, and then discussed in more detail in his Compton lecture (1965) and in the lecture given at Emory University in 1969 (See discussion in Aronova (2007)) .

[106]K. Popper to J. Bronowski, Jul 24, 1965. Karl Popper Archive, Stanford University, Hoover Institution Library and Archives (**Thereafter KPP**), box 346, folder 8

In advance of Popper's visit, Bronowski asked him to give a seminar and to present his analysis of Darwinism as a scientific theory to the Fellows of the Institute.[107] Popper enthusiastically agreed to do this, noting that he "should very much like to discuss some of my problems (and I hope results) ... in the lines of my Spencer lecture ... with members of your Institute." [108] As he further elaborated,

> I have been working hard on a development of evolutionary theory on strictly Darwinian lines, with the following minimum aims: (1) avoid the almost-tautological character of the survival of the fittest (where "the fittest" are whose that survive); (2) avoid abandonment (with which I blame the Neo-Darwinists) of the Darwinian attempt to explain what may be vaguely and intuitively called "evolutionary ascent" or "evolution towards higher forms"; (3) explain ... "higher" in non-anthropomorphic terms.
>
> I have, I believe, not only succeeded in this minimum programme, but I have, actually, obtained some extremely interesting *testable* results which, it seems to me, should have a considerable effect on the direction of experimental research in genetics."[109]

In November1966 Popper was appointed a Visiting Fellow for up to three months, "to work with the Fellows of the Salk Institute on the problems in the philosophy of science as they apply to biology in general and in particular to the understanding of evolution."[110] Popper spent two months at the Institute. He gave two seminars, one entitled "Still fighting against fashions in science," and another simply "Evolution." In addition, a joint seminar by the Salk Institute and the Philosophy Department of UCSD featured Popper's seminar on scientific method, "Three idols of empiricism: precision - definition - senses and their data."[111] Most of his time at the Institute, however, Popper spent writing prolifically: in 1967

[107] Jacob Bronowski to Karl Popper, 27 Jan 1965. KPP box 82, folder 10
[108] Karl Popper to Jacob Bronowski, 12 Feb 1965. KPP, box 82, folder 10
[109] Karl Popper to Jacob Bronowski, 24Jul 1965. KPP, JSP, box 346, folder 8
[110] "Certificate of eligibility for exchange visitor status," signed by Jonas Salk. 3 Nov 1966. KPP, box 346, folder 8
[111] Seminar flyers, 1967. JBP, box 105

Popper published eight papers, most of them written, finished or conceived during his staying at Salk Institute.

For Bronowski Popper's stay promised to give weight to his own program at the Institute. A series of Bronowski's publications were influenced by his discussions with Popper. Immediately after Popper's visit, Bronowski gave a series of lectures on *Nature and Knowledge: The Philosophy of Contemporary Science*, published as a book in 1969. His essay "Humanism and the Growth of Knowledge," written in 1968 and published in 1974, was his contribution to a collective tribute to Popper in the *Library of Living Philosophers*. These two sets of writings constitute Bronowski's major statements in philosophy, in which he identified Popper as his closest philosophical ally. At the same time, Bronowski was critical of Popper's concern primarily with the testability (or, falsifiability) of theories rather than with their specific content. In Bronowski's view, Popper approach to scientific method, with its focus on theories and tests, and with its view of theories as a series of propositions and challenges, could only advise scientists about testing their theories, but could hardly give any useful insight on how to arrive at a coherent explanation of the world. In his lectures delivered at Columbia University in 1969, later published as *Magic, Science, and Civilization* (1978), Bronowski remarked:

> And though I have a great admiration for my friend and colleague Karl Popper, in his recent work he has begun to stress the notion that there is a great problem solving element in making laws of science. I think he suffers, as so many of his colleagues do, from the fact that he really isn't used to know how a laboratory carries on. There aren't any clear-cut problems; there certainly aren't any decisions in which you set up an experiment and you say, "Here's a law, here's a hypothesis, I challenge it, I am going to negate it." Instead, it all works by a highly tentative and experimental process.[112]

[112] Bronowski (1978), p. 54

All in all, the first experience with the visitors in the humanities was considered a success. Melvin Cohn recollected that Fellows found Popper and Jakobson quite congenial, especially Popper who, in Cohn's view, was "a great addition to the Institute," introducing "an aspect of the way how to think about science and what to believe as a result of the experiment that we never would have considered."[113] At the same time, as Cohn noticed, he was the only scientist at the Institute who really interacted with Popper.[114]

Regardless of the level of the actual cooperation between Popper and scientists of the Salk Institute, the philosopher's residency was highly praised as "a basis of cooperative effort with biologists at the present time." [115] The report on Popper's stay in the Institute's *Newsletter* announced that more visitors in the humanities will follow, as

> Dr. Popper's residency was part of a program to bring leading international thinkers and scholars to the Salk Institute [that] has been made possible as a result of the recent move of the Institute staff into its new laboratories where space necessary for such programs is available. As a result of the move, this ... program of the Institute is gathering momentum. [116]

Indeed, the completion of the construction of two laboratory buildings and the activation of the North building in 1966 revived some of the initial plans and aspirations of the Institute's founders with regard to the possibility to bridge the two cultures of sciences and the humanities in this high-profile setting.

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[113] Melvin Cohn, interview with the author, Nov 2004.
[114] Melvin Cohn, interview with the author, Nov 2004.
[115] *Salk Institute Newsletter.* Dec 1966, vol. 1, no. 4
[116] *Salk Institute Newsletter.* May 1967, vol. 2, no. 1

4.5 Molecular Biology as a Unifying Framework: Expansion of the Scientific Programs at the Salk Institute, 1966-1968

By mid-1960s, the unification discourse had gradually shifted its rhetoric from the "unity of science/humanities/arts" discourse typical for "scientific humanism" to the "unity of biology" rhetoric centered on molecular biology. Thus, in 1967 the program delineating the expansion of the Institute into new areas of neurobiology and behavioral sciences presented the Salk Institute as the institute designed for "unified research," emphasizing that molecular biology for the first time "has created a new unity of outlook and method for the life sciences as a whole. ... opening unique opportunities to study from the same standpoint and by a coordinated method all the expressions of the life of man - his bodily development, his nervous and mental activity, and his social conduct and aspirations."[117]

In 1966, following the move into the permanent laboratory building, the Institute entered a period of intense discussions of the long-term plans and expansion of its scientific programs. In February 1967, summarizing these discussions and the proposals received from different Fellows, Renato Dulbecco and Jonas Salk presented the Annual Fellows with their joint memorandum, which stated:

> We have reached the general conclusion that the Institute cannot survive, or even justify its existence, as an institute for molecular biology. The projected addition of neurobiology would add, undoubtedly, great significance to the Institute. However, we have been seeking an even broader justification, in terms of the objectives of the Institute as they were formulated at its inception, namely that the Institute should be an organism *in the service of man.* [118]

[117] "Fellows' Plan of the Institute, 1966-1967," Aug 1967. JSP, box 362, folder 8

[118] Renato Dulbecco and Jonas Salk, "Proposals for the Academic Development of the Institute,"

The dramatic advances in science and technology made science “a major moving force of the modern world.”[119] At the same time, as Dulbecco and Salk elaborated, one of the pressing problems of the time was the growing “gap between the scientists and the other group of men who play a major role in the development of the world, such as politicians, business people and executives with important responsibilities, judges, writers, etc. ... – the ‘action world”’:

> It seems that the gap between scientists and the action world arises because each group performs its activities by using different capabilities of man’s mind. These aspects of the human mind appear totally unrelated to each other because their connections are deeply buried in the complexity of the mind. If either group asked what the universal or unique nature of man is, in a way that question could be answered, a far greater understanding between them could result. As present, the reasons for man’s motivations are usually unknown, and it is impossible to make a prediction of the future consequences on man of both discoveries of scientists and decisions of a political or economic nature. But if these motivations could be understood, the operation of the groups that represent major moving forces of the world would cease being extraneous or even hostile to each other, and could become constructively coordinated.[120]

Towards this end, they argued, the new goal for molecular biology that emerged at this juncture might be seen in the development of new experimental approaches to the study of the human mind, as well as the expansion of molecular biology into social sciences dealing with mind and its workings. As Jonas Salk put it, “biological scientists can contribute to the advance of what is often referred to as the ‘soft sciences’ ... advancing our understanding of man’s uniquely human condition as these are influenced by his genetic constitution and the environmental influences.”[121] Dulbecco agreed, suggesting that the time has come for molecular biology to contribute to the understanding “the causes of individuals’ behavior.

22 Nov 1966. Document E, Annual Fellows Meeting 7-8 Feb 1967. JSP, box 362, folder 8

[119]ibid

[120]ibid

[121]Jonas Salk, “Statement on Institute Objectives,” 29 Dec 1966. JSP, box 402, folder 12

... When the mechanisms that bring about human reactions (e.g. hostility, friendship, suspicion) are known, ... on this basis the behavior of individuals can be predicted."[122]

Other Fellows were more cautious with regard to the expansion of the institute's programs into the areas traditionally occupied by the social and human sciences. Leslie Orgel voiced a shared sentiment suggesting a more modest strategy, explaining to Jonas Salk: "While all the Fellows have a deep interest in social problems ... we do not at present feel we have much to offer to, say, development psychology."[123] Orgel suggested to adopt a strict "basic-science" approach and narrow the focus to scientific programs at the moment:

> The unique contribution which the Institute could make would be the development of a unified approach to the study of brain function based on neurophysiology and neuroanatomy on the one hand and genetics and molecular science on the other. In the short term we should pursue this goal single-mindedly. If money becomes available, so much the better; if it doesn't we shall at least have the satisfaction of having tried to do something we believe in. In the short term, I am now strongly opposed to appointments in such areas as child-development which have no natural tie-in with our professional interests. ... [After] we have established ourselves in the basic neurosciences ... we could then consider work in selected areas which have direct social relevance, but again only by remaining within the most quantitative areas of psychology. ... I would like to state my conviction that even if our achievements are to be measured

[122] Renato Dulbecco, "Material for Institute's plan. Memorandum for Jonas Salk," 27 Jan 1967. JSP, box 403, folder 12. The Salk Institute, Dulbecco argued, was in a unique position to bring molecular biology to the elucidation of the mechanisms of the working of human mind, and to revolutionize traditional approaches and disciplines dealing with these questions, above all psychology. The Salk Institute's scientists, many of whom, as Dulbecco underscored, the founders of the new science of molecular biology themselves coming from diverse disciplinary backgrounds, were "not afraid to move from a comfortable scientific niche [that] they have themselves created, into a new area [since] they have done this in the past already; each scientist has had experience with more than one discipline, like physics and biology, or physics and medicine, theoretical chemistry and biology, or mathematics and humanities" (ibid). The structure of the Salk Institute was an advantage as well, as Dulbecco pointed out: "Since the Institute has no internal barriers, it can make a unitary effort in this direction better than most other Institute and Universities" (ibid).

[123] Leslie Orgel, "Memorandum to Jonas Salk," 1967. JSP, box 403, folder 12

> in terms of application to human problems, over a ten year period we are most likely to be judged successful if we adopt the "basic-science" approach in a quite rigorous way.[124]

Among these two competing views, Orgel's was in a minority. Dulbecco's and Salk's proposal became the basis for the refined plans of the new phase of the Institute's development. The major rationale behind moving ahead with a more ambitious and socially relevant agenda of the Institute was a pragmatic one: a more ambitious program promised to attract new funds to the Institute, both from private foundations and from governmental funding bodies like NIH. Moving into the "big ventures" was seen as the only possible solution to the budgetary crisis that the Institute was facing, as Louis Kahn's masterwork consumed the substantial part of the initial grant from the National Foundation that was intended to create an endowment. Mingling pragmatic and intellectual rationales for the institute's expansion, Warren Weaver in his interview to *Scientific Research* in July 1967 praised the Salk Institute as the harbinger of the molecularisation of brain research, which was also opening new funding opportunities:

> There is one area which I personally think is being missed by foundations at the moment. .. Just as it seemed entirely clear in 1932 that the next great wave of science was going to be in biology, I am now convinced that molecular biology has now reached a point where it can make over the next five to twenty-five years some extraordinary advances in the general field of molecular analysis and interpretation of neurophysiological problems. We are going to learn something about the mind, the brain and behavior. For the first time we are going to learn it with our feet on the ground. We're going to find out whether the mind is in the head or isn't. We're going to find out how we learn. We're going to find out how we remember. We may even find out how we forget. ... But we really need large imaginative free support at the present time and here's one of the very best places in the world - the Salk Institute.[125]

[124] ibid

[125] "Extract from an interview with Dr. Warren Weaver by *Scientific Research*, July 1967," Appendix 7, "Plan of the Salk Institute for Biological Studies." Aug 1967. JBP, box 98

This plan adopted at the Fellows' meeting was approved by the Board of Trustees in August 1967. It endorsed the view that "the breakthrough in biology has created a new unity of outlook and method for the life sciences as a whole," as the time yielding "an integrated and coordinated method of research ... to study life whole, from a single biological viewpoint."[126] The plan for the development of the Institute stressed the unifying role of molecular biology, as "the recent breakthroughs in biology have initiated unique opportunities to study from the same standpoint and by a coordinated method all the expressions of the life of man - his bodily development, his nervous and mental activity, and his social conduct and aspirations":

> The field of social and human studies has been separated from the exact sciences in the past. But it is a cardinal feature in the Institute's plan to break down this separation, and to establish a modern union between biological principles and the psychological, social and ethical study of human problems. One of the responsibilities of science to society today is basic and rigorous research into the human nature of society; and this is an overall responsibility which all the Fellows share. ... The Salk Institute has been created ...by assembling ... a body of Fellows with a record of achievement in many fields, to work together as a community of scholars with a common purpose: to understand and to foster the full potential of life.[127]

The program for the next several years of the Institute's activities foresaw strengthening the research programs in the areas where the Institute was already active - the research focused on the study of intracellular regulation and its mechanisms at the genetic and molecular level, while expanding it first to the level of intercellular regulation (the study of regulation involving multi-cellular systems: immune system, developmental biology, the nervous system), and then moving to the organismic level (the study of regulation of complex systems).

[126] ibid
[127] ibid

The areas of immediate interest in the last two categories, highlighted in the Institute's plan, included: patterns of regulation of embryonic development, aging, sexual behavior, the organization of language, and "artificial intelligence with computers."[128] Finally, as the last stage of this expansion into the new areas, the Institute planned to turn to human and social patterns. The Institute's plan highlighted the following areas of study as its most feasible foci of research: "animal communication, group and ritual behavior, population pressure and control; selected studies in human relations (social aspects of shelter, food, and sleep; stabilities of interaction in work, trade, entertainment, and other associations; self-organization in group movements); and implications for the individual and for society of scientific (particularly of biological) discovery."[129]

The new ambitious plan was to be realized through the new appointments envisioned both on the level of faculty, where several major appointments in the area of neurobiology were in order, and on the top administrative level. The search for the new President was already underway, as the Institute now needed a figure of stature who could effectively lead the Institute during the next phase of its development. The person selected by the Search Committee was Joseph Slater, the Ford Foundation senior officer who ran its International Affairs program.

[128] ibid
[129] ibid

4.6 Joseph Slater, the Congress for Cultural Freedom, and the New Agenda for the Salk Institute

In congratulating Slater with his new appointment Robert Sheeks, an executive from the Asia Foundation wrote:

> If I were asked to name a person who could lead an institution ... and to develop it from one addressed to cellular and molecular research to one addressed fully to life processes and to mankind's potential, it would be Joseph Slater. The opportunity calls for something that you have been doing, and for which you have unique talent. This is the effective linking of ideas and efforts across conventional boundaries, whether these involve nations, professional fields or other seemingly sacred units.[130]

Indeed, the new Salk Institute's president seemed to be the best choice the Institute could possibly make to implement its ambitious plans. Before joining the Salk Institute in 1967 as its new President, Joseph Slater held various posts with the U.S. government in the U.S. and abroad. A Berkeley graduate with degree in economics, Slater received his first major post in the aftermath of WWII, being appointed as the Secretary General of the Allied High Commission in Germany responsible for drafting the economic recovery plan for West Germany. In 1952, he moved to Paris where he served as the executive secretary in the office of the U.S. representatives to NATO and the Organization for European Cooperation. He subsequently shifted to the diplomatic service, and in 1957 joined the Ford Foundation, which was building its International Affairs Program, under the directorship of Shepard Stone, Slater's future boss.

[130]Program of Action. A Supplement, volume 1. May 1968. Section 20: Letter from R. Sheeks to J. Slater. 12 Nov 1967. JBP, box 98

The Ford Foundation's International Affairs program was established in 1957 with the primary aim of countering the threat of totalitarianism and Soviet political, military, intellectual and cultural expansion.[131] In the mid-1950s, the Soviet Union was no longer viewed as a serious competitor of the U.S. in the intellectual and cultural realms, although still a threatening force in power-political terms. Yet, the decline of Soviet influence didn't mean that the cultural and intellectual hegemony of the U.S. in Cold War competition with the Soviet Union was secure. Conflicts in American domestic politics and the beginning of American involvement in the Vietnam War caused public outcry, at home and abroad, targeting what was increasingly seen as American "imperialism" - cultural, military, and economic. Against this background, the support for cultural and scientific exchange through grants for research, conferences, and exchanges of information — the primary activities of the Ford Foundation's International Affairs program - acquired a renewed significance, being seen as a means to re-build bridges across the Atlantic.

From its inception the Ford Foundation's International Affairs was in close contact and collaboration with the Congress for Cultural Freedom, with which it shared an agenda as well as the network of key people involved. The Ford Foundation funded the CCF activities through its Intercultural Publications program, launched in 1952, which supported many of the Congress' initiatives, including the funding of the CCF journal *Der Monat* (Saunders 1999). In 1967, after the revelation of the CIA connection and the following moral crusade of the CCF, the reorganized Congress, now named International Association of Cultural Freedom (IACF), was funded entirely by the Ford Foundation (see chapter three).

[131] On the politics of the Ford Foundation see Krige (1999, 2001, 2006), De Greiff (2001).

The key link between the Ford Foundation and the Congress was Slater's supervisor, Shepard Stone, who, Frances Saunders has put it, "was so closely connected to the CIA that many believed he was an Agency man" (Saunders 1999, p. 143). In 1953, Stone spent a month in Europe, visiting leading Congress people, including Michael Josselson who invited Stone to get acquainted with the Congress' operations. The director of the Ford Foundation's International Affairs since 1954, Stone was in a key position to broker millions of dollars of Foundation's funds for the Congress. Not surprisingly, Stone was Josselson's first candidate for his successor when he had to resign from the Congress in 1967. Shepard Stone became the head of the reorganized Congress, the IACF.

The legitimation crisis of the CCF that followed the revelation of the organization's connection to the CIA in 1967 coincided with the period of major turnover within the Ford Foundation. All senior program executives left their positions by the end of 1967.[132] In the fall 1967 both Stone and his closest associate Joseph Slater had left the Ford Foundation, Stone for Paris where he became president of the International Congress for Cultural Freedom, Slater for California where he assumed the presidency of the Salk Institute. Their paths crossed again in the early 1970s when Slater, who became the president of the Aspen Institute for Humanistic Studies in Colorado in 1969, helped Stone to establish the Berlin Aspen Institute. Both Aspen Institutes, in Berlin and in Colorado, played major

[132]The International program of the Ford Foundation was quite successful; however, the arrival of the new Foundation's president, McGeorge Bundy, in 1965, led to the reorganization of program and the replacement of an older group of officers with his own trusted people. 1967 Ford Foundation annual report noted: "The end of 1967 found us nearing the end of the period of major turnover among our senior officers which began in 1965. In this last year ... four of our senior program executives have moved to other major assignments. John Coleman left us to become president of Haverford College ... Malcolm Moos has gone to the presidency of the University of Minnesota ... After thirteen years and ten years, respectively, Shepard Stone and Joseph Slater have left us—the former for Paris, to lead the International Association for Cultural Freedom, and the latter for California, to lead the Salk Institute." (*http://www.fordfound.org/elibrary/documents/1967/021.cfm*, accessed 20 May 2012)

role in the Congress' activities after its reorganization in 1967 (see chapter three).

As a former foundation executive, Slater had all the necessary contacts and standing to raise funds necessary for ambitious new programs at the Salk Institute. By the 1960s, the Ford Foundation's philanthropic activity expanded considerably into the territory of the biological sciences. In the 1950s and 1960s it made "behavioral sciences" one of the prioritized areas of its support, distributing grants for research in such controversial areas as political, social, and individual behavior, stressing that problems of human behavior could be approached from the vantage point of psychology, anthropology, sociology and related fields rather than political science or economics (see Appel 2000, Kay 1993). Studies of the biological basis of human behavior fitted this agenda very well. Leading practitioners in molecular biology research became beneficiaries of the Ford Foundation at that time. Thus, for example, Linus Pauling received nearly $1 million in grants from Ford Foundation for biochemical studies of mental deficiency and biological studies of human behavior (Kay 1993).

Slater moved to the West coast in January 1968, and started his term as President by hiring consultants to strategize the Institute's move to the next phase. One of the consultants recruited by Slater was John Hunt - the former executive secretary of the CCF and Josselson's secretary in charge of the Congress' day-to-day operations – and the CIA's second man inside the CCF office in Paris.[133]

[133] See on John Hunt: Nilsson (2011), Saunders (1999), Coleman (1989). Despite the fact that Hunt's association with the CIA was revealed during the scandal over the CCF in 1967, when Stone assumed presidency of the reorganized Congress he decided to keep John Hunt at least during the first year of his presidency, "to help with the budget"(Saunders 1999, p. 411). By the end of 1967 Hunt left the reorganized Congress and joined the Salk Institute, initially as a consultant but already a month later appointed as the Institute's Executive Secretary, replacing William Glazier. Hunt's experience as intellectual entrepreneur at the CCF – his primary role within the Congress was to help running the CCF seminar program and to participate in the formulation and execution of the seminars from beginning to end - apparently equipped him well enough to be able to quickly win the necessary respect and establish good working relations with the scientists at the Institute, notwithstanding their awareness of Hunt's association with the CIA through leaks in the press (See recollections of Edgar Morin, a visitor in the humanities at

Other consultants invited by Slater included Joseph Goldsen, the head of the Department of Social Sciences at the RAND Corporation, sociologist Richard Brown and a writer and film-maker Stephen White.[134] This team was to work together with the Salk Institute's scientists on plans for expanding the Institute's programs beyond the disciplinary boundaries of biological sciences, as it was envisioned by the Fellows prior to Slater's arrival.

By no means did Slater see himself as a mere administrator and a fund-raiser. The ambition of the Institute finally to launch a full-fledged humanities component appealed to Slater as it coincided with his own interests and long-term goals. As Slater explained in a letter to his friend Lester B. Pearson, Canadian Prime Minister in 1968,

> I have recently left the Directorship of the Ford Foundation's International Programs to become President of the Salk Institute. As you may know, The Institute is going into new phase... In addition to the Presidency, I will have special responsibility for the non-scientific phase of the Institute's activities which have heretofore been devoted primarily to fundamental biological research. I am told the biological field is probably the most promising of modern science, holding out possibilities not only for the greater control of health and physical well-being, but human intervention in genetic processes and in human behavior. It was that promise, and in some respects that threat that led to the initial concept of a research institute in which natural scientists would be joined by behavioral scientists and humanists to take cognizance as a group of the roads which this new science was opening up. Many eminent scientists believe that the burgeoning of the biological science in the 1970s and 1980s will probably have a greater impact on the individual and society than the nuclear developments of the 1930s and 1940s. The Salk Institute, therefore, hopes to involve in its new activities scientists and scholars as well as political, business, mass media, cultural and other leaders both here and abroad in an effort to understand more clearly these rapid scientific developments and to study their

the Salk Institute in 1969: Morin (1970). See also *Saunders (1999)).*

[134] On the history of the social sciences at the RAND Corporation's think tank see Collins (2002), Hounshell (1997).

> implications and the action programs required to meet contemporary and future challenge.[135]

Although Slater did not have a background in biology, he recognized the complexity of the issues and attempted to involve as many of the Salk Institute's scientists as he could, in discussions of concrete ways to implement the ambitious plans. To this end, he launched an institute-wide planning exercise, in order to bring multiple perspectives to the table. Within first months of his presidency, Slater and his team set up working groups, which involved all laboratories, each contributing two scientists to sit on an Institute-wide nominating committee. Every committee then selected additional people to be included in the working groups. The result of the planning exercise, which involved almost anyone at the Institute, was a four-volume report to the Board of Trustees entitled the "Program of Action," accompanied by a fifth volume of supporting documents that presented the detailed plan for the new phase of the Institute's development.[136] This large mass of material included a number of specific proposals and recommendations, largely falling into two groups. The first group of proposals suggested ways to broaden the scientific core of the institute, by adding new senior and junior positions in molecular biology and by extending the research agenda of the Institute into the field of neurosciences. Another group of proposals concerned the entry of the Institute into new fields dealing with the expansion of biology into the fields traditionally occupied by the social and human sciences, in order to address a broad spectrum of social issues, behavior and communication from the standpoint of the recent developments in biology.

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[135] Joseph Slater to Lester B. Pearson, 19 Jan 1968. JBP, box 99

[136] "Program of Action. A Report to the Trustees of the Salk Institute," May 1968. JBP, boxes 98 and 99.

In this program of the expansion of the Institute's agenda Slater skillfully mediated between biological research that was, on the one hand, scientifically significant, and on the other hand, could be presented as politically salient for its relevance to public goals. In a style characteristic of the CCF Study Groups, Slater described the planning of the future programs "related to Man and the Life Sciences" to the Salk Institute's Board of Trustees:

> ...the bringing together of scientists and non-scientists through intensive ... working groups, and special task forces on both exploratory and concrete projects. ... In addition, "experimental" confrontations should be organized to explore the lessons to be learned from biological phenomena which might be applicable to the development of the individual, his identity, and his relationships, such as the construction of a more satisfactory urban life; the mitigation of problems arising from racial and group differences; radically improving the teaching-learning process and the more effective handling of the information-communications explosions; and the development of new systems of national and international justice, organization and arms control.
>
> ...The Institute should serve as a clearing house of information on work throughout the world involving attempts to utilize insights derived from biological studies which are relevant to man's development and covering synthesizing efforts between the scientific and non-scientific fields.[137]

Over the course of the next several years, between 1967, when the Institute set out to fully develop its humanities component, and 1975, when the idea to have humanities represented at the Salk Institute was largely abandoned, the Institute went through a period of unprecedented activity in the development of the programs of studies of social, legal, political, and ethical implications of contemporary biology.

[137] Joseph Slater, "Essential conditions to ensure the balanced development and vitality of the Salk Institute during the next four years," 11 Sep 1967. Program of Action, I-A Program Supporting Documents, p. 4. JBP, box 99

The vision of the unifying function of molecular biology as a bridge between natural sciences and the human sciences was still at heart of this program; however, the focus shifted from the heuristic potential and efficacy of molecular biology, which was the main preoccupation of the "humane studies" throughout the early and mid-1960s, to the focus on its political power in the late 1960s. Molecular biology became important in domestic policy, national security, and foreign policy, with molecular biologists gradually replacing physicists as advisers in domestic policy and national security issues. With this change, the "humane studies," which were largely the "one man show" of Jacob Bronowski, were transformed into a broad and ambitious effort. The "Council for Biology in Human Affairs," established under the auspices of the Salk Institute, sought to connect the Salk Institute's scientists with the humanities scholars, social analysts, legal scholars, and policy-makers, on a large scale.

4.7 The Rise and Demise of the Council for Biology in Human Affairs, 1969-1975

Slater's plans for the Council for Biology in Human Affairs (CBHA) were bold and imaginative. The CBHA was launched at the Salk Institute in 1968 and aimed at establishing a web of formal and informal contacts between the nation's political establishment and leading molecular biologists, scientists, legal scholars, social scientists, and public-policy makers. The goal of the Council was to coordinate public opinion on the societal implications of molecular biology by communicating ideas through the web of formal and informal contacts between leading molecular biologists on the one hand, and "the national legislative and executive community, and the leadership in business, industry and labor, law, education, communication and foundations," on the other. [138]

In many different ways, Slater was building his plans based on the model provided by the CCF, with its study groups and seminar programs intended to influence policy and decision making. The Salk Institute, with its international composition and a number of world-class scientists affiliated as resident and non-resident fellows, appeared to be an excellent institutional setting to implement an agenda promulgated by the CCF following its reorganization in 1967 (see chapter three), while focusing it exclusively on the biological sciences and the social effects of the recent molecular biological revolution. [139] Indeed, in the 1960s, on the eve of the molecular revolution and in the wake of the modern evolutionary synthesis, the biological sciences emerged as the leader in fostering international cooperation as well as meta-reflection by scientists on the social implications of their research, in a way physics could no longer sustain. In Slater's view, the Salk Institute and its Council for Biology in Human Affair were to become the center of the international network of molecular biologists and public intellectuals concerned with the social implications of the molecular revolution.

As Slater explained the idea of the Council to NSF director William McElroy at their informal meeting in August 1969,

> It will be nexus of an international network of bright, well-known [natural] scientists and social scientists concerned with the implication of the biological revolution and willing to work toward improving public and private policy needed to deal with the effects of this revolution. ... The Council will help define research needs, see that the research gets done at the Salk Institute and elsewhere, that the findings are disseminated to elite and other groups, and that they have impact on public and private policy.[140]

[138] "Biology in Human Affairs at the Salk Institute," Jan 1972. JBP, box 105

[139] Joseph Slater, "Memo on meeting with Lee Du Bridge." 19 Mar 1969. JBP, box 108

[140] Memo "Meeting of Joseph E. Slater and Rowan A. Wakefield with Director of National Science Foundation," 1 Aug 1969. JSP, box 435, folder 2.

The CBHA relied upon the network of intellectuals associated with the Congress of Cultural Freedom, listing among its members Daniel Bell, Saul Bellow, Herbert Passin, and Jacques Barzun, all active members of the Congress for Cultural Freedom. Relying on his connections, Slater hoped eventually to form a network of branches of the Salk Institute, with a stronger humanities component, on the East Coast and even in Europe, along with affiliated smaller centers world-wide. Slater started with the idea to establish "an outfit called Salk Europe" to ensure its international outreach.[141] While the idea of "Salk Europe" took a long-term perspective, Slater proceeded with a smaller project of establishing the "Salk East," as a pioneering international effort launching studies of the social implications of modern biology.

The first preparatory meeting of the "Salk East" took place on April 8, 1969. Its participants included sociologists from Columbia University, Bernard Barber, Robert Merton, and Herbert Passin; biologists Eugene Galanter, Daniel Lehrman, Cyrus Levinthal, and Edward Tatum; the law scholar Joseph Goldstein, and Slater himself representing the Salk Institute. As Slater reported to Jonas Salk soon after the meeting, all participants expressed strong interest and support for the project, while "individuals of such diverse professional interests as Herbert Passin, Arthur Trottenberg, Cyrus Levinthal, Eugene Galanter and Robert Merton each emphasized that universities were not now effectively facing this task and, moreover, that as we look to the next decade, current developments within the university system portend even greater difficulty in this regard."[142]

A few months later, two follow-up meetings, exploring "The Entry of Biology into Humanistic Studies," were organized by Slater and Hunt in August and October 1969 - one on the East Coast and another in La Jolla. The goal

[141] Slater's correspondence with J. Robert Schaetzel, 1968. JBP, box 107
[142] JSP, box 376, folder 2

of the meetings was to reassess biological sciences, as they were unfolding at the time, from the point of view of different specialties in the social sciences and the humanities. As the invitation letter sent to the prospective participants stated:

> One of the results of the emergence of biology as the currently dominant science is that the attempts to bridge the "two cultures" now take on an entirely different cast. Biology is not only different in its methods and thought, but it is asking different questions, questions about man much closer to those asked by scholars in the humanities. The meeting [will] be held to discuss the points at which biology does come into contact with humanistic studies and to help design programs which will effect a closer union between the two fields.[143]

Participants of the first meeting in La Jolla included the Salk Institute alumnus Roman Jakobson, moral philosopher Daniel Callahan, Berkeley anthropologist Burton Benedict, psychologist Arnold Mandell, and the Salk Institute's scientists and staff members: Salvador Luria, Jonas Salk, Michael Crichton (then a postdoctoral fellow at Jonas Salk's laboratory), and John Hunt, among others. A second exploratory meeting was hosted by the Harvard Club in New York, and listed Thomas Kuhn among its participants.[144] In writing to Kuhn, Bronowski used the occasion to publicize the plans underway at the Salk Institute:

> As you know, The Salk Institute has initiated research into the impact of modern biology on society. The project which we have began lie in these general fields: learning and behavior, environment, legal and ethical aspects of biology, and the humanities. In the latter field, we wish to look at the ways in which biological research is changing the fundamental assumptions about human nature on which our culture has been based. If these assumptions have not withstood the test of twentieth century biology and anthropology, how has our conception of man changed? Our understanding of man's evolution, for example, now reveals it to be a self-guiding process, with culture and language influencing as well reflecting man's development. The behavioral sciences are reexamining, and

[143]Stuart Ross to Joseph Slater, 10 Sep 1969. JBP, box 105.
[144]JSP, box 261, folder 5

> on occasion upsetting, notions about human behavior which we once took for granted; these new insights clearly have implications for the day-to-day conduct of society. Moreover, in the future, advances in biology will probably allow man to take a more direct hand in guiding his own evolution and in the control of individual behavior. What effect will these developments have on our values and our social institutions? We intend to conduct a program to study these issues. ... We are ... inviting your participation in the further planning of this program.[145]

Kuhn attended the meeting, but withdrew from the further discussions of the Salk Institute's initiatives. Many other scholars, however, stayed involved. Within the year following these meetings the Council was formed. Slater used his contacts and the Institute's scientific standing to involve a squadron of the noted East Coast scholars who had connections in the American political and policy-makers' circles:

> I have discussed and received the endorsement of this project from a number of leaders in the humanities, including Sir Isaiah Berlin, Dr. Rene Dubos of Rockefeller University, Dr. Frederick Burhardt, President of the American Council of Learned Societies, Dr. Alan Bullock, Vice Chancellor of Oxford and member of the Salk Institute Board of Trustees, sociologists Daniel Bell and Robert Merton of Harvard and Columbia respectively, historian Arthur Schlesinger and Dr. Edward Shils.[146]

In Slater's view, as he put it in the application submitted to Alcoa Foundation in April 1970, "one of the major benefits of the proposed humanities programs at the Salk Institute will be the effect it will have on the preparation of people in the new field and the establishment of a model for programs in the traditional university setting." [147] As Slater emphasized in the grant application to the NEH,

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[145] Jacob Bronowski to Thomas Kuhn, 22 Aug 1969. JBP, box 105.

[146] Joseph Slater to A. M. Doty, 2 Apr 1970. Grant proposal submitted to the Alcoa Foundation. JBP, box 107

[147] ibid

> The principal element from the beginning and one which makes this effort unique, has been the strong desire to involve humanists at every stage and at every level in the work of the Council. ... – historians, philosophers, writers, poets and artists ... must become full members of the Council. .. Humanistic studies are too often disregarded by programs in 'science and public policy', with evident lost opportunities for both sides. This money would be allocated from the beginning expressly for the involvement of humanists.[148]

The CBHA was officially approved by the Board of Trustees on July 30, 1969, and was formally established as a permanent part of the Institute in February 1970.[149] The members of the Council included a number of distinguished scientists: Nobel Prize winners James Watson, Peter Medawar, Jacques Monod, Salvador Luria, and Robert Holley, (Monod and Luria were the Non-Resident Fellows of the Salk Institute, while Holley had recently joined the Institute as a Resident Fellow), Jacob Bronowski, Jonas Salk, Roger Guillemin (another recent addition to the crown of the Salk Institute's scientists), a Harvard-based chemist Paul Doty, psychologist Eugene Galanter, and biochemist Cyrus Levinthal, both from Columbia, the director of the Rutgers University's Institute of Animal Behavior Daniel Lehrman, geneticist James Neel, physicist David Pines, and director of the Center for Advanced Study in the Behavioral Sciences Meredith Wilson. The humanities were represented by an equally celebrated group: sociologists Daniel Bell and Robert Merton, writer Saul Bellow, Japan scholar and writer Herbert Passin, the legal scholars Abram Chayes from Harvard and Joseph Golstein from Yale, along with the representatives of foundations, Emilio Daddario and Oscar Ruebhausen.[150]

The Council consisted of six commissions, each dealing with a particular aspect of the biological revolution and its social implications that included the

[148]Grant application submitted to the NEH. 4 Nov 1969. JSP, box 137, folder 1.
[149]JSP, box 435, folder 6
[150]JSP, box 435, folder 6

most pressing issues of the time: abortion, artificial insemination, biological weapons, clonal reproduction, drug addiction, eugenics, genetic engineering, embryo transplantation, and the legal, ethical, and social implications of biological revolution. Each Commission sponsored conferences, seminars and conducted a limited amount of research. However, the primary goal of the commissions was "to recommend policies and courses of action for consideration by private and public agencies."[151]

The Commission on Biology in International Affairs, chaired by Harvard biochemist Paul Doty, aimed to study "the different effects of the biological revolution on the industrialized as well as the less developed nations," and had as a more narrow goal the elaboration of a treaty banning the use of biological weapons (the work in which Salvador Luria was most active).[152] The Commission on Biology, Ethics and Law, chaired by the legal scholars Abram Chayes and Joseph Goldstein, focused on social, ethical, and legal issues associated with genetic engineering, organ transplantation and, more broadly with "the legal response to increased biological knowledge about behavior and personality."[153] The Commission on Biology, Learning and Behavior, chaired by Eugene Galanter, was concerned with such topics as "human violence, mind control and machine intelligence, as well as the social and humanistic consequences of the growth of knowledge and the manipulation of human behavior in these areas."[154] The Commission on Ecology, Environment and Population dealt with the problems of population, genetics, and demography. Chaired by Cyrus Levinthal, the Commission's work was conducted largely by a study group set up at Columbia

[151] JSP, box 435, folder 2
[152] Grant application submitted to the NEH. 4 Nov 1969. JSP, box 137, folder 1.
[153] ibid
[154] ibid

University.[155] The Commission on Biology, Medicine and Health Care sought to contribute to the establishment of a universal health care system, so that the studies initiated within this Commission were intended to indicate "those elements of change required within our educational, economic, political and social institutions, as well as regarding changes in our philosophy and mentality required if every person in this country were to have adequate medical care available by 1980."[156] Finally, the Commission on Biology in Contemporary Culture was chaired by Bronowski who attempted to integrate the traditional "scientific humanist" agenda of his "Department of Humane Studies" within the framework of the "new programs," focusing the Commission's work on "the changing image of the scientist, a contemporary account of human nature, new foundation for ethical concepts, and the evolution and transformation of human institutions."[157]

As a *Nature* reviewer commented soon after the inauguration of the Council in February 1970,

> By all accounts, the formation of the council is the outcome of the preoccupation of Mr. Joseph Slater, the president of the Salk Institute, since he took over that job two years ago, with the implications of the contemporary research in biology. He says that he would like the council to be able to respond to the 'problems that emerge from the biological revolution' more perceptively that was possible twenty years ago with the growth of nuclear power.[158]

Overall, the Council organized more than twenty large and small conferences, seminars and workshops in the period from 1968 until 1973, with leading American intellectuals, politicians, and national leaders involved. The work of the Council was initially supported by the grant from the NEH, which awarded the Salk Institute $200, 000 to help launching the Council in 1969. Smaller grants

[155] ibid
[156] ibid
[157] ibid
[158] Anon (1970).

were secured the same year from the Hasen Foundation, the US Steel Foundation, and from the philanthropists Robert O. Anderson and E. Keene Wolcott.[159] Negotiations and proposals were initiated with a host of other foundations as well. By 1970, the applications were sent to Carnegie, Rockefeller, Danforth, and the Kettering Foundations, seeking funds to "support a four-year program to establish the humanistic component of the Salk Institute and thus transform it from an institution almost completely devoted to laboratory research in biology into an institution equally concerned with the humanistic and social consequences of the biological revolution."[160]

The response of the foundations was generally favorable, sometimes genuinely enthusiastic. The strength of the Council appeared to be, as many noted, in its reliance on the expertise coming from both the natural sciences - molecular biology - and from the humanities and social sciences. As the *Nature* reviewer underscored, "the intention [of the Council] is that ... each of the six commissions should include a lawyer and an educator among its members" (Anon 1970). Apparently this was what the foundations wanted to see funded. Indeed, the Director of NSF, William McElroy, ensured Slater that he would give the Salk Institute's proposal full consideration, since the Salk Institute considered the social sciences component as integrated with the natural sciences research at the Institute, rather than portraying it as a separate expertise on science coming from social scientists.[161] As Slater reported to Jonas Salk,

> [Dr. McElroy] is concerned as he sees social scientists trying to take the lead in the nation in many science policy areas, he said. The lead must come from the "hard scientists" just as being done at the Salk

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[159] "Council for Biology in Human Affairs," Minutes of the Business Meeting 9, 10 and 11 Feb 1970. JSP, box 435, folder 2.

[160] Joseph Slater to A. M. Doty, 2 Apr 1970. Grant proposal submitted to the Alcoa Foundation. JBP, box 107

[161] See Solovey (2012) for the discussion of the NSF and federal science establishement hostility towards the attempts to create a separate agency for the social sciences during the 1960s.

> Institute, he believes. They must make sure they have good social scientists working with them on science policy problems.[162]

Despite positive reaction, the big money did not come. By mid-1970 Slater became so concerned about the future of the Council that he tried to push his contacts at the Ford Foundation to get the necessary funds. Although the personal conversation with McNeil Lowry, Vice President of the Division of Humanities and the Arts at the Ford Foundation, was encouraging, the official response to Slater's request for funds was negative. Richard Sheldon, the program officer of the Division, explained the reason for the decline of the award:

> We have no question at all about the importance of the subject you intend to address nor about your ability to get good people to work on it. We have, however, been spending a good bit of time during the past year in universities talking with humanists, including a number who want to work in the same general area you are concerned with - the humanities as related to the pressing problems of our times - and we have concluded that the needs in the universities are so great that we are going to have to put our priorities there.[163]

The replies from other foundations were ambivalent as well, and did not bring the necessary funds. Thus, William Archie from the Babcock Foundation wrote to Slater in January 1971:

> I have read and reread your letter and the brochure which you have sent along. I find it fascinating, very difficult to comprehend completely, but very likely a worthwhile idea. I mean specifically the establishment of a Council for Biology in Human Affairs seems entirely valid to me.... My reply implies that this Foundation might be interested in helping you match the NEH grant.[164]

Few days later, however, he informed Slater about the negative final decision of the foundation with regard to the Salk Institute's proposal.

[162] JSP, b. 435, f. 2

[163] R. Sheldon to Joseph Slater, 25 May 1970. JBP, box 105

[164] W. Archie to Joseph Slater, 13 Jan 1971. JBP, box 109

Nevertheless, all in all in 1969-1970 the Council's budget was approaching a round sum of $500,000 and Slater announced the submission of a $2.5 million grant proposal to the NSF. By 1973, however, the CBHA had almost ceased to exist. By this time Slater already gave up the hope to realize his ambitious plans at the Salk Institute. Instead, in 1970 he assumed the presidency of the Aspen Institute for Humanistic Studies in Colorado, the position he occupied simultaneously with his presidency of the Salk Institute. In 1972 Slater finally resigned from the presidency of the Salk Institute and focused on developing the Aspen Institute into the center he had aspired to create in La Jolla. A small program financed by the residuary funding was run by Bronowski, but his untimely death in August 1974 became the final blow for the Council. The Council for Biology in Human Affairs was quietly disbanded in 1975, along with the humanities component of the Salk Institute.

4.8 Conclusion to Chapter Four

Why were the broad programs in the humanities at the Salk Institute disbanded in the mid-1970s? Obvious reasons were Slater's resignation in 1972, followed by Basil O'Connor's death in 1973 and that of Bronowski in 1974. O'Connor, the head of the March of Dimes Foundation, was the Institute's and Jonas Salk's major patron, and his death in 1973 dramatically decreased the influence of Jonas Salk on the Institute's policy and its agenda.

However, this is hardly the full story. One of the factors underlying the major reorganization that the Salk Institute underwent in the mid-1970s was the change in its system of patronage that led to the reconsideration of its original concept and, eventually, to the demise of its humanities component. Since the Institute's inception in the wake of the success of Salk's polio vaccine, the system of patronage shaped the Institute's outlook and its research agenda. The March of Dimes, as a philanthropic foundation and the Institute's major patron, supported and enhanced the Institute's broader humanistic outlook presenting the Salk Institute as an experimental "melting pot" institution which would bridge the C.P. Snow's "two cultures." During the 1970s the March of Dimes' financial backing was gradually replaced by the funds coming largely from the National Institutes of Health and, to a lesser extent, the NSF, which favored a more conventional vision of the Institute as a high-profile specialized research center in modern biology.[165]

Another factor that increasingly impeded the CBHA's initiatives and the support for its activities was a dramatic change in the public debate on biotechnology in the mid-1970s. After the possibility of genetic manipulation by recombinant DNA technique was first demonstrated in 1972 DNA technology moved, in words of historian Hans-Jörg Rheinberger, from the "mode of discovery" into the "praxis of molecular bio-construction," while scientists took over the debate about the risks of genetic engineering (Rheinberger 1997, see also Jasanoff 2005, Wright 1986, 1994, Kevles and Hood 1992). The Ad Hoc Panel to assess the problem was set up at the National Academies of Sciences in 1972, followed by the "first" Asilomar conference in January 1973, which involved almost 100 biomedical scientists. In both settings there was no effort to invite the press or public representatives. The emphasis on the "self-regulation" of the social implications of molecular biology research by scientists themselves was likewise behind the work of the two national commissions - the Recombinant DNA Advisory Committee, formed as a follow-up to the Asilomar conference about the hazards of the recombinant DNA technology (1975), and, a decade later, the Ethical, Legal and Social Implications (ELSI) Working Group of the National Center for Human

[165] See on the changing priorities and patterns of patronage of science in the U.S.A in the 1960s and 1970s: Appel (2000), Fries (1984).

Genome Research set up under the directorship of James Watson.

In contrast to the CBHA conferences on the ethical, legal and social implications of genetic engineering, these later structures were not intended to provide a forum for legal-ethical-philosophical reflection on the recent advances in molecular biology and genetic engineering research, which was the main rationale behind the activities of the Council for Biology in Human Affairs. The Asilomar conferences were attended mainly by scientists who actually worked in recombinant DNA research, with only a few lay members (see Wright 1994, Grobstein 1979). As Sheila Jasanoff put it, the decisive absence of the nation's representative political institutions and intellectual and cultural leaders in framing the DNA policy, could "speak more to the scientists' community skills in evading legislation than to the legislature's institutions inability to take action" (Jasanoff 2005).

The CBHA episode thus belonged to the pre-Asilomar period of these debates. Indeed, after the first successful experiments in introducing foreign DNA into bacterial cells, the very scientists who were the most active members of the Salk Institute's Council for Biology in Human Affairs, and especially its Commission on Biology, Ethics and Law, such as James Watson, played major roles in the institutionalization and professionalization of the debates over recombinant DNA, while the Council, with its emphasis on representation of the lay public, lost support among the scientific community of molecular biologists.

Bruno Latour's visit and research stay at the Salk Institute in 1975-1977 marked the end of the broader humanistic agenda of the Institute. As Latour noted, his choice of the Salk Institute for his empirical study of "laboratory life" was determined, at least in part, "by the tradition maintained at the Salk Institute of somehow supporting humanities."[166] By the time of Latour's visit, the humanities

[166]Bruno Latour, "Progress Report 1975-1977. Anthropological Study of a Laboratory of Biology at the Salk Institute." JSP, box 450, folder 4 (original emphasis).

component at the Institute largely returned to where it began: a "humanistic tradition" that annoyed Latour as outdated and detached from the scientific work done in the laboratories:

> The main obstacle ... I came across in the course of my program ... is the isolation of philosophical research inside the Institute. This isolation has been for a long time the fault of philosophers, since, instead of studying scientists, they contended themselves by talking about science. Although at the beginning my program was supposed to be linked with Dr. Bronowski's work, I always intended to stay with scientists in their own laboratories and resist any temptation to work independently of them.[167]

Reflecting on the experience of his two year study of Roger Guillemin's laboratory at the Salk Institute in 1975-1977 that resulted in his classic *Laboratory Life* (1979), Latour located the failure of the humanities programs at the Salk Institute in their detachment from actual scientific research done at the Institute:

> The relative failure of the program Biology and Human Affairs is not surprising since it was not an objective study of science but a general discourse *about* science. ... So far, non-scientific programs at the Institute have been presented as a sort of luxury for bridging the gap *between* science *and* society. The approach I took and I propose to extend is that no such a bridge is necessary, because science is society and does not require any ad hoc explanations to be accounted for. But science requires empirical studies which are not a luxury, but the very aim of a scientific Institute. I propose to place future efforts not under philosophical or a humanitarian heading, but somewhere between cognitive sciences and cellular biology.[168]

Somewhat ironically, Latour's prescription would have been perfectly congenial to the founders of the Institute. Rather than being a luxury, the humanistic component was seen by the Institute's visionary founders indeed as the very aim of the intellectual space Jonas Salk aspired to open up. Perhaps unwittingly, Latour defined his own vision of the studies of science in the same

[167] ibid
[168] ibid

scientistic terms Jonas Salk and the other founders of the Salk Institute defined it, ending with the recommendation to situate the field "somewhere between cognitive sciences and cellular biology" (ibid). The difference between Latour's vision and that of the founders of the Salk lay not so much in their map of the disciplines, as in their attitude towards the different locations on that map. The Salk Institute visionary founders, in their aspiration to "bridge two cultures" by developing various programs to study modern biology by means of the social and human sciences felt a deep reverence for natural sciences, sharing a conviction in the universality of scientific knowledge. In contrast, Latour and his generation of science studies scholars challenged this very conviction, questioning science's claim to a privileged access to knowledge. Indeed, as Michael Lynch put it, "a signal feature of much current STS research is an aversion to universalistic claims about science [and] knowledge" (Lynch 2008, p. 9). At the same time, as Sergio Sismondo noted, the history of STS might be seen, at least in part, as "a history of increasing scope - starting with scientific knowledge, and expanding to artifacts, methods, materials, observations, phenomena, classifications, institutions, interests, histories, and cultures. With those increases in scope have come increases in sophistication, as its analyses assume fewer and fewer fixed points draw on more and more resources to understand technoscientific constructions" (Sismondo 2008, p. 13). With this view in mind, the Salk Institute's initiatives in initiating a sustained inquiry into social studies of modern biology were more than mere "pre-history" of the STS as we know it today. The programs that the Institute housed were diverse and varied, driven by the impulse to accommodate both the framework of "scientific humanism" with its "universalist" agenda, and the recent trends towards post-positivist epistemology, focusing them on modern biology. In this sense, rather than being a moment of rupture as Latour had presented it, the developments that led to the Science Studies of which Bruno Latour's study was one of the pioneering works, were continuous with at least some of the Salk Institute's forgotten initiatives under the auspices of the Council for Biology in Human Affairs.

Chapter 5

In Search of the Soul in Science: The Hastings Center and its Quest for Philosophy of Science Relevant to Medical Ethics, 1976-1980

. . . The topic we want to explore is the present relationship between the sciences and the humanities. Of course C.P. Snow initiated a debate on that subject over a decade ago in the "two cultures"; Snow may have started the discussion off on the wrong foot, but we believe that the problem is worth returning to. The styles of intellectual argument in the "two

> *cultures," the willingness or unwillingness to take on certain questions, the approaches to political issues, the standards of responsibility and professional loyalty, in short, what Snow describes as a culture ... are divergent enough to create serious obstacles to the ethical evaluation of scientific and technological developments. In other words, our own concern for the problem stems not only from an intellectual interest. ... Our Institute is interdisciplinary in its focus and composition, with most of our research involving an attempt to move back and forth between the humanities and the biomedical sciences.*[1]
>
> *...I often hate to see us casually lumped together by some with "science for the people," since it seems to me that that group represents a very different spirit and direction from the kind of work we are trying to do.*[2]

British sociologist of science Trevor Pinch, reviewing the responses to Kuhn's *Structure of Scientific Revolutions* in the 1970s and early 1980s, suggested that the various interpretations of Kuhn's work diverged into two distinct paths, each inspiring a particular mode of analysis of science that would shape the future

[1]Daniel Callahan to Lionel Trilling, 11 Jul 1975. The Hastings Center Records, Yale University Library, Manuscripts and Archives (**Thereafter HCR**), box 23

[2]Daniel Callahan to Bernard D. Davie, 24 Oct 1975. HCR, box 21

development of science studies (Pinch 1997). As Pinch observed, one of these two paths, which he called the "conservative interpretation" of Kuhn, emphasized the social dimension of science and associated the notion of a paradigm with an identifiable social group or scientific community. This interpretation of "paradigm" implied that the cognitive and social realms of science could be separated and that paradigms can be identified and studied by means of sociometric methods such as co-citation and social network analyses. Pinch characterized this conservative interpretation as "Kuhnian Mertonian," because the sociometric studies inspired by it were consistent with Merton's analysis of science, which explicitly dissociated the social, religious, and cultural factors that regulate the behavior of scientists from the content of scientific knowledge or scientific method.

Another interpretation of Kuhn's work, according to Pinch, emphasized the unity of the "socio-cognitive nature of scientific activity," suggesting that social and cognitive aspects of science cannot be studies separately. This reading of Kuhn was informed by the later philosophy of Wittgenstein (in its particular interpretation by Peter Winch) and provided a means for the understanding of scientific actions that resonated with the post-colonialist or interpretative anthropological accounts of the others' (or natives') points of view. As Pinch pointed out, the nexus of ideas that came from Kuhn, cultural anthropologists, and Wittgenstein/Winch's writings formed the basis of the "radical" interpretation of Kuhn's *Structure* that provided the social constructivist school with its philosophical agenda (Pinch 1997).

This chapter sketches the developments that can be characterized as a third path, as the discussion of Kuhn's *Structure* by philosophers attracted to medical ethics in the 1970s didn't follow either of the interpretations and reading suggested by Pinch. The medical ethicists' philosophical deliberations in the wake of Kuhn, although constituted by loosely connected individual voices, reflected a certain discourse of philosophizing about science that emerged and flourished in the 1970s and mostly faded away by the end of the decade. For a short time, the discourse of medical ethics became a fertile ground for a dialogue between philosophically minded bioethicists and the philosophers of science who responded to Thomas Kuhn's challenge of traditional philosophy of science with which the field of science studies came to be associated. In their discussion of the validity of Kuhn's work, these bioethicists suggested a distinct interpretation of Kuhn, emphasizing the elements in his account that had been independently developed by Michael Polanyi, and propelling a view of science that retreated from idealizations of scientific method without sacrificing philosophical realism. Appropriating Polanyi, they extended his account of science to biology and medicine.

To elucidate this mode of philosophizing about science I focus on the deliberations of philosophers who in the 1970s and 1980s participated in the series of conferences organized under the auspices of The Hastings Center — the world's first institute of bioethics, founded in 1969 by the Roman Catholic philosopher Daniel Callahan.[3] The Hastings Center's conferences attracted philosophers who had a parallel interest in ethics and in science and were versed in both moral philosophy and the philosophy of science. In their search for philosophy of science relevant to the issues of concern to medical ethics the philosophically-minded medical ethicists associated with the Hastings Center in the late 1970s and early 1980s raised questions that have been rarely raised in traditional philosophy or history of science: What role does personal engagement play in science? How far can rationality serve as a "motive" for scientific work? What is the place of passion in science? The attempt of medical ethicists to reconcile ethics, medicine, and the philosophy of science was in a way a search for the "soul in science," and also "a bit

[3]On the history of the Hastings Center see Stevens (2000).

of soul-searching" for philosophers-bioethicists themselves, in their concern with the practical policy implications of the epistemology they were endorsing, as well as political appropriations of their work. The contribution of Karl Popper to the debate on the applicability of philosophy of science to the issues of medical ethics provides me with the opportunity to discuss the ways in which political agendas of different epistemologies of science intertwined with questions of concern to medical ethics.

5.1 In the Wake of Kuhn: the Hastings Center's Conferences "The Foundation of Ethics and its Relationship to Science," 1976-1980

As a contemporary discourse, medical ethics emerged in the decades after WWII, in the wake of the "responsible science movement" and the trials of Nazi medical doctors.[4] The first professional centers for bioethics, the Hastings Center and the Kennedy Institute of Ethics, were founded in 1969 and 1971, partially in response to the advances in biology and medical technology, partially as a consequence of a series of medical scandals that provoked public outrage[5]. In the late 1960s, with the professionalization and institutionalization of bioethics, the field traditionally dominated by physicians became increasingly populated by academically trained philosophers-turned-medical ethicists. Philosophers who entered the field of biomedical ethics found that the philosophical idioms of

[4]On the history of medical ethics in the US and Western Europe see: Jasanoff 2005; Jonsen 1998; Rothman 1991; Stevens 2000.

[5]Pivotal for the emerging field of medical ethics were the Thalidomide scandal following the identification of the side effects of this drug in 1961, Henry Beecher's 1966 exposé that revealed the mass violations of the principles of beneficence and informed consent, and the revelation of the Tuskegee Syphilis Experiment in 1972 (see Jonsen 1998; Rothman 1991).

traditional moral philosophy were only loosely connected to the moral problems of the "new medicine." Contemporaries usually depicted the field of ethics and moral philosophy in the 1960s as intellectually somber and impotent, unable to provide the philosophical ground for the newly emerged field of medical ethics. Thus, Callahan admitted in 1973: "there was nothing whatever in my philosophical training which had prepared me to make a flat, clear-cut ethical decision" (Callahan 1973, 67).[6]

Such an image of a "perceived impotence" of ethical theory was not shared by all philosophers, though. Some of them, agreeing that bioethical decisions cannot be drawn from "philosophers' first principles," started to see modern medicine and biology as a new resource for "philosophers' first principles" rather than the other way around. Thus, Stephen Toulmin, who had a parallel interest in ethics and science, asserted retrospectively that "it was medicine—as the first profession to which philosophers paid close attention during the new phase of 'applied ethics' that opened during the 1960s—that set the example which was required in order to revive some important, and neglected, lines of argument within moral philosophy itself" (Toulmin 1982, 746).

One of these apparently fruitful lines of argument, to which the philosophically minded medical ethicists turned their attention in the wake of Kuhn, was the analogy between the nature of scientific judgments and the nature

[6]Another early bioethicist, Albert Jonsen, described moral philosophy as hardly an exciting field in American university curricula in the 1960s: "Graduate students who did take a course in moral philosophy [in the 1960s] might glance at Thomas Hobbes, John Locke, and Immanuel Kant, then ...would touch on the naturalistic fallacy, non-naturalism, and emotivism, a tour sufficient to suggest to the bright student that moral terms could be evacuated of meaning and that moral reasoning could be (to use contemporary language) deconstructed. Despite this devastation, the principal theories of normative ethics would be reviewed, often grouped as two major theories of obligation—teleological and deontological. ... The main example of a teleological theory, utilitarianism, would be presented as ostensibly compelling but, on closer examination, as suffering from conceptual weaknesses. The deontological approaches would be revealed as standing on the shifting sands of intuition" (Jonsen 1998, 7).

of moral judgments. The explorations of this analogy became the focus of a series of conferences on "The Foundations of Ethics and its Relationship to Science" launched by The Hastings Center in 1976.[7] As Callahan noted in his introductory remarks to one of the volumes of the proceedings of the conferences,

> ...when The Hastings Center ... was founded in 1969, ... few were prone to deny the reality of the issues or the knotty dilemmas: the care of dying patients, psychosurgery, genetic engineering, behavior control, excessive population growth, and the allocation of scarce medical resources. What many were prepared to deny, however, was that ethics could be a fit subject for intelligent, even illuminating, discourse. ... Fortunately, the very urgency of the issues we set out to examine helped to overcome many of the doubts. ... Something had been missing in contemporary ethics — a systematic attempt to develop modes of practical moral reasoning sufficient to allow moral decisions to be made. ... Our project has been forced to ... nothing less than a full-scale interdisciplinary inquiry... [8]

The project was very interdisciplinary indeed. Participants included philosophers Alasdair MacIntyre, Samuel Gorovitz, Tristram Engelhardt, Daniel Callahan, Hans Jonas, Thomas Nagel, Marjorie Grene, Marx Wartofsky, Kenneth Schaffner, Tom Beauchamp, Gerald Dworkin, Robert Solomon, Joseph Margolis, and Gregory Vlastos; theologians Paul Ramsey, James Gustafson, David Burrell, Stanley Hauerwas, Ronald Green, and Rabbi Jack Bemporad; philosophers of science Stephen Toulmin and Patrick Heelan, historians Loren Graham and Steven Marcus; clinicians turned to medical ethics, such as Edmund Pellegrino, Eric Cassell and Bernard Towers; historians of medicine Guenter Risse and Lester King, and biologists Richard Alexander, Gunther Stent and Marc Lappé. From 1976 to 1979, the discussions were centered upon the interplay of evaluation and explanation in biomedicine, the role of religion and science in the formation of

[7] Engelhardt and Callahan (1976, 1977, 1978, 1980). The general overview of the conferences was publicized in The Hastings Center Report (Engelhardt 1976).

[8] Callahan, Engelhardt (1978), viii-ix.

ethical judgments, and the psychological and cultural contexts crucial for framing both scientific and ethical views (Engelhardt 1980).

The merits and salience for medical ethics of the Kuhnian and other contemporary developments in the philosophy of science received ample attention in these discussions. Kuhn's name as well as Ludwik Fleck's were mentioned regularly in the discussions at the Hastings Center, being a reference point any time the issues of objectivity, rationality, or epistemological relativism with regard to medical knowledge came up in the discussion.[9] Their deliberations on the philosophical foundations of medical ethics constituted a particular discourse, a mode of analysis of science that emerged in the 1970s partially in response to Kuhn's work, and found a receptive audience among the philosophically-minded medical ethicists. This particular mode of analysis of science appropriated Polanyi's epistemology, singling it out among other post-positivist accounts of science, placed Kant at center stage, and emphasized the "dramatic" rather than the "normal" dimension of science.

Michael Polanyi's philosophy of science was featured prominently at the Hastings Center's conferences. Polanyi's commitment to realism combined with the prominence he gave to the moral and emotional dimensions of science made him, rather than Kuhn, the philosopher of science most appealing to medical ethicists. As the moral philosopher Alasdair MacIntyre, a fellow of the Hastings Center and a participant in the Hastings Center's conferences, emphasized, Polanyi's account of the community of scientists as a community of faith, with its emphasis on the

[9] Although Kuhn acknowledged Fleck for anticipating many of his ideas (for example, in his foreword to the first (1979) English translation of Fleck's *Genesis and Development of a Scientific Fact*) Fleck, who grounded his epistemology in the fine-grained analysis of bacteriological and immunological practices, remained in the 1970s barely known by most of philosophers of science interested and versed in the physical sciences. Philosophers lured to medical ethics presented a sharp contrast as they read Fleck and equated his epistemological contribution to that of Kuhn.

role of consensus, tradition, and authority in the scientific community, seemed to secure the stability of scientific knowledge in a way that "resembles the stability of systems which modern Western thought takes to be nonrational, such as Zande witchcraft. ... This account of the continuity and stability of science yields as its obverse Kuhn's account of science's discontinuities and instabilities. It yields also the same kind of conclusion about the relationship of theory to observation which Kuhn formulated in terms of incommensurability" (MacIntyre 1978, 26).

Marjorie Grene, a philosopher who worked for years as Polanyi's assistant and who helped him relate his ideas to established epistemological traditions, was another participant who highlighted the importance and promises of Polanyi's epistemology of science.[10] Reflecting, presumably, a common sentiment among the participants of the Hastings Center's gatherings, she contrasted Polanyi with Kuhn (to whom she referred as "Polanyi's alleged derivative"), suggesting that "Kuhn's relativism ... appears to result ... from an overemphasis on one aspect of Polanyi's theory of science: the logical gap ... between evidence and theory and between one conceptual framework and another" (Grene 1978, 46). Hence, she

[10]Polanyi and Grene met in 1950 and this meeting turned out to be a crucial event for both of them. Marjorie Grene was then professionally and intellectually at the very periphery of the philosophical milieu, working as a teaching assistant at the University of Chicago after being outside of the profession for fifteen years (Grene 1995). After attending Rudolf Carnap's seminar at the University of Chicago she became disillusioned with the logical positivist program in its American reincarnation (ibid). In Polanyi Grene found both a like-minded thinker and an employer. A renowned physical chemist with an early training as a physician, who gravitated towards economics and politics and in his middle age towards philosophy, Polanyi relied on Grene to help him articulate his own ideas in the context of Western philosophical thought, "to articulate the inarticulate"—a new way of analyzing and thinking about science (Grene 1995, 91). In many ways, Grene has shaped some of Polanyi's philosophical views and publications, recognizing in his effort to create what he called a "post-critical" philosophy a potential to transform many of the suppositions and interests of modern philosophers (Mullins 2002). Polanyi praised Grene's contribution in the preface to his magnum opus, *Personal Knowledge*: "This work owes much to Dr. Marjorie Grene. The moment we first talked about it in Chicago in 1950 she seemed to have guessed my whole purpose, and ever since she has never ceased to help its pursuit. Setting aside her own work as a philosopher, she has devoted herself for years to the service of the present enquiry. Our discussions have catalyzed its progress at every stage and there is hardly a page that has not benefited from her criticism. She has a share in anything I may have achieved here" (Polanyi 1962, ix).

concluded, "to put Polanyi center stage in the development of the philosophy of science is ... a welcome move" (Grene 1978, 42).

Although finding the developments in philosophy of science stimulating for moral philosophers, MacIntyre was convinced that "the philosophy of science may have something to learn from ethics, rather than vice versa" (MacIntyre 1978, 22). In his paper "Objectivity in morality and objectivity in science," presented at the Hastings Center's conference, MacIntyre argued that contemporary philosophy of science can be seen as a "recapitulation" of some of the classical themes articulated by the moral philosophers long before modern philosophy of science did (MacIntyre 1978). Thus, Kuhn's preoccupation with the incommensurability of paradigms and the question of how the choice between different paradigms is made by the natural scientist, MacIntyre argued, resurrected Kierkegaard, "who held that the choice between rival sets of universal propositions prescribing ways of life cannot be decided by rational argument, but only by a fundamental, unargued and unarguable choice "in the making of which there are no rational criteria" (MacIntyre 1978, 24). Likewise, Feyerabend was claimed to be a "revival of Emerson's ... individualism and the rebellion against authority" and Polanyi as a "version of Burke [with his emphasis on consensus and tradition]" (MacIntyre 1978, 23-27). MacIntyre suggested that the roots of these parallels between nineteenth-century moral philosophers and modern philosophers of science can be found in Kant's teaching, obscured within the analytical philosophy but restored unwittingly by the new philosophers of science like Kuhn and Polanyi (MacIntyre 1978). The restoration of the unity of Kant's teaching, MacIntyre argued, was what philosophy of science can learn from ethics, since "moral theorists as different as Kierkegaard and Emerson were responding to Kant in a radical way that their contemporaries in the philosophy of science never paralleled" (MacIntyre 1978, 23).

The re-reading of Kant became the context for reading Kuhn, and a recurrent theme in the Hastings Center's discussions. MacIntyre argued that the Kantian "oddly asymmetrical division of reality," which separated science from ethics, in fact goes beyond Kant's genuine thesis, representing "a vulgarized and unacknowledged version of Kant" (MacIntyre 1978, 23). He illustrated this point by the example of Newton's achievement and its role in Kant's philosophy. Newton's *Principia*, MacIntyre explained, being a model of objective knowledge for Kant, had a neglected but entirely fitting sequel in Newton's interpretations of the *Book of Daniel*, and for Newton, both were the two sides of the same inquiry. Kant's "unintended achievement was to make the intellectual unity of Newton's life unintelligible to later generations," thus producing an "oddly asymmetrical dichotomy," epitomized later in an uneasy tension between science and ethics (MacIntyre 1978, 23). The parallel structure of Kant's first and second Critiques – *The Critique of Pure Reason* and *The Critique of Practical Reason* – mirrored an apparent contradiction between two parts of Newton's life: one that allows to depict Newton as a man who had been for so long a totem figure of "rationality," and the less known and neglected side of a man who worked under the impulsion of religious convictions and hermeneutical beliefs. The "restoration of Kant's genuine teaching" and the denial of the unjust separation of science from ethics that resulted from a "vulgarized version of Kant," would provide, MacIntyre argued, an account of reality that is not to be lost in relativism—a danger moral philosophers saw in the Kuhnian account of science. Science does not fall apart into various communities of researchers with incompatible views of reality, as Kuhn imagined. On the contrary, a scientific community is bound together by an interest in the internal goods, which include the pursuit of the true representation of nature.

The moral philosophers were wary of putting scientific objectivity at "the risk of being fragmented into many somewhat discontinuous domains of reason, with disparate views of reality" (Engelhardt 1978, 3). Facing Kuhnian relativism, MacIntyre argued that Kantian realism designated ethics as the foundation of science, and not the other way around. The major lesson for the philosophy of science to be learned from ethics, MacIntyre concluded, is that the fragile objectivity of science "is a moral concept before it is a methodological concept and the activities of natural science turn out to be species of moral activity" (MacIntyre 1978, 37).

In this argument about scientific objectivity as a moral concept, MacIntyre and other medial ethicists extended Polanyi's account of science with its strong emphasis on the moral dimension of science.[11] What made Polanyi especially attractive for medical ethicists of the Hastings Center was the prominent place he allotted to emotions, passions, and moral energies triggered by scientific activity. Polanyi believed that science can exist only if carried by what he called "intellectual passions." Drawing his "theory of commitment" on the analogy between religious faith (especially Augustine's faith) and scientists' faith, Polanyi argued that a scientist must become obsessed by his work, approaching it with heuristic passion akin to the "fear and love" of a believer. For Polanyi, scientists are believers who seek only through faith. Like a believer, a scientist must have "a love for the truth" (Polanyi 1962, 204). While the post-positivist philosophers of science excluded

[11]Lorain Daston and Peter Galison suggested an account of the "moralization of objectivity" as reflected in scientific image making in the nineteenth century (Daston, Galison 1992). In this account the moral aspect of objectivity is grounded in emotional detachment and the ethos of self-restraints and self-command "triumphing over the temptations and frailties of flesh and spirit" (Daston, Galison 1992, 83). This ideal of "mechanical objectivity," manifested in "wordless science" of scientific atlases, attempted to eliminate the mediation of observer and subjectivity of human emotional, intellectual and moral virtue, in order to attain empirical reliability of scientific images. Contrary to this image of "mechanical objectivity," perceived as ascetic emotional detachment, MacIntyre, following Polanyi, argued for the full inclusion of human emotional virtues into the account of science and objectivity.

emotions from the method and substance of science, Polanyi saw emotions as being an indispensable element of science. In Polanyi's account of science intellectual passions were not merely psychological 'by-products' of scientific activity that can be ignored, but a "logical function which contributes an indispensable element to science" (Polanyi 1962, 134).[12] As philosopher Richard Allen pointed out, in his account of science Polanyi offered an elaborated "theory of commitment" that rehabilitated emotions as an object of epistemological relevance (Allen 2005).

The epistemological relevance of the emotions and the rehabilitation of passions, motives, and love ("love for rationality" or "love for the truth") —all these marginalized subjects within traditional philosophy of science—surfaced as legitimate themes in the Hastings Center's deliberations. Toulmin called what he saw as an emerging new field at the intersection of philosophy of science and ethics "The Moral Psychology of Science" (Toulmin 1978, 50). Emotions, as Stephen Toulmin argued, "make rationality effective" (Toulmin 1978, 50). "Curiosity and competitiveness, spontaneous joy and the wish to please, internal censorship and delight in play, grandiosity and openness ... all the components normally involved in the development of an individual's general personality, character, and mode of life can presumably find expression in the particular activities of his intellectual life ... In short, the natural sciences exist at all only because there is something in the work of science *for* the individual scientists involved" (Toulmin 1978, 54).

Being not only a belief system, but also a cultural system, science for Polanyi was both a paradigm of and a model for society. It is against this background that

[12]Scientific passion, according to Polanyi, has certain functions that trigger scientists' activity and thought. Its "functions" can be "that of distinguishing between demonstrable facts which are of scientific interest, and those which are not ... [and] also as a guide in the assessment of what is of higher and what of lesser interest" (Polanyi 1962, 135). Scientific passion can also function as heuristics, sustaining the effort over a long period as "the force which impels us to abandon an accepted framework of interpretation and commit ourselves, by the closing of a logical gap, to the use of a new framework," as well as "link[ing] our appreciation of scientific value to a vision of reality" (Polanyi 1962, 159).

the prominent place of Polanyi's account of science in the nexus between medical ethics and philosophy of science must be seen. Polanyi's work was appropriated by the bioethicists as a system of thought that included both Kuhn's and Fleck's accounts, and at the same time secured scientific objectivity through the inclusion of the moral dimension of science as a legitimate part of philosophy of science.

In their discussion of the validity of the Kuhnian analysis of science, the philosophically-minded medical ethicists and philosophers with a parallel interest in science and ethics offered a very distinct interpretation of Kuhn that emphasized the elements in Kuhn's account that had been independently developed by Polanyi. In this way, the bioethicists articulated a particular way of interpreting Kuhn and a particular path in the post-Kuhnian developments of philosophy of science, which was, in paraphrasing Pinch and his categorization, neither "Kuhnian Mertonian" nor "Kuhnian Wittgensteinian" but rather "Kuhnian Polanyian."[13]

Appropriating Polanyi, bioethicists also expanded the philosophical points and themes that Polanyi offered into the area that Polanyi himself addressed only scarcely—biology and medicine—and articulated the value of the "alternative ontology" of medicine and biology for philosophy of science and the ways in which the focus on medicine and biology promised to change the agenda of the post-Kuhhian philosophy of science.

[13]It might be not a coincidence that the Hastings Center's series of conferences seeking the common foundations of science and ethics have taken place almost immediately after Polanyi's death in 1976. It seems that the prominence of Polanyian discussions and Marjorie Grene's presentation and interpretation of Polanyi's thought for medical ethicists were to a certain degree influenced by the feeling of the untimely loss and the necessity to explore the legacy of Polanyi's epistemology.

5.2 Medical Ethics as a Means of Epistemological Deliberation: Biology and Medicine as the New Object of Post-Kuhnian Philosophy of Science

In 1976, several participants in The Hastings Center's conferences on the foundation of ethics and its relation to science presented their papers at the bi-annual meetings of the Philosophy of Science Association of science. Tristram Engelhardt, Marx Wartofsky, and Marjorie Grene pointed out that the practices and concerns of medicine were inadequately developed in contemporary philosophy of science. At the same time, they argued, biology and medicine, representing a particular mode of scientific practice, distinct from physics on which the philosophy of science was classically based, not only could pose critical questions for the field but also become a particularly fruitful arena for post-Kuhnian philosophy of science (Grene 1977; Engelhardt 1977; Wartofsky 1977).

As Wartofsky put it, "philosophy of medicine offers the philosophy of science an opportunity to begin again, after almost a century of development in its present form, on condition that the therapy which philosophy of medicine suggests is taken seriously" (Wartofsky 1977, 109). Modern philosophy of science seemed to exclude "too much of what is included ... in the historical and actual practice of science" (Wartofsky 1977). Based on physics as the paradigmatic science, with its clearly defined and axiomatically organized knowledge and powerful results derived from few assumptions, it showed severe limitations when applied to scientific practices other than physics. A focus on medicine, Wartofsky argued, put the basis of philosophy of science in question, at the same time offering "the philosophy of science ... an occasion and an opportunity to begin again: to radically reconstrue both the object and the method of scientific inquiry; to consider anew the relation between theory and practice in science, and to recognize the fundamentally historical character of scientific knowledge..." (Wartofsky 1977, 110).

Grene reinforced this philosophical manifesto. "If we don't look at medicine, we are very likely to miss, in our addiction to a particular model of physics as model for science, a characteristic, if not of science in general, certainly of the biological sciences in so far as they are rooted in the perception-mediated understanding of what goes on in the real natural world" (Grene 1977, 91). She urged philosophers to follow the lead of Georges Canguilhem in his single-handed effort to develop the epistemology of the experimental sciences by examining the very messiness and complexity of medical practices (Grene 1977). In her paper, Grene dissected a work of the physician John Murray, *The Normal Lung*, trying to demonstrate to her fellow philosophers that the conception of science, its presuppositions, its methods, and its epistemic claims would be different from the prevailing one if we had "the physic, not the physics, of Padua" as our model of modern science (Grene 1977, 81).

Grene argued elsewhere that an "alternative ontology" of medicine and biology, characterized by the centrality of perception-mediated practices rather than deductive reasoning, would have significant implications for the "new philosophy of science," leading to an alternative to both empiricism and relativism. She called this epistemological position "comprehensive realism" (a position she attributed to Polanyi's account of science), opposing it equally "to the phenomenalism and to the 'thin realism' of the older philosophy of science" (Grene 1985, 6). She maintained that comprehensive realism would lead the "new" philosophers of science to a focus on the pluralism of scientific practices rather than on the unity of science, and to a "swing toward a noncognitive account of science" (Grene 1985, 6). At the same time, she accentuated, such an account of science "does not make science illusory, arbitrary, or merely pragmatic. This turn ... is not a turn to relativism in either the sense of Feyerabend or in the sense in which Kuhn's doctrine is usually understood" (Grene 1985, 8).

Mapping what she saw as an emerging "new philosophy of science," Grene described the position of the "new philosophers" as being between the 'extremes' of post-Kuhnian relativism and positivistic 'realism'. As she put it in the opening remarks to the collected volume of the essays *Evolution at a Crossroads: The New Biology and the New Philosophy of Science*, "Current issues in the philosophy of biology illustrate, and are most fruitfully approached, in terms of philosophical perspective less monolithic than the once regnant logical empiricism but less revolutionary than the adherents of what was taken to be Thomas Kuhn's position" (Grene 1985, 1).

Philosophers' criticisms of the "classical" philosophy of science (based on physics) for it apparent failure to account for the practices of medicine had its parallel within the medical profession. In the 1970s the "medical model"—the dominant model of contemporary Western medicine that regards medicine largely as an application of the biological sciences to the problem of disease—came under attack for its "epistemological deficiencies" and its tendency to reduce medicine to biology. Critics (for example, Engel 1977; Seldin 1977; Veatch 1973) argued that the "medical model" focused narrowly on etiological factors of diseases, excluding the social conditioning of both the concept of illness and the practices of medical diagnosis.[14] Although The Hastings Center's ethicists did not explicitly refer to

[14]The resurrection of this criticism can be seen in a current debate over the evidence-based medicine and its attempts to create universal rules for medical practice. As the critics of EBM argue, the medical model with its emphasis on the "logical etiology" of disease imposes the hypothetico-deductive model of scientific method onto the practices of medical diagnosis and

the medical model and the criticism raised by the physicians, their attempts to reconcile modern developments in philosophy of science with what was previously excluded from the concept of science were responding to these concerns.

The "Procrustean bed" of the hypothetico-deductive model of scientific method was indeed imposed on medicine by many scientists and practitioners who advocated the application of the robust scientific methodology in such boundary disciplines as immunology. Peter Medawar, for example, appealed to Karl Popper's philosophy of science in his assessment of medicine as a "true" science. In his lecture, "Scientific Method in Science and Medicine," delivered at the New York Hospital Medical Center in 1974, Medawar argued that the scientific methodology is the same in the sciences and medicine: "My purpose ... is to outline my conception, founded essentially on Karl Popper's, of the process of intellectual enquiry as it occurs in both science and medicine and to try to convince you that scientists and clinicians use essentially the same method of enquiry" (Medawar 1975, 346). Medawar compared the process of decision making in clinical practice to an "archetypal cybernetic process," where hypotheses concerning the diagnosis might be refashioned and discarded "in the light of the degree of correspondence with reality of its implications and logical predictions," similarly to the logical output being modified by negative feedback in a cybernetic control system (Medawar 1975, 352). Medawar presented this cybernetic vision of a clinician as a Popperian alternative to the application of the inductive method to the diagnosis of a disease: "When we ask if the act of clinical diagnosis is inductive or "Popperian" in structure the answer must surely be "Popperian." Confronted with a sick patient, the clinician is not observing passively; he is exploring, forming tentative hypotheses which further guide observation and which in the light of

thus presupposes an inaccurate and deficient view of medical knowledge (Henry 2006).

observation have often to be discarded: a typical process of hypothesis formation corrected by negative feedback. It is the inductivist view that diminishes the clinician and makes him seem rather like an inefficient computer" (Medawar 1975, 350).

Many physicians dealing with mental diseases (most notably, psychiatrists) were not happy with such an image of a physician, and found the medical model reductionist, as it narrowed the medical diagnosis of mental illness to biological brain dysfunctions. These practitioners, for whom the exclusion of psychological, interpersonal, and societal causes from the concept of mental illness threatened their professional autonomy and identity[15], felt that their "problems ... [have] arisen from the abdication of the theologian and the philosopher," and appealed to sociology and ethnography instead (Engel 1977; Veatch 1973). Starting with Parsons' and Merton's pioneering work in the 1950s, illness was comprehended within the sociology of medicine as a socially assigned and value-laden category. Writing in the 1970s and referring to Parsons and Merton's works, the long-time Hastings Center's associate Robert Veatch argued that the medical model should be put under philosophical, sociological, and legal scrutiny, so as to acknowledge the fact that illness is not only biologically but also socially and culturally constructed deviance (Veatch 1973). Contemporary philosophy, Veatch pointed out, didn't even start to address the vital problems of medical practice: "The philosophical problem which we might call the 'baseline problem' —the idea of normalcy from which one can measure health and illness, benefit and harm, commission and omission of an act, ordinary and extraordinary means, or positive and negative incentives, is not well explored in the philosophical literature — and

[15]As Engel prognosticated, "Disorders directly ascribable to brain disorder would be taken care of by neurologists, while psychiatry as such would disappear as a medical discipline" (Engel 1977).

should be..." (Veatch 1973, 75).

Philosophers gathered at the Hastings Center in the 1970s started to explore this nexus between medicine, biology, and the philosophy of science, and to fill this gap. In their attempts to formulate a new, post-Kuhnian, philosophy of science, based on biology and medicine rather than on physical sciences, these philosophers claimed their retreat from both the "old empiricism" and positivist ideals, without sacrificing philosophical realism. With the professionalization of the philosophy of biology, many of these early insights were lost or faded away, coinciding with the end (by mid-1980s) of what Stephen Toulmin called "the philosophical phase" of the development of bioethics (Toulmin 2001, 121). As Toulmin has noted, the debate on medical ethics initiated in the 1950s by theologians and restated in the 1970s by philosophers in philosophical terms, was appropriated in the next decade by a new class of professional bioethicists and translated into the down-to-earth language of policymaking, divorcing "the concrete, particular questions of practice to which doctors and bureaucrats needed answers, and the abstract, universal issues of theory that philosophers were interested in disentangling" (Toulmin 2001, 120).

5.3 Medical Ethics as a Means of Democratic Deliberations: Political Agendas of Biomedical Ethics' Epistemologies

The 1970s proved to be a pivotal period for bioethics in the United States. Having emerged as a forum for communication, by the early 1970s bioethical discourse was institutionalized to become an instrument of public policy (Jasanoff

2005; Stevens 2000). In the mid-1970s official bioethics started to take shape on the national level, beginning with the congressionally mandated National Commission for the Protection of Human Subjects of Biomedical and Behavioral Research, formed in 1974 and resulted in the influential Belmont Report[16]. The Recombinant DNA Advisory Committee was formed in 1975, followed by the establishment of the President's Commission for the Study of Ethical Problems in Medicine and Biomedical and Behavioral Research in 1980.

Institutionalization of bioethical practice authorized "a new class of professional bioethicists" in the US (Jasanoff 2005, 177). As Jasanoff has noted, these developments also produced a tension between bioethics as an instrument of public policy (and thus a disciplinary discourse dominated by the experts), and bioethics as a medium for communication and reflection, open to expert "bioethicists" as well as to public intellectuals. The Hastings Center's medical ethicists' discursive strategies in the 1970s were shaped by this internal tension. The Hastings Center, although founded as "independent" organization, aimed to provide a *professional* reflection on the ethical issues in medicine. In the mid-1970s the Institute promoted itself as a mediator between different points of view rather than an advocate of one particular position (Stevens 2000). In a report of the Center (1972-1973), Callahan explained:

> I do not believe that we should move in a much more 'activist' direction, except under very special circumstances. ... I think that any movement to take specific Institute stands on particular issues... would eventually (and quickly) lead to serious factionalizing within the Institute, making it exceedingly difficult for people of different ethical views and persuasions to continue working together... Our public reputation might well change. At present I believe that we are looked upon as a non-ideological group, grinding no polemical axes, available as a trustworthy resource for all factions and ethical

[16]The Belmont Report (1974) set out three basic principles of medical ethics: respect for the person, beneficence, and justice.

schools of thought.[17]

By the mid-1970s the Hastings Center presented itself as a "neutral," explicitly "nonideological" organisation. As the Center's founder, Daniel Callahan, put it in 1976, the Center's standing is "ethical rather than political," with social critique "moved from protest to management": "the reform/radical groups in science and medicine are action-oriented, classically activist; the bioethics groups are not" (Callahan 1976, xxi).

It turned out, though, that the problems in the philosophy of science discussed by the philosophers-bioethicists with respect to such seemingly apolitical issues as the "alternative ontology" of medicine and the uncertainty of medical knowledge, resurfaced in the explicitly political plane. The questions of whether medical ethicists should or should not be the advocates of particular policies and whether or not they should take sides in the debates concerning medical policy issues became closely linked to the particular account of the philosophy of science they subscribed to.

The issue of uncertainty (fallibility) of knowledge in its relationship to the questions of medical ethics was treated quite differently by Alasdair MacIntyre and Samuel Gorovitz in the US, and by Karl Popper in the UK. The following example allows us to see how the political implications of the accounts of science and moral philosophies espoused by Karl Popper and Michael Polanyi with respect to the ways in which their political agendas and moral philosophies intersected with questions of concern to medical ethics.

In their 1976 essay, "Toward a theory of medical fallibility," MacIntyre and Gorovitz, both fellows of the Hastings Center, explored the nature of errors in medicine, arguing that traditional philosophy of natural science, by restricting

[17] Cit. in Stevens (2000), 59.

science to abstract universals and law-like generalizations, failed to account for fallibility of medical knowledge; nor did it provide the plausible explanation of mistakes in clinical practice and hence malpractice and compensations policy (Gorovitz, MacIntyre 1976). The time had come, Gorovitz and MacIntyre argued, to "reinterpret natural sciences." In medicine, they suggested, the acknowledgement of the "necessary fallibility" of scientific knowledge required the reconsideration of the concepts of harm, sanctions, culpability, and compensation, as well as their application in medical practice (Gorovitz, MacIntyre 1976). In considering the specific policy reforms that might follow from these theoretical considerations, the philosophers discussed two alternative policies, a "liberal" and a "conservative" one, making it clear that they were not taking sides on this issue: "No specific policy follows from our theory of medical fallibility, nor will we argue for any specific policy. Rather, we will focus attention on the question of what sorts of policy make sense in light of that theory and thereby argue that a revision of current policy is in order. To do so, we will describe two alternative policies for which one could argue with the support of our theory—one in what might be called a liberal social welfare tradition, the other in the spirit of what might be called a more conservative individualist viewpoint..." (Gorovitz, MacIntyre 1976, 67).

The response to this article, explicitly articulated in what might be called the "liberal tradition," came from the other side of Atlantic. Late Karl Popper, in co-authorship with Neil McIntyre, professor of medicine of London's Royal Free Hospital School of Medicine, in an article published in the leading medical journal in the UK, questioned such a "methodological asceticism" and a posture of neutrality. Replying directly to MacIntyre and Gorowitz's 1976 essay and agreeing wholeheartedly with the thesis that the predominant attitude in medicine is based on "old views of the growth of knowledge," McIntyre and Popper advocated the introduction of the firm medical audit policies (in Britain) and new professional ethics in medicine (everywhere), invoking a Popperian theory of conjectural knowledge (McIntyre, Popper 1983).

Popper and McIntyre called for "the new professional ethics" in medicine, which they presented in the form of ten principles, including, for example: (Principle 1): "Our present conjectural knowledge far transcends what any person may know, even in his own specialty. It changes quickly and ... not by the accumulation but by the correction of erroneous doctrines and ideas. Therefore there can be no authorities..." (Principle 5): "Our attitude towards mistakes must change. It is there that ethical reform must begin. For the old attitude leads to the hiding of our mistakes..." (Principle 7): "It is therefore our task to search for our mistakes and to investigate them fully. We must train ourselves to be self-critical"; (Principle 8): "We must ... learn to accept gracefully, and even gratefully, criticism from those who draw our attention to our errors" (McIntyre, Popper 1983, 1920). The implications of these principles for medicine, the authors argued, would challenge the antagonism towards medical audit and establish "a new type of confidence" in the medical profession that would bear not upon the "old ideal of authority" but on tolerance and "rational criticism" (McIntyre, Popper 1983, 1922).[18] The article's major target was the opposition to medical audit in Britain, with the arguments for making medicine "open and accountable" and "to bring medical practice under closer scrutiny" set forth to advocate the introduction of audit and peer review into medical profession (McIntyre, Popper 1983, 1922).

The article had an uneasy path to publication. The manuscript was initially

[18] The authors referred here to Popper's earlier publications where he expressed his views on "new professional ethics", mainly in "The Moral Responsibility of the Scientist," first published in *Encounter* in 1969 and then reproduced in (Popper 1994) and his 1981 lecture "Toleration and Intellectual Responsibility" published in (Popper 1992).

rejected by the *British Medical Journal* (BMJ). In two anonymous reviews dated 1981, the paper (which didn't differ considerably from the published version) was called "crashingly naïve and superficial irrespective of the eminence of its second author."[19] Most damningly, the very idea of the application of Popperian philosophy of science for the issues of medical ethics was found problematic. As one of the reviewers noted,

> It has been a serious weakness of Sir Karl Popper's approach to life that he has failed to distinguish reality from the ideal. His views about scientific philosophy are well nigh impeccable and few would deny that in the abstract idealist world of our Bishop Berkleyian minds new knowledge is a product of conjecture and refutation. However in real life things are not so simple and psychosocial factors (to which Kuhn has, as we all know, drawn attention) may predominate. Because of these two separate views of reality, medical practice as distinct from medical science may diverge. Reconciliation of the two is possible but is it no way achieved.[20]

The impact of the Kuhnian account of science is hard to assess basing on this reviewer's remark, but the very invocation of Kuhn shows the extent to which philosophy of science became a new resource to develop arguments concerning the practice of medical science and its implications for policy making in medicine.

Rejected by the *BMJ*, McIntyre sent the manuscript to *The Lancet*, where it was received more favorably with respect to the idea of applying Popperian reasoning to medical practice. What came under criticism, though, was the authors' stance on the issues of "authoritarianism, self-criticism, and audit." The editor of *The Lancet* wrote to McIntyre:

> A year or two back we gave thought to the possibility of a series on scientific method in medicine. We were unable to decide how far the hypothetico-deductive method applies across the spectrum from

[19]Karl Popper papers, Hoover Institution Archives (**Thereafter KPP**), Correspondence, McIntyre. b. 325, f. 14

[20]The Critical Attitude in Medicine," KPP, Speeches and Writings, "b.250, f. 22

> basic science to clinical problem-solving. ... We cannot disagree entirely with the B.M.J.'s advisers. Your article should provide a fuller account of the basis of reasoning and rational behaviour in medicine. Where we differ from the B.M.J. is that we believe that it does have the making of a good article for a general medical journal. Would you and Sir Karl be prepared to re-cast the paper so that it discusses this aspect more fully, at the expense of some of the space given to authoritarianism, self-criticism, and audit? This latter discussion strikes us as unsatisfactory... In short, I would like to put at your disposal five *Lancet* columns for a more sharply focused account of the relevance of the popperian method to medical science and clinical practice.[21]

Apparently an idea to cut off the central point of the paper was not appealing for the authors, since the paper was eventually published in the *BMJ* in a slightly revised form, but preserving the authors' initial position.

The apparent difference in the positions of the philosophers on both sides of Atlantic and their stances concerning the practical policy implications of the uncertainty of knowledge might be attributed to the different roles medical-ethical discourse played in the United States and in Britain in the 1970s and 1980s. As Sheila Jasanoff has pointed out, while in the U.S. bioethics became the province of professional medical ethicists by the late 1970s, in the UK the role of bioethics and medical ethics was to educate and to promote "public understanding" in order to "head off public anxiety before it materialized" rather than to offer professional expertise on public policy issues concerning the questions in the area of expertise of medical ethicists (Jasanoff 2005). In the early 1980s the discourse of medical ethics in Britain was far from being professionalized and institutionalized. Moreover, as Jasanoff noted, "the response to official bioethics ... has been to resist the professionalization of this discourse and to reestablish ethical deliberation as a field of democratic engagement, accessible to ordinary people as well as experts"

[21] Robin Fox to Neil McIntyre, July 1, 1982. KPP, Correspondence, McIntyre, b. 325, f. 14.

(Jasanoff 2005, 191).

Jasanoff called bioethics "a new language of democratic deliberation" (Jasanoff 2005, 172). As she pointed out, bioethics in both American and European democracies was shaped by the political agenda rather than the other way around. The epistemologies the bioethicists endorsed had the different political agendas as well, encompassing different visions of democracies and "free society." Popper's vision of democratic society reconciled a ruthless intellectual rivalry[22] — a Darwinian struggle of ideas — with the regulation of economic competition imposed to protect the interests of the weak (Hacohen 2000). For Popper, ethics couldn't be stripped from its political meaning. Although his foray into medical ethics was limited to a single article, which was entirely his co-author's initiative,[23] Popper's engagement with moral theory was a life-long enquiry, going back to his formative years in interwar Vienna and then to his time in New Zealand during WWII. As Popper's biographer Malachi Hacohen noted, Popper's introductory lectures on ethics in 1938-9 became the historical framework for *The Open Society and its Enemies* (1945)—a pervasive critique of totalitarianism and a defense of liberal democracy written during WWII and considered by Popper himself as his contribution to the war effort (Hacohen 2000, 385). Popper's major target in the *Open Society* was the "scientific ethics" of Marxist moral theory. Endorsing the "scientific attitude towards ethics," Popper distinguished it from Marxist "scientific ethics": "'Scientific Marxism' is dead. Its feeling of social responsibility and its love for freedom must survive" (Popper 1962, 216).

Popper's response to MacIntyre and Gorovitz's article underlines the different views on political implications of the epistemological uncertainties of

[22]For Popper, a culture of critical debate intrinsic for democracy had a character of war: "It is the great tradition of Western rationalism to fight our battles with words rather than with swords" (Popper 1962, 396).

[23]Correspondence with McIntyre, 1980-1983. KPP, Correspondence, McIntyre, b. 325, f. 14

medical knowledge these philosophers held. If, for Popper, ethics could not be separated from politics, on the other side of the Atlantic the bioethicists associated with the Hastings Center deliberately attempted to present themselves as neutral observers not taking stances upon the issues under study. Their endorsement of Polanyi's account of science at the Hastings Center's conferences could suggest that not only Polanyi's epistemology, but also his vision of democracy and "free society" fitted into pragmatic philosophy of the US bioethics.

Polanyi's account of science as first presented in his *Science, Faith and Society* (1946) was shaped by his deep concern about what he considered to be the real threats to science from social and political philosophy and practical politics on both the left and right sides of the political spectrum. His account of science as a self-contained, self-sufficient enterprise, governed by its own rules and free market economy, represented an ideal of science "for its own sake" and a model of a democratic society. However, in his view, contemporary Western philosophy and philosophy of science did not provide any better justifications for science than did Marxist accounts (Polanyi 1946). For him, most philosophical accounts of science misrepresented the fragile relationships between the scientific community and the political and social order and therefore left science as vulnerable as it had shown itself in the Soviet Union (Polanyi 1946).

Steve Fuller has noted that the specific conception of democracy Polanyi subscribed to was *pluralism*, "an alternative to the pseudo-democracy of self-styled socialist regimes, whose idea of "equality under the law" was equal subordination to a central authority" (Fuller 1992, 261). *Pluralists* see themselves as presenting a version of "open society," but instead of questioning authority, as Popper advises, pluralists advocate tolerating authority. This subtle, but significant difference in the meaning of "openness," Fuller noted, was "required of the open society in a world of, so to speak, Big Democracy. In the case of science, it marks a shift away from criticizing products to understanding practices" (Fuller 1992, 262).

Whereas Popper and Polanyi differed in their visions of "free society," they shared a deep political engagement, which shaped their political and epistemological writings alike.[24] Unlike them, Kuhn, as Hollinger and Fuller have pointed out, stands out for having published a book that had no clear political affiliation (see Fuller 1992; Hollinger 1990; Hollinger 1995). Kuhn's account of science boosted the *laissez-faire* ideology and supported an ideal of science that should be left for scientists and maintained in its current state. The suppressed character of the political in Kuhnian account, as Fuller argues, made a decisive impact on the development of the Sociology of Scientific Knowledge, and made Kuhn "[succeed] where even autonomous science's most eloquent champion, Michael Polanyi, had failed. Despite the persuasive case he made for the existence of a "scientific community" ... and the admiring audience he secured for penetrating the "tacit dimension" of scientific practice, Polanyi typecast himself as upholding the culture of science in the face of barbarous and creeping socialists. Participation in the quotidian battles that science fought in the public arena did not permit Polanyi the sort of detached "scientific" rhetoric that characterizes most of the writing in [Kuhn's] *Structure*, a point in no small measure responsible for the book's longevity" (Fuller 1992, 260).

Yet, if Polanyi failed to influence the future development of the studies of science because he was not sufficiently detached from his political agenda,

[24]Both Popper's and Polanyi's views on science and democracy were shaped by their respective political engagements. Like Popper, Polanyi, who by the time he formulated the first account of his vision of science had fled two countries, was acutely aware of and disturbed by Marxist accounts of science. In England, Polanyi had led a movement to resist "planned" science in Western Europe, being the driving force behind the formation of the Society for Freedom in Science in the 1940s, and was engaged for years into economic analysis of the problems of centrally planned economies.

he fully succeeded within the group of scholars involved in the discussion of the philosophical foundations of medical ethics. For the medical ethicists, epistemological claims cut loose from their practical, political and ethical roots—thus becoming a metatheoretical academic exercise—were not congenial. The Polanyian account of science, which converted cognitive virtues to moral ones, diminished and countered the elements in Kuhn's account that were deemed relativistic. While in the Kuhnian account the criterion of scientific truth was the province of scientific practice and self-contained uncommensurable paradigms, in Polanyi's account of science truth and objectivity became moral concepts not less than the matter of internal consensus of scientific community. This position made Polanyi congenial for the medical ethicists, and this position was adopted and further developed by Alasdair MacIntyre in his argument concerning scientific objectivity as a moral concept. In this way, medical ethicists explored and expanded both the philosophical and the political dimensions of Polanyian writings

5.4 Conclusion to Chapter Five

The nexus between political, ethical, and epistemic concerns that animated the philosophical discourse of medical ethics in the 1970s appears especially pertinent for the discipline today, as the field finds itself at the crossroads. During the "science wars" of the 1990s when social constructivist theories came under intense criticism, the STS project appeared as a rebellion against the belief in rationality and objectivity of scientific knowledge. The "lack of scientific certainty" was appropriated to become a powerful rhetorical weapon to be deployed as a means to achieve certain political ends by anybody except for STS scholars.[25]

[25]As Oreskes and Conway have shown, "scientific uncertainty" was used to challenge the scientific evidence of anthropogenic global warming, to the extent that it was singled out as

Leading science studies scholars immersed themselves in a "soul searching," as Bruno Latour put it. "What were we really after," Latour asks, in response to the appropriation of STS insights and methods by deniers of global warming, "when we were so intent on showing the social construction of scientific facts? ... Should I reassure myself by simply saying that bad guys can use any weapon at hand..? Should we apologize for having been wrong all along? Should we bring a sword of criticism to criticism itself and do a bit of soul searching here...?" (Latour 2004).

Indeed, the principle of impartiality, highlighted by the Strong Programme of social constructivism, exhorted the student of science not to take sides or be involved in the debates s/he studied, nor to provide her/his judgment or impose moral values on the explanatory project (Bloor 1976). During the 1970s, the methodological stance of "impartiality" separated the social constructivists from the radical critics of science, with their explicit political commitments. The social constructivist program was presented as a contribution to social understanding, and not as a contribution to social change. By contrast, radical critics of science, including environmentalists and feminist critics, explicitly questioned the separation between "intellectual" and "political". As feminist historian Joan Scott put it, "the two [feminist politics and academic studies of gender— EA] are part of the same political project: a collective attempt to confront and to change existing distribution of power" (Scott 1999, 6).

The "methodological asceticism" embedded in the impartiality principle is now being challenged. The field of science studies is now trying to find place for commitment and encouraging scholars to take sides in the debates they are studying. In the introductory remarks to the last edition of the *Handbook of*

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the official strategy of the Republican party in the 1992 mid-term elections, with the candidates being urged by a Republican pollster "to continue to make the lack of scientific certainty a primary issue in the debate [on global warming]" (cit. in Oreskes and Conway 2008).

Science and Technology Studies, the editors, in comparing the present field of science study with the representation of the field in previous editions, note: "Normativity, relativism, and evaluation of expertise and scientific knowledge endure from previous volumes but in new ways: no longer just problems for philosophical reflection, such concerns are now posed in terms that seek collective political or sociological resolution" (Hackett et al. 2008, 3). Still, the place of ethics remains modest within the science studies project, being limited to the "ethical implications" of science and the possible contribution of STS scholarship to ethical inquiry, rather than assuming the possibility of an equal contribution of both fields to each other. Characteristically, the chapter in the *Handbook* that is aimed at placing ethics within the STS project is called "STS and Ethics: Implications for Engineering Ethics" (Johnson, Wetmore 2008).

Such a modest and intellectually impoverished place for ethics in the STS project was far from what medical ethicists associated with the Hastings Center in the 1970s and early 1980s were looking for. Their ambitions went much further than to tackle the "ethical implications" of medical and scientific advances, but rather to work out the possibilities for a real dialogue between the two fields of intellectual inquiry divorced by the "Kantian unintended achievement" (MacIntyre 1978). With their efforts to formulate the new, post-Kuhnian, philosophy of science, with which Marjorie Grene and other philosophers attracted to medicine and biology in the 1970s associated themselves, these philosophers hoped to propel a view of science that retreated from "old empiricism" ideals without sacrificing philosophical realism (a position Grene called "comprehensive realism").

Historian Betty Smocovitis suggested that the philosophy of biology came out of both the 1960s "antireductionist movements in biology and anti-positivist movements in philosophy" (Smocovitis 1996, 105). Philosophers Werner Callebaut, Elliott Sober, and Alexander Rosenberg also explicitly tie the emergence of philosophy of biology to the decline of logical positivism in the 1960s and 1970s (Callebaut 1993, 73-74). The nexus between medical ethics and the philosophy of science constituted an area where the antireductionist movement in biology and anti-positivist movement in philosophy intersected, providing a viable ferment for the discussion of what the philosophy of biology should look like. Philosophers' anticipation of the "new philosophy of science" that would emerge on the nexus between medicine, medical ethics, and post-Kuhnian philosophy of science didn't live up entirely to their expectations, though. Although philosophy of biology was institutionalized as a distinct field, it didn't attain the epistemic authority that Marjorie Grene and others deemed this field would achieve.[26]

Some of these early insights in the philosophy of biology of the 1970s sound prophetic now. Thus, in the recent *Isis* Focus section on "Changing Directions in History and Philosophy of Science," Lorraine Daston highlights the fundamental importance of the "ontology of scientific observation" for the history and philosophy of science, endorsing Fleck's conception as an original and highly relevant insight for history and philosophy of science (Daston 2008). Marjorie Grene's emphasis on perception as an active process, an achievement, a product and continuation of previous learning patterns (Grene 1985) reads as foresight of some of the same issues, which the field she was trying to map out, is now readdressing.

Likewise, the medical ethicists' inclination towards exploring the epistemological relevance of emotions and passions, following the lead of Polanyi, faded away in the course of the further developments of the field. These issues

[26]Margorie Grene's efforts at developing since the 1960s the then barely existent field of philosophy of biology were well recognized, though. As Richard Burian has put it in 1986, she played "an extraordinary role" in shaping philosophy of biology as a discipline (Burian 1986, 23).

also are resurfacing in today's "soul searching." Giving an example of political epistemology, which succeeded in "depoliticizing political passions ... to the point of leaving citizens nothing but gloomy asceticism...," Latour suggests to take seriously the "psycho-social entry into the problem" of environmental political epistemology, that is to say, to tackle "this philosophical issue as a psycho-social question, namely as a question of emotion, of feeling, ... of political passions" (Latour 2009). In their attempt to appropriate Polanyian epistemology in order to reconcile philosophy of science, ethics and medicine, bioethical philosophers were in the position to create such a "psycho-social entry" into the political epistemology of medical ethics.

The dialogue between philosophically minded bioethicists and the philosophers of science who responded to Thomas Kuhn's challenge in the 1970s - early 1980s thus offers important insights allowing to readdress some of the issues that were at stake twenty years ago, when the field of STS was taking shape, to see what was left behind when STS became dominated by "fruitful but thoroughly intellectualist methodology," to borrow Robert Westman's expression (Westman 1975). Although largely elided from the subsequent developments of the post-Kuhnian studies of science, some of these early insights and deliberations seem strikingly pertinent to the today's "soul searching," showing that the ways in which STS has proved itself vulnerable to political appropriation were already identified as potential weaknesses when Kuhnian and post-Kuhnian philosophy of science was applied to the field of medical ethics.

This chapter, in full, is a reprint of the material as it appears in *History and Philosophy of the Life Sciences* 2009. Elena Aronova. 2009. In Search of the Soul in Science: Medical Ethics' Appropriation of Philosophy of Science in the 1970s,

History and Philosophy of the Life Sciences 31: 5-34. The dissertation author was the only investigator and author of this paper.

Chapter 6

The Politics and Contexts of Soviet Science Studies (*Naukovedenie*): Soviet Philosophy of Science at the Crossroads

> *[Naukovedenie] ... is a domain of scholarship which is situated on the borderline between the natural sciences and philosophy. Hence, in our country, where scientists are constantly improving their philosophical competence, we have the optimal conditions for the development of the science of science.*[1]

[1] Stoletov (1966: 422). Vsevolod N. Stoletov, the Minister of Higher Education of the Russian Federation, used the occasion of the translation of the collection of essays in honor of John D.

Naukovedenie (literarily meaning the 'science of science' or 'science studies'), was first institutionalized in the Soviet Union in the 1920s, then resurfaced and was widely publicized in the 1960s, as a new mode of reflection on science, its history, its intellectual foundations and its management, after which it dominated Soviet historiography of science until Gorbachev's *perestroika*. The seemingly deep internal transformation of the field of the history of science in Russia and the high expectations that the Soviet leaders of *naukovedenie* placed on it added to its visibility outside the Soviet Union.

In the 1970s and 1980s the institutionalization of this new field in the Soviet Union occurred simultaneously with the dramatic reconfiguration of the studies of science in the Anglophone West, where it was associated with the proliferation of the sociology of knowledge and the extension of cultural anthropology, critical theory, and literary studies into the studies of science. Soviet *naukovedenie*, not surprisingly, almost immediately attracted the attention of Western science studies scholars.[2] However, seen against the backdrop of what came to be known as *science studies* in the Anglophone West, the Soviet project appeared as a rather bleak version of its Anglo-American counterpart. Thus, in 1984 the leading journal in science studies, *Social Studies of Science*, published a review entitled "Soviet Science Studies: A Dissident View," written by Alexey Levin and introduced by Steven Shapin, who was in contact with the Soviet dissident philosopher, by that time fired from his position at the Moscow Institute of Philosophy. The publication reproduced extracts from the correspondence between Shapin and Levin, which included the following dialogue:

Shapin: Could you comment on the general state of history of science

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Bernal, *The Science of Science: Society in a Technological Age*, originally published in 1964, to publicize the new discipline in the Soviet Union. The quote is from his afterword to this collection.

[2]See useful review of the field of *naukovedenie* in the Soviet Union: Rabkin (1976).

and science studies in the Soviet Union?

Levin: This is a poorly developed field in the Soviet Union. To begin with, it is under-developed institutionally: there is at present not a single department, unit, faculty, or the like, which could provide an appropriate training in any branch of science studies. ... The only university departments that are able to offer at least some formal training suitable for science studies are departments of philosophy, with the result that the field is overcrowded with philosophers. You have, I know, your philosophical rationalists in the West, but you could hardly imagine a typical Soviet philosopher [Levin goes on to describe ideological constraints on Soviet philosophy - S.S.]. On the other hand, the education furnished by even the best philosophical faculties can hardly be regarded as adequate: too much classical philosophy, too much ... Marxist disciplines..., too much formal logic, and too little general history. ...

Shapin: In the West, the most recent empirical Marxist study from the Soviet Union with which many historians are familiar is Boris Hessen's 1931 'Social and Economic Roots of Newton's *Principia*.' What, if anything, has become of that tradition of work?

Levin: Work in Hessen's style is now very rare...

Shapin: What, then, is the official line in Soviet science studies?

Levin: ... Soviet history and philosophy of science ... has never produced its own special models of scientific change or practice other than abstract and over-universalized pictures constructed in the framework of dialectical and historical materialism. ... I cannot name any sound, middle-level methodological, sociological, or historical conception of science developed by Soviet scholars.[3]

As Levin further attested, "the approaches which dominate Soviet historiography are traditional and intellectualist. Little attention is paid to modern Western perspectives in the social study of science. ... the references to modern Western authors ... are ritualistic ... [and] there is no significant orientation to what is occurring in the West" (Levin 1984: 462).

Several years later, in the late eighties, an anthropologist visited the Moscow Institute for the History of Science and Technology (Institut istorii

[3]Levin (1984: 464).

estestvoznanija i tekhniki, thereafter IHST), the country's leading center of *naukovedenie.* Alessandro Mongili used Bruno Latour and Steve Woolgar's classic *Laboratory Life: The Construction of Scientific Facts* - the core work in the laboratory studies tradition within the field of science studies - as a model for his own anthropological study of Moscow Institute (Mongili 1998a). What resulted was a fascinating account of the daily activities and routine practices of the researchers of this Institute, and the ways in which Soviet *naukovedy* constructed their "scientific facts." Mongili suggested that Soviet *naukovedenie* was "a phantom science" ("une science fantôme"), as a nod to Latour's point on the significance of paper documents and texts - literary inscriptions - in the "normal" operation of science (220). As Mongili described this phantom, or fictional, character of Soviet *naukovedenie,*

> The research units were far from being the real teams of research. More often than not it was a matter of a fictional collective: each individual researcher tended to be occupied with his own personal research or did no research whatsoever. ... Other structural conditions, such as control structures that [reduced the possibilities to publish] and ... the difficulties and dangers for publishing outside the USSR ... reduced the importance of publications. ... Publication was transformed into a gadget for those researchers who already enjoyed high-level careers. ... The publications were [obsolete] and were not read, at least not by the professional public. ... In the climate of control, censorship and self-censorship, texts tended to become more and more irrelevant, little more than conveyers of cryptic messages... Science therefore found itself in a situation where publication played only a marginal role: it lived in an almost *pre-gutenbergian world.*"[4]

Both Levin and Mongili compared and contrasted Soviet science studies with what is called "science studies" in the Anglophone tradition, and found it wanting. In contrast, in this account of the history of "Soviet version" of science

[4]Mongili (1998b: 170-180, emphasis added - EA).

studies I attempt to understand *naukovedenie* in its own terms and situate it within its own local context – the culture of late-socialism in the Soviet Union during the Cold War - asking how this discourse functioned and what it meant for its creators and practitioners. As historian Slava Gerovitch pointed out, Soviet historiography of science followed closely the political and social evolution of Soviet society, changing its thematic and methodological outlook according to the changing political demands of the time (Gerovitch 1996, 1998). I seek to extend Gerovitch's account, focusing on one aspect of Soviet science studies – the role of philosophers and philosophy in the Soviet science studies project.

Contrary to what came to be known as *science studies* in the US and the UK, with its explicit juxtaposition of the new approaches in the studies of science to traditional philosophy of science dominated by logical positivism, and the ambition to replace philosophy of science by sociology of knowledge (the credo of the so-called Strong Programme and the Edinburgh school),[5] in the Soviet Union *naukovedenie* was a preeminently philosophical project. As Levin lamented, the field of *naukovedenie* was "overcrowded with philosophers" (Levin 1984: 464). Who were these philosophers who came to occupy this discursive space in the sixties? What discourse on science did they produce and why? The mainstream Soviet philosophers, to whom Levin's referred to as "typical Soviet philosophers," wrote lots of undeniably boring and uninspiring pages, as Levin's comments vividly testified. However, making these "typical" philosophers and their discursive strategies the focus of analysis is revealing, as it allows us to see to what they were responding. What these non-dissident adaptors and survivors

[5]As John Zammito noted, "this disputation of the authority of philosophy of science emanated most forcefully from David Bloor. Bloor's book *Knowledge and Social Imagery* was aptly characterized as 'a sustained tirade against philosophers.' Bloor 'sets out to redefine the disciplinary boundaries for the study of science, giving sociology pride of place ... and dealing philosophers ... largely out of the game" (Zammito 2004: 137).

produced reflected the relationship between science, society, and the state.

In this account of Soviet *naukovedenie* I use the history of IHST as a case-study. Since its inception in the twenties, the Institute became a prototype of a country-wide "model" institution in historiography of science, conceived as part of the bigger ambition to develop and institutionalize meta-studies of science, under the names of "studies of methodology of science," "general studies of science," or *naukovedenie*. The case of this single institute allows me to trace the history of meta-studies of science in the Soviet Union from its early institutionalization in the twenties, when various political, theoretical and institutional struggles set the stage for the development of the new field in the new Soviet republic, to the sixties when the field resurfaced within the particular political context of the Cold War. I argue that rather than being a "fictional" or "phantom" discipline existing in an entirely different world of Soviet socialism incommensurable with Western democracies ("pre-gutenbergian world," in Mongili's words), the story of Soviet science studies is both comprehensible and intriguing in the same terms in which historians have come to understand how the political concerns of the Cold War formed the backdrop of "the science studies renaissance" in the U.S. and the U.K. in the sixties and seventies.[6]

[6]Recent studies highlighted the political context of the emergence and institutionalization of science studies as an academic discipline during the Cold War. Thus, as Steven Fuller argued, Thomas Kuhn's account of science fitted the demands of the time in successfully promoting the conservative cause of maintaining science's status quo in the hostile political environment of the Cold War, forging the "conservative agenda" and a "neutral," apolitical stance of science studies (Fuller 2000). Similar argument was made by David Hollinger (Hollinger 1995). On the ways the Cold War anxieties and concerns affected the intellectual agendas of historians and philosophers of science in the U.S. see Reisch (2005), Solovey (2001).

6.1 Soviet Philosophy and the Studies of Science in the 1920s and 1930s

Philosophy always had a special status in the Soviet Union. Marxist philosophy provided explicit legitimation of the Soviet system of the party-state. Moreover, Soviet Marxism was proclaimed to be a truly "*scientific* philosophy," in the sense that both science and philosophy were seen as the foundations of the Soviet system. By the same token, Marxism was also proclaimed to be distinct from all former socialist theories, as it was perceived neither as "utopian socialism" nor as "ethical socialism" but as *scientific socialism.* As Lenin famously stated in his 1902 *What is to be Done?*, one of the central tasks of the workers' party was to educate the working class in the *science of socialism.*

The relation between Marxist philosophy and science, i.e. the question of whether and in what sense Marxism was a science, was the subject of major debates in Soviet philosophy since its inception.[7] The special relation, however understood, between Marxist philosophy and science also authorized the expansion of Marxism into the natural sciences, both theoretically and institutionally. Studies of science as a social institution and the basis for a "knowledge society" attained high visibility within educational curricula of the newly formed educational institutions.

[7] These debates came to partition the opposing groups of leading Bolshevik theoreticians in the twenties. The group of scientifically inclined Marxists (the so-called "mechanists," most prominently Aleksandr Bogdanov-Malinovskij and Nikolaj Bukharin) argued that the methodologies of natural sciences required no "working over" by Marxism, because Marxism was compatible and indeed continuous with all genuine science. The other, more defined group lead by Abram Deborin ("Deborinists"), prompted in part by the publication, in 1925, of Engels' *Dialectics of Nature* by the Institute of Marxism-Leninism, called for systematic application of "dialectical method" to natural sciences, denouncing the "mechanists" for their alleged failure to appreciate the importance of Hegelian dialectics. In 1931, however, the dispute was terminated by the resolution of the Party's Central Committee and both positions were officially condemned as "right" and "left deviations" respectively. Nevertheless, the same discussion, with the same positions, resurfaced in the sixties, although without the old labels (on the debate between "mechanists" and "Deborinists" see Joravsky (1961), Scanlan (1985).

Moreover, meta-studies of science were promoted as a special discipline throughout the twenties and thirties in the Soviet Union. A special term for such a discipline – "*naukovedenie*" (a term which resurfaced in the sixties) – was suggested by philosopher I. A. Boričevskij in 1926 (Boričevskij 1926).[8]

The major institutional niche for the development of the meta-studies of science was provided by the Communist Academy, which was established in 1918 as the center for the education of the new, Communist, intelligentsia – educated, dedicated, and disciplined professional revolutionaries who should combine the attributes of both revolutionaries and scholars.[9] The Communist Academy was founded with the aspiration of becoming the Party's "theoretical center" as well as the country's premier center of Marxist research and study. Initially formed as the Socialist Academy of Social Sciences (Socialističeskaja Akademija Obščestvennykh Nauk), in the first years of its existence the Academy focused on the social sciences. During the twenties "the social sciences" (obščestvennye nauki) were re-defined in the Soviet Union in relation to the component parts of Marxism. Thus, for example, sociology was redefined as being identical with historical materialism, that is, the study of the general laws of development of human history and the specific laws of socio-economic formations. The authoritative text for Marxist sociology was Nikolaj Bukharin's in his *Teorija istoričeskogo materializma - populjarnyj učebnik marksistskoj sociologii.*[10] According to Bukharin, because Marxist sociology is equated with historical materialism and is thus a component of Marxist theory, there is no need for sociology as an independent science.[11]

[8] Boričevskij's visionary proposal was materialized, at least to some extent, in the establishment of the Institute for the History of Science, headed by Nikolaj Bukharin, with which Boričevskij was affiliated from its inception in 1927.

[9] On the history of the Communist Academy see David-Fox (1997), Krementsov (1997).

[10] Published in English translation as *Historical Materialism: A System of Sociology* (Bukharin 1925).

[11] The institutional consequence of these discussions was the elimination of the Department of Social Sciences at Moscow University that included the chair of sociology, which was disbanded

By the mid-twenties the Communist Academy expanded to include the natural sciences, being transformed from an exclusively educational institution into what was conceived as a "scientific-methodological center." History and philosophy of science as well as studies of science "as a whole" (*naukovedenie*) were promoted by leading Bolshevik theoreticians who became the major patrons of this new offshoot of Marxist philosophy. In 1920 the first institute focusing entirely and exclusively on meta-scientific issues – the Institute of Scientific Methodology – was established on the basis of the Communist Academy's Section of methodology of science. The Institute of Scientific Methodology affiliated many prominent Russian scientists – mathematicians N.N. Luzin and O. Ju. Šmidt, physicists A. K. Timirjazev, V. K. Arkad'ev, and Ju. V. Vul'f, economists E. S. Varga and S. G. Strumilin, among others – many of whom were not Marxists, let alone Party members.[12] As such, the formation of this Institute within the Communist Academy signaled that the Academy was moving beyond the boundaries of social sciences, "... gradually turning into a scientific-methodological center ...," as the resolution of the 12th Party Congress stated in 1923 (David-Fox 1997: 211). During the twenties, several specialized research institutes devoted to meta-studies of science were established under the auspices of the Communist Academy, supplementing teaching curricula in history and philosophy of science, which became a prominent part of the educational programs at the Communist Academy's institutions, notably its Institute of Red Professors and Sverdlov

in 1924, after five years of existence. For an overview of the history of sociology in the Soviet Union see Weinberg (1974).

[12]The research themes listed in the institute's research agenda ranged from "mathematical method in biology" and "the application of quantum theory to the theory of chemical reactions" to "the disciplinary structure of biology" and "the role of statistics and abstract analysis in scientific research." Although the Institute never became an influential center of theoretical coordination, as initially envisioned, it was, in words of one of the Institute's members, "the first attempt at collective meta-scientific study" (cit. in Bastrakova 1978: 37).

University, with the research component.[13]

In 1927 the Communist Academy's Section of the Natural Sciences petitioned for the establishment of the Institute for the History of Science, affiliated with the Communist Academy. The program of the Institute included the studies of the relations between science, on the one hand, and technology, art and literature, on the other, as well as studies of the social organization of science – institutional history of science, history of education, and the "... governmental politics of science in different countries at different times."[14] As the 1927 program further suggested, the research at the new institute would focus on the "history of science and specifically the use of science as a tool in the class struggle of the exploiters against the exploited (the cases of racial hygiene, chauvinistic anthropology, pseudo-Darwinian eugenics, etc); ... the phylogeny of scientific ideas and discoveries; study of the conditions and impact of different factors on the growth of scientific thought and scientific discoveries; ...study of individual, social and genealogical backgrounds of scientists (as exemplified by de Candolle, but revised according to Marxist theory); ... a systematic book-keeping of scientists' genealogies and social status, as well as statistical study of data on scientific discoveries in different countries; and the development of methods and ways of popularization of science."[15]

[13]On the institutionalization of meta-studies of science in the Soviet Union in the twenties and thirties see Bastrakova (1978), Ilizarov (1993), Dmitriev (2002), Gindilis (2009), David-Fox (1997).

[14]*Programma kabineta po istorii estestvoznanija*, on p. 4.

[15]*Programma kabineta po istorii estestvoznanija*, on p. 4-5. The reference to Alphonse de Candolle (whose *Histoire des Sciences et des Savants* (1873) drew on genealogical and statistical data to explore how various variables influenced the rates at which different European countries produced eminent scientists) concealed a peculiar appropriation by the new Institute of the studies of the social composition of Russian scientific and intellectual elites, conducted in the twenties by geneticist Jurij Filipčenko as part of the work of his Bureau of Eugenics in Petrograd. Filipčenko and his students collected and statistically analyzed data on the genealogies, demographic composition, sex ratio, etc, of the co-opted Russian Academy of Sciences for more than 80 years, the time span that covered both the Academy's pre-revolutionary and post-revolutionary periods. At the time when the future of the Russian Academy of Sciences

The plan of the Institute was adopted by the Presidium of the Communist Academy, and what was initially called The Cabinet of History of Science of the Communist Academy was established in December 1927 under the directorship of Nikolaj Bukharin.[16] Its first research topics included Boris Hessen's "social and economic roots of Newton's mechanics," which would make such a lasting impact at the 1931 International Congress for the History in Science in London; "Haeckel and monism;" " the history of geology in its relation to natural philosophy in the first half of the 19th century;" along with more abstract and theoretical topics, such as "the general history of the methodology of science," "the history of historiography of science," as well as several projects to elucidate and develop the views of the founders of Marxism on the history of science and technology.[17]

With the consolidation of Soviet system of the party-state Marxist philosophy, once a productive field of scholarship, was turned into an official canon and became gradually dogmatized. During the thirties, which were marked by ideological battles and "public debates" on science, Soviet philosophers actively

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was undecided, Filipčenko highlighted the "essentially democratic" composition of the Russian Academy, stressing its difference in this regard from foreign Academies of sciences: "in comparison with de Candolle's statistical data on foreign members of Parisian Academy of Sciences, our data ... [show] that our outstanding scientists are descendent from much more democratic background than the members of Parisian Academy of Science..." (Filipčenko 1922). On Filipčenko's eugenics program see Adams (1990).

[16]In 1932 The Cabinet for the History of Science was merged with the Commission for the History of Knowledge, created in 1921 by the initiative of Vladimir I. Vernadskij under the auspices of the All-Union Academy of Sciences, to form the single Institute for the History of Science and Technology (IINT). The creation of IINT was preceded by the replacement in 1930 of Vernadskij, in his role of the head of the Academy of Sciences' Commission for the History of Knowledge, by Bukharin, as a consequence of 1929 "cleaning" of the Academy of Sciences from 'bourgeois" elements (the so-called "1929 affair of the Academy of Sciences" - "delo Akademii Nauk"). IINT was established under the directorship of Bukharin, and now was formally affiliated with the All-Union Academy of Sciences, ceasing its earlier affiliation with the Communist Academy. This reorganization signified the major trend started in 1932 (the beginning of the Second Five-Year Plan) towards the centralization of Soviet scientific and educational institutions. By the mid-thirties, the parallel existence of the two academies, All-Union Academy of Sciences and the Communist Academy, ceased to exist, and in 1936 two academies were merged into one, centralized system – the Soviet Academy of Sciences.

[17]Bastrakova (1978), esp. on p. 44. On Boris Hessen see Graham (1985).

participated in the ideological surveillance and policing of the scientific community, "translating scientific theories into ideological language for the politicians and transforming political slogans into research agendas for the scientists" (Gerovitch 2002: 26).[18] Historian Slava Gerovitch has noted that under Stalin the relations between philosophy and science were turned around: while in the early Soviet period the Bolshevik party-state drew its legitimacy in part from its claim to be *scientific,* thus putting science "on the top" (that is, higher or equal to philosophy), by the time when the Soviet party-state was finally firmly established under Stalin, science had to base its legitimacy on its declared compliance with Marxist philosophy (Gerovitch 2002: 29).

The terror of the 1937-38 turned out to be detrimental for the nascent field of the meta-studies of science in the Soviet Union. Almost all leading Bolshevik theoreticians who became the major targets of Stalinist purges were affiliated with the Communist Academy. Many members and all the heads of the centers for the meta-studies of science created within the Communist Academy during the twenties and early thirties were arrested during the purges, and their institutes were disbanded by 1938, thus ending this first stage of the institutionalization of the studies of science as an independent discipline in the Soviet Union. Bukharin's Institute for the History of Science was disbanded in 1938, following his arrest in 1937 which was followed a few months later by the arrest of the Institute's second director, academician V.V. Ossinskij. The remnants of the Institute's staff and facilities were reorganized in the same year of 1938 to form the Commission on the history of the Academy of Sciences, which was structurally subordinated to the Academy of Sciences' Archives. The Commission was staffed by illustrious

[18]Most infamously, Soviet philosophers attacked the theory of relativity, quantum mechanics and genetics, during the "public debates" on science, in search of various "philosophical deviations" of scientific theories (Krementsov 1997).

members of the Academy of Sciences, such as V. I. Vernadskij, A. N. Krylov, O. Ju. Šmidt, L. S. Berg, among the others, but it functioned more as an administrative unit rather than an intellectual or research center (Ilizarov 1993: 17-19).

6.2 The Cold War and Soviet Philosophy

The Cold War gave the major incentives for the re-emergence of *naukovedenie* in the Soviet Union. The Cold War had a profound effect on the status of Soviet philosophy. While WWII was instrumental for the rise of the status of natural scientists, the beginning of the Cold War was crucial for the re-definition of the social sciences in the Soviet Union as disciplines independent of philosophy. Within months after Winston Churchill's "iron curtain" speech in November 1946 several new journals and institutes were established "... to enlarge and improve the personnel in the social science disciplines."[19] The institutional measures to strengthen the "ideological front" included the establishment, in November 1946, of the Academy of Social Sciences under the Party's Committee for Agitation and Propaganda (Agitprop). The creation of the Academy of Social Sciences indicated that with the beginning of the Cold War the USSR was in need of highly qualified social analysts who could provide tools and categories for defining and interpreting the new postwar situation, and who could develop the framework for understanding the basic notions of the Cold War.[20] Soviet scholars – philosophers in the first place – were assigned the key roles in the battle on the "ideological front" of the Cold

[19] As proclaimed by Andrej Ždanov in 1946, cit. in Krementsov (1997), on p. 130.

[20] During the following decade, various departments and "laboratories" for training and research in sociology, social planning, and "concrete social research" (konkretnye social'nye issledovanija) were opened in several universities and research institutes. Sociology as an independent field with its discrete functions was legitimized officially in 1966 at the Twenty Third Party Congress, and two years later the separation of sociology from philosophy was institutionalized in 1968 with the transformation of the Division of Social Research of the Institute of Philosophy in Moscow into a separate Institute of Concrete Social Research (see Weinberg (1974), Greenfeld (1988)).

War: on the one hand, their function was to criticize Western ideas, on the other, they were expected to come up with compelling visions of Soviet modernity that can be exported to Eastern Europe and Third World countries. [21]

In the early years of the Cold War the emphasis was on criticism. In the wake of the patriotic campaign started during the war, patriotism and its opposite, "servility to the West," became the main notions that framed the thematic profile of Soviet historians and philosophers of science. In the late forties and fifties, Russian history, in particular the history of Russian science, became the important element of the new doctrine of Soviet patriotism, which portrayed "Soviet culture" as advanced, modern, progressive, and European, but at the same time based on and continuous with Russian culture.[22] The Cold War rhetoric emphasized the strict dichotomy between East and West. Scholars from different disciplines were busy at juxtaposing the "two camps" with their incommensurable sets of values – "Soviet values" *vs* "Western values." The notion of "world science" came under attack and became the common accusation in the public debates on science that erupted in the forties and fifties. As one of the speakers at the 1950 Academy of Sciences meeting exclaimed, "There is no place in Soviet science for those who, under the slogan of 'a single world science', openly or secretly try to hamper the development of our science. ... They are unworthy to bear the exalted title of Soviet scientist."[23]

Following the death of Stalin in 1953, and during Khrushchev's Thaw, however, the emphases changed. After Stalin's death in 1953 and the successful testing of the Soviet hydrogen bomb in the same year, scientists, especially those

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[21] See discussion in Pollock (2006).

[22] See discussion of the strategies of Soviet historians of science and technology during the rise of Russian nationalism in the forties and fifties in Gerovitch (1996). On the politics of historical profession in the Soviet Union and the appropriation of historical memory under different Soviet rulers over the course of the twentieth century see Koposov (2011).

[23] G. K. Khrushchov, cit. in Krementsov (1997), on p. 219.

associated with the atomic bomb project, used their new prestige to renegotiate the power relationships within the Soviet academic community. Philosophers (or, rather, 'a philosopher'), in words of Gerovitch, were targeted by leading Soviet scientists as the "major evil" for Soviet science. As Gerovitch put it, "liberal intellectuals personified the enemy in the figure of a 'philosopher'," when in the fifties and sixties the leading Soviet scientists called into question the dominant status of philosophers in Soviet scientific community (Gerovitch 2002: 163). As David Holloway has observed, however, "...the term 'philosopher' was used to describe not only professional philosophers and officials in the ideological apparatus, but also those who employed the Stalinist 'techniques of persuasive argumentation' in natural scientific debate" (Holloway 1974). "Philosophy" thus came to signify the entire Stalinist political and ideological discourse with its "ideologisation" of Soviet science. Historian David Gillispie put it succinctly when he noted that "to some extent, Soviet philosophers were made the scapegoats for various lags in science and technology."[24]

Philosophers responded to this legitimation crisis and reorganized themselves in various ways.[25] During the sixties and seventies many philosophers turned to the in-depth studies of science, changing their disciplinary identities and affiliations.[26] Others, on the other hand, turned their efforts to the need to project

[24] Cit. in Gerovitch (2002), on p. 166.

[25] For a discussion of the strategies used by scientists to unsure a greater autonomy and independence from the control of the party ideologists, see Gerovitch (2002).

[26] For example, as historian Douglas Weiner discussed, philosophers became a distinct subgroup of the "environmentalist community" in the sixties and seventies. Environmentalism came in a variety of flavors in the Soviet Union. For Soviet philosophers, however, the engagement with environmentalist issues was mostly rhetorical. As Weiner noted, "social scientists could not engage in Marxist analysis of the political economy of their own society," which made nature protection a purely rhetorical exercise for them. Even 'unmasking' the "myths" of inexhaustibility of nature or the desirability, let alone possibility of "man's domination of nature," philosophers could not expose the structural or socio-economic causes that led enterprises and ministry officials to externalize environmental costs. As Weiner pointed out, "nature protection-as-rhetoric" led to the promotion of "picaresque new careers" of these new men of environmentalism, at the same time making environmentalism safe for the Soviet regime (Weiner 1999: 399-401).

a positive image of the socialist modernity, by developing a new philosophical discourse on science, which was now informed and shaped by the new political realities of the Cold War.

The Cold War was, among many things, about different visions of how to organize science. The basic premise of Soviet Marxism was that in the socialist system scientific rationality is perfectly matched by the rationality of social and economic planning. This acclaimed congruence between scientific and socialist rationalities acquired renewed significance in the context of the Cold War confrontation. Social scientists were expected to provide an intellectual coherence to the *apriori* premise that communism or socialism was the best way to organize society as well as science rationally and efficiently.[27] This role of philosophers and social scientists grew even more important in the sixties and seventies, when the arms and space race heated up.

In the late fifties and during the sixties, in the wake of the sensational achievements of Soviet science in nuclear physics, the pioneering construction of the first nuclear power station and nuclear ice-breaker in the Soviet Union, and, above all, the Soviet exploration of space, a new term entered the Soviet political and philosophical discourse – the "Scientific-Technological Revolution" (Naučno-Tekhničeskaja Revoljucija, thereafter STR). The notion of STR came to denote these recent scientific achievements and technological innovations that had taken place since the WWII, and which had such a critical impact on the arms and space race, promising crucial changes to the socioeconomic conditions in the USSR, and globally.[28]

[27]See discussion in Pollock (2006).

[28]In the seventies and eighties the literature on STR was thoroughly reviewed by American and Western European Sovietologists and political analysts. The most important overviews of the literature include: Hoffmann and Laird (1982a), Hoffmann and Laird (1982b), Hoffmann and Laird (1985), Black (1979), Buchholz (1979), Buchholz (1985), Hoffmann (1978), Rapp (1985).

A comprehensive theory of STR was introduced in the Soviet bloc in the sixties by philosophers and social theorists in Eastern Germany and Czechoslovakia – technologically the most advanced countries in the Soviet bloc. In Czechoslovakia, the theory of STR, developed by Czechoslovak philosophers, provided the reform movement led by Alexander Dubček with its philosophical agenda. The program of Czechoslovak economic reforms ("socialism with a human face") was the result of the team work of philosophers, economists, sociologists, psychologists, engineers, and natural scientists, commissioned by Dubček's government and led by philosopher Radovan Richta, who assumed the directorship of the Institute of Philosophy in Prague in 1968.[29] The programmatic collective monograph that resulted from this team work, *Civilization at the Crossroads: Social and Human Implications of the Scientific and Technological Revolution*, was published in Czech in 1966, followed by an English translation in 1969.[30] The central argument of Czechoslovak reformers was that the modern STR delimited a new epoch "in the evolution of productive forces" and required the adaptation of the socialist economic system to the demands of modern industrialization and scientific-technological development. As Czechoslovak reformers argued, however, while the newest STR was critically different from the "first industrial revolution" in many important aspects, in the socialist countries the qualitatively new possibilities of the STR were still combined with an economic system that had traits of the first industrial revolution. As the Richta report stressed, "these considerations underscore the vital need for radical economic reforms now being introduced in the socialist countries"(Richta 1969: 19). In practice, such an

[29] As historian of Czechoslovak reform movement has noted, "The Dubček leadership could, from the very beginning, count on unprecedented expertise of a kind that the previous regime was in part denied and in part refused to accept. ... Few regimes had ever been able to rely on such formidable theoretical support" (Kusin 1971), see also Kusin (1977).

[30] Richta (1966), published in English as Richta (1969).

adaptation would mean a more flexible and transparent economic system, and openness to the world-wide exchange of information and ideas.

The Soviet invasion in Czechoslovakia in August 1968 and the end of Dubček's "new way toward socialism" had immediate consequences for the philosophical discussion of STR. The specialists in Czechoslovakia who played leading roles in the debates of the sixties and those who were active in the reform movement per se were removed from their positions and posts, although most if not all of them continued to work in visible positions (Kusin 1971). Some theoretical innovations of Czechoslovak reformers, particularly those concerning the role of social groups and the nature of social relations under socialism, were explicitly criticized and rejected both in Czechoslovakia and in the Soviet Union. However, the criticism did not include the theories of STR developed by Richta and his team (Kusin 1971, 1977). Rather, the major effect of the events of 1968 on the theories of STR was a shift of focus from the discussion of the reformist cause and the need to adjust socialist societies to the requirements of the STR to the discussion and evaluation of anticipated (or unanticipated) social and political consequences of STR. [31]

The seventies marked the beginning of what might be called the "era of STR" in Soviet political discourse, when the STR became a central notion in the official statements of Soviet leaders. The greatly increased official commentary on the STR was part of the campaign to formulate more effective national policies and

[31] In the Soviet Union, in the economic sphere, the mid-sixties was a period of innovative thought and experimentation. New institutes created at this time, such as the Central Economic and Mathematical Institute and the Institute for Concrete Social Research, along with the Institute of Economic in Novosibirsk, formulated ideas and proposals calling into question many established Soviet economic principles (see Josephson (1997)). Numerous debates and positions were adopted on such issues as pricing, value, the plan and the market. The broad conclusions which slowly filtered out of these discussions - the emphasis on efficiency, intensification and productivity, need for greater autonomy for the lower levels of the system, more scope for individual initiative and incentives - formed the core of the Kosygin economic reforms of 1965, adopted soon after Brzezhnev replaced Khrushchev.

to mobilize bureaucratic support for the major economic and political decisions of the time: to pursue the politics of détente, which marked a new phase of the Cold War, accompanied by increased economic cooperation and West-East trade. The economic situation was a crucial factor in these political moves. The oil crisis of 1973 created new opportunities for the export of Soviet oil and opened new conditions for the integration of the USSR into the world economy. However, although the oil windfall was keeping the Soviet economy running, the seventies were marked by the beginning of a slow economic decline, which became only more obvious in the course of the next decade. [32] By the mid-seventies the Soviet industrial economy largely ceased to grow, causing Brezhnev's government to adopt the policy of technology-transfer from the highly industrialized capitalist West to the socialist East, as the way to promote economic growth and productivity.[33]

In this new political climate of détente, the official STR discourse offered different images of the future socialist utopia.[34] Soviet philosophers and social scientists were in a unique position to interpret and to give meaning to the official pronouncements on STR on the part of Soviet party leaders. Thus, for example, almost every account of the STR referenced or quoted Brezhnev's speech at the Party's 24$^{\text{th}}$ Central Committee Congress in 1971. The line from the speech where Brezhnev referred to the STR literally stated: "The task we have, comrades, is one of the greatest historical importance: *to fuse the achievements of the scientific-technological revolution with the advantages of the socialist economic*

[32] See Boyd and Caporale (1992), Reinsh, Lavrovsky and Considine (1992).

[33] The technology transfer involved the importation of entire factories. Thus, during 1965-1972, two giant vehicle manufacturing plants, one for cars (Tolyatti) the other for trucks (Kama River plant), were constructed by purchasing the equipment and general technical services from Italian and American companies. On the cultural history of cars manufacturing in the Soviet Union see *Siegelbaum* (2008).

[34] For Western contemporaneous perspectives on Soviet forecasting of the future see Bell and Mau (1971), Gilison (1975). For the discussion of Soviet views of future and Soviet utopian theorizing during the Khrushchev years, see Vail' and Genis (1989).

system."[35] This wording, however, left ample room for interpretation of what exactly this unity of STR with the socialist system would mean. The job of philosophers and social theorists was to give a concrete meaning to this proclaimed "fusion," and to develop fresh ideas about a possible and desirable synthesis between modern scientific and technological advances (often imported from the capitalist West) and the socialist principles of centralized economics and social planning, without challenging the system too far.

The emphasis of Brezhnev's government on the importation of entire systems of technology from the West made theories of STR the key element in the evaluation of the effects (especially the undesired and unanticipated ones) of the infusion of foreign technology into socially and politically different societies. In this context, Soviet theorists of STR developed a comprehensive discussion of the relations between technology and society, postulating analytical distinctions between the form and the content of modern technologies, between "technique" (gadgets and machines) and "technology" (social and economic relationship embedded in apparently value-free machines), and between direct and indirect effects of technology on society. For example, philosopher Genrikh Volkov contended in 1972 that some technological innovations, like computer technologies, increase labor productivity indirectly through changes in production relations, while other technologies have a direct effect on labor process "relatively independently of socio-economic operations" (Volkov 1972: 7). Alluding to the very real and present day situation of technology transfer from West to East he concluded that such technological systems as "an assembly line would require the same type of highly specialized, mechanical operations, no matter whether it is installed in a Detroit plant or in a plant in Sverdlovsk" (Volkov 1972: 38).

[35] *Materialy* (1971), on p. 57, emphasis in the original.

The postulated distinctions reflected deeper disagreements on the social and political implications of technology transfer from the West. As the authors of 1972 volume *Naučno-tekhničeskaja revoljucija i social'nyj progress* contended, new technical hardware cannot simply be grafted onto existing processes of labor, production and management. Rather, these processes themselves needed to undergo considerable change in order to generate and accommodate new machines. Hence, a technical breakthrough can be effectively utilized only if adjustments are made in the larger social systems of which the new techniques are a part. Thus, for example, the installation of computers would not produce "revolutionary" changes in the forms and organization of production, or decision-making practices, unless they would be accompanied by changes in the organization of the flow and content of technical and social information (Kedrov, Mukulinskij and Frolov 1972). The underlying argument was that scientific and technical advances are not value-free, since they are embedded in the value-laden technologies that enabled them to perform social and political functions.

Ideas about STR served various ends. On the political level, theories of STR were deployed by both the conservative Party leaders to justify and rationalize the preservation of the status quo, as well as those Party officials who sought ways to reduce organizational "irrationality" and to "optimize" economic decision-making. Thus, some Soviet leaders, like Mikhail Suslov, who was especially apprehensive about the undesirable consequences of the infusion of Western technology, were less than enthusiastic about the growing interdependence between capitalist and socialist states. Others, like Alexej Kosygin (as well as Leonid Brezhnev himself), believed that capitalist technical advances could be effectively adapted without compromising socialist values, and seriously attempted to anticipate and to take advantage of opportunities the STR presented, and to fuse the achievements of predominantly Western scientific and technological advances with the social and economic structures of the USSR.[36]

On the theoretical level, the discourse of STR was not merely a post-facto justification of certain economic decisions nor utopian forecasting. Theories of STR offered a general theory of social change, and hence represented an important modernization of Soviet Marxism.[37] In contrast to classic Marxist-Leninist theory of the thirties, which emphasized class conflict as the motor of social change, the basic premise of the STR theory was that the advancement of science and technology had become the principal source of societal transformation. Unlike standard Soviet Marxism, this new outlook didn't take for granted that socialist society was the most advanced simply because of the public ownership of the means of production. Instead, STR promoted a new image of competition between the two world systems based not on class conflict or military victory, but rather on the superior ability to develop, manage and apply advances of science and technology.

The theories of STR thus extended the meaning of Soviet Marxism. As the major evolving part of official Marxist-Leninist theory in post-Stalinist Soviet Union, STR discourse also restored to a certain degree the political function to Soviet philosophy, in the sense that political struggles were not solely about positions and personal power but also about ideas and the meaning of Marxism-Leninism. The theories of STR gave the major incentive and the legitimation for the re-emergence of *naukovedenie* in the Soviet Union.

[36]See discussion in Hoffmann and Laird (1985).
[37]This point was made in Black (1979).

6.3 Local Contexts and Politics of *Naukovedenie*: Bonifatij Kedrov and "Philosophical Turn" of the Institute for the History of Science and Technology

IHST was the major producer of literature on the STR. During the seventies, the Institute produced or supervised a large number of collectively authored book-length accounts of STR, which played the key role in generating philosophical reflection on the STR.[38] The history of this Institute provides a vantage point from which to look at the local politics and contexts of Soviet studies of science. Framed as a direct response to the growing importance ascribed to the STR, the project of "Soviet science studies" – *naukovedenie*, which resurfaced in the Soviet Union in the sixties – was aimed at providing a comprehensive expertise about Scientific-Technological Revolution.

The Institute's history mirrored the life trajectory of *naukovedenie* project. The Institute for the History of Science (IHS), founded by Bukharin in 1927 and disbanded in 1938, was founded anew in 1945. The main incentive for the re-establishment of the Institute was to provide historical depth to the new doctrine of Soviet patriotism, which had emerged during the WWII. As the resolution of the Scientific Council of IHS adopted in 1949 stressed, "nowadays ...at the time

[38] The "collective volumes" on STR published by IHST researchers or under the supervision of IHST included: Stokova et al. (1967), *Naučno-tekhničeskaja revolucija i social'nyj progress* (1972); *Čelovek – Nauka – Tekhnika* (1973); *Naučno-tekhničeskaja revolucija i izmenenie struktury naučnykh kadrov* (1973); *Naučno-tekhničeskaja revolucija i obščestvo* (1973); *Partija i sovremennaja naučno-tekhničeskaja revolucija v SSSR* (1974). Only few names of these volumes' authors, however, appeared on the title pages, usually the supervisors, but sometimes collective monographs were published without any names of the authors listed. In the case of the 1973 volume *Čelovek – Nauka – Tekhnika* the omission of the names concealed the collaboration with Czechoslovak philosophers who were involved in the Czechoslovak reform movement and were displaced (and in some cases disgraced) after the crushing of the Prague Spring.

of the fierce and violent struggle between the two worlds, the history of science has acquired critical significance" (Ilizarov 1993: 27). Indeed, already in 1943, in the wake of the patriotic and nationalistic campaign, various commissions of the studies of Russian science were organized under the auspices of Soviet Academy of Sciences.[39] In 1949 the Scientific Council of IHS adopted the program of the preparation of multivolume (more than 20 volumes) ambitiously entitled "History of Science and Technology in the USSR, From Antiquity until Today," which was approved as the main research project of the Institute (Ilizarov 1993: 27). The mammoth project was never implemented as conceived, but during the fifties numerous works were produced by the researchers of the IHS, asserting the superiority of Soviet science, and supplying the official discourse of the opposition between two "camps" with historical "evidence" - the function that the Institute dutifully performed starting in the late forties and throughout most of the fifties.

Following the death of Stalin in 1953, the Institute was reorganized. The first reorganization, in 1953, merged all the Academy's Commissions on the history of Russian science, along with the Commission on the history of technology formed during and immediately after the war, with IHS, to form the single Institute (IHST), located in Moscow, with the "branch" of the Institute in Leningrad. This reorganization signified the centralization and consolidation of the field of history of science, as IHST became the country's premier center in history of science. [40] During the sixties, the Institute was reorganized again. While the reorganization of 1953 mostly invoked structural changes, only slightly affecting

[39]Commissions established at the end of the war at the Academy of Sciences included the "Commission on the study of scientific legacy of D.I. Mendeleev," the "Commission on the study of scientific legacy of A.M. Butlerov," and the "Commission on scientific legacy of M.V. Lomonosov," among many others that were focused on the study of scientific heritage and publication of the works of prominent Russian scientists.

[40]During this reorganization the first director of the institute after WWII, physiologist and the corresponding member of the Academy of Sciences, Kh. S. Koštojanc, was replaced by the vice-minister of education, A.M. Samarin.

researchers' individual agendas, the reorganization of the sixties was primarily aimed at framing the new agenda for the Institute that would respond to the new political situation. This new phase in the history of IHST was marked by the appointment, in 1962, of the new director of IHST, philosopher Bonifatij Kedrov.

Bonifatij Mikhajlovič Kedrov (1903-1985) was a controversial figure in the history of Soviet philosophy. [41] A philosopher and an "old Bolshevik," with background in chemistry, Kedrov had been involved in major political debates over science since the twenties. His early career was in many ways similar to that of Boris Hessen. [42] Like Hessen, Kedrov represented the type of a 'sophisticated Marxist', knowledgeable both in Marxism and in science, and with strong ties to the scientific community. Kedrov saw the studies of science as a philosophical project, and as a program of de-Stalinization of Soviet philosophy.

Son of a distinguished Old Bolshevik and professional revolutionary Mikhail Kedrov, Bonifatij Kedrov was 14 when his family returned to Petrograd from Switzerland soon after the October revolution (Kedrov 1986). In 1918, Kedrov began his studies at the newly opened Communist University (so-called "Sverdlov University") - the first center of Marxist education for the future generation of Party elite. He interrupted his education volunteering to the Red Army during the civil war, after which he decided to turn to natural science. In 1922 Kedrov entered Chemistry Department of Moscow University from which he graduated in 1930. After his graduation Kedrov became a deputy director of the "Chemical Institute" at the Moscow University[43] - the position similar to that of Boris Hessen, who was a deputy director of the "Physical Institute" of Moscow University (Graham 1985). During the thirties Kedrov gradually gave up chemical lab work and focused instead

[41] See useful sketch on Kedrov and his career path in Hahn (1982). For the analysis of Kedrov's philosophical views see Blakeley (1966).
[42] On Hessen's background see Graham (1985).
[43] Kedrov's autobiography [1949], reproduced in Lektorskij (2005), on p. 699-701.

on theoretical and historical research in chemistry. Simultaneously, in 1931-32 he was studying at the Institute of Red Professors of the Communist Academy. In 1935 Kedrov earned his Ph.D. degree in chemistry for the dissertation on the history and his original solution of the Gibbs Paradox, which he defended at the Institute of General and Inorganic Chemistry. The same year he received the position of instructor at the Science Section of the Communist Party's Central Committee (Lektorskij 2005).

By the mid-thirties Kedrov was firmly established in the burgeoning field of meta-studies of science, both within the Communist Academy and at Moscow University. Along with Hessen, Kedrov taught at the Institute of Red Professors and Moscow University: in both institutions Hessen lectured on the history of physics while Kedrov covered history of chemistry.[44] Kedrov's major works in the history of science were focused on the history of atomic theory in chemistry. Kedrov saw his project as bridging history, philosophy and theory of chemistry, holding that through the reconstruction of the historical development of the categories of [chemical] composition, structure and function one would progress eventually from history of chemistry to theory of chemistry.[45]

The terror of 1937-1938 impacted Kedrov's immediate milieu. The majority of the first graduates and professors teaching at the Institute of Red Professors

[44]In 1938 the program of lectures on the history of physics of the Institute of Red Professors in Philosophy, taught by Hessen, included following topics: (1) the law of conservation and transformation of energy; (2) the problem of determinism in classical physics; (3) second law of thermodynamics; (4) the problem of matter in classical physics; (5) time and space in contemporary physics. The readings included texts by Engels, Lenin, Einstein, and Rosenberger. Kedrov's program on the history of chemistry highlighted the following topics: (1) The metaphysical period in chemistry; (2) Atomic theory in the first half of nineteenth century; (3) Atomic theory in mid-nineteenth century; (4) Periodic law; (5) The development of physical chemistry and the crisis of chemistry in the twentieth century. The readings included texts by Engels, Lenin, Boyle, Lavoisier, Mendeleev, Ostwald (Gosudarstvennyj Arkhiv Rossijskoj Federacii (**Thereafter GARF**), fond R-5205: Institut Krasnoj Professury – Filosofija, opis' 1, delo 511)

[45]Kedrov (1940, 1948, 1949, 1956, 1969).

were arrested and disappeared during the purges, including Kedrov's colleague and comrade Boris Hessen.[46] At the same time, those who survived – mostly younger graduates of the IRP - constituted in later years the elite of the party-state nomenklatura and headed many of the Central Committee's Departments.[47] As Sheila Fitzpatrick has observed, the massive and rapid social promotion of proletarian cadres begun during the Great Break (the shift from the NEP to the Stalin era), followed by the Great Terror, gave rise to a single "cohort" of party leaders who came to power as the "Brezhnev generation."[48]

Kedrov's own career well illustrates this rapid social promotion during the time of terror. Despite the fact that his father and younger brother were executed as "the enemies of the people" during the Stalin's purges,[49] Kedrov was appointed in 1939 as a senior researcher at the Institute of Philosophy, the Party's elite institution, which was formed on the basis of the Institute of Philosophy of the Communist Academy after the Academy's dissolution in 1936.[50] He left the

[46]On the former graduates of the Institute of Red Professors see Berendt (2002).

[47]Former "ikapists" (graduate of IRP), such as Suslov, Pospelov, Il'ičev, Pelše, made astonishingly rapid and successful careers, since the very terror made many vacancies available and facilitated the rapid promotion of those who escaped the purges. The surviving graduates of the IRP became the politicians who shaped the politics of the Soviet Union after Stalin. From 1927 to 1989 at least one former graduate of the IRP was in the Central Committee. The Party's main organ supervising science policy, Agitprop (Committee for Agitation and Propaganda), was headed by the former graduates of the Institute of Red Professors throughout its history: A. I. Steckij (1930-1938), M. A. Suslov (1947), D. T. Shepilov (1948-1952), F.V. Konstantinov (1955-1958), and L. F. Iličev (1958-1965). Many of the foreign graduates of IRP (which was truly an international institution admitting students from all over the world) later played an important role in shaping the political agenda of East Germany, Bulgaria, Hungary, and Poland (Berendt 2002).

[48]Fitzpatrick (1974).

[49]Kedrov's father, Mikhail Kedrov, was one of the founders of CheKa (later NKVD), who, along with his youngest son Igor was arrested and executed following their alleged attempt to reveal the compromising facts about Beria (Hahn 1982). The fate and terrible torture of Mikhail Kedrov during the purges figured prominently in Khrushchev's "secret speech" of 1956. As Loren Graham recounts, the fate of his father was something that Bonifatij Kedrov never forgot (personal communication).

[50]The Communist Academy's Institute of Philosophy was organized in 1923 initially as a section headed by G.G. Špet. In the twenties and thirties the members of the Institute included A.M. Deborin, Bukharin, Ja, E. Sten, L.A. Akselrod. In the thirties a Hungarian Marxist philosopher and one of the founders of Western Marxism, Georgy Lukacs was affiliated with the Communist

Institute of Philosophy in 1941 to join the Red Army, and upon his demobilization in 1945 he received the prestigious position of deputy Director of the Institute of Philosophy.

Kedrov survived Stalinist purges, but he figured prominently in what was called the "anti-cosmopolitan" campaign in the late forties, coinciding with the beginning of the Cold War and which targeted scientists who maintained ties with foreign colleagues and promoted the ideals of international, universal and "world science." Kedrov was in the center of this patriotic "anti-cosmopolitan" campaign of the late forties. He endorsed the notion of "world science" and even made it the title of his new book, *World Science and Mendeleev: On the History of the International Collaboration of Physicists and Chemists of Russia (USSR), Great Britain and USA*, scheduled for publication in 1949.[51] In the wake of the anti-cosmopolitan campaign the publication of Kedrov's book with such an unfortunate title was cancelled and Kedrov became the target of violent attacks. He ritualistically admitted all his "ideological errors," but was forced to resign from the position of deputy director of the Institute of Philosophy and was dismissed from his position of editor-in-chief of the journal *Voprosy Filosofii* he had helped to create. [52]

Kedrov's defeat didn't lead to his complete removal from the scene, though. He remained on the editorial board of *Voprosy Filosofii*, and retained his professorship at the Department of dialectical and historical materialism within

Academy's Institute of Philosophy during his time in the Soviet Union, working on his doctoral dissertation on young Hegel, which he defended at the Institute (Gusejnov and Lektorskij 2009).

[51]Kedrov's book, *Mirovaja nauka i Mendeleev: k istorii sotrudničestva fizikov i khimikov Rossii (SSSR), Velikobritanii I SŠA,* was not published until 1983

[52]"Stenogramma zasedanja 23 i 28 fevralja 1949 g.," Papers of the Institute of Philosophy, Archives of the Russian Academy of Sciences (ARAN) (**Thereafter IPP**), fond 1922, opis' 1, delo 286.

the newly created Academy of the Social Sciences.[53] Moreover, Kedrov actively participated in major ideological campaigns of the time.[54] In 1949, at the meeting of the Academy of Sciences convened to frame new directions in the history of science, Kedrov delivered a speech arguing that Marxist history of science should be written in the spirit of "Soviet patriotism." Only such history, Kedrov explained, can "claim to be a truly scientific and at the same time truly partisan approach to the explication of the events of the past. Only such [patriotic] history can help our Party and our people in their struggle with hostile forces that try to humiliate, blacken and defame our Soviet people and its great past, and only such history can help Soviet people win new victories on their path to communism."[55]

After Stalin's death Kedrov gradually regained his former stature. He rejoined the Institute of Philosophy in 1958 as head of the prestigious department of dialectical materialism. In 1960 he was elected to the Academy of Sciences. In 1962 the Presidium of the Academy of Sciences nominated Kedrov the director of the newly reorganized IHST and approved Kedrov's program for that Institute.

Kedrov's appointment marked the major change in the profile of IHST. His program included a strong philosophical component and established strong connections between two Moscow Institutes, IHST and the Institute of Philosophy. One of the newly formed departments of the Institute, with the somewhat cryptic

[53]Kedrov also found a refuge at the *Great Soviet Encyclopedia* where he held a modest position of a researcher, simultaneously being affiliated with the Academy's "Commission for the study of Mendeleev's scientific legacy." Work in this Commission led to Kedrov's perhaps most original work in the history of science – a meticulous hour-to-hour reconstruction of Mendeleev's discovery of the periodic law (Kedrov called his method "the *microanatomy* of scientific discovery"), in which he pointed out to the role of textbooks in the construction of the periodic system – a line of reasoning quite resonant with the present day science studies approaches: see Kedrov (1958, 1970), discussed in Gordin (2002).

[54]Most infamously, Kedrov wrote several pro-Lysenko articles and contributed to the so-called "anti-resonance campaign" in chemistry in 1951. On the controversy over resonance theory see Graham (1987), on pp. 294-319. On Kedrov's infamous involvement in anti-resonance campaign see Sonin (1991).

[55]*Voprosy Istorii Otečestvennoj Nauki* (1949), on p. 662.

name "sector of the general problems of the history of science," which was renamed in the late sixties as the "sector of the logic of the development of science" and then transformed into the huge "department of *naukovedenie*," was staffed by philosophers from the Institute of Philosophy, who were expected to provide the direction and theoretical framework to historians of specialized branches of science.[56]

The "philosophical turn" of the Institute troubled many IHST researchers. In 1965, a historian of chemistry and a researcher at IHST Georgij V. Bykov wrote in a letter he sent to the directorate of the Institute:

> Who are we now? According to the formal stratification of specialties we are historians. By our training, past work, and our diplomas most of us are natural scientists. However, recently our Institute became affiliated with the Division of Philosophical Sciences [of the Academy of Sciences] and hence now we are philosophers. What does this mean? ... From our Director we have heard only confusing answers to this question...[57]

The philosophical turn of the historians' Institute served several goals. One concerned the new role for the history of science, as it was redefined in the sixties. During the 'anti-cosmopolitan' campaigns of the 40s and fifties, the Institute dutifully produced or supervised numerous works that claimed the priority of Russian science in almost any significant scientific discovery and declared the superiority of Soviet science. At the same time, as a result of these militant nationalistic campaigns many historians retreated to descriptive and antiquarian

[56]During his time at the Institute (from 1962 to 1973 as a director, and from 1974 until his death in 1985 as the head of the "section of the logic of science") Kedrov cultivated close connections between the Institute for the History of Science and Technology and the Institute of Philosophy. From the sixties through to the eighties the Institute served as an alternative institutional affiliation for leading Muscovite philosophers from the Institute of Philosophy.

[57]G. V. Bykov, "Ob osnovnom napravlenii v dejatel'nosti našego instituta," 22 janvarja 1965, Archive of the Institute for the History of Science and Technology (**Thereafter IHSTP**). Before joining IHST Bykov was the Scientific Secretary of the Academy of Sciences' Commission on the scientific heritage of A.M. Butlerov, and became the researcher at IHST after the Commission was merged with IHST in 1953 (Ilizarov 1993: 48)

modes of scholarship, or to exotic fields like Arabic mathematics, or ancient science, as a way to continue their careers without contributing to what they considered false scholarship. [58] In the sixties, neither of these strategies suited the new political goals. The rhetoric on STR, placed the emphasis on the analysis of recent science, and encouraged the study of major Western industrial nations and their scientific and technologic achievements, rationalizing in this way the new emphasis of Soviet economics on the transfer of Western technology and know-how.

Kedrov's 1962 program emphasized that the Institute's structure and research focus had to be changed in order "to correspond to the present day situation" and to ensure the production of up-to date "synthesizing and analytical work on the development of recent science and present-day science."[59] The new departments - "the department of the scientific-technological revolution" and the department "of general problems of the history of science" – were created to ensure the implementation of these changes. [60]

Although the intellectual production of the Institute was by no means confined to the accounts of STR, the discourse of the STR legitimized the re-emergence of the field of *naukovedenie*, now conceived as the investigation of recent science, with the emphasis placed onWestern scientific and technological developments. At the same time, Western works on scientific and technological developments were read by Soviet scholars against the background of their own local cultural wars. The reading and "staging" of the works by Thomas Kuhn and Karl Popper in the Soviet Union is especially illustrative of the ways in which

[58]The retreat to descriptive and factological style, or to "textology" (the publication of original texts with only minimal commentary and with no interpretation) was widely employed by historians and literary critics (*literaturovedy*) in general (see discussion of the strategies of Puškin scholars during Puškin Centennials in the late thirties when Puškin was redefined and mythologized as a Soviet hero: Petrone (2000), on pp. 113-149).

[59]Priloženie "O napravlenii naučnykh issledovanij i strukture Instituta istorii estestvoznanija i tekhniki AN SSSR" k postanovleniju Prezidiuma AN SSSR ot 12 oktjabrja 1962, IHSTP).

[60]ibid.

the local contexts and politics of *naukovedenie* set the stage for the "geography of reading" of these two towering figures in Western science studies, as Kuhn and Popper came to signify the radically different images of science, almost opposite in their epistemological premises and practical as well as political, implications.

6.4 Geography of Reading: Staging Kuhn *versus* Popper in the Soviet Union

Kuhn's seminal book, *The Structure of Scientific Revolutions* (1962), is usually highlighted in historical accounts of science studies as having challenged the received view of science and triggering the professionalization of science studies as an academic field in the following decades, particularly in the U.S.[61] In the Soviet Union Kuhn's *Structure* was discussed and sympathetically reviewed by IHST researchers almost immediately following its publication.[62] Despite that Soviet reaction to Kuhn's book gained momentum only a decade later, in the mid-seventies. The Russian translation of Kuhn's *Structure* by IHST researchers Markova and Mikulinskij appeared in 1975, and this publication stimulated the response and vivid discussion of Kuhn's model of science.[63]

In many respects Soviet discussion and criticism of Kuhn's work resembled

[61]On the reception and the responses to Kuhn's *Structure* in the United States see Zammito (2004).

[62]In 1963 American historian of science from Cornell University, Henry Guerlac, visited IHST and gave a lecture on "The development of the history of science in the USA" ("Otčet o rabote Leningradskogo otdelenija Instituta istorii estestvoznanija i tekhniki AN SSSR v 1963 g." IHSTP, Protokoly zasedanij direkcii). The first Soviet review of Kuhn's *Structure* was published in 1965 by a researcher of IHST L. Markova and offered a sympathetic summary of Kuhn's book, stressing its significance as a turning point for the history of science (Markova, 1965).

[63]The postscript to Russian translation of Kuhn's *Structure* by the researchers of IHST highlighted the significance of Kuhn's book pointing to the "antipositivistic bent" of the Kuhnian account and emphasizing its proximity to Marxist thought through its dialectical interpretation of the revolutions in science (Mikulinskij and Markova 1977). See summary of many positions presented by Russian readers of Kuhn's *Structure* in Josephson (1985).

Western criticisms. Soviet philosophers and scientists were troubled, just as their Western counterparts were, by the "incommensurability" thesis and argued that scientific change is a rational and logical choice, not a somewhat mysterious and irrational "gestalt switch." [64] Another common facet of Soviet critics was Kuhn's failure to discuss the sources of the new knowledge. From the perspective of Soviet critics, Kuhn's concept plausibly accounted for the survival of paradigms but didn't provide a sufficient explanation for the arrival of new paradigms.

Many Soviet reviewers pointed out that Kuhn's model of scientific change through periodic radical transformations was not new. They argued that Marx, Engels and Lenin had recognized the phenomenon of revolutions in science, and the view that the development of science occurs through leaps can be found in Engel's law of the transformation of quantitative changes into qualitative changes. As Kedrov put it bluntly, "Kuhn put forth a view, long established in Marxism, that the progress in science occurs through periodic shifts from the evolutionary to the revolutionary periods of [scientific] development, through the constant transition from one step to the next one, in the infinite progressive path to the absolute truth" (Kedrov 1969). At the same time, Soviet critics stressed that the apparent proximity of Kuhn's concept of 'paradigm shifts' to the laws of dialectics was deceptive, since Kuhn's concept was largely concerned with how scientists come to *agreement* on what is true, rather than with how science produces objective

[64] As John Zammito has noticed, Kuhn primarily sought to reach philosophers of science as his major audience. Their reaction (at least in the US and Britain) was rather hostile and largely dismissive. As Zammito put it, "The philosophical community Kuhn sought to join continually rejected his ideas. By contrast, the discipline he invoked somewhat cavalierly to illustrate his views, sociology, took up his ideas in their most drastic formulations and launched a research program in his name, a 'Kuhnian' sociology of scientific knowledge" (Zammito 2004: 123). Contrary to the Anglo-American response to Kuhn's work, the philosophical reception of Kuhn in the Soviet Union was far from being hostile. One of the reasons was that while in the United States Kuhn emerged in the context of a philosophy of science with a strong logical empiricist tradition, this tradition was largely absent in the Soviet Union and the countries of Soviet bloc (with the exception of Poland) until the sixties.

knowledge about the reality. [65]

The most prominent criticism, however, concerned Kuhn's focus on the universal features (structures) of scientific revolutions. As Kedrov argued, Kuhn's analysis, seeking for universal patterns, does not account for the unique features of individual revolutions. As Kedrov argued, "Each scientific revolution must be studied separately as a unique and non-repetitive phenomenon" (Kedrov 1976). By late seventies, directly responding to Kuhn, Kedrov developed an entire "typology" of scientific revolutions in his several books, one of which was characteristically entitled *Scientific Revolution: Substance. Typology. Structure. Mechanism. Criteria* (1980).[66] As Kedrov concluded, "Although we could say, following Kuhn, that in each case there was a radical breaking of the interconnected system of concepts and views (what Kuhn calls "paradigms"), such a generalized (and hence abstract) approach can hardly be fruitful for the study of the revolutionary development of science" (Kedrov 1976: 71).

Overall, Kuhn's work was received in the Soviet Union with sympathetic interest but without any particular enthusiasm. One of the reasons that made Kuhn's work to some degree irrelevant for the Soviet *naukovedy* was the competing discourse of scientific revolutions – the theories of STR. The STR theorists, many

[65] As one of Kuhn's critics put it, "It is not difficult to find certain points where Kuhn's concept comes into a contact with dialectical materialist theory of knowledge. These points of apparent proximity between the two include the implied interconnection and interdependence of theoretical and experimental practices in science, the protest against the absolutisation of logical methods of studies of science, the assertion of the social conditioning of scientific research, etc. However, it would be a mistake to talk about any proximity of Kuhn's views to the basic tenets of the Marxist theory of knowledge. One of these major tenets lies in the answer to question, what is the relation between science and [objective] truth. This question is out of the scope of Kuhn's analysis, as the notion of "truth" does not play any role in his concept" (Legostaev 1972: 136).

[66] Kedrov had characterized four "types" of revolutions: the first type was the Copernican revolution characterized by Kuhn. Then there was the "Kantian Revolution" that forged the ideas of evolution. In the late nineteenth early twentieth centuries, the "New Revolution in the natural sciences" consolidated representations of nature based on mathematical abstractions and probability. Finally, there was the Scientific-Technological Revolution, a new phenomenon, which, as Kedrov argued, could not be understood by reducing causes, effects and outcomes to the previous revolutions in science (Kedrov 1980).

of whom were affiliated with IHST, were focused almost exclusively on the period since WWII. History for them was happening here and now, in the post-atomic age, and promised the revolutionary transformation of the world, qualitatively different from the effect of the previous revolutions in science. Politically primitive and counter-revolutionary, Kuhn, with his focus on normal science as a stabilizing social practice, was largely irrelevant for the theorists of the STR.

In his book *Kuhn versus Popper* Steve Fuller reflected on the history of the famous debate between Kuhn and Popper organized by Imre Lakatos in London in 1965. As Fuller argues, this event played an important role in the creation of Popper's reputation as a conservative and authoritarian proponent of an out-dated positivism, at the same time helping frame Kuhn as a "scientific radical," an intellectual rebel and critic of the scientific establishment of his time, and a precursor of the contemporary post-modern social studies of science. The truth, however, as Fuller provocatively tells us, could be read as the exact reverse, at least with regard to the issue of science, democracy, and radicalism. Contrary to their respective reputations, Fuller argues, "Popper was a democrat concerned with science as a form of dynamic inquiry and Kuhn an elitist focused on science as a stabilizing social practice" (Fuller 2005: 68).

The Soviet reading and staging of Kuhn *versus* Popper created reputations of Popper and Kuhn in the Soviet Union which were almost the reverse of those that they, somewhat unwillingly, acquired in the Anglophone West. For Kedrov, as well as for philosophers from the Institute of Philosophy who moved to staff Kedrov's Institute, philosophy of science had a special significance; it was seen as a vehicle for de-Stalinization (in some cases even de-Marxification) of Soviet philosophy in the wake of the legitimation crisis of Soviet philosophy in the fifties. It is against this background that formal logic and its applications to the analysis of the language of science became an especially attractive field among philosophers who tried to avoid issues of Marxist dialectical materialism altogether. During the sixties many philosophers retreated to philosophical logic and, along with mathematicians, were actively extending formal methods to various "special sciences."[67]

At IHST the logical studies of science became one of the prominent areas. [68] In this logicians' milieu, the works of Karl Popper were wildly popular. In the sixties IHST became a formidable center of "Popperian studies" in the Soviet Union. Popper's works in philosophy of science were closely read, reviewed and even made into the subject of doctoral dissertations.[69] The prominence of "Popperian studies" in Soviet philosophy of science made it possible to selectively introduce Popper's social philosophy to the general Soviet reader. Popper's *Open society and Its Enemies* (1945) and his *Poverty of Historicism (*1944-45), which presented a sharp critique of Marxism, were not translated into Russian until after *perestroika*. In the seventies the Soviet general public could learn about Popper's social and political philosophy only from the publications of British Marxist philosopher Maurice Cornforth, whose defense of Marxism against Popper's criticisms was widely publicized and made Popper the official "anticommunist theoretician number 1" in the Soviet Union (Sadovskij 2002: 181-189). The occasional expositions of Popper's *Open Society* by philosophers of science were also

[67]The integration of logic with general philosophy through the analysis of language, in the manner of Western analytical philosophy, was not developed in the USSR until the sixties. The logical positivist perspective was officially rejected in Soviet philosophy since the thirties an approach deemed idealistic and not compatible with the materialist focus of Marxism, as being focused on the analysis of language rather than on material entities.

[68]Logical studies of science were on the agenda of the "sector" (department) of the "general problems of history of science," which was renamed in the late sixties to become the "sector" of the "logic of the development of science" (headed from 1972 by Boris Grjaznov and from 1974 by Kedrov) (IHSTP, Otčety o rabote sektorov, individual'nye plany raboty, 1965-1973).

[69]Thus, in the late sixties a graduate student at IHST T.N. Khabarova defended a dissertation on Popper's philosophy (summarized in Khabarova (1968)). The philosopher Boris Grjaznov, a researcher and for a short period head of the IHST sector of the "general problems of history of science," published prolifically on Popper throughout his career (see for example, Gorskij and Grjaznov (1975), Grjaznov (1976), Grjaznov (1978), Grjaznov (1982)).

accompanied by standard critiques (Mongili 1998a: 198). Yet, Popper's writings in philosophy of science had contributed to the very possibility of these expositions. [70]

Thus, somewhat ironically, the Soviet staging of Kuhn and Popper produced reputations very close to the de-mythologized picture that Fuller has presented. The efforts of Soviet philosophers to rehabilitate the "anti-communist number 1" made Popper's *Logic of Scientific Discovery* and his other writings on science sound much more unorthodox, radical and appealing in the Soviet Union than they appeared to his American audience in the seventies and eighties.

6.5 Conclusion to Chapter Six

As I argue in this chapter, the Soviet science studies project was articulated in response to the local economic and political situation and the needs of the Soviet state during the Cold War. Science studies (*naukovedenie*) as a specialized discipline were promoted and used as a vehicle for changing the role of philosophers in the post-Stalinist Soviet Union. Scientists' harsh attacks on "philosophy" targeted the Stalinist order of things rather than philosophy or philosophical theory per se. A popular Soviet joke from the sixties attributed to the physicist Lev Landau captured this perfectly, stating: "There are only three types of science: natural science, unnatural science (i.e. social sciences), and anti-natural science (i.e. philosophy)." Philosophers-turned-*naukovedy* did not succeed in challenging the hierarchy that privileged natural sciences over social sciences and the humanities in the Soviet Union, but they did succeed in reestablishing

[70] The first Russian translation of Popper's works on scientific method appeared in 1978 thanks to Grjaznov of IHST and V.N. Sadovskij of the Institute of Philosophy. The translation was the compilation of Popper's articles published in Boston Studies in the Philosophy of Science in 1963-1971 (Grjaznov and Sadovskij (1978)). Popper's *Logic of Scientific Discovery* appeared in Russian translation in 1983 (translated by Sadovskij). See the history of publication of Popper's works in Russian in Sadovskij (2002), on pp. 186-189

connections with the scientific community and the state.

A creature of late socialism, *naukovedenie* was neither a form of resistance nor conformism with the state. Such a binary representation was itself the creature of the Cold War mentality that many recent works of historians of the Soviet Union have convincingly questioned.[71] The Cold War-inspired accounts that made Soviet experience appear incommensurable with the world of Western democracies, are now giving way to more nuanced and complex readings showing various forms of ideological accommodation as well as pragmatic and institutional cooperation between different groups of scientists and politicians (a picture not that different from "Western science"). The story of "Soviet science studies" project demonstrates that Soviet *naukovedy* responded to the same anxieties and concerns of the Cold War as their Western counterparts, while adapting and transforming them in highly specific and often peculiar ways.

This chapter, in full, is a reprint of the material as it appears in *Studies in East European Thought* 2011. Elena Aronova. 2011. The Politics and Contexts of Soviet Science Studies (*Naukovedenie*): Soviet Philosophy of Science at the Crossroads, *Studies in East European Thought* 63/3: 175-202. The dissertation author was the only investigator and author of this paper.

[71]See discussion in Yurchak (2006). On the problem of resistance in Stalinist Russia see the special issue of *Kritika* (David-Fox, Holquist and Poe 2003). See also Adams (2001), Kotkin (1998).

Chapter 7

Conclusion

The historical episodes examined in this dissertation suggest that the promotion of the studies of science as a politically relevant area of expertise, undertaken within existing powerful institutional structures outside academia, helped to legitimize the disciplinary identity of science studies in the age of the Cold War. When it comes to the discussion of the Cold War contexts and politics of the history of science in this period, historians usually highlight the turn to internalist history (or intellectual history) within academia, coupled with concern for disengagement from politics and ideology, along with the general depoliticization of the social sciences as one of the symptoms of the Cold War university (Engerman 2003).[1] As I show in this dissertation, however, outside academia the picture was different. Within the organizations that combined professional and political

[1] As Anna Mayer, for example, showed in her work on the history of history of science profession in Cambridge, anti-Marxism became the defining feature of the professional identity that formed the intellectual agenda of the Department of History of Philosophy of Science in Cambridge in the early Cold War years, which promoted the image of scientific work as a disinterested journey of the mind, and institutionalized this image through its appointments policy in this period (see Mayer 2000). Many other historical accounts of the development of history of science in the Cold War has demonstrated, likewise, that the politics of representing science as an impartial neutral and apolitical affair was part of the cultural narrative of the Cold War, coupled with historians of science concern for constructing the disciplinary identity for themselves (see, for example, Enebakk 2009, Porter, 1990).

commitment, such as the CCF and UNESCO, there was no turn to internalist history. To the contrary, these transnational and overtly political organizations promoted the studies of political and social-economic dimensions of science, stimulating the studies of the issues on the science-society-politics nexus and in various ways promulgated the view that science is inseparable from politics.

The newly established institutions struggling to establish their reputations and to distinguish themselves from the traditional institutional settings likewise promoted studies of science emphasizing the social, political and ethical dimensions of science. During the first decade of its existence, the Salk Institute for Biological Studies developed a wide range of programs, many of them pioneering, in order to examine broader social and political implications of molecular revolution in biology. Likewise, the Hastings Center, during its short-lived "philosophical phase," promoted an active dialogue between scientists and physicians, on the one hand, and philosophers of science, on the other, to chart a common ground between the emerging field of medical ethics and philosophy of science in the wake of Kuhn, and exploring, in different ways, the "normative" and ethical dimension of science.

All these settings, I argue, construed a semi-institutional niche for Science Studies before the discipline became institutionalized in academia during the 1970s and 1980s. This loosely connected network of intellectuals helped to construct a public space in which the relations between science and politics were debated and discussed. In the process of these discussions, they helped to invent a new subject, or set of subjects, in the 1950s and 1960s, reconceptualizing science as a social and political activity, promulgating the view that science is inseparable from politics, and in various ways exploring the science-society nexus.

The vision of "science studies" these scientists and scholars were advocating was different from the Science Studies (or Science and Technology Studies, STS) as we have it today. Yet, these alternative visions, in which the issues of science politics appeared inseparable from those of science policy, organization of science, science governance, ethics of science, and the planning of science, constituted the "pre-history" of science studies as a discipline, and a base for establishing an early, semi- institutional, niche for it.

With the development of Science Studies as an academic discipline the issues of science policy became marginalized, with Science Studies primarily focused on knowledge production – the central concern of what Fuller called the "High Church" in science studies, since the 1970s. However, before Science Studies became a predominantly intellectual project, the political developments and political concerns had a central role to play in legitimizing studies of science as a distinct and politically relevant area of expertise, in the age of Cold War. In this way, I argue, rather than being a moment of rupture, Science Studies grew out of these early projects and intellectual programs driven by political developments and political concerns.

7.1 Big Science and "Big Science Studies"

One of the overarching themes running through the case-studies discussed in different chapters concerns the rise of Big Science and its role in legitimizing studies of science as a distinct area of expertise within the social and human sciences. As I show in this dissertation, in the 1960s, on both sides of the Iron Curtain, social theorists problematized the phenomenon of Big Science, articulating the awareness that the large-scale growth of science after WWII had significant implications, for better or for worse, for modern societies. In the United States, the discussion of political and social consequences of Big Science was largely framed by the network of intellectuals associated with the Congress for Cultural Freedom. In the post-Stalinist Soviet Union, a counterpart of the American discussion of Big Science was epitomized in the concept of the Scientific-Technological Revolution, which became the center of theoretically significant discussions of the conditions and consequences of scientific-technical, social, and economic change in different political systems.

Big Science, as a cultural phenomenon and a particular mode of organization of science, was deployed as a resource to debate, negotiate, and rationalize the concerns and anxieties of the Cold War. Throughout the Cold War, both the United States and the Soviet Union advocated their ability to offer and display different visions of modern industrial society, and Big Science, with its paradigmatic Manhattan Project, played a major role in these powerful Cold War imageries. In these different political settings, on the opposite poles of the Cold War geopolitical divide, social theorists conceptualized the phenomenon and consequences of Big Science, relating their country's preferred model of society to a certain image of science and its organization, and at the same time creating certain common tropes in their respective conceptual systems built around modern technology. In their quest for a middle ground and compromise, they suggested various reconciliations of two "ideal types" - the *free market* (a cherished ideal of capitalist system) and *centralized planning* (during the Cold War firmly associated with Soviet economic system).

Reflection by natural scientists and social analysts on the social and political consequences of Big Science in its relation to state and politics, and the articulation of the need for independent expertise on science as a social institution and "political instrumentality," was an important context for the nascent field of "science studies" both in the United States and in the Soviet Union. On both sides of the Iron Curtain, Big Science had been seen by social and political analysts as a cultural and political phenomenon: not merely as a mode of organization of scientific research, but as a complex phenomenon requiring assessment by social analysts. In both political settings, as I argue, the discussion of the social and political consequences of Big Science provided legitimation for the disciplinary identity of science studies as a distinct - and politically relevant - area of expertise. The story of "Soviet science studies" demonstrates that Soviet *naukovedy* responded to the same anxieties and concerns of the Cold War as their Western counterparts, while adapting and transforming them in highly specific and often peculiar ways.

As the term "Big Science" gained currency after the end of the Cold War, it gradually lost its original, political, connotations. "Big-ness" became the chief characteristic of Big Science, an association which suggested that Big Science should not be necessarily identified as a contemporary phenomenon.[2] This dissertation, by exploring the context in which the concept of "Big Science" was framed originally - in Cold War America as well as in the post-Stalinist Soviet Union, with its counterpart concept, the theory of Scientific Technological Revolution – suggests that the extension of the notion of "Big Science" to periods beyond its original contexts may be problematic. Although specific 'characteristics' of Big Science as a mode of organization of science can be discerned since the time of the Scientific Revolution, such extension draws attention away from the explicit Cold War connotations of this term - and in certain ways from the "political instrumentality" that, as Stephen Toulmin warned, Big Science embedded, as it was informed and shaped by the specific concerns and general climate of the Cold

[2]Thus, for instance, astronomy of the 16th century, with its large telescopes and observatories, is often referred to as the earliest form of Big Science (see Vermeulen and Penders 2007). Another example, widely considered as a "big science of the nineteenth century" is oceanography (Deacon 1997, Mills 1989).

War.

As the question of the governance of Big Science is now becoming an issue of discussion among contemporary scholars[3] and science policy issues are becoming a 'legitimate' part of current science studies, its "pre-history" can provide important insights, both into the Cold War "roots" of science studies and the concerns articulated during the early discussions of political and social consequences of Big Science, as they resurface today when Big Science is becoming a widespread mode of research in academia world-wide.

7.2 Challenges to a Universalist Ideal of Science

Most of the early promoters of "science studies" had a deep reverence for science, sharing a conviction in the universality of scientific knowledge. Yet, in their attempt to find a middle ground between the traditional frameworks of the unity of knowledge (epitomized in the scientific humanist agenda of historians of science like Joseph Needham or the "humane studies" of Jacob Bronowski at the Salk Institute) and the post-war trends towards post-positivist epistemology, the very ideal of the universality of science had been profoundly challenged. The UNESCO *History of Mankind* project, driven by its visionary founders' aspirations to reflect the universal nature of science and civilization, failed to achieve its goal, stimulating reflection, instead, on the methodology of science history writing that would escape the pitfalls of universalistic stories. Nowhere was the commitment to the ideal of universality of knowledge more prominent, among the historical cases under the study, than at the Salk Institute. The Institute's programs in the humanities were explicitly committed to the goal of "bridging the two cultures" of science and the

[3] See, for example, Fuller (2000).

humanities, amalgamating the traditional scientific humanism with the American concerns of the time. At the same time, the Institute developed a broad array of programs some of which were seeking to incorporate the post-positivist framework, within which science appeared as a matter of negotiation and consensus resulting in questionable truth value, pursued by scientists who work in disunified disciplines toward short-lived achievements in a world in which scientific, social, ethical and political components cannot be sorted out or privileged. Within the CCF, the vision of "science studies," while embedding a "unification" ethos, at the same time carried a strong sense of "disunifying" discourse, especially in the 1970s - emphasizing diversity, pluralism, complexity, ambiguity, and discontinuities in the development of science that led to what was identified as Big Science, as a form of organization of "scientific life" with which these "disunifying" features started to be associated.

Similar trends came to be associated with the so-called "postmodern" consciousness. The two notions, "postmodern" and "postindustrial," were often conflated by the theorists of "postmodernity".[4] For example, J.-F. Lyotard, in his *Post-Modern Condition,* referred to both notions as parts of definition of culture and society under the very different social and political circumstances the 1960s opened. Whether or not "post-industrial" society and "postmodern" culture actually emerged in the 1950s and 1960s, the *ideas* of "post-industrial" society and "postmodern" culture surfaced at that time, most prominently within the network of the intellectuals associated with the CCF. And although the validity of this most abused term, "postmodern," as a descriptor of a historical period or

[4]As literary critic Frederic Jameson noted, postmodernism is the "dominant cultural logic of late capitalism" (Jameson 1991). By "late capitalism" Jameson denoted the phase of capitalism after World War II, when a radical break occurred in the span of historical experience roughly at "the end of the 1950s or the early 1960s," the watershed often described as the rise of "post-industrial society" (Brick 1992).

intellectual trend, remains controversial, Science Studies as an intellectual project is often located within the intellectual framework associated with "postmodernity." The fact that this connection can be traced to the "cultural cold wars" of the intellectuals involved in the CCF provides an additional rationale to suggest a strong continuity between the "science studies" promoted by the intellectuals associated with the CCF and the field we know today.

In his *Interpretation of Cultures* Clifford Geertz said about himself: "my own general ideological (as I would frankly call it) position is largely the same as that of Aron, Shils, Parsons, and so forth, that I am in agreement with their plea for a civil, temperate, unheroic politics" (Geertz 1973, p. 200). In the sense identified by Geertz, most of the promoters of "science studies" discussed in this dissertation largely shared (an ideological) commitment to the "end of ideology," coupled to the commitment to post-industrial society in a world of big science. Although still holding dear the notions of cooperation and unity rather than conflict in global scientific and technological developments, as well as the notion of the advancement of universal scientific knowledge, these latter notions were profoundly challenged within the framework of the "end of ideology," "post-industrial society," and Big Science. The struggle of scholars and scientists, who promoted "science studies" in the 1960s, to come to terms with the inherent tension of the positions between these two sides, greatly contributed, I would argue, to the "watershed" change in the perception of science that came to be associated with the "epistemological revolution of the 1960s."

7.3 The Cold War and its Legacies

The case of the CCF may also illustrate the complexities and ambiguities of the Cold War. Both CCF and UNESCO, given the framework they offered for the history of science and "science studies," sought to overcome the polarities of the Cold War. Being constrained by the Cold War at every turn, both organizations at the same time were re-defining and re-shaping the early Cold War ideological agenda dominated by the Cold War dichotomies and coupled with the Euro-West-North biases. UNESCO, functioning as a tool of nation-states and an instrument of legitimation of both late colonial and post-colonial nation-state building, was still capable at times of maintaining its earlier, transnational agenda. The CCF, with its overt anti-communist agenda and covert connection to the CIA, produced the outcome which was shaped by Cold War anxieties and concerns, but not determined by political demands directly, let alone unequivocally.

As the cases discussed in this dissertation demonstrate, there was no single Cold War "party line." The CCF-associated intellectuals, as well as the Soviet philosophers who promulgated the STR rhetoric, were assigned - or self-assigned - the role of giving meaning to the basic notions of the Cold War in the same way other social scientists were doing. The claims by the CCF intellectuals that they were "independent" and "free" in their thinking would not survive the 1960s, as the revelation of CIA sponsorship of the Congress shattered comfortable assumptions of scholars in the service of the state. Yet their quest for a "middle ground," reconciliation, and compromise, as part and parcel of their conceptions of scholarship and service, effectively shifted the debate away from simplistic Cold War narratives of East-West competition.

As I have attempted to show in this dissertation, the "science studies" project was situated at the very center of the cultural narrative of the Cold War.

In this regard, the history of Science Studies is similar to other social sciences that received ample support during the Cold War – area studies, behavioral science, human relations, development studies, American studies, and a host of other disciplinary and interdisciplinary fields – serving the national interest, but not necessarily in predictable ways.

Chapter 8

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