THE INTERCONNECTEDNESS OF THE SOCIAL AND NATURAL SCIENCES: HUNGARIAN REFUGEE SCIENTISTS IN DEFENSE OF THE AUTONOMY OF SCIENCE

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ABSTRACT

The contribution of foreign born scientists to the building of the first Atomic bomb is well-known. That a number of theoretical physicist involved in the Manhattan Project continued taking an active part in the politicization of nuclear physics of post war America were also refugees is, perhaps, less so. This essay considers the public engagement of Hungarian born scientists in a broader historical context that has shaped their professional trajectories. Discussing themes as family backgrounds, (forced?) migration, totalitarian systems, personal ambitions, technical brilliance, socio-economic relations of science and government as well as nuclear defense politics, I point out that my protagonists have become agencies in and of the transformation that science politics had been undergoing at the time. The shared experience as Atomic scientists of Eugene Wigner and Leo Szilard is the departure point from which an analysis of personal and socio-political factors is used to interpret any discrepancies in the rationale behind, and the nature of, their public engagement. A chapter is devoted to another Hungarian refugee scholar-scientist at the time living in Manchester, England, who, not unlike his two colleagues across the Atlantic, was also championing the autonomy of science but in a different context, by different means, and to different ends. Michael Polanyi's case is meant to be invoked to provide an outlook of how his, then nascent, philosophy of science, applied to pushing back against Marxist-materialist science in England, overlaps with Szilard's efforts to defend the autonomy of science in the American context and whether it has any relevance to Wigner's conceptions of the relation between science and politics.

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Introduction

1. Hypothesis

In 1934, experimental physicist Pyotr Kapitsa (1894-1984) travelled to the Soviet Union to visit his parents. He had then been working for about ten years at the Cavendish Laboratory in Cambridge under the tutelage of Ernst Rutherford (1871-1937). It was going to be a temporary stay but Stalin's government denied his return to England. The next year (in April) the first Theoretical Physics Conference in Washington DC was organized, with Leo Szilard (1898-1964) and Paul Dirac (1902-1984) in attendance. Although the two theoreticians had hardly known each other, they raised the issue of Kapitsa's house arrest and tried to have their American colleagues boycott the Soviet Union. Since it was only Robert A. Millikan (1968-1953), then President of Cal Tech, steadfast anticommunist, who supported the plan, Szilard went further in his efforts to help get Kapitsa back to Cambridge: he suggested that the Russian Physicist be smuggled out of the country in a submarine.[1] The proposed rescue operation of Kapitsa (it came to nothing) was not the first instance of the Hungarian scientist's political (humanitarian) activism. Three years earlier, in September 1931, the Empire of Japan invaded Manchuria and established a puppet state there. In the year following, on April 24, Szilard was reading in the *Manchester Guardian* that "Japan had rejected all interference, by the League of Nations or by any country, in its invasion of Manchuria.[2] He then called upon some of the most notable natural scientists to boycott Japan and the plan was that "scholars would refuse to send scientific and technical information and journals to Japan or to cooperate with Japanese students.[3] His impulsive reaction to the news

[1] William Lanouette, *Genius in the Shadows: A Biography of Leo Szilard, The Man Behind the Bomb* (Chicago: The University of Chicago Press, 1992), p 151.
[2] Lanouette, 141.
[3] Ibid.

did not mean for it to be without a subsequent scheme: he had calculated that for the proposal to be effective "eight-tenths" of all Nobel Prize winners should sign the petition.[4]

Szilard's partner in urging American physicists in Washington to help Kapitsa, the English born Paul Dirac, was going to become in 1937 Eugene Wigner's (1902-1995) brother in law. Wigner was not in Washington at the time of the Conference but if he had been, he would have regarded Szilard's brazenness with alarm and a great deal of concern. They were friends, colleagues, and their lives had a largely similar progression, a cornerstone of which was their involvement in the creation of the first Atomic bomb. Post war, Wigner 'joined' Szilard and they were among those who went on to become actively engaged technical experts in public affairs. It is their contributions, qua natural scientists, to nuclear politics that will be the broader subject of this essay. Complementing the theme of Hungarian born refugees acting as publicly engaged scientists, I will also discuss some aspects of the (early) philosophy of science of Michael Polanyi (1891-1976) in the framework of his defense of the autonomy of science against the concept of centrally planned science as advocated by British leftist scientists during the 1930s. Combining the extra scientific activities of the two Manhattan Project (MP) scientists with those of Polanyi serves the purpose of tracing both similarities and discrepancies in the means and ends of their efforts to bridge the technical divide between science and politics (Szilard and Wigner) and engage the philosophy behind Polanyi's exaltation of the freedom of science. Central to my argument are the two distinct approaches to the concept of the autonomy of science: In the main, I will analyze Szilard's standing up to the authority of American scientific organizers and Michael Polanyi's defense of pure science against the ideologized championing of applied (or planned) science in Britain. In a comparative analysis focusing primarily on the agendas of two historically

[4] Ibid.

significant characters (of very similar backgrounds) who, with Wigner's more subtle contributions also discussed, I will argue, were advocating the *same* concept in the hope of achieving *different* aims.

2. Context

Establishing a platform for a meaningful investigation of the background of the intellectual curiosity and consequently the achievements of the protagonists to demonstrate the nature of their ambitions will be done in a variety of different geographical and social contexts. As all three were subjected to multiple exiles, a transnational analysis is indispensable involving also some gesturing towards the most important developments in theoretical physics of the period when relevant to the ways in which their professional trajectories were shaping up. I do not claim for the genre of this essay to be that of a history of science proper; it is, admittedly, not unproblematic to determine: I hope to give a fair, balanced and detached account that could serve as a segment in the social histories of three natural scientists. Besides identifying the development of some aspects (institutional, organizational and, to a lesser extent, technical) in the (physical) science history of the early to mid-20th century, discussing the relevance of the natural sciences in contemporary socio-political trends in Hungary, Germany, and the United States to which Szilard, Wigner and Polanyi were all exposed, subjected to, and (when in America) to a certain degree shapers of, will, it is hoped, somewhat aid the understanding the role they played in what was a remarkable transformation in the relation of science and government during the exhilarating years of the atomic age.

3. Literature

The broader subject of this essay comprises a part of 20th century history that has over the years drawn an immense amount of both popular and scholarly interest. Consequently, the scope of literary outputs (again, both popular and academic) is vast. Comprehensive histories of the interwar period, the second world war, and the Cold War period abound and, I would risk to say, almost all imaginable subfields have been thematized and treated profusely across the discipline as well: the social, political, diplomatic, military and science are just the most important aspects in the international historiographical canon whose richness places this field of interest among the most researched areas in the scholarship. Zooming in on the more immediate topic of the interconnectedness of the social and natural sciences, the memoirs and biographies of the MP scientists as well as the specialized, scholarly papers are the most useful secondary sources to consult. As for the primary sources, I will be relying on the personal correspondence of Szilard, Wigner and Polanyi as well as their own articles and essays, and almost never on documents that have been written *about* them. Since I structure the chapters in a way that the secondary sources provide the framework of the prose, I will not itemize them here. The copious amount of literature does present a challenge, the biggest danger being that the narrative will be in the end reduced to a mere mapping of events already told. I hope to avoid this by a disproportionate use of primary literature to support what I think to be a sound working hypothesis.

4. Origins and Its Historiography

The "secret" behind the remarkable intellectual prowess of the Hungarian-born refugee scientists has drawn plenty of popular speculation and a somewhat fewer scholarly analyses. Conversely, works in the otherwise quite meagre Hungarian historiography are, however, rather rich in theorizing about the fact that a disproportionally numerous group of individuals coming from the same, apparently unlikely, place, with a few years between their births, being thus subjected to the same historical contingencies, ending up discipline defining scientists and, some of them, crossing social (having crossed national and disciplinary) boundaries, became reputable public intellectuals. At the center of the major tropes being invoked to explain the 'genius of the Hungarian Group' is the transforming social-cultural landscape in the Hungarian part of the Habsburg Empire around the turn of the 20th century which saw reforms in education, an increase of internal migration, the appearance of new social classes, and eventually the strengthening of the urban intelligentsia (in Budapest especially, after the unification of Pest, Buda and Óbuda in 1873).[5] In the entanglement of these social phenomena, the quality of secondary education in particular is often referenced as the crucial foundation from which many of the future greats emerged. Hungarian born American historian, John Lukacs, himself a beneficiary of such an education, said of the teachers that "the level of their training, not to speak of their dedication, was at least comparable to that of senior professors at the most reputable American universities now."[6] To date, there are two comprehensive studies of Hungarian refugee scholar-scientists. One is a prosopography by Istvan Hargittai titled *The Martians of Science: Five Physicists*

[5] The 'Hungarian Group' is a casual denomination of a number of natural scientists (mostly physicists) in which the protagonists of this essay also belong. It usually correlates with the individuals Hargittai [and György Marx before him in his *A marslakók érkezése* (The Arrival of the Martians, Budapest: Akadémia kiadó, 2000)] calls 'Martians': Theodore von Karman (1881-1963), Leo Szilard (1898-1964), Eugene Wigner (1902-1995), John von Neumann (1903-1957) and Edward Teller (1908-2003)

[6] John Lukacs, *Budapest 1900. A Historical Portrait of a City and Its Culture*, (London and New York: Weidenfeld & Nicholson, 1988), p 145.

Who Changed the Twentieth Century, the other Tibor Frank's *Double Exile: Migration of Jewish-Hungarian Professionals through Germany to the United States, 1919-1945*, which is a wider ranging consideration of not only Hargittai's heroes but interwar intellectual émigrés in general as well. Staying with the scholar's favorite trope of education, in the former, the author is more reserved in assigning the *gimnázium* experience a "miraculous impact" that it had on their careers but contends that it was "an important enough ingredient" in their lives.[7] In an essay, Tibor Frank, "conjectures" that the roots of what he calls "Hungarian creativity" can be found in the combined corollary of the volatile history of Hungary ("constant entanglement with internal and international conflicts, wars and revolutions") and the strong influence of German culture and civilization. Although his epistemology of German creativity is not clear, the proximity of German culture coupled with the precarious existence of the individual (living in Hungary) yielded, according to him, a long line of brilliant problem solvers, many of whom attained world reputation as first rate natural scientist in physics, chemistry, mathematics and engineering.[8] Frank sets the narrative of his book by creating a framework for his subjects, and more broadly, for all living in the territory of Hungary, in which he sees them as "predestined losers […] yet shrewd survivors" which characterization is meant to underline what seems to be the general departure point of the historiography in general. Extremely reduced, it could be summarized like this: Hungary's geographical location has exposed its inhabitants for centuries to the conquest and rule of empires (Ottoman and the Habsburg) until the gradual process of nationalization brought with it a favorable climate for social transformation the most important consequence of which were reforms in education that eventually determined the future success of a great many

[7] Istvan Hargittai, *The Martians of Science: Five Physicists Who Changed the Twentieth Century* (Oxford, Oxford University Press, 2006), p 11.
[8] Tibor Frank, "Acts of Creation: The Eotvos Family and the Rise of Science Education in Hungary" in *The Nationalization of Scientific Knowledge in the Habsburg Empire, 1848-1918,* Eds. Mitchell G. Ash and Jan Surman (London: Palgrave Macmillan, 2012) pp 113-137.

talented students.[9] The reason I briefly rehearse here the central tenet of the Hungarian scholarly discussion of the refugee scientists, three of whom are also the subject of this essay, is because it almost always centers around general tropes of "intellectual migration", "Hungarian genius", "World changing scientist", while explorations of more specialized themes are very rare. Frank did publish on individuals (Szilard and Polanyi) and there is a short piece by Gabor Palló on the intellectual legacy of Polanyi on his student, Eugene Wigner.[10] The way this essay aims, if at all possible, to steer clear from the above keywords is by considering its protagonists as pursuers of a particular "agenda" through a comparative perspective in an effort to demonstrate the argument that, as first rate technical experts, all three had principled convictions regarding the valences of natural sciences to broader society – and that they were not necessarily the same. In order to facilitate a narrative that considers family, social, national, political and professional contexts as virtually equivalent in importance to make up their "culture", fragments, deemed crucial for the argument being made, from virtually all phases of their lives will be presented (to different degrees) in the narrative.

[9] It is to note here that among the main contributors to the Hungarian historiography of Hungarian-American scientists only one author is a professionally trained historian, Dr Tibor Frank. Drs Marx, Hargittai and Palló are themselves natural scientists, a physicist, a chemist and a chemical engineer as well as a trained philosopher respectively.

[10] Tibor Frank, "Ever Ready to go: The Multiple Exiles of Leo Szilard" in *Physics in Perspective* 7 (2005): 204-252 and Tibor Frank, "Cohorting, Networking, Bonding: Michael Polanyi in Exile" in *Tradition & Discovery*, 2001, Vol. 28 Issue 2, pp 5-19, 15p. For the brief recount of the Wigner-Polanyi dynamics, see Gábor Palló, "Kép a falon: A Wigner-Polányi kapcsolat" [Picture on the Wall: The Wigner-Polanyi relation], In: Fizikai Szemle, 2002/10-11: 293-296.

Chapter 1 Against Materialist Planners – Michael Polanyi's Autonomy of Science

1.1 Introducing the Two Camps

Physicist J.D. Bernal (1901-1971) and physical chemist turned philosopher of science Michael Polanyi (1891-1976) were, like all scientists, curious men. What set them apart from the majority of their colleagues was that they were not only curious about their own science but also about the impact of their science on society. The role of government in science and the relation of science to government as well as industry were questions that intrigued them both. The analogies, however, more or less end here. Bernal, born in Ireland to an Irish father and an American mother, studied in Cambridge and grew into an eminent X-ray crystallographer. C.P Snow had this to say about his qualities as a natural scientist: "People have asked, just how will he rank in scientific history in the narrow sense. I think the answer is that in natural gifts he stands very high; he is the most learned scientist of his time".[11] He was also a steadfast Communist, extolling the merits of Soviet science, and retaining his loyalties even when most of his comrades found particular manifestations of the Stalinist variant of Marxism (Lysenkoism is a case in point) no longer tolerable.[12] His seminal book, *The Social Function of Science* (1939), is a comprehensive study (and a history) of science, which he regarded as a social institution,

[11] Quoted in Peter Trent, "The Scientist" in J. D. Bernal: *A Life in Science and Politics* Eds. Francis Aprahamian and Brenda Swann (London: Verso Books, 1999), p 100.

[12] On the effects of Stalin's decision in 1947 to endorse T. Lysenko as head of Soviet biology on the English Left(ist scientists) see Gary Werskey, *The Visible College: Scientists and Socialists in the 1930's* (New York: Holt, Reinhardt and Winston, 1979), p 293. On the nonreaction of the English left to the Nazi-Soviet Pact of 1939 and Bernal's personal struggle to make sense of Bukahrin's torture and execution (in 1938) see Andrew Brown, *J D Bernal, The Sage of Science* (Oxford: Oxford University Press, 2005) p 136.

as well as his main thesis on the virtues on the central planning of science to satisfy the needs of society. For the purposes of the broader theme, I will be discussing the prewar social interplay between English leftist scientists advocating an ideologized conception of science and the liberal agenda as its force of opposition, led by one of the most vocal defenders of the "freedom of science", Michael Polanyi. Polanyi shared a largely symmetrical trajectory with the other protagonists, two of the Manhattan Project alumni, Leo Szilard and Eugene Wigner. He also was a refugee scientist, but his flight from the intensifying threat of *Gleichschaltung* in Germany ended not in the United States, but in Manchester, England in 1933. Also, the multiple geographical relocations of the Hungarian MP scientists, in his case coupled with a triple intellectual transformation as well. Having obtained a degree in medicine at twenty-two, his interest began to shift towards physical chemistry, and by 1926 he was a professor of his new discipline at the Kaiser Wilhelm Institute in Berlin-Dahlem (where he was Eugene Wigner's doctoral advisor). Then, fifteen years after his arrival in England, his third country, his increasing occupation with social issues, economics, political-philosophy and the philosophy of science, prompted Manchester University to create a new chair for him in Social Science. Also, like the members of the "Hungarian Group", he was a natural scientist of a wide ranging, formidable intellect. However, throughout his life he would keep away from open politics. Never having become a member of, or been officially affiliated with the public political forces of British Liberalism, toward the end of the decade, Polanyi nevertheless shared in the faith of those in opposition to the conservatives' policies, particularly on issues of tariffs and protectionism under the leadership of Conservative Prime Ministers Stanley Baldwin (1935-1937) and Neville Chamberlain (1937-1940).[13]

[13] Mary Jo Nye, *Michael Polanyi and His Generation* (Chicago and London: The University of Chicago Press), p 186.

At stake for him primarily, as one of his biographers, Mary Jo Nye, argues, was "the fate of Liberalism […], the defense and validation of 'pure' science independent on social and economic needs, the maintenance of scientists' individual autonomy in choosing their scientific research against the claims of centralized planning, and the protection of stable scientific traditions […] and, more broadly Polanyi sought to do what he could to ensure freedom from oppression of individual rights and duties by a centralizing regime."[14] In what follows, I will be focusing on Polanyi's promotion of the individual autonomy of the scientists not only in terms of them being free to establish their research agendas as a prerequisite to retain professional integrity and to shield the disciplines of natural science from political ideology, but also in pragmatic terms. Since the 'autonomy of science (scientists)' is the main theme along which I will be building up my argument, I will here start by forwarding three fundamental assertions regarding Polanyi's defense of the freedom (autonomy) of science. It will be discussed at some length and established that 1) Polanyi's defense was a reaction but not, first and foremost, to Communism but the advocacy of planned science as claimed to be the foundation of social progress, 2) his technocratic interventionism regarding government regulation of patent rights is a contradiction to his philosophy he advanced about the autonomy of science, and 3) that a conflation of Szilard's defense of the autonomy of science with that of Polanyi's is, to a certain degree, tenable and would make a good case for historical comparison. To track down the drive behind the contentiousness of the key actors, some instances of the discourse is necessary to rehearse. Revisiting the debate, I shall highlight the key factors of the antagonism which pitted against a libertarian Hungarian refugee scientists and his communist British counterpart(s), arguing along the way that the nature of the debate was not essentially political. At the core was the question of the role of science in society

[14] Ibid., 189-190.

around which an entanglement of economic, cultural and philosophical considerations accumulated. While I demonstrate in some detail the effect of Soviet communists on a group of people who came to be called Red scientists, Polanyi's intellectual trajectory and personal background will be given a comparatively greater weight.

1.2 The Nature of the Debate

Bernal, of course, was not a solitary warrior championing the leftist cause in 1930s Britain. In *The Visible College* Gary Werskey gives a remarkable portrayal of the social-political environment through the lens of his five protagonists, the English natural scientists shaping the things to come, through (and thanks to) an increasing visibility as leftist technical experts acting in the public domain.[15] Although they had already begun shaping it in the 1920s it was the first years of the new decade that saw an intensification of their political activities. The facts that in contemporary Britain one in every five people was unemployed and that Germany, the home of then Europe's strongest labor movement, was passively observing the solidification of National Socialism had "even the formerly apathetic Joseph Needham begin to write pamphlets, give speeches and serve on numerous committees."[16] (For instance at gatherings of grassroots organizations

[15] Werskey's collective biography of J.D. Bernal, Joseph Needham, Lancelot Hogben, J.B.S. Haldane and Hyman Levy whom the author identifies as the most vocal of the Red scientists extolling the virtues of Soviet Marxism as they considered it the desirable political context providing the best environment in which to do science. More importantly, for them, planning and collectivist science were to ensure that the practical application of scientific work would eradicate (at least ameliorate) societal ills and bring about progress – without the inequalities characteristic of capitalist societies. Werskey's prosopography is justified by a comparable life and career trajectories; a mere six years between their births, all of them studied at Cambridge, became natural scientists to whom their chosen profession provided a point of departure towards becoming publicly engaged figures conjoining social and natural sciences. [It is to note here that one of the semi-comprehensive treatment in Hungarian historiography of the four most important Hungarian born MP scientists, Istvan Hargttay's *The Martians of Science: The Physicists Who Changed the 20th Century (Oxford: Oxford University Press, 2006)* is built on a very similar strucure.]

[16] Joseph Needham, biochemist, perhaps the most important historian of Chinese science. Also one of the followers of Bernal, Werskey, 68.

strongly connected to the Communist Party of Great Britain CPGB, like the Cambridge Anti-War Council whose first public meeting was chaired by him.[17]) A major discrepancy of the British advocates of planning and their Hungarian born counterpart is the political-ideological alliance of the former group which underlined, and to varying degrees determined, the characteristics of their public engagement. It was an adherence to Marxism, the solidification of which is "traceable to a single event at the Science Museum, South Kensington, on Saturday morning, 4 July 1931."[18] Lead by Nikolai Bukharin, the Soviet delegation to the Second International Congress of the History of Science and Technology had among its members, physicist A.F. Joffe, biologist N.I. Vavilov and physicist-historian of science Boris Hessen. The almost immediate consequences of the event are best captured by Werskey, contemplating the effect it had, particularly on Bernal: "[t]hey were becoming not only more involved in socialist politics, but also more aware of what socialism was about and how it related to their science. Much of the credit for the rapidity of their political evolution must of course be given to the Soviet delegation, and more particularly to Bukharin."[19] Indeed, the dilemma which J.D. Bernal had subsequently posed in his "The Freedom of Necessity" in the form of a question was never really to be a dilemma for him the same way that it was an easy question for Polanyi. It was the *public* that was addressed in the hope they would consider whether it was "better to be intellectually free but socially ineffective, or to become a component part of a system where knowledge and action are joined for one common social purpose?"[20] The answers given by the two socially engaged scientists could not have been more different. Polanyi's stance on the freedom of science is worth quoting in full:

[17] Brown, 120.
[18] Gary Werskey, Visible College, 138.
[19] ibid., 148.
[20] Quoted in Werskey, 146.

> I am not regarding Marxism among scientists as a "bolt from the blue". It is, as you say, a symptom of their unsatisfied social conscience. The craving of intellectuals to participate in mass enthusiasm has produced in the past twenty-five years many forms of depravation, but I have never felt inclined to excuse these on grounds of their relation to social conscience. For the artist and scientist seclusion is the state of high responsibility. I can see no excuse for abandoning such a state in order to indulge in some nationalnist or socialist trash which provides the creative person with the cheap illusions of fulfilling a more concrete social duty. No, as I said before, I have seen too much of this in all sorts of colours (most of it in August in 1914 in the days when Hitler thanked God in tears for the great deliverance) to feel anything that can be politely expressed, towards social emotional needs of this kind.[21]

The interpretation, however, of Polanyi's conflation of "intellectuals", "artists" and "scientist" having assigned to them the domain of seclusion is not without challenges. Notwithstanding the curious designation of nationalism or socialism as "mass enthusiasm", Polanyi and his liberal allies repeatedly crossed the boundaries also. By his reasoning, scientists should not abdicate their responsibilities as scientists to partake in advancing political causes, but push back against those colleagues who do politics as well. And Polanyi was not alone in this. In a letter to him dated 1 July 1941, F.A. Hayek, the Austrian born economist, while urging him to review J D Crowther's book[22], he also had this to say:

> […] I attach very great importance to these pseudo-scientific arguments on social organization being effectively met and I am getting more and more alarmed by the effects of the propaganda of the Haldanes, Hogbens, Needhams etc. etc. I don't know whether you have seen the latest instance, C.H. Waddington's Pelican [the publishing house] on The Scientific Attitude. I think this last specimen is really quite contemptible but like all the sixpennies it will probably be read by the hundreds of thousands. I am seriously thinking of writing to NATURE to point out how much scientists discredit the reputation of science by such escapades.[23]

Hayek did eventually write the article in which he projects an equal amount of vexation and a great deal more militant rebuttal of Waddington's thesis.[24] The distinction between intellectuals and scientists is unequivocal with him; that latter group must not have a say

[21] Letter, Polanyi to Hogben, November 23, 1939, Polanyi, Michael. Papers, [Box 04, Folder 117], Special Collections Research Center, University of Chicago Library
[22] J G Crowther (1899-1983), author, science correspondent of the Manchester Guardian.
[23] Letter, Hayek to Polanyi, July 1, 1941, Polanyi, Michael. Papers, [Box 04, Folder 0444], Special Collections Research Center, University of Chicago Library
[24] F.A. Hayek, "Planning, Science and Freedom" in *Nature*, vol 148 no 3759 (1941): 580.

in public (to him that is mostly political-economic) affairs. This is one of the theses of his short piece. The other is that "planning" (again, of economic activities, of which science is a part) leads inevitably to the establishment of totalitarian societies: "[i]n practice every kind of collectivism consistently carried through must produce the characteristic features which Fascism, Nazism, and Communism have in common."[25] The claim of Socialist societies desiring to enhance freedom is also effortlessly dismissed:

> If the plan is to succeed or the planner to appear successful, the people must be made to believe that the objectives chosen are the right ones. Every criticism of the plan or the ideology underlying it must be treated as sabotage. There can be no freedom of thought, no freedom of the Press, where it is necessary that everything should be governed by a single system of thought. [26]

A distinct cleavage in the economist-philosopher alliance was generated by Hayek's audacious claim, according to which the rapid advancement of German National Socialism was down to publicly engaged physicists (he named two, J. Stark and P. Lenard) without whose social interference Nazism would not have attained the success it eventually did: "It would be hardly an exaggeration to say that in Germany it was the scientists, and the university teachers generally, who have led the way to totalitarianism."[27] In his desperate effort two keep the 'two cultures' separate, Hayek used an extreme(ly distorted) example of two *Deutsche Physiker* to demonstrate the dangers of the amalgam of science and politics. For Hayek, central planning was a devilish plan of would be dictators, while Polanyi reserved a more nuanced approach: for him it was impractical and unfeasible.

[25] Ibid., 583.
[26] Ibid.
[27] I accessed the full book review of C.H. Waddington's *The Scientific Attitude* (Harmsworth: Penguin Books, 1941) in Polanyi, Michael. Papers, [Box 04, Folder 0454], Special Collections Research Center, University of Chicago Library

1.3 Polanyi and Materialist Science: Origins, Animosity and Reactions

Polanyi's allergy to Bernalism reached its apogee after the publication of *The Social Function of Science* in 1939 shortly after which the Society for Freedom in Science (SFS) was, with his active cooperation, established.[28] Bernal's book, then, the *magnum opus* of the British movement of leftist scientist, was a catalyst that steered Polanyi's career in new directions and this brief chronology of the 'scientific' ethos, and what it meant in England for Bernal and his comrades on the one hand, and Polanyi with his conservative-liberal allies on the other, serves 1) to qualify (the somewhat reductionist) notions whereby Polanyi's antagonism to the idea of applied (or planned) science emanates from its associations with totalitarian systems and 2) to demonstrate a claim that his campaign for the freedom of science was not thought of by him as a clash of theories within a larger context against Nazism and then Communism. Despite alluding to the Nazi leader, the vehement attack of the rankled Polanyi, I assert, is not directly (or exclusively) fed by his experiences of the authoritarian regimes he had fled by then twice in his life. He shared in Hayek's argument about the inevitable link between planned science and totalitarianism but he had little interest in confining the debate solely in the framework of political ideologies. Much the same way, his personal experiences of forced migration hardly have the most forceful explanatory powers to justify his antagonism to the British leftist scientists and what they represented. In an answer to a letter of support by *Nature* editor, astronomer, and an ally in the exaltation of pure science, Sir Richard A. Gregory, Polanyi defended the attacking of "certain writers" by alluding to his exasperation. As he intimated that the correctness of his stance on the status of science "seems so obvious"

[28] Nye, 184.

and that "most scientists think as I do", he began considering the argument of the other camp to be quite tiresome and that the "line of [his] thought may gain some philosophical interest on its own".[29] Although no single event, or antagonistic idea to refute, is responsible for Polanyi's (second) change of discipline and the eventual, full time dedication to the philosophy of science, and in particular to the social impact of the natural sciences, as being publicly debated in 1930 Britain, the momentum of the Red scientists was arguably a catapult. A general, historiographical consensus (literature by Nye, Frank, Hargittai, Palló) seems to be that his escapes from two countries, the persecuted family members and scientific colleagues in the Soviet Union and elsewhere have influenced his emotionally charged reactions and this is not an assertion to be dismissed but, I maintain, he would most probably have been just as forthcoming in trying to discredit any claims for the validity of any "kind" of science that is not done for its own sake if the political circumstances had been different in Hungary, Germany or in England. Yet, this is not to claim that Polanyi's keen interest in the social sciences and in economics in particular was engendered in a vacuum. His brother, economist Karl Polanyi, the future writer of *The Great Transformation*, had been involved in their youth with the formation of a group of intellectuals in Budapest who called themselves the "Galileo Circle" [Galileo Kör] and contributed regularly to the Circle's magazine "Freethought" [Szabad Gondolat].[30] After Bela Kun ousted Károlyi, Karl Polanyi left for Vienna where, from 1924 to 1933 he worked as a senior editor for *Der Österreichische Volkswirt*. Their correspondence was a symbol of a continuous entanglement, not a

[29] Letter, Polanyi to Sir Gregory, January 30. 1942, Polanyi, Michael. Papers, [Box 04, Folder 0541], Special Collections Research Center, University of Chicago Library

[30] In what is oftentimes labelled as the 'crisis period of late dualism' the significance of the Kör, as a prominent player in the contemporary 'culture wars', have not yet been made justice to by Hungarian historiography. There are only three monographs and a PhD dissertation discussing its history. For the most comprehensive academic treatment of the Galieists, See Csunderlik Péter Tibor, "A Galilei Kör (1908-1919) története és recepciótörténete" [History and Reception of the Galilei Circle (1908-1919)] PhD Dissertation ELTE 2016. I accessed the document from here: http://doktori.btk.elte.hu/hist/csunderlikpeter/tezis.pdf

manifestation of sporadic exchanges.[31] Eventually, Polanyi was to be crucial in helping Karl and Adolf and his sisters Laura and Irene to flee the Nazis and settle in England The exposure to the intellectual exchanges by other individuals like Karl Mannheim, George Polya, Duczyńska Ilona (future wife of Karl Polanyi) and others may have been relevant during the formative years of the young Michael Polanyi yet the general lack of subsequent references by him precludes the teller of his story from credibly reconstructing the foundation of his wide ranging interest, his intellectual loyalties and especially, his antagonism to Communism, as rooted in the company of the civic radicals of Budapest. An ideological brethren to "Freethought" was another journal of civic radicalism titled "20th Century" [Huszadik Század]. The young Polanyi published, in what was largely Jászi's journal, one article in the "20th Century" [Polányi Mihály, "A békeszerzőkhöz," ("To the Peacemakers") Huszadik Század, no. 2 (1917), but not in the "Freethought". However, among the members of the Circle and its regular contributors to both journals we find, besides Karl Polanyi, individuals suchas George Lukacs [Lukács György] and Oscar Jaszi [Jászi Oszkár]. The Circle and the two journals served as a largely militant outlet to produce pieces (mostly in the form of comments on Western articles) on natural science, arguments for the separation of Church and State, egalitarianism and equal rights to (anonymous) vote. There were calls for free of charge, non-denominational public education and polemics against racism, superstition and metaphysics as well. These politically liberal ideas are the legacies of the fin de siècle Hungary; during the declining years of the Austro-Hungarian Empire, in a city to undergo a significant social-political transformation, the young physician-physical chemist was one among those whose involvement in and exposition to contemporaneous ideas of

[31] The University of Chicago houses the Polanyi papers. In the collection there is a box of letters "covering over fifty years of family history."
https://www.lib.uchicago.edu/e/scrc/findingaids/view.php?eadid=ICU.SPCL.POLANYI

change and reform is generally held to have been decisive in the formation of their social consciousness.

Caution must, however, be exercised when allying him with the atheistic-materialist circle of positivists (or democratic radicals). For one thing, they had among its members several individuals who went on to become prominent Communists. Indeed, the contemporary nationalistic rhetoric of the Hungarian government labelled the student alliance as "rootless" and "nationless" (alluding to the fact that members of Jewish origins had been overrepresented) and clearly considered them to be a dangerous force of the radical leftist counterculture championing historical materialism and freethinking. It is important that the adult Polanyi never related to any of the above. By the time he reached England he had associated himself with Christian libertarianism with loose ties to the Liberal Party in Britain – and with a clever definition of liberalism. Nye reports how he envisaged the liberal conception of freedom: "he said that conservative means traditionalist and that he had no quarrel with this kind of conservative philosophy, because '"in England tradition is Liberal.'"[32] For him, "the two leading political philosophies, the Liberal and the Marxist, are struggling for supremacy within European nations", and, consequently, that struggle was between ideologies rather than economics.[33] Polanyi's liberalism never manifested in him actively advocating its ideologies by seeking political positions, party membership or otherwise aiming to become a public figure advertising liberal virtues as desirable attributes of social progression. In this sense, he always remained apolitical. Pure science – Polanyi's pet theme in life – was to be defended and promoted against applied science, but his convictions regarding its merits were not born on the train fleeing Budapest or Berlin, but

[32] Mary Jo Nye, Michael Polanyi and His Generation, 185.
[33] Ibid.

in the laboratory doing *normal science.* And it is this experience qua scientist, more than anything else, that had lent authority to his intellectual output on what the social function of science should be. His British contemporaries, the Red scientists, whether card carrying members of the CPGB or voters of the Labor party, were dialectical materialists of the Soviet mold making efforts to rendering the practical application of scientific results to be the single most important objective of (materialist) science of which, again, a socialist system was the ultimate framework. In short, Polanyi's rejection of (Soviet style) planned science was not necessarily a product of his social consciousness i.e. it was not personal-ideological in nature but rather intellectual-professional.[34] Furthermore, his philosophy on the freedom of science must be understood only as a part of a larger set of claims on the virtues of the liberal tradition and (his conceptualization of) freedom in general. Totalitarian sensibilities were anathema to such freedom: belief in truth, justice, tolerance and charity are some of Polanyi's concepts which, when not upheld and secured by society – as they certainly were not in Soviet Russia – will be replaced by fanaticism and skepticism. Such societies will be testament to failure in their oppression of intellectual freedom and denying what their basic function should be: to provide moral and intellectual order and a framework for its members to make a living. His studies of socio-economic issues will be further elaborated but it is to be noted here that contrary to Bernal and his followers who, when framing their ideas about the desired role of government involvement in science and economics, acted as agencies of a materialist

[34] Wersky identifies J. D. Bernal as the staunchest promoter of the social relations of the science movement in Great Britain, having emerged from the group as the one to whom "science was communism" and, quite possibly, "communism was to become a science". (Wersky, 137.) Polanyi's 'freedom of science' is, by the early 1940s, became the center focus of his intellectual and political life with which Bernalism was to be defeated. On the personal level, the clash was quite cordial, at least at the beginning. Unfortunately, their sporadic correspondence between them reveals very little of the antagonism with which the two approaches to how the social function of science were playing out at the time.

ideology not as detached social scientists – which may be postulated to characterize Polanyi's contributions.

Since the British left, by the eve of the war, was essentially led by Communists – whose numbers totaled at about 60.000 – everybody, militants and socialist intellectuals alike, had to tailor their politics to the implicit requirements of the common cause. As for the scientists, they looked to the Soviet Union, the world's first socialist experiment on a state level, and there, Bernal in particular, they were able to "identify the social bases of their own intellectual work."[35] In contrast, the Liberal Party had been on the wane since well before the time of Polanyi's arrival in Manchester in 1933. The growing momentum in the late 1930s of British leftist scientists had seen Polanyi's irk intensifying but his interest in and concern with the intellectual debate on what he saw as the freedom of science (as practiced, or as must be practiced, in liberal – and conservative – societies in a Capitalist social framework) against planned, practical science (as it had been connected by him, Hayek, Baker and others, to totalitarian regimes) had begun to develop already in the late 1920s in Berlin when, along with Szilard and von Neumann, he became a regular attendant at Jacob Marschak's seminars on economics.[36] His interest deepened after a number of visits to the Soviet Union during which he personally acquainted the notable physicist Abram Joffe (in 1928), the electrochemist Alexander Frumkin (director of the electrochemistry department at Moscow University), the chemist-physicist Nikolay Semenov and Nikolai Bukharin.[37] These visits engendered papers on Soviet economics,

[35] Werskey., 137.

[36] John von Neumann (1903-1957), mathematician, MP scientist, consultant to a number of government and military organizations. Also, one of Hargittai's Martians.

[37] He visited the Soviet Union at least on four occasions, first in 1928 and lastly before the war in 1935, when Bukharin personally elaborated the merits of Soviet science to him "the distinction between pure and applied science made in capitalist countries was due only to the inner conflict of a type of society which deprived its scientists of their social function, thus creating in them the illusion of pure science. […] The distinction between pure and applied science was inapplicable in the U.S.S.R. […] This implied no limitations on the freedom of research; scientists could follow their interest freely in the U.S.S.R. but owing to the complete internal harmony of Socialist society they would, in actual fact, inevitably be led to lines of research which would benefit the current Five Years' Plan." Michael Polanyi, *The Contempt of Freedom: The Russian Experiment and After* (London:

"to understand the Soviet phenomenon better", the first of which was published in the 1930 May issue of *Der Deutsche Volskwirt.*[38] What Polanyi saw there was that, "the economic system functions so badly that one cannot judge from the result what its fundamental and dubious principles are. Everything is permeated by brutal and stupid fanaticism considering all other opinions as devilish nonsense. The tone of voice in public is a distasteful, monotonous cursing".[39] As discussed earlier, not long after the History of Science Conference in London, exhilarated Red scientist, and science writers, from England (Bernal and Crowther among them) made their pilgrimage as well, and had an entirely different experience. In fact, as Werskey reports, "all of them were deeply impressed with the political, scientific and economic activities that they encountered there."[40] Bernal especially was overwhelmed: "it was grim but great. Our hardships in England were less; theirs were deliberate and undergone in an assurance of building a better future. Their hardships were compensated by a reasonable hope."[41] The discrepant interpretation of what had been observed in the Soviet Union by the two camps prompted Polanyi to accuse the red scientists of myopia, or, even sheer dishonesty about the Soviet conditions. A little more than a year before the History of Science Conference in London, Polanyi wrote a review of a book and sent it to his brother, Karl.[42] What Harvard historian William L. Langer called "one of the best German studies of Russia"[43], was for

Watts and Co., 1940), p 3. In 1930 he was offered by Semenov a position at Joffe's institution in Leningrad, which he declined, but agreed to return to occasionally lecture on X-ray crystallography and solid-state physics. Polanyi had family ties in the Soviet Union as well. His niece, Eva Striker was married to Alex Weissberg, a mathematical physicist from Vienna, working from 1931 at the Ukranian Physical Technical Institute (now the Kharkov Physical Technical Institute). She was arrested by the NKVD on bogus charges and later, by then her ex husband shared the same fate as well. His ordeal at various concentration camps, prisons and his eventual escape is partially portrayed in the imperishable novel, *Darkness at Noon* by Arthur Koestler – Eva Striker's childhood friend. Nye, 198-200.

[38] Nye, 155.

[39] Ibid., 154.

[40] Werskey, 148.

[41] Ibid.

[42] The book was Arthur Feiler's, *The Experiment of Bolshevism* (London: G. Allen & Unwin, 1930), Letter, M. Polanyi to K. Polanyi December 12, 1929, in Polanyi, Michael. Papers, [Box 02, Folder 0001], Special Collections Research Center, University of Chicago Library

[43] In a book review for Foreign Affairs 1931 January issue.

Polanyi not faultless at all ("it does not have a conclusion" and "it does not have a sense of quantity") but it, as well as his own studies, was sufficient enough for him to characterize the Bolshevik's quantitative analyses of economics as lunatic.[44] In his comments he complements what he regards as the shortcomings of Feiler's book about the quantitative analyses of Soviet industry including figures of the agricultural sector. As his calculations conclude, the size of Soviet industry is 1/5-1/7 that of the German, producing ten times less industrial products than Germany (which has approximately half the Soviet population). [45]

He had cultivated a cordial relationship with many of the Leftist scientists and their correspondences were based on mutual respect and courteousness. P.M.S. Blackett (1897-1974), with whom he also "always disagreed", was even counted among his friends.[46] That friendship had begun to be jeopardized by a heightened sense of tension stemming from the dichotomy of their relationship ("I begin to doubt whether you still believe in this un-Marxist distinction") which Polanyi vocalized in a letter written to him on 28th October 1941. Insisting on applying an objective lens through which he had made his observations, he reminds Blackett of his (Polanyi's) prediction he had written "in the enclosed little book" about the U.S.S.R. possibly becoming a "very powerful country". And also lamented the changing nature of the intellectual rivalry, **"**I dare say if you and your friends had been as insistent on finding out the truth concerning the Soviet experiment, as I have been myself, we would now all face the future in a different world of mutual confidence. As it is, there seems to be little tradition left of sober and

[44] "Az oroszok gondolatai egy joreszt elmebajhoz hasonlo alakot vesznek fel, ami legfeltunobb a meroben ertelmetlen szamok kultuszaban". [The thoughts of Russians assume a character that is largely similar to lunacy, which is most spectacular when seen in the cult of thoroughly incoherent numbers. (My translation.)] Letter, M. Polanyi to K. Polanyi December 12, 1929, in Polanyi, Michael. Papers, [Box 02, Folder 0001], Special Collections Research Center, University of Chicago Library

[45] Ibid.

[46] Nobel laureate (1948) experimental physicist. Key figure in developing operational research. Also a left wing scientist.

considerate agreement, and only the desire left to deal a blow. Such is my profoundly sad impression today."[47]

The institutional framework of the planners was the Association of Scientific Workers that "has worked for the proper utilization of science and […] demanded that science be adjusted to social needs."[48] The 'mission statement' of the ASW is reported here verbatim as it was response to a Polanyi article written three days earlier in which he had repudiated the concerted effort of a movement that "rejected the traditional conception of science as the disinterested search for truth".[49] The brief dialogue demonstrably proves the fundamental differences. According to Polanyi, the scientist must choose between tailoring his/her work to the requirements of an organization that is directing its efforts to the creation of particular applications that satisfy social (military, governmental etc.) needs, or conducting free research to produce new knowledge irrespective of the future applicability of the novel "product".[50] Remarkably, this exchange took place during the second world war in late 1942 – after the Manhattan Project had recently taken off (the next month would see the creation of the first 'Atomic pile' – the forerunner of the nuclear reactor – by Fermi and Szilard in Chicago) but, of course, Hiroshima was yet to come. As for the war efforts at home (in Britain), Polanyi's reluctance to get involved remained in alignment with his philosophy of seclusion**.** In November, 1939 he received a letter from a professor of physical chemistry, Eric Rideal, the founder of the Colloid Science Laboratory, later a world class institution of surface science to be used for war work during WWII, on behalf of the Advisory Research Council of the Chemical Society.

[47] Letter, Polanyi to P.M.S. Blackett, October 28, 1941, Polanyi, Michael. Papers, [Box 04, Folder 0487], Special Collections Research Center, University of Chicago Library

[48] Wrote W.A. Wooster, Honorary General Secretary of the ASW in the Manchester Guardian. Newspaper article by W.A. Wooster, November 10, 1942, Polanyi, Michael. Papers, [Box 04, Folder 106], Special Collections Research Center, University of Chicago Library

[49] Quoted in a Newspaper article by W.A. Wooster, November 10, 1942, Polanyi, Michael. Papers, [Box 04, Folder 106], Special Collections Research Center, University of Chicago Library. (As a note of interest, R. K. Merton published his thesis on "The Normative Structure in Science" the same year.)

[50] See p. 32.

The Society's function was to "suggest problems of some national importance to laboratory workers" and Polanyi, with two other British chemists to whom the same letter had been sent, was tentatively requested to cooperate on a particular technical problem and to advise the Society on current German and American methods as well as the "present state [in those countries] of published scientific or technical knowledge".[51] A year later the ARCCS was still unclear about the extent to which its scientists were to be required to aid the war efforts. The confusion was unmitigated by the compulsory registration on the Central Register of the Ministry of Labor (up until then applied only to engineers) of chemists, physicists and quantity surveyors. In an article in *Nature* the ARCCS issued a plea addressing the government by requesting that "it would be appreciated if research committees under Government auspices could make known to the Council the existence of chemical problems just below the level of priority which justifies the expenditure of public funds in the attempt to find a solution."[52] Government involvement in synchronizing work capacities with tasks was also requested: "The ARCCS still finds itself unable to suggest work of national importance to all of the large number of chemists who have volunteered their services, although more than a hundred topics have already been allocated."[53]

[51] Letter, Rideal to Polanyi, November 10, 1939, Polanyi, Michael. Papers, [Box 04, Folder 106], Special Collections Research Center, University of Chicago Library
[52] "News and Views" in *Nature*, Vol. 146, no 3691 (1940): 125.
53 Ibid.

1.4 The Role of Science in Society and Polanyi's Inconsistencies

Although Polanyi felt he could not be of much use in solving problems of the oxidation hydrocarbons of fatty acids, he offered his help to Rideal, provided he could quit any time. Regarding the requested information on the status of German and American science, he also gave his consent but not without saying that, "investigations on anything but a directly technological line would be useless under present circumstances".[54] His reluctance to involve himself with Rideal's (and the ARCCSs) project may only be partially explained by the seemingly unclear relations of government and science at the time. It is more probable that Polanyi's own projects interested – and occupied – him to a much greater extent. It is even more probable that, regardless of war preparations, he, as a scientist, intended to remain "in seclusion". The disinterested scientist was not to be swayed by either ideological considerations nor the lure of professional prestige, or even personal fame, both of which an involvement in government-directed research (for a good cause during the war efforts, for instance) could have brought about. Polanyi's defense of the autonomy of science aimed at the preservation of the independence of the process of knowledge production irrespective of the characteristics of the political environment – and of the results, the practical applications that scientific research may yield. In contrast, Bernal's uncritical admiration of Soviet science was derived from the notion that, in practice, it was directed towards achieving an unprecedented scale of economic growth. Science and politics, he advocated, must be closely connected and for this to happen, science policy (too) must foster a research environment based on central planning – which was what the Soviet

[54] Letter, Polanyi to Rideal, November 14, 1939, Polanyi, Michael. Papers, [Box 04, Folder 108], Special Collections Research Center, University of Chicago Library

Communists had been doing. It was remarkable that Bernal, for whom Marxism *was* science, attained a widespread recognition and appreciation in Britain in spite of his controversial ideological alliance with Soviet style Communism.[55] Economist, Chris Freeman recounted Bernal's contribution to what in the 1940s was a novelty in economic studies; his systematic research into the proportion of Research and Development to the GDP was done by Bernal, and his helpers, for the first time in British history. His advocacy of an increase by an order of magnitude the scale of R&D resulted in an unparalleled economic growth not just in Britain, but in Europe and Japan as well. Between 1945 and 1970 R&D did increase substantially, almost to current levels, which stands, on average, at around 2-3% percent of the GDP. And Bernal did not promote the expansion of science for science's sake. His agenda was, according to Freeman, to "improve upon the human condition."[56] Polanyi's conclusion in his theoretical framework of economics was that the issue of unemployment needed addressing first, which during the era of great depressions, was a hotly debated, recurring theme. His ideal society, one that caters for its members by creating an environment of free enterprise and competition and one in which possibilities are provided for all to make a decent living, seemed to have been a far cry from reality. At issue was the degree of government involvement in regulating the Capitalist market. Polanyi agreed with Keynesian theories in that the state must maintain a sufficient level of demand by which full employment was to be attainable.[57] Bernal's call, on the other hand, for centrally organized (i.e. government financed) R&D programs is more than advocating interventionist economic policies, but, just to stay with the issue of eradicating mass unemployment through government

[55] Alexander King, who played a major part in formulating science policy during and after the second world war, in his tribute to Bernal on the 50th publication of *The Social Function of Science*, rated Bernal's influence on government policies of the post war world recognizing his advocacy to the expansion of resources directed to scientific research.
[56] http://vega.org.uk/video/programme/86
[57] Michael Polanyi, *Full Employment and Free Trade* (Cambridge: Cambridge University Press)

intervention, Polanyi's advocating the same should be surprising given his relentless objection to planning. Economists, which this writer is not, now probably would cry out in unison to dispel such an erroneous conflation of central planning and interventionism but it is beside the point here. The contrast between the vehemence with which Polanyi defended the autonomy of science and the ease with which he anticipated governments to step in to improve on societal ills is striking. After all this is the same reasoning the Red scientists forwarded when advocating the notion that science must be organized and directed to only produce practical applications that are of use for society. If economics can be centrally tempered with for the betterment of society, why can't science? It is because of the different objective (purpose?) of the social and natural sciences. It is precisely because "pure" science must be left pure.[58] At the outset, there is no distinction between pure science and applied science: for Polanyi, it is a free intellectual exercise of a community of likeminded people who are connected by their faith that their theorizing and experimentations, concentrated to deciphering the mechanisms of the natural world (to create new knowledge), will yield results. The purpose of science is not and, Polanyi thought, cannot be the satisfaction of social needs. But social sciences in general, and economics in particular could and should assume that role. And this is where he projects some inconsistencies in his philosophy of science regarding the role of government in science policies. Because, if the optimum environment for the scientist in which to do research is one where they are free to pursue science for its own sake, the question regarding research financing remains open still. According to Polanyi, scientific research is no different from other sectors of the economy (in a liberal capitalist society) in that it should also be structured around, or based on, open competition. And he does expect the

[58] It is to be noted that Polanyi was not entirely hostile to the idea of relinquishing some freedom thereby "legitimating" applied science. According to him, extra-scientific authorities rightly assume the leadership in directing research processes in time of war, for instance. But then, it ceases to be free science. See Michael Polanyi, "The Foundations of Freedom of Science" *Bulletin of Atomic Scientists* Vol. 2 Issue 11/12 (1946): 6-7.

government bureaus as well as industry, universities and private benefactors to grant finances. More to the point, he also demands government intervention through a set of actions to help provide free publicity of and accessibility to new scientific results: patent right legislations were to be amended "in order that inventions may be used freely by all" and for that to happen the state "must relieve [inventors] of the necessity of earning their rewards commercially and must grant them instead the right to be rewarded from the public purse".[59] It was, as Polanyi said this area where he "was not at all opposed in principle to an extension of centralized control over economic activities." What is more, he was "entirely in favor of it whenever the conditions require it."[60] Yet another instance when the fierce debate reached beyond the nature of a clash between Communism vs Capitalism is connected to the changes taking place in the postwar British economy. Polanyi suggested that the strengthening of conglomerates of the industrial sector as a result of injection of state Funds for R&D programs (as advocated for by Bernal for instance) be curbed because it will stifle free competition and the scene will start becoming to resemble something akin to what is favored by a planned economy.

[59] Michael Polanyi, "Reform of the Patent Law in Britain", in *Nature*, 7/14/1945, Vol. 156 Issue 3950, p54-54.
[60] Letter, Polanyi to Mannheim, March 6, 1945., Polanyi, Michael. Papers, [Box 04, Folder 1210], Special Collections Research Center, University of Chicago Library

1.5 Polanyi's "Culture" Revisited

I have in this chapter revisited two interpretations of the desired role scientific research was to play in interwar British society, demonstrating the twofold nature of the debate. To a certain degree, it was a clash of political ideologies, but in more subtle ways Polanyi's, occasionally self-contradictory, defense of free science transcended politics and 'reduced' the antagonistic propositions – and sharpened his own reactions to them – of the Red scientists to matters of economics, and general differences of views on the conditions of human development. I have also briefly alluded to the fact that Bernal, a communist scientist and social thinker in a liberal Democracy, attained in their lifetimes a greater recognition than Polanyi and I will in part conclude this section by offering to engage in a brief discussion of the 'cultural' factors that may help account for the idiosyncrasies of Polanyi's social contributions in this regard.

Having qualified the notion of 'influence' of the *ideas* of radical-intellectual circles in Budapest as foundational on Polanyi's defense of the autonomy of science, I continue asserting that the *exposure* to a socially sensitive group, combined with his innate interests that were wide ranging, was very much a part of Polanyi's tradition. But more importantly, the culture of education in (German speaking) Central Europe carried over a form of sensibility for him which was lacking in his British counterparts. Max Weber's historical atmosphere of the German university, and consequently, the profession of the *Privatdozent*, (after a long, arduous track, oftentimes full of contingencies), brought with it obligations that frequently implied responsibilities outside academia.[61] (In contrast, at Cambridge where scientists were effectively sealed off for larger society, there were

[61] Max Weber, "Science as a Vocation", published as "Wissenschaft als Beruf," Gesammlte Aufsaetze zur Wissenschaftslehre (Tubingen, 1922), pp. 524-55. It was originally a speech at Munich University, 1918, published in 1919 by Duncker & Humblodt, Munich. I accessed the document from here: http://anthropos-lab.net/wp/wp-content/uploads/2011/12/Weber-Science-as-a-Vocation.pdf

hardly any obligations to so much as to publishing papers.) Although the empirical effectiveness of science was not a point of contention in either Britain or Germany, the rhetoric of the exaltation of it, as the key to progress, became much less responsive to other epistemic claims in Weimar Germany.[62] Further, the tradition of scientists' public alliance with leaders of industry was older and better established and, from the second part of the 1920s, Germany reentered the community of international science organizations as well.[63] What *is* Polanyi's tradition then? I make no argument that he is the exemplar 'Weimar scientist' and that his 'culture' is firmly rooted only in his experiences at the Kaiser Wilhelm Institute, or more broadly, in the German setting. But thirteen years of immersion in that setting should be accounted for more than his political alliances or his formative years in Budapest when attempting to reconstruct his epistemology. One reason he cannot be considered as the exemplar 'Weimar Scientist' is found in his philosophy regarding the distinction between pure and applied science. His insistence on restricting his involvement to "technical matters" and reserving the option "to quit any time" from government directed research is in alignment with his scorn of applied science – and, if one can generalize, entirely uncharacteristic of the Weimar scientist (or the Wilhelmine one, for that matter).[64] Also plausible that he was not a "kaiser's chemist" in ways it is described by Steve Fuller. Polanyi did think the minimum requirements, (or the greatest merits) of a scientist, endowed with *tacit knowledge*, was, besides their technical proficiency, the belief (he would say faith) shared by the community of scientists in that the working of the natural world are fathomable. And, in a

[62] For a brief summary of the historical moment of Weimar science, see Cathryn Carson, "Method, Moment, and Crisis in Weimar Science" in *Weimar Thought: A Contested Legacy* eds., Peter E. Gordon and John P. McCormick (Princeton and Oxford: Princeton University Press, 2013) pp. 179-199.

[63] Polanyi was known to have regularly consulted with industries in Hungary (with Tungsram, for instance, up until 1937), Germany as well as in Britain.

[64] See page 10. Also, in terms of scientists' involvement in government or military affairs, consider the efforts of the German chemists during the First World War in manufacturing poisonous gases. Also, it was Fritz Haber, at the forefront of German 'war chemistry' who invited Polanyi from Karlsruhe to join the Kaiser Wilhelm Institute in Berlin.

way, this was also the moral persuasion of his scientific community. He also thought that theiir responsibilities were limited but not for the same reason as the one Fuller invokes about the kaiser's chemists – or "Weimar scientists" in general, They did not consider themselves as a moral community obliged or required to extend their expertise beyond the demonstration of their technical expertise.[65] By contrast, Polanyi's consideration of the scientific community, bound together by faith, expertise and, more to the point here, a moral persuasion, profusely enabled them to cross social boundaries. Yet, as we have seen, it was discouraged as interference with social matters by having a vested interest in the potential consequences of scientific work would corrupt the purity of the craft.

Additionally, in terms of the public (social) engagement of both Bernal and Polanyi, mention must be made of Karl Mannheim's theory of the free-floating intelligentsia in which, in part, he dismisses Weber's warning of the social scientist who, in choosing their research topic, is led by personal motives and interests. For Mannheim, the intelligentsia "is floating above society", which ensures a detached and objective view of the social phenomena under investigation.[66] For Mannheim's social scientist, then, the personal stake in the outcome of his investigation likely disappears, which is not alien to Polanyi's (and Merton's) disinterestedness of the scientists. Although whether Polanyi ever acknowledged allegiance with Mannheim's social theorizing in general is not known, he did find fault in Mannheim's sociology of knowledge. Equally important, they both belonged, and were the "products" of the Central European intellectual tradition.[67] It is of interest that until the Spring of 1944 they had not known each other personally. Polanyi contacted him first in January the same year to inquire whether Routledge – Mannheim

[65] Steve Fuller, *Thomas Kuhn: A Philosophical History for our Time*, (Chicago and London: The University of Chicago Press, 2000), p 144.
[66] Karl Mannheim, 'The Problem of the Intelligentsia, An Enquiry into its Past and Present Role' *in Essays on the Sociology of Culture*, ed. by Bryan S. Turner (London, New York: Routledge, 1992), pp. 91-170.
[67] Mannheim also shares a similar trajectory to Polanyi's, Szilard's and Wigner's.

had been affiliated with them – would consider publishing his collection of essays.[68] After mutually expressing pleasantries and some regret for the lack of personal acquaintance, and interest in the other's work, it was Polanyi who hinted at the prospect of a closer acquaintance ("because our views are, I think, in closer harmony now than they were at earlier times"). Soon, what later became a discourse on practicalities regarding publishing issues came to be an exchange of disagreement initiated by Polanyi regarding Mannheim analyses of the development of ideas. For Polanyi, what any analysis of history should have considered was the nature of and the degree to which social circumstances created, or assured "opportunities for development of thought".[69] Also, his elitist crusade for the legitimacy of moral judgments is also found here: Mannheim's view, according to which "thought is not merely conditioned, but determined by a social and technical situation" is something that Polanyi could only "strongly reject." For him, the moral judgement of history is an axiom, for Mannheim, as Polanyi accused him of thinking, it was "ludicrous". Elsewhere also, his principled defense of the supremacy of morality by elevating it to levels of virtue of the truth seeking scientist is tellingly present, and is also apolitical: "as moral beings we [natural scientists] are dedicated to an interpretation of human notions in terms of right and wrong [and] as Christians – and Westerners, I suggest that we are dedicated to seek and uphold human interpretations more especially in terms of our own moral tradition."[70] In passionately safeguarding the domain (culture?) of the natural sciences against communists and planners, which, it is important to note, was not manifested in arbitrarily

[68] The correspondence comprises 29 letters written between 1944 January and 1945 September.
[69] Letter, Polanyi to Mannheim, April 19, 1944., Polanyi, Michael. Papers, [Box 04, Folder 1006], Special Collections Research Center, University of Chicago Library
[70] Letter, Polanyi to Mannheim, May 2, 1944., Polanyi, Michael. Papers, [Box 04, Folder 1023], Special Collections Research Center, University of Chicago Library

flashing out ideas, but it was a reaction to what he had perceived as an attack on the autonomy of science, the conflation of virtues with morality was a recurring theme.[71]

The key phenomena, then, that may be invoked to aid the deconstruction of the roots of the antagonism between the British "planners" and Polanyi's autonomy of science (or, as I discuss later on, that of Szilard's and the American science bureaucrats) centered on cultural-epistemological differences. From the point of view of the subjects of this essay, when considered as non-native scientists (social or natural) in a native context, their epistemology was culture. The concept of "culture", however, is notorious for evading a universally satisfying definition. Yet "culture" does become a concept that is somewhat tangible and discernible when considered within, and compared across, national boundaries. Although my brief treatment of the Weimar context and Polanyi's Budapest background is not intended to be a proper comparison with the British setting, it does touch upon a set of national practices regarding higher education, the nature of the profession of university faculty members, the technical expert and the social expectations from them. These are the aspects of "culture" which are here considered to be crucial. Furthermore, although Polanyi did not have any "conception of the true nature of civic liberty" before going the England in 1933, he had been a natural promoter of it from a very young age.[72] His outspoken dissent to any measures taken by autocratic regimes culminated in Britain and was directed against the Bernalists as, again, a reaction to attempts at corrupting the culture of natural sciences and its function in society. To a certain degree, Polanyi still projected the image of a nineteen century elite scientist, with

[71] Fuller designates the conflation of virtues and morality as "a Polanyiesque trope". See Fuller, p 141. Also, on the conflation of morality with virtue, see Karl Hall, "The Younger Polanyi", *European Journal of Sociology*, LIV, 3 (2013), pp. 582.

[72] On his brief involvement in politics in Hungary, serving as Secretary to the Minister of Health under Károlyi, for instance, see Nye, pp. 11-13. At twenty-six years of age, he wrote his first attack on the materialist conception of history, he was the only male member at the Budapest University who refused to "volunteer" for the Red Army – and suffered the consequences. Letter, Polanyi to Mannheim, April 19, 1944., Polanyi, Michael. Papers, [Box 04, Folder 1006], Special Collections Research Center, University of Chicago Library

the exception that he had to have a community of scientists sitting with him in the ivory tower. Science for him, though very much a craft, was still a gentlemanly activity whose value was itself the everyday pursuit of it and not the results of the research – when there were any. And a siege on that tower in the form of an encroachment of materialist scientists was a clear and present danger.

Chapter 2 The Public Engagement of Szilard and Wigner

2.1 An "Old Topic" Briefly Recounted

By the time Eugene Wigner (1902-1995) asked in a letter the opinion of his mentor and friend, Michael Polanyi (1891-1976), about the "East-West controversy" both scientists had been well-established figures in their chosen environment, geographical and intellectual. [73] Also, by 1950, the year in which the letter was written, the subject of the inquiry by a theoretical physicist addressed to a physical chemist was not out of the ordinary; both individuals had been among those natural scientists who regularly, and publicly, commented on socio-political issues.[74] As discussed in the previous chapter, Polanyi led the liberal movement of pure science against the socialist planners in England and I highlighted the different angles of the agenda the two sides had forwarded. While the Bernalists had an ax to grind for ideological reasons Polanyi's defense of what he considered free science emanated from outside the political domain; his indictment of the materialist scientists was buttressed by his philosophical argument for the optimal (Polanyi would say exclusive) context in which scientific work should be done. The relevance and role of science in the public domain on the other side of the Atlantic

[73] Letter, Wigner to Polanyi, April 10, 1950, General Subject/Correspondence Files; (Box 66 Folder 2) 1927-1991; Eugene Paul Wigner Papers, Manuscripts Division, Department of Rare Books and Special Collections, Princeton University Library

74 According to Wigner, "Polanyi had taken up economics around 1939 and by 1946 he had become largely a philosopher." In Andrew Szanton, *The Recollections of Eugene Wigner as Told to Andrew Szanton* (New York: Plenum Press, 1992), p 135.

assumed a different character but it was in no way inferior in its scope or significance to the divide between "leftist" and "free" science in England.

John Dewey's optimistic outlook in 1927 on an empowered public capable in the near future of making scientifically informed decisions in political participation whereby the creation of a dreaded "rule of experts" would be prevented was still in the coming decade very far from becoming reality. [75] In America of the 1930s, the Great Depression provided the social context in which the role of the natural sciences came under lively debates across the disciplines, centering around, unsurprisingly, the shortcomings and failures of the social sciences, particularly those of (laissez-faire) economics. It eventually dispelled the general notion that scientific progress would simultaneously mean, through the development of the economy, the advancement of society and an improved standard of living. Politicians' and economists' loss of confidence in technology and their belief that the more widely accessible practical applications of scientific results would increase social welfare was preceded by the skepticism of most scientists and philosophers.[76] Yet, because of the very visible contrast between the economic downturn and the rapid advancement of the natural sciences – especially physics – some observers opined that the solution lay in the application of the scientific method in social affairs. Chemist (and rival to Michael Polanyi), Irving Langmuir is reported to echo the popularity of such an idea: "The striking increase in knowledge of the physical world and the technical advances that have resulted from the progress of science have led to a rather widespread belief that the methods of science should be capable of solving most human problems." [77] The left leaning theologian and public intellectual, Reinhold Niebuhr, contented that social ills – and the "traditionalism of the social sciences" – resulted from the desire of the

75 John Dewey, *The Public and Its Problems* (New York: Henry Holt & Company, 1927; Athens: Ohio University, Press, 1991)
[76] Don K Price, *The Scientific Estate* (Cambridge, MA: The Belknap Press of Harvard University Press, 1965)
77 Qtd in Peter J. Kuznick, *Beyond the Laboratory: Scientists as Political Activists in 1930s America* (Chicago and London: The University of Chicago Press, 1987), p 54.

ruling class of Capitalism to retain its privileged status.[78] This is a kind of reasoning that was not alien to the English scientists of the left to whom it was a Marxist society in which the application of the scientific method in social affairs was interpreted as desirable, yet Price equates the application of the scientific method in human affairs with materialist dialectic "which is supposed not merely to let the Communist system make the best use of science in technical matters, but to give the scientific intellect a general dominant role in the society of the future,". Considering the arguments of the American philosophers and public commentators or the British communists (communist scientists) promoting the 'scientific method' to cure social ills, both camps advocated what amounted to a little more than empowering the bearers of technical knowledge without elaborating the particulars of *how* the scientific method would be beneficial in socio-political affairs. [79] Back across the Atlantic, for the American scientists on the left ("some of them pro-Communists, others pacifists"), while they were not at all immune to the "Soviet experiment", its style of conflation of Marxism and science remained somewhat distant.[80] As Kuznick elaborates, the interest of some American intellectuals post-Depression in the Soviet experiment went beyond publications of cultural criticism and it gradually transformed into a lure as an alternative to the Capitalist system (with its meltdowns and gross inequalities) that elevated science in celebrating it as the sole determinant of a prosperous future for all. Eventually, the alternative the Soviet model would have offered to remedy the aftereffects of the Depression (and liberal Capitalism in general) on American society yielded little transformative power in a society still bent on upholding the idea of an unfettered economy and the divide between science and politics. The advent of the militarization of the German society even strengthened Democratic sentiments and the idea of

[78] Ibid., 55-56.
[79] Price, 6.
[80] See Kuznick, pp. 106-143. Notably, the Oppenheimer brothers took a keen interest; Frank even joined the Communist party and Robert "behaved like a prototypical fellow traveler", who devoured *Das Kapital* and he knew his Lenin as well. Haakon Chevalier is quoted as saying that "Oppenheimer [Robert] is better read [in dialectical materialism] than most party members." Ibid., 140.

intellectual freedom. As Kevles reports, "they proposed instead to ease the material wants of mankind, keep the international scientific community an apolitical beacon of peaceful corporation, and, in the last resort, '"go on strike"', as *Scientific American* urged."[81] It is Jessica Wang, in a brief historiographical survey on the history and sociology of the relations of science-public-government (democratic governments), who takes us from Merton's thesis on the closely knit and mutually beneficial relations of post war science and democracy (with the two entities being intertwined into a functioning system of a whole), to an intellectual discussion led by Don K. Price aiming to refute the benefits of science to democracy. At the core of Price's refutation of Merton's argument is the conclusion that "collapsing the difference between science and politics and redefining the political estate as a truth-seeking operation to make politics more scientific would destroy democracy, not save it." [82] In the 1980s however Price himself began to reconsider the validity of the divide he had promoted, yet Wang chose to call upon the thesis of Yaron Ezrahi to demonstrate the presentation of his argument to construct a new theory to synthesize science and politics which is "appropriate to a postmodern age."[83] Ezrahi, reports Wang, considered science as ideology (when applied in the political domain) and, consequently, rejects the Mertonian reification of scientific truth. Wang reaffirms in the survey the role and the importance of political theory in the study of scientists and politics but concludes that the integration of scientists themselves as political actors in investigations of actual political matters would make theorizing superfluous, and yield more benefits of scholarly efforts in contemporary context.[84] In what follows, the fundamental agenda will be

[81] Daniel J. Kevles, *The Physicists: The History of a Scientific Community in Modern America* (New York: Vintage Books, 1979), p 287.
[82] Jessica Wang, "Perspectives on Science and Democracy since 1940" in *Historical Studies in the Physical and Biological Sciences* Vol. 30, No. 1, Physicists in the Postwar Political Arena: Comparative Perspectives (1999), p 296.
[83] Ibid., 298.
[84] Ibid., 306 For Merton's call for "a new technical form of organization" in order to "preserve and extend equality of opportunity" thus sustaining universalist standards, see Robert K. Merton, "A Note on Science and Democracy," *Journal of Legal and Political Sociology, 1* (1942), 115-126.

to present Leo Szilard as an actor balancing on the boundaries of the two cultural domains trying to bridge the technological divide**.**

2.2 Szilard and the Autonomy of Science

Revolutions in the natural sciences, the outbreak of the second world war (*especially* the outbreak of the war) and the transformations of both *normal science* and the nexus of science and politics had immensely amplified Dewey's (and other important social commentators', like Walter Lippmann's') dilemma, yet, as Jessica Wang reminded us in 2002, "the conflict between science and democracy, expertise and public rule" was by historians of science still "not understood as one of civil society and the problem of the public".[85] From WWII on, but especially from the early days of the Cold War, science, the domain of technicality, would transform into something so complex that the boundary between the popular mediation of it and the dealings with the experts would become secluded. It was going to shape up to be a post enlightenment era when science became largely inaccessible for the general public. Up to the interwar period, science and the knowledge it produced remained an essential part of public life, influencing morals, culture, even religion. Then, through broad institutionalization, and also in the ways personnel were trained, science fed into a dynamic in which it came to be seen as confined to merely technical matters. It would cease to carry those moral and cultural persuasions that it did in the post Newton era.[86] And soon politicians became disquieted. "Scientists should be on tap not on top" – quipped Churchill. Eisenhower, in his famous farewell address, warned of what he had perceived was a threat that "public policy

[85] Jessica Wang, "Scientists and the Problem of the Public in Cold War America, 1945-1960", *Osiris*, 2nd Series, Vol. 17, Science and Civil Society (2002), pp. 323-347.
[86] Based on my lecture notes. *Atomic Age 2017 Winter term, Central European University*

could itself become the captive of a scientific-technological elite."[87] The same sentiment expressed in different ways was a message that scientists' participation in the nature of public reason must be curtailed.[88]Technicality, then, as the prestige of specialized knowledge, was to become a political problem when it came to the role of the scientist in the creation of public reason. However, scientific prestige and the ability to shape an enlightened public discourse was not equated automatically in all cases. The ideal of a scientist in the white lab coat being detached from politics and public discourse was not dissolved.

The social antecedent to what eventually led to "big science" and the "military industrial complex" saw the authority of science beginning to increase the social esteem of the scientist and consequently the institutions of science were called upon to aid nuclear politics. This was the context in which a number of refugee scientist to the U.S, predominantly from Germany, found themselves in the 1930s. In what follows, I will revisit some instances of the public engagement of two such refugee scientists, Leo Szilard (1898-1964), and Eugene Wigner considering as I am doing so Szilard's misgivings of the transformation of the organized work of the MP scientists, the dilemmas concerning the deployment of the Atomic bomb, as well as the control of Atomic energy and early Cold War developments in nuclear politics, particular issues of Wigner's reluctance to assume the leadership of a scientis-politician.[89] The main objective here is to rehearse some aspects of the valences between science and politics in light of the actions and ideas of the two Hungarian born physicists. The characteristics of the trajectory of their professional lives will serve as the framework to

[87] https://www.youtube.com/watch?v=CWiIYW_fBfY

[88] C.P Snow warned of the perils of the great divide between science and politics (the humanities in general) which might endanger functioning democratic systems, especially if scientific expertise gets a monopolistic position in advising political authorities. See Charles Percy Snow, *Science and Government* (Cambridge, MA: Harvard University Press, 1961)

[89] Charles Weiner distinguishes between émigré and refugee scientist, the latter group having arrived in the United States (generally) between 1930 and 1941 from "fascists dominated countries". See Charles Weiner, "A New Site for the Seminar: The Refugees and American Physics in the Thirties," in *The Intellectual Migration: Europe and America, 1930-1960*, eds. D. Fleming and B. Bailyn (1969), pp. 190-228.

reinforce, qualify or maybe even dispel some of the arguments that have been raised to explain antecedents of and motivations for their contribution to the transformation of American, and implicitly, international science and science politics. In gauging the extent and the effectiveness of their public role, there will be no claim made that either Szilard or Wigner was the most accomplished Manhattan Project scientist (although Wigner won the Nobel Prize in 1963 and Szilard has a number of groundbreaking patents to his name[90]) or that they became the most influential science administrators, government consultants or public intellectuals. The onus of the investigation is on the claim that their relevance outside the laboratory and eminence as public figures do not only come from the prestige they had acquired as first rate scientists (while not refuting the claim that MP scientists acting as public figures do so borrowing their disciplinary authority qua scientists, at the time of the Einstein letter, hardly anyone in America knew the names of Szilard or Wigner outside their immediate academic circles[91]) but was the combined result of personal interest and the contingencies of an already changing landscape of science-politics – with all the opportunities it had offered to contribute or, more characteristically of Szilard, to challenge the status quo. I will try to explore what it was that had developed and cultivated that interest.

Why were some MP scientists more inclined to cross the boundaries of science and politics and others less so? Is a distinction between native and foreign born scientists worth making in this regard? Was there a consensus across the disciplines? And, in general, what was the rationale of MP alumni to engage in public affairs? I do not propose to fully answer all these questions (it is not unreasonable to think some of these are unanswerable) but an analysis of the two individuals will yield at least some explanatory power to get a fuller understanding

[90] The linear accelerator (patented in 1928), the electron microscope (1931), and the nuclear chain reaction (1934). About the contradiction of Szilard as defender of free science and taking out patents see page 48.
[91] Or even within it. Wigner recalls Karl Compton, Wigner's one-time boss at the Institute for Advanced Study in Princeton, who was unable to tell apart Wigner and von Neumann even after six months of acquaintance. Szanton, 207.

of those issues. The personal and professional histories of refugees (scientists or not) involve transnational analyses and in case of individuals who are knowledge producers, the challenge lies in the fact that they are presumed to bear certain national or cultural identities, yet they produce knowledge *outside* their culture. Instead of considering whether knowledge production is neutral to personal backgrounds or include important cultural idiosyncrasies when researching, "doing science" in the laboratory, this investigation seeks primarily to explore personal backgrounds in an effort to interpret their inclinations for public engagement. Invoking first, for instance, the responses of MP scientists to the moral and military-practical dilemmas concerning the use of the Atomic bomb over Japanese cities, Szilard reports his own bewilderment at the apparent divide between Met Lab chemists (pro-deployment) and physicists (generally against deployment).[92] The lack of signatures on his petition by the former group he attributed to their – by Szilard's standard misconceived – utilitarianism which had apparently been unshaken by his strongly worded document whose main message was thinly veiled in an appeal to morality.[93] A commonly supported argument for using the bomb on Japanese cities had been that it would prevent further loss of lives inevitable in a long drawn out military engagement in the South Pacific that was predicted. That his chemist colleagues had failed to consider the killing of innocent people greatly baffled him. It is here that he refers to his experiences in Germany.[94] In Berlin in 1933 Szilard had failed to convince Polanyi that he should accept the offer to work at the University of Manchester. Unlike his friend, Szilard was entirely pessimistic about the future of Germany and he never subscribed to the hopeful suggestion that civilized Germans would prevent a full-blown Nazi takeover from happening.

[92] "Reminiscences by Leo Szilard" eds. Gertrud Weiss Szilard and Kathleen T. Winsor, in *The Intellectual Migration: Europe and America, 1930-1960,* eds. D. Fleming and B. Bailyn (Cambridge, MA: The Belknap Press of the Harvard University Press, 1969), p 130.
[93] See Appendix III "Szilard to Group Leaders of 'Metallurgical Laboratory' July 4, 1945". In "Reminiscences", 149.
[94] Ibid., 131

His convictions came from "the observation of small and insignificant things".[95] He pointed to the utilitarianism of Germans as a general national character in which "the moral point of view was completely absent or very weak […] and on that basis did I reach the conclusion in 1931 that Hitler would get into power, not because the forces of Nazis revolution were so strong, but rather because I thought that there would be no resistance whatsoever."[96]

The official body to consider the advantages and limitations of using the bomb during the war was set up in May 1945 and was called the Interim Committee.[97] Szilard was not a member (nor was Wigner) of either the committee or its scientific panel but this of course did not preclude him from thinking about what the purpose of continuing the development of the bomb was and also from contemplating "how would the bomb be used if the war with Japan has not ended by the time we have the first bomb."[98] According to his biographer, William Lanouette, Szilard had correctly surmised the level of uncertainty of the success of his own initiatives in matters that were strictly decided by political or military leaders.[99] Regardless, he was not just thinking; in the fight (his word) to get his messages across, he kept petitioning the government ("his constitutional right to do so") and bombarding with his memoranda the leaders of different sections at the Met Lab in Chicago where, from 1942, he was also working as "Chief Physicist". Sometimes it was historical contingencies that prevented his plans to come to fruition, other times it was his own errors of judgment: He had an appointment with President Roosevelt scheduled for May 8th to present and discuss the memorandum and less

[95] Ibid., 95
[96] Ibid., 96
[97] It was headed by Secretary of War, Henry L. Stimson and the members of the committee were Vannevar Bush, James Conant, Karl T. Compton, Ralph Bard (Undersecretary of the Navy), William Clayton (Assistant Secretary of State) and James Byrnes (acting as personal assistant of President Truman, he held no official position at the time). Oppenheimer, Compton, Fermi and Lawrence sat on the scientific advisory panel to the committee. See "Reminiscences by Leo Szilard" eds. Gertrud Weiss Szilard and Kathleen T. Winsor, in *The Intellectual Migration: Europe and America*, 1930-1960, eds. D. Fleming and B. Bailyn (1969), p 129.
[98] Ibid., 123
[99] William Lanouette, *Genius in the Shadows: A Biography of Leo Szilard, The Man Behind the Bomb* (Chicago: The University of Chicago Press, 1992), p 259.

than three weeks before, Compton's assistant broke the news to Szilard that the President had passed away (April 9 1945)."[100] Three years later, when the War Department was about to pass a bill "without much discussion" on the future control of atomic energy, Szilard saw no other alternative to prevent it from happening than to amass popular support.[101] He had agreed to be interviewed by the Chicago Sun and the Chicago Tribune telling both that the *physicists* were going to "see to it" that the bill would not pass, but for fear of the details being confidential, Szilard did not disclose all particulars of the matter or identified either himself or Compton as the main players behind the issue. Under these circumstances the Tribune decided not to publish the story.[102] Earlier when he contemplated the future of the project (the Manhattan Project) virtually Compton was the only person Szilard had regularly consulted – he saw "no point in discussing these things with General Groves or Dr. Conant or Dr. Bush."[103] The ambivalent – and often acrimonious – relationship of Groves, the leader of the Manhattan Project, a military man, with the foreign born scientists is well documented and Szilard in particular had always been regarded by him as outright intolerable. ("the kind of man that any employer would have fired as a troublemaker").[104] As for Vannevar Bush (1890-1974),

[100] Arthur H. Compton (1892-1962), Nobel Prize winner in physics (in 1927 for demonstrating the particle nature of electromagnetic radiation) and the Head of the Met Lab in Chicago. Szilard ran the memorandum by him before sending it to the President. "Reminiscences by Leo Szilard" eds. Gertrud Weiss Szilard and Kathleen T. Winsor, in *The Intellectual Migration: Europe and America, 1930-1960*, eds. D. Fleming and B. Bailyn (1969), p 124

[101] "The Interim Committee's draft legislation reached President Truman via the State Department shortly after the armistice. After affected federal agencies approved, Truman advocated speedy passage of the congressional version of the bill, the May-Johnson bill, on October 3, 1945. Groves, Bush, and Conant testified at hearings in the House of Representatives that the sweeping powers granted the proposed commission were necessary and that only government control of atomic power could prevent its misuse. Although Lawrence, Fermi, and Oppenheimer (with some misgivings) regarded the bill as acceptable, many of the scientists at the Met Lab and at Oak Ridge complained that the bill was objectionable because it was designed to maintain military control over nuclear research, a situation that had been tolerable during the war but was unacceptable during peacetime when free scientific interchange should be resumed. Particularly onerous to the scientific opponents were the proposed penalties for security violations contained in the May-Johnson bill-ten years in prison and a $100,000 fine. Organized scientific opposition in Washington slowed the bill's progress, and Arthur H. Vandenberg of Michigan held it up in the Senate through a parliamentary maneuver." I accessed the document from here: http://www.atomicarchive.com/History/mp/p6s6.shtml

[102] Ibid., 134.

[103] Ibid., 123.

[104] Richard Rhodes, *The Making of the Atomic Bomb* (New York: Touchstone Book by Simon and Schuster, 1988), p 502. Rhodes asserts that Groves' dislike of particular characteristic traits of Szilard ("brashness") was attributable to the MP Chief's anti-Semitism. The General's intense dislike of Szilard culminated in writing a

electrical engineer, science administrator and the director of the Office of Scientific Research and Development at the time, Szilard's distrust is well detectable as one originating from a largely negative experience of collaboration. In their correspondence there is a lengthy exchange on what Szilard proposed to be the optimal organization of work on unseparated uranium, his chief problem being "the division of authority along the wrong lines".[105] In January 10, 1943, after some eight months of relatively courteous wrangling, the frustrated Szilard had this to say:

> I am addressing this letter to you rather than Dr. Conant who is perhaps devoting more of his time to our work at present than you do […] At that time I believed that our troubles could be remedied merely by a change in organization, now I see that no real improvement is possible without a fundamental change of attitude on the part of the Washington end of your organization towards the *creative scientists* [my italics] who are carrying on this work. One of the consequences of this attitude is the fact that those in Washington whose decisions vitally affect our work, are often laboring under misconceptions about the relative importance of the various conflicting points of view and of the various tasks which we have to solve.[106]

More than a year after the first letter, Szilard still did not see the problems of compartmentalization of the scientific work properly addressed and went on pleading with Bush and requesting that he

> take the scientists who are engaged in this work in the various projects into your fullest confidence for I fear unless you do this our work will seriously suffer. By the word "scientist" I mean not only the administrative heads of the various projects, namely Urey, Compton, Lawrence, Oppenheimer but I also mean men like Fermi and others [in the first version of the letter, before proofreading and correction, instead of "others" Szilard wrote "myself"] who have shown in the past foresight and balance of judgement and a good number of very able, devoted, upright and sincere members of the various projects whom you might class with the rank and file.[107]

By January the next year, Bush grew reluctant to discuss the matter further and declined Szilard's proposal for a personal interview. He was going to delegate the handling of the

secret letter to the Secretary of War, Henry L. Stimson, to suggest that Szilard ("an enemy alien") be interned. Stimson refused. In Laniuette, 240.

[105] Szilard's issue was a disadvantageous allocation of expertise among divisions. Letter, Szilard to Bush, May 22, 1942, Correspondence, Leo Szilard Papers. MSS 32. Special Collections & Archives, UC San Diego Library

[106] Letter, Szilard to Bush, January, 10, 1943, Correspondence, Leo Szilard Papers. MSS 32. Special Collections & Archives, UC San Diego Library

[107] Letter, Szilard to Bush, August 11, 1943, Correspondence, Leo Szilard Papers. MSS 32. Special Collections & Archives, UC San Diego Library

negotiations to Dr. Conant which Szilard declined.[108] Bush was loath to confer because he knew a personal discussion would be futile; war time secrecy in practice meant the compartmentalization of research fields, and, accordingly, the head of the OSRD was not going to divulge information ("cannot depart form the organizational arrangement") that was not strictly pertaining to Szilard's direct scientific work, irrespective of his recalcitrance.[109] On January 26th Szilard wrote his last letter to Bush giving in to the administrator's position to which came a reply, a well-considered change of heart to appease, with some-time points being suggested for a consultation in person. Unsurprisingly the meeting changed very little, if anything, in the organizational structure of scientific work. Government secrecy continued being of utmost importance and an integral part of that policy was to prevent scientists (and engineers) from trespassing each other's domain. Szilard had sought to change that arguing all along by pointing to impractical arrangements and the ill-conceived structuring of the scientific organization that he saw were (or could have been) directly responsible for inefficiency and heavy financial loss. In his frenzied effort to make an impact, Szilard occasionally resorted to very strong language (calling Bush "aloof" as well as implying that the OSRD chief was not doing anything he could) and even to suggesting that compartmentalization might mean "that important potential possibilities (sic) have remained undetected."[110] In fact, he was making the government ("the Washington end of your organization") responsible for the potential failure of or delay in the project. His standing up to the autonomy of science, I argue, goes beyond a sheer objection to the characteristics of the topography of power in which bureaucratic

[108] James Bryant Conant (1893-1978), chemist, President of Harvard University and the first U.S Ambassador to West Germany (1955-1957). "I fear that no indirect method of communicating with you could achieve the purpose which I had in mind therefore I do not feel justified to ask Dr. Conant for an interview […] The very fact that Dr. Conant was lately much closer to this general program than you were is one of the reasons that I wrote to you rather than to him." Letter, Szilard to Bush, January, 1944, Correspondence, Leo Szilard Papers. MSS 32. Special Collections & Archives, UC San Diego Library

[109] Letter, Bush to Szilard, January 18, 1944, Correspondence, Leo Szilard Papers. MSS 32. Special Collections & Archives, UC San Diego Library

[110] Letter, Szilard to Bush, February 1944 ("Rough Draft"), Correspondence, Leo Szilard Papers. MSS 32. Special Collections & Archives, UC San Diego Library

administrators, like Bush, but especially General Groves, aimed at keeping the flow of information from scientific workers. Szilard, a practicing scientists, was aware that compartmentalization also meant deskilling as movements between different spheres of research were disallowed. The shortcomings of such arrangements (how do administrators maintain compartments when one scientific worker is still closely connected to another – for feedback for instance?) he continuously tried to draw attention to and push back against, but my main assertion here is that the dissolving of compartmentalization was ancillary to Szilard's grander agenda. In the correspondence I reviewed, he implicitly, but emphatically, campaigned for an elevated status of scientists and blamed the organizers for rendering "the work crippled from the start by a false attitude toward the scientists". [111] For him, the competence of natural scientists (but not engineers!) to weigh in on administrative and organizational matters using their "foresight" and "balance of judgement" was beyond question. And why not allow them to do the same when it comes to Washington politics? That was probably what he meant, for instance, when saying that the physicists would see to it that the May-Johnson bill would not pass – although it is a question whether or not he only referred to himself.

2.2.1 Szilard and the Authority of Science

[111] Ibid.

As his biographer testifies, Szilard had a difficult personality. His individualism and eminence as a theoretical physicist were matched by the intensely proactive and provocative attitude with which he engaged associates, superiors, authorities, even governments – and friends. But this personality was also reflected in his relentless pursuit of an unfettered environment in which scientists could work and a free pass for them to enter the political-administrative domain was, he thought, a corollary of their capabilities as physicists. It was essentially a cry for a work environment of self-regulation. Yet, he cannot entirely be considered as the champion of the freedom of science but rather an advocate of the free scientists – and not just any kind of scientist. He lobbied for an unfettered research environment to liberate the practical scientist who was "unfree", only fulfilling his functions "mechanically". And since war time pursuits of scientific research was fertile ground for the curtailing of such freedom, Szilard considered himself both technically and rhetorically gifted to challenge the new status quo; he saw himself as one of the best representatives of a body of knowledge which happened to be extremely relevant for the government at the time. Consequently, the relevance of the proprietor of that specialized body of knowledge, he thought, grew as well, or should have grown, accordingly. With regards to the idea of "free science" he should be considered the defender of the "authority of science" rather than the "autonomy of science", Even more precisely, perhaps, it can be concluded that he had envisioned the scientific community as being a meritocracy in which the best representatives, say, those who individually, create new knowledge, have the sole authority to manage the application of the consequences of the research. Consider his application, in 1934, for a patent describing the laws governing chain reaction. What he thought was his extended authority meant not just registering the "ownership rights" for his idea but, since "he knew what it would mean", he was adamant to exclude the public, or even more importantly, fellow scientists who would have been apt to develop his idea further. Similar actions of secrecy were disguised as

precautionary measures when, now in America with Fermi and Walter Zinn, they proved through experimentation that neutrons are actually emitted in the fission of uranium. Szilard was against publishing the results.[112]

Also, as an aside, it is of interest to note that the freedom he demanded for himself and (potentially) his colleagues qua scientists was an intuitive conviction not necessarily born out of experience.[113] And the merits of his demands were defended by argumentations rather than practical results, which is uncharacteristic of the well-considered and empirically constituted philosophy by Polanyi on the freedom of science, the essence of which, when compared to Szilard's notion of the role of scientists, is this:

> The obvious fact of the matter is that any research which is conducted explicitly for a purpose other than the advancement of knowledge, must be guided ultimately by the authorities responsible for that outside purpose. Such external purposes are usually practical, like the waging of war, or the improvement of some public service […] If the research worker is to serve any of these purposes, he must submit his own contribution to the judgement of those who are ultimately responsible for waging a war, running a telephone system [...]. He must accept their decision as to what is required of him for these purposes. […] Broadly speaking, you must choose between dedication to the advancement of a system of knowledge which requires freedom, or the pursuit of applied science which involves subordination.[114]

[112] Szilard, pp. 107-109.

[113] Wigner, who considered him his best friend, lamented that Szilard never accomplished in science what he could have, should he have been more focused on his research in physics or even in biology (which he took up in 1949) and less enthusiastic in his other – social – endeavors. See Andrew Szanton, *The Recollections of Eugene Wigner as Told to Andrew Szanton* (New York: Plenum Press, 1992), pp. 282-286. I will add that apart from his work at the Met Lab (during which he virtually spent more time in Washington than at the lab in Chicago) Szilard has no extensive work experience as a scientist being part (or a leader) of a community of scientific workers "doing" science.

[114] Michael Polanyi, "Freedom in Science" in *Bulletin of the Atomic Scientists* Vol. 6 Iss. 7 (1950): 195-224.

2.3 Szilard's Base of Authority: A Scientist on a Mission to Save the World

As Polanyi's article was published in 1950 it can't have influenced Szilard's conception of the nature and degree of his own involvement in the scientific war efforts, and there is no evidence that he had read it later on. In any event, it is probable that it would not have made a lasting influence on how he saw himself as a physicist in relation to the power structure supervising research works at the Met Lab. It is not, then, conceivable that Szilard would have ever subordinated to any systems of decision making procedures without questioning the merits, the logic, the utility, or at least having a say in the implementation of, the directives of scientific work. (And often non-scientific work as well.) In this, he repeatedly repudiated Polanyi's imperative. Szilard *was* dedicated to advancing a system of knowledge to yield practical applications (and the seriousness of war time applications of scientific works is not necessary to accentuate, and nor is the fact that it was this seriousness that had attracted him the most) *without* sacrificing the autonomy of the scientist. And, especially, his own – as a technical expert of great political-administrative ideas: Lanouette's biography does not contradict Wigner's sentiments about Szilard. He acted freely and individually often at the expense of his commitments as a physicist.[115] It is also true that his superiors, scientific administrators like Compton and Groves, never lamented his absence very much. In fact, as he was seen as an obstacle to the effective operation of the organized scientific research, Bush especially seemed to have been most content when Szilard was not around. And in this, he often satisfied them. War time scientific research, and especially the potential future applications of its results excited him beyond measure as it had created and opportunity to be

[115] In October 1945 Szilard gave a testimony at a Congressional committee meeting regarding the deliberations of the May-Johnson bill. In November, still in Washington, he wrote to Chicago requesting permission for (unpaid) leave of absence from the Met Lab. Lanouette, 295.

involved in processes with world changing possibilities. And simple involvement was never enough: he had to be seen as an important man doing important things ("I have always been a great man").[116] Irrespective of missed opportunities (Roosevelt's death), unfinished projects (several, technical as well as social, most due to the inability of raising funds), personal failures (dismissal by Rutherford), he never stopped considering himself as a heroic figure who was on a personal mission to save the world.[117] The war imperative in general meant that technical experts, borrowing their authority as scientists when acting as public figures, had to adjust. Szilard never did. It is true that on rare occasions he started his articles with a disclaimer saying he is "not particularly qualified" as a scientist when he was setting out to write about "the problem of peace" for instance. But such statements are difficult to take at face value when they are followed by the reasoning to justify the merit of his scholarly contribution, "Yet a scientist may perhaps be permitted to speak on the problem of peace not because he knows more about it than other people do, but rather because no one seems to know very much about it." [118] This and similar statements of false modesty achieved two purposes at one go: it denigrated the public expert while elevated the technical one. The balance of judgement and foresight Szilard referenced as extant characteristic traits in scientists making them admissible, indeed desirable, in the public domain is, I maintain, a plea predominantly for his own acceptance not just as a mediator between science and politics but a shaper of science politics. Although he was aware of his own brilliance as a scientist, he rarely claimed his prestige as such when dealing with either the military or the government. One reason for this was his familiarity with the American context in which the tradition of a well-defined theoretical framework of the relationship of science and politics was lacking (which meant plenty of

[116] Ibid., 279.
[117] His "sense of proportion" was offended by Secretary of State to be, James F. Byrnes when the politician hinted that Hungary would likely be in danger from the Soviets when the war ends. Szilard was not presently concerned with the fate of Hungary. According to him, the new atomic age would herald a new system in world affairs and discussing America's role in it, as a nuclear power, was more important than anything. Lanouette, 265.
[118] Leo Szilard, "Calling for a Crusade" in *Bulletin of the Atomic Scientists* Vol. 3 Iss. 4/5 (1947): 102-125.

legroom to maneuver in), and another, which was not only an American idiosyncrasy, was that the only scientist who could get things done relying on his prestige and fame alone was Albert Einstein.[119] His "balance of judgement" could be interpreted to mean, when referred to in discussions with administrators or politicians, an ideology free, non-partisan readiness to act while "foresight" was what he deemed more important than anything in public affairs – and as important as technical brilliance in his own personality.[120] Furthermore, America was the environment in which Szilard found himself disentangled from a European like traditional tie binding the professional scholar-scientist to the state and he responded to what was a tacit expectation by Americans: ideas and profound theories without actions counted for little. It was a fertile ground for Szilard's brilliance to take off and his individualistic-proactive character and can-do attitude, however difficult to put up with, fit the milieu.

[119] The traditional valences of science and politics in the American context is described by Price as being largely based on the American's "faith in the combination of democracy and science as a sure formula for human progress." This is the legacy of a nation, some of whose "founding fathers" were themselves natural scientist. This legacy has then gradually translated into practices whereby the ties between science and politics came to mean the tackling of individual problems. Then, the great optimism that the advance of science would simultaneously mean social progress ended first because of the great depression, and also when it became apparent that science can also do harm on a scale never seen before. By the end of the first part of the 20th century, at a time when scientific revolutions followed one another in quick succession, Americans found themselves without a theory of the politics of science. Price, pp. 1-5.

[120] He certainly lived by the meaning of his own famous adage, "You don't have to be much cleverer than other people, you just have to be one day earlier than most people". In "Reminiscences", p 97. Some of his "firsts" include: Among the first (in 1930) to have thought Germany was doomed (and having failed to persuade Polanyi to leave), he left for Britain, where, sooner than anybody else, even Rutherford (in 1933), he became convinced that a nuclear chain reaction was not "moonshine" (patent in 1934). Then, independently from Lord Beveridge initially, but around the same time, took early efforts in organizing an escape route for scientists fleeing the Nazis (Academic Assistance Council). He is one of the leaders of MP scientists to lobby for a bill establishing civilian control over the peaceful development of nuclear energy, and also one of the main initiators of the Pugwash conferences on Science and World Affairs. While in America, he was the initiator of the Szilard-Einstein letter, and "designer" of the Council for a Liveable World (1962).

2.4 The Background of a Nuclear Politician: Wigner

In the 1930s there was no expectation from nuclear physicists to have their research funded by the government. Famously for example, Ernst Lawrence's particle accelerator was the product of a fundraising effort as a result of which industry (in his case the medical) became closely connected to, and acquired a vested interest in, the science of physics.[121] Szilard's adversity to the administrative methods, perhaps even the persona, of Vannevar Bush did not seem to have been mitigated by the fact that the OSRD Chief had previously been instrumental in mobilizing civilian science and coordinating military research programs, i.e. securing considerable financial backing for the projects the Office was involved with. Also, Szilard may not have known of Bush's admiration and defense of scientists working under him against military bureaucrats ("they were singlehandedly remaking the defense posture of the army and the navy")[122]. A very gifted entrepreneur of organization, Bush "was convinced of the need to mobilize science under a new *federal* [Kevles's italics] agency […] funded by the government and reporting directly to the President.[123] Bush amassed the support of Compton and Conant who were in agreement with him to make the new Agency durable and effective (which meant manning it with members of the Academy) and most importantly "to keep science safe from politics". [124] In 1940 The National Defense Research Committee was born with the chairmanship of Bush himself, and a little more than a year following its inception, the Committee (and Bush) obtained authority to research and design weapons including all stages of production.[125] Later, a new operating agency under the same direction, the Office of

[121] In interwar America, physicists, in fact, thought that industry had too much influence on physics but by the onset of the war, concern shifted that the military was going to dominate research agendas. In the end the Atomic Energy Commission proved to be the biggest financier.
[122] Kevles, p 296.
[123] Ibid., 300.
[124] Ibid., 296.
[125] Ibid., 298.

Scientific Research and Development superseded the NDRC. The Metallurgical Laboratory in Chicago was not the first venue for a government funded science-military project (it was the MIT based Radiation Laboratory) but it was the first where a large number of refugee scientists were employed and worked together with American born colleagues.[126] Both sites were operating under the NDRC, later the OSRD. Compton personally recruited Wigner (in September, 1941), who at the time had been affiliated with the Institute for Advanced Study in Princeton, and probably not because the Hungarian physicist "urged him 'almost in tears' to help our nation build the atomic bomb". In his memoirs fifty years later, Wigner reports the episode in a different way ("I certainly did not sob") but does not deny the emotional nature of his inspiration to join the Manhattan project.[127] In fact, it is of interest to note that he *expected* his future American superior to be not surprised at his emotionally charged eagerness to fight the Nazis. It is unclear whether a specification can be made here out of Wigner's open declaration of his motivation to participate in war efforts but, as I hope to point out in the following, the drive and keen ambition had always been reinforced by his dread and ideological contempt of both Nazism and Communism. A consideration of the general effects of Nazism on the experiences of refugee scientists as an exclusive source to explain and point to different degrees of readiness for public engagement would only lead to reductionism, or worse, platitudinous claims, but in case of Wigner, the apprehension and defiance of the Nazis, and especially, the Communists with which he had continued coping is well traceable. As is true for the other Hungarian born refugee scientists, experience of totalitarianism had been present early on: already in Germany, for instance, his discussions with Edward Teller (1908-2003) on politics ("Teller and I were both interested in politics"), when centered around Communism, they yielded claims by Wigner such as "communist dictators might try to subdue the earth;

[126] In 1941 Wigner, living in the U.S. since 1933, naturalized in 1937, still referred to himself (and other European born scientists) as "foreigners." Szanton, 208.
[127] Szanton, 208.

halting them would be difficult but crucial" although back then it did not seemed to have resulted in an engagement of deeper analysis of it as a political ideology.[128] While both friends had a wide ranging interest outside the sciences from an early age, lamenting the woes of Communism amounted to a little more than reminiscing about a volatile political system which made it easier for them to consider (or think less about any potential drawbacks of) leaving Hungary and take the opportunity to study in Germany.[129] Those were hardly serious intellectual exercises about a totalitarian ideology in practice, but talks and wistful musings on what had been a negative personal experience ("some people have asked me if the intensity of my anticommunism is an emotional reaction to childhood events. It must be so, in part. I saw the crimes of communism").[130] Associated with political conservatism in the United States, Wigner's aversion to communism is well-known and is rooted indeed in his formative years in Budapest. When referencing "personal experiences" however, precision is sometimes found wanting. In Wigner's case, it should be noted that anti-Communist sentiments had been present in the Wigner home well before March 1919. Antal Wigner, his father, then in a managerial position at the Mauthner tannery, "deeply opposed them" and "he felt that they would cruelly restrict our lives".[131] And since a number of Communist leaders had been Jewish ("my father found this quite disturbing") he thought best to convert his family to Christianity. As Eugene Wigner remembers: "I think my father enjoyed Lutheranism, but at heart our conversion was not a religious decision but an anti-communist one.[132] The father's fear of collectivization and

[128] Ibid., 123.
[129] At 16, Wigner did read Marx as well as Lenin. "I found Karl Marx's work quite unconvincing. And Lenin was even worse. That all power should rest in the state was an idea with quite obvious and serious flaws. Lenin's work clearly brimmed with a lust for power and a grotesque urge to regulate human life by the tenets of communist ideology. Rigid organization may create a perfect antheap, but even at 16 I knew that human beings are not ants and need far broader freedoms." Szanton, 39.
[130] Ibid., 40.
[131] Ibid., 38.
[132] Ibid. It shall be noted here that Wigner's experience as a youth of what he had understood (and recalled in his memoir) as some aspects of Communism as an ideology in general and seen in the Hungarian context in particular is rather sketchy and not without its inaccuracies. Here, for instance, he relates Communism with anti-Semitism and claims that "about 1915 the Communists began gaining real strength in Hungary." Ibid., 38.

its potential consequences – his dismissal from the tannery – did not prove unfounded. Upon losing his job, he took the family to Austria where the prolonged unemployment meant a gradual decline of the family's comfortable living. The father's influence is key here and it is what instilled in the young Wigner his distaste for and opposition to Communism which subsequent events were to reinforce rather than shape or qualify. This might have been the real "personal experience" having a lasting impact on Wigner's personal drive later on, probably more so than the actual regime of Bela Kun or the long forgotten, and rare, assaults putatively triggered by anti-Semitism in Budapest. In fact, somewhat in conflict with Teller's memory, Wigner claims he was uncertain of the real motifs of the attacks he had been (once) the target of in his youth.[133] Furthermore, the advent of the Horthy regime did not seem to neutralize the family's anti-Communist sentiments. In between the carefully worded, and balanced, lines of the recollection ("the new regime turned out to be bitterly, unreasonably anti-Communist", but also, "we had now seen dictatorships of both Left and Right, and heartily disliked them both") there are no unequivocal references of or hints at the young Wigner's deep engagements with socio-political issues at the time. It then remains a challenge to consider his memory a reliable source that would help trace the origins of what was to become his firm political-ideological alliance with forces opposing communism. Horthy was elected Regent (*Kormanyzó*) of Hungary in March 1920. Wigner after one year of enrollment at the Budapest Technical University of the Sciences started the new academic year in Berlin at the *Technische Hochschule* in 1921. It is the brevity of his exposure to the regime(s) and possibly the five decades that passed at the time of his recollections that may explain the lack of a more thorough comparison of the two types of systems and their effects on his political preferences ("our

[133] "One biography of Edward Teller says that 'I was beaten by a mob,' probably for being Jewish. Well, when I was 16, I was in a fight, yes. I do not remember it well, nor do I want to. But this 'mob' was no more than three. Such hoodlums do not bother to define the source of their anger before assaulting their victims. But I doubt it was my Judaism." Ibid.

primary feelings of 1919 were joy and relief at the fall of the communists").[134] It is my assertion that in terms of the formative years in Budapest, his social consciousness was being developed primarily at the influence of his father and was not an innate, generic interest – as were the natural sciences (mathematics, physics and chemistry in particular).

At seventeen years of age, Wigner started considering his choices for his future profession and after systematically weighing out the possible benefits and drawbacks of some (from becoming a farmer, a doctor, clergyman or, probably in jest, a professional singer) he came to the conclusion that it would have to be physics ("I asked myself 'what am I principally interested in?' It was clearly physics and mathematics. So I decided to become a physicist"), and the father's response to his career plans has been quoted many times.[135] The young Wigner had no choice: ("at 17 I was still securely under the influence of my father") he was to study chemical engineering and join Antal Wigner in his tannery later. There was, however, also encouragement for the young Eugene Wigner to "see the word" as the father had known, and during a family trip to the *Technische Hochschule* in 1920 he made a point about it, that his son "would learn more in Berlin than […] in any Hungarian technical institute".[136] The reason I include verbatim some fragments of the conversation is because they are nontrivial for the argument being made. They well demonstrate Antal Wigner's instrumentality on both the attitude to politics and future career of his son. But, at the same time, I make no claim for draconian paternalism: the young Wigner was free to leave the tannery when the opportunity arose.[137]

[134] Ibid., 43.
[135] Ibid., 62. The clever way of dissuading the young Wigner from becoming a physicist was a simple question that his father had put to him ('how many jobs for such physicists exists in our country?' 'Four.' 'Jeno, do you think you will get one of those jobs?').
[136] Ibid., 63.
[137] While it was Antal Wigner's decision to send his son in 1921 to study in Germany, he still anticipated that upon returning his son would continue working at the tannery. Sitting in on the physics colloquia however (with the likes of Einstein, von Laue, Max Planck, Walter Nernst, Rudolf Landenburg, Richard Becker, Werner Heisenberg and Wolfgang Pauli) had sealed Eugene Wigner's future. Back in Budapest again in 1925, the

There is very little likelihood that either Szilard or Wigner (or Polanyi for that matter) would have remained in, or even returned to, Hungary irrespective of the political situation. Pursuing advanced studies in the sciences in Weimar Germany was not an opportunity to pass up on. And the appeal of cutting edge training at prestigious institutions by world class scholar-scientists had been incomparably greater than personal grievances in an unstable political environment in Hungary. German universities (especially in Berlin, Munich, Gottingen and Leipzig) and research institutions comprised the center of higher learning with an appeal to not only bright young individuals (of means) from Eastern but from the whole of Europe – and indeed from the United States as well, contributing a great deal to the internationalization of the natural sciences.[138] Arguably, exposure to totalitarianism had varying degrees of repercussions on the thoughts and actions of the refugee scientists. An in depth, collective psychological analysis cannot of course be offered here but pointing to the different manifestations of such experiences in the two individuals is of interest and is, I propose, doable. I also make the (easy) claim that those who had gone through double exile had a heightened sense of the precariousness of their existence in general and of the threat of National Socialism in particular. For both Szilard and Wigner, relocation to the United States, then an entirely different social and professional environment to Europe, was not without its challenges despite the fact that it had been their second country they had emigrated to.[139] Recounting the circumstances of their integration, it may seem curious that it was more seamless for Szilard who, unlike Wigner, prior to his arrival had not been contracted by a university or a research institution already, landed in the U.S a lot later than his friend (January 2, 1938) and initially

opportunity came in 1926 when, through Dr. Weissenberg, a chrystallographer working at the time at the Kaiser Wilhelm Institute, Michael Polanyi, Wigner's future PhD advisor sent an offer for him to go back. The father grudgingly supported his decision. Ibid., 102-103.

[138] When considering physics, to the list of premier German centers of learning and research must be added the Dutch Leyden University, Rutherford's Cavendish Laboratory in Cambridge, and Bohr's institution at the University of Copenhagen. For a near-complete list of who studied where and with whom (including the Americans), see Weiner, pp. 194-195.

[139] It was the third for Szilard as he first went to England from Germany (after a short stay in Vienna).

he "did nothing but loaf".[140] Indeed for the first few months he was not looking to find work. He visited friends until one day, his famous foresight was at work again: listening to the radio "giving news about Munich" he convinced himself that another war was now inevitable and there was no point anymore to return to England.[141] And, in spite of co-initiating the Academic Assistance Council, Szilard was not among the 1500 scholar-scientists whose relocation and integration efforts were directly aided by it. On a closer look at their personalities and general characters however, there is little curiosity is left when considering Szilard's easier transition. In short, Szilard did not seem to have been overly bothered by the challenges to integrate. It was his fourth country, as opposed to Wigner, he knew the language, especially the vernacular of physics, thanks to the internationalizing nature of the discipline and personal collaborations (or at least acquaintances) with fellow scholar-scientists already there.

2.4 Wigner as an Apolitical Politician

Invoking my earlier assertions that the multiple exile the two scientists (three including Polanyi) had to go through was an experience that had compelled them to constantly reinventing themselves which, as regards adaptability, translated into various degrees of accomplishments and successes – especially in the public domain. The pedigree, which the technical brilliance of both Wigner and Szilard brought about, is not necessary to elaborate here, yet Wigner proves comparably more elusive as a historical character when it comes to deconstructing his sensibilities and clear objectives regarding the role of science in the modernizing world.[142] The somewhat detailed recounting of his youth above serves the purpose

[140] Szilard, 105.
[141] The Munich Crisis, September, 1938.
[142] In 1958 he was requested to write an article on „Society and Science" by Leeds University which he turned down saying, "it would be extremely difficult for me to write an article [on society and science] which would be

of complementing the understanding that can only laboriously be derived from his own publications, manuscripts or even private letters in this regard. If the claim about refugee scientists, according to which their epistemology equals culture, is sustainable and relevant in Wigner's case as well, he will be seen as the one among the trio (even among the quintet if we include Hargittai's Martians) for whom intellectual discipline ought to be counted as central to his culture. [143] He was not an impulsive man; his subtle skills of reasoning and well-thought out arguments coupled with a characteristic modesty, even self-proclaimed shyness, (that was essentially broken only once by anger during the first meeting of the Uranium Committee when Wigner rather aggressively interrupted Colonel Adamson, a military man, when he said that not armaments but the morale of the civilian population wins wars), and, also he keenly avoided publicity.[144] Although fiercely anticommunist, and ready to support any war in which the Soviet Union was involved, it would have been inconceivable from him to suggest, for instance, to smuggle Kapitsa out of the U.S.S.R. in a submarine.[145] Contrary to Szilard's frenzied activism and unfinished projects, his focus on the task ahead was hardly interrupted or derailed.

more than a repetition of well-known slogans." Letter, Wigner to Greene, 9 December, 1958, Series 2: General Subject/Correspondence Files; 1927-1991; Eugene Paul Wigner Papers, Manuscripts Division, Department of Rare Books and Special Collections, Princeton University Library

[143]. As the elusive definition of "culture" was briefly discussed in the previous chapter, an addendum to the problem here is that culture cannot be treated, in science history as universal; here epistemic universalism is an obstacle. Akira Irye, „Internationalization of History", *The American Historical Review*, Vol. 94, No. 1 (Feb., 1989), pp. 1-10.

[144] In 1961 he politely declined giving his permission for a lecture series to be called "Eugene Wigner Lectures". Letter, Wigner to Callihan, May 29, 1961 Series 2: General Subject/Correspondence Files; (Box 21 Folder 3) 1927-1991; Eugene Paul Wigner Papers, Manuscripts Division, Department of Rare Books and Special Collections, Princeton University Library

[145] His bitter antagonism to the Soviets probably excluded his physicist colleagues. For a conference to be held in April 23-25, 1956, titled "Quantum Interaction of the Free Electron" a Russian delegation of physicists were to be invited. Wigner was in favor of the invitation and insisted their stay should be extended to include a few days after the conference. He told John S Toll, the Head of the Physics Department at the University of Maryland (the conference venue), that "quite frankly, if the State Department insist that your visitors leave immediately after your meeting, I would find it less embarrassing not to invite them than to send them home right after the conference." Letter, Wigner to Toll, February, 15, 1956 Series 2: General Subject/Correspondence Files; (Box 21 Folder 5) 1927-1991; Eugene Paul Wigner Papers, Manuscripts Division, Department of Rare Books and Special Collections, Princeton University Library. Eventually, the Russians never showed up. Apparently, the invitation sent to the Soviet Academy of Sciences (with a copy sent to the Embassy in Washington) was not forwarded to the scientists.

His preference to rely on reason and rationality was maintained even in personal matters.[146] Postwar, he published extensively on nuclear energy, arms control and civilian defense, regularly addressed meetings and conferences and sat on commission boards. Probably the most important office he ever held was the directorship of the Research and Development Department at Oak Ridge National Laboratory (now the Clinton Laboratory), a post which he assumed in 1946. However, he had no patience for what he feared was going to be an overwhelmingly managerial (i.e. bureaucratic) role to implement directives coming from Washington, and he relinquished his position and stayed on as consultant to Alvin Weinberg less than a year later when the Atomic Energy Commission started supervising research operations.[147] His rationale to engage in nuclear politics was clearly built on his reputation he had acquired as an MP scientist, and the seamless transition from technical expert to political opinion leader was in no small measure welcome, thanks to his agreeable demeanor, and nuanced style of reasoning (besides the obvious cognitive capabilities) in the public domain. While Szilard, as we have seen, considered himself as well-suited in the politicized domain of science, Wigner was the opposite. Instead of placing himself at the forefront to negotiate against the overbureaucratization by civilian (non-scientific) agencies to direct technical research – which Szilard did do and Wigner could have done as Head of the R&D Department at Oak Ridge – he opted to step down. Yet, deciding against getting personally entangled in open politics did not mean withdrawal from contributing. More suited to his character was not

[146] To illustrate this is an example about the nomination of his former mentor, Michael Polanyi for the Nobel Prize in chemistry, for which his written recommendation had been sought. As he considered Polanyi's contribution as part of a group effort and his not necessarily the single most important, as well as the fact that Polanyi by then was known a lot more about his contributions as a philosopher of science, he did not recommend him. When there accumulated what Wigner considered enough recommendation by other scientists, he changed his mind and joined the recommenders. Consider that Polanyi was not just a mentor but a dear friend as well: there used to be two photos on the wall in Winger's office at Princeton, one of his former school teacher, Laszlo Rátz, the other of Polanyi. Gabor Palló, "Kép a falon: a Wigner-Polányi kapcsolat" [Portrait on the Wall: the Wigner-Polanyi Relations], *Fizikai Szemle* 2002/10-11. 293. I accessed the document from here: http://fizikaiszemle.hu/archivum/fsz0210/pallo0210.html

[147] Frederick Seitz, Erich Vogt, Alvin Weinberg, "Eugene Paul Wigner". *Biographical Memoirs*. (National Academies Press, 1988) pp. 20-21. I accessed the document in June 5, 2017 from here: http://www.nasonline.org/publications/biographical-memoirs/memoir-pdfs/wigner-eugene.pdf

confrontation, especially in person, but the presentation of well-thought out arguments mostly in the written form. Consider, for instance the issue of expanding the American Institute of Physics (AIP) to include non-physicist organizations as well, particularly, in 1956 (Wigner was then President of the American Physical Society, APS) the Society of Exploration Geophysicists (SEG). In a letter dated August 25, 1956 to Hans Bethe (then at Cornell) Wigner is elaborating his misgivings in the following manner:

> I do not believe it is such a good idea for the AIP to expand if it wishes to continue to speak for physicists like you or I, or for that matter, Fred [Fred Seitz, physicist, formal doctoral student of Wigner]. If three or four institutions similar to SEG [Society of Exploration Geophysicists] are admitted, the governing board will consist of fifty people and the majority will have a different outlook on life, will have different ideals and different matters will be more important for them than for us. I have seen many cases in which scientists tried to collaborate with the more experienced and shrewd non-scientists and they lost out in every case. Not that the non-scientists have anything against us, it is just that they do not know what makes us tick and wish to substitute their more efficient methods for ours. I hope you know what I mean although I hope you know it not from experience. This problem is serious, at least I consider it so and I am not reassured.[148]

That the matter was serious and not just for Wigner is attested by the fact that several reputable members of both the Society and the Institute weighed in (I. Rabi opposed the extension, Bethe was supportive of it) and deliberations were not concluded until a statement was issued on the proper role of the AIP the next year in February.[149] The issue of the admission of SEG was deferred until the next session in the following Spring. Wigner's distrust of "the shrewd and

[148] Letter, Wigner to Bethe, August 25, 1956 Series 2: General Subject/Correspondence Files; (Box 2 Folder 3) 1927-1991; Eugene Paul Wigner Papers, Manuscripts Division, Department of Rare Books and Special Collections, Princeton University Library

[149] Excerpts of the document:

"Scientific societies have two types of functions: the first type is "material" because the further the interest of the respective branches of science in a direct, immediately perceptible way. The most important functions of this nature are:

1. Publications
2. Organization of meetings
3. Official representation of the branch of sciences in relation to other institutions, in particular, Government organizations
4. Upholding of the professional standing of the members of society

The second class of function of scientific societies is "emotional". These are more difficult to describe and classify but in importance I would not place them second to the material functions.

1. Scientific societies provide their members a sense of belonging
2. Scientific societies form a basis of a public opinion. (Within the branch of science concerned they give or withhold approval and thereby regulate the relation of the members of the group.)

Scientific societies form a repository of scholarship and the unity of the science concerned. February 1, 1957 Series 2: General Subject/Correspondence Files; (Box 2 Folder 5) 1927-1991; Eugene Paul Wigner Papers, Manuscripts Division, Department of Rare Books and Special Collections, Princeton University Library.

more experienced" non-scientists, then, upheld well into – more than ten years – into the Cold War period. And it is not entirely impossible to claim that he included himself among the scientists who had found themselves, at some point, on the losing side when collaborating with non-scientists.

What were the elements in Wigner's "culture" that he did not necessarily share with Szilard or Polanyi? One of the things that may be pointed at is that, unlike his two colleagues, he clearly had two individuals he had considered as more than teachers, mentors or friends. The quality of secondary education in Hungary during the formative years of these individuals has been echoed by both historians and those who lived through it (Teller and Wigner, for instance) and whenever the subject arises, Wigner never failed to remember to invoke his school ("*a fasori gimnázuim*") and, especially, the name of his teacher of mathematics, László Rátz (1863-1930). Apart from the first rate training Wigner (and von Neumann, also, Rátz's pupil) received, the love for and the discipline necessary to seriously engage with mathematics – and consequently the natural sciences – was instilled by Rátz. The teacher of mathematics and physics was appointed to be Principal of the school but shortly after a year he chose to resign as bureaucratic tasks had taken too much time for what he loved doing the most, teaching. I do not intend to use the analogy between Rátz's resignation as Principal and Wigner's doing the same when he stepped down as Head of the R&D Department at Oak Ridge (and went back to doing what he really enjoyed and loved) because convincingly identifying the origin of one's morals and ethics is too contentious an issue to consider here, but Wigner's ethos is probably a derivative of what he had been exposed to at school as a youth. And then, also in Berlin-Dahlem, at the Kaiser Wilhelm Institute where "worked a man who decisively marked my life: Dr. Michael Polanyi", he was to be subjected to a different kind of exposure.[150]

[150] Szanton, 76.

While the *gimnázium* determined his love for mathematics and the sciences, the Weimar years established his foundations of his career in theoretical physics. Also, the proximity of Polanyi meant an outlet for Wigner to witness, and learn from, an individual whose own wide ranging interest, formidable intellect and developing ideas concerning economics and the philosophy of science were, to varying degrees, keenly absorbed by him. In an interview given to Gábor Palló, Wigner, when asked about his influences and whether he had kept contact with any of them remarked that, "his son [Michael Polanyi's, John] received the Nobel Prize last year. John Polanyi is a very decent lad, but his father was truly incredible, I did my doctorate with him [my translation, see the original in footnote]."[151] Although identifying the two mentors as a clear influence on Wigner's principled professionalism is impossible, there is however a logical consequence that transpires in Wigner's disciplined pursuit of his science and his amateurish fondness in philosophy:

> Modern philosophy dismayed me, clinging to lifeless categories […] while declining to take on the meaning of human life, the motives and dilemmas of human society. The hectic modern world seems to no longer allow philosophers leisure enough to paint a full picture in primary colors. I knew that I lacked the talent to change the whole direction of modern philosophy. So I decided to concentrate on physics.[152]

This excerpt is taken from his recollections and what I intend to demonstrate with it is that his future career as a natural scientists turned opinion leader was not entirely devoid of the philosophical considerations of physics, as a discipline, and science, as a broader social domain which, by fulfilling crucial functions in society does have a lot to offer for the philosophically inclined mind. Consider his rationale that he would build up systematically in explaining the *cultural* and *economics-technological* benefits of science to argue for the extension of government finances for scientific research and development projects. Since the date of the

[151] "John Polanyi nagyon derék fiú. De az apa valóságos csoda volt, nála csináltam a doktorátust." In "A fizika érdekessége csökkent, mert túlságosan nagyra nött" [The Curiosity of Physics has Diminished as the Discipline has Grown too Large], interview transcript, Palló with Wigner, November, 1987, General Subject/Correspondence Files; (Box 63 Folder 2) 1927-1991; Eugene Paul Wigner Papers, Manuscripts Division, Department of Rare Books and Special Collections, Princeton University Library

[152] Szanton, 308.

unpublished manuscript to be referred to is not specified, and in their correspondence I found no references, it cannot be established with certainty whether Polanyi knew of the text, but if he did not, he would have loved it. It is a glorification of the natural sciences in in the broadest sense ("contribute to human welfare in general") and also a subtle defence of pure science (he calls it "basic research") in particular – by also elevating physics as the most comprehensive, therefore the most important (although, he is not as explicit about it as Szilard would be) discipline. This is how he tackles the dilemma of why government should increase financing of scientific research whose benefits are unpredictable:

> […] they can seldom be recaptured by any single institution or group to the degree necessary to justify the optimum investment in terms of potential benefits to society as a whole. As the interconnections between different parts of science broaden and deepen with expanding knowledge, the diffuseness of unpredictability of the benefits tends to increase, and in a purely market economy the allocation of resources to the advance and dissemination of knowledge would tend to fall further below the optimum required for long term growth of the economic system as a whole and for the furtherance of the general welfare. [153]

In contrast to Szilard, then, who primarily sought to preserve, or, even extend the boundaries of, the autonomy of science, primarily on account of the cognitive capabilities of the natural scientists, but also, for what he thought was a general, objective worldview they tended to have, Wigner, to achieve the same objective, envisioned the necessity of maintaining the idea of "free science". His reasons, unlike those of Polanyi's, were not purely philosophical but practical; given the many emerging disciplines in the natural sciences, the predictability of research outcome would therefore result, he claimed, in economic advancement and the "general welfare of society" to be attained if government would "furnish an increasing fraction of the total support for basic research".[154] This invitation of large scale government subsidies would likely bring with it an infiltration of bureaucrats ("non-science men") in managerial positions and Wigner seems to have acquiesced to fighting a losing struggle (consider his

[153] Manuscript, date not specified, General Subject/Correspondence Files; (Box 70 Folder 1) 1927-1991; Eugene Paul Wigner Papers, Manuscripts Division, Department of Rare Books and Special Collections, Princeton University Library
[154] Ibid.

relinquishing his post as R&D Chief at Oak Ridge, or his argument presented to Bethe regarding the extension of AIP), but all was not lost. In 1961 President Kennedy appointed Glenn T. Seaborg, a chemist and an MP scientist, Chairman of the Atomic Energy Commission, which Wigner duly celebrated.[155]

Like all émigrés and refugees, Wigner also had to reinvent himself both as professional, and private person. His transition, however, from the Technical University in Budapest to the *Technische Hochschule* in Berlin was a seamless process of minimal adaptation. With no language barriers, and having been, as both Szilard and Polanyi, exposed to the German influences Hungarian education and of what we would now call best practices, he experienced nothing of the culture shock that the American environment would have in store for him. Unlike Polanyi, who was already well-established in German academia, and a regular consultant with German industrialists, Wigner spent his years in Germany as a student and therefore, quite possibly, had not immersed himself in the political-scientific culture of Weimar the way he was compelled to do so in America. While Szilard almost reveled in, and thoroughly utilized the loose (or undetermined) boundaries between science and politics, Wigner had joined in the hopes of those Americans who still believed capitalism and democracy is the proper framework for – government backed but free – science (at least nuclear physics) to flourish. His tactics to cope with the increasing politicization of scientific research was to become an advisor, rather than an executive with decision making responsibilities – towards non-scientific men. He remained an opinion leader and a public intellectual, but one that shunned both politics and publicity.

[155] In a note of congratulation, Wigner reminded Seaborg that "the chairman of the Atomic Energy Commission is the highest position yet in the United States Government occupied by a scientist". Letter, Wigner to Seaborg, January 18, 1961 General Subject/Correspondence Files; (Box 23 Folder 1) 1927-1991; Eugene Paul Wigner Papers, Manuscripts Division, Department of Rare Books and Special Collections, Princeton University Library

Conclusion

I have considered in this essay some instances of public engagement of three Hungarian born natural scientists to engage the nature of their attitude to the role of science in two different historical and geographical contexts. The premise of the study has been to demonstrate that Leo Szilard, Eugene Wigner and Michael Polanyi, all of whom had been considered seminal figures within their respective technical disciplines, were readily crossing the social domain of science and politics to defend the autonomy of their chosen profession. To show what I asserted to have been the origins in the discrepancies of their tactics and methods to forward their agendas, I revisited the socio-political contexts of Germany, Britain and the United States and designated the individuals as agencies in the transforming relations of science, public and politics. Since my protagonists had shared a very similar life and career trajectories, I considered it necessary to gesture towards "influences" they may have had as youth in Budapest and as students in Weimar Germany. I showed as key factor Wigner's exposure to a disciplined upbringing which included a transmission of professional values and, to a degree, political orientation as well. I highlighted the importance of what had been an excellent secondary education in Wigner's case only; all three had received high quality training in the natural sciences from an early age in Budapest but in Wigner's case the inclusion of the impact on his personality of his mathematics teacher – and later Polanyi – is, I asserted, indispensable in any discussion aimed at understanding his principled stance on the values of science. The Atomic age brought about the robust politicization of the natural sciences – especially physics – and during that transitional period Wigner, compared to Szilard, remained the more considered, rational public intellectual. He, like Szilard, also had great optimism in the natural

sciences, but was more pessimistic, and less confident, regarding the role of science in a democratic political system.

I was presenting virtually no historical facts about Szilard's background, or impactful events in his (early) year since his actions, as a natural scientist, outside the confines of the physics laboratory were both plentiful as well as more telling for the student to discern his clear objectives. He saw himself as a heroic figure who was on a mission to save the world. I showed that his "fight" for the autonomy of science was in fact an attempt to elevate his own authority as a scientist (or, in all fairness, the authority of the best scientists) for which his rationale had been the combination of his technical brilliance and the nature of his field of knowledge – nuclear physics – of which he was indeed an expert. I also argued that the American context, with a historically more flexible framework of science and politics, as opposed to a more traditionalist, Central European one, which was also facilitating Szilard's assertive and proactive character.

The agency of Michael Polanyi as the defender of the freedom of science was presented to contrast a different context with the American, in which the nature of the pressure also assumed a different character: the challenge to the autonomy of the natural sciences, in Britain, came from fellow scientists. The clash of ideas of which I highlighted fragments, is usually remembered, and presented, as that of partisan ideologies – liberal and communist – and I demonstrated the fact that Polanyi was not attacking the Soviet variant of Marxism (as promoted by his British opponents) but strove to prove that in a society of planned science, progress was impossible. Polanyi's argument, however, about the ill-effects of materialist science was only part of his greater agenda. I argued that the momentum of the Red scientists was coterminous with his increasing interest in the philosophy of science at the core of which, then, stood his championing of pure science – the free pursuit of scientific research to produce

new knowledge about the mechanisms of the natural world. The publicity Polanyi sought was the result of the momentum the materialist scientists were gaining; his public campaign was a reaction to the advocacy of applied science. Also, I hinted at Polanyi's elitism to counter the reductionist views of the debate as being the clash of "mass enthusiasms" comprising the antagonism of two opposing parts of the political spectrum. An analogy was also tentatively drawn of the implicit discrepancies between Polanyi's ideas of interventionist economics and Szilard's taking out patents. While Polanyi maintained the desirability of (new) government legislation on patent rights – he sought financial contributions from the public – to establish the material independence of the scientist as well as a free accessibility to "new inventions", Szilard was wielding what he thought was the authority of the scientist regarding the application, or utilization, of his "invention".

The burden of this essay has been to consider three geniuses of Hungarian origin from a somewhat different aspect from what I think generally comprises the main tropes in the Hungarian historiography. Through assigning them a main theme, I investigated their attitudes to, philosophies about and strategies regarding the changing role of the natural sciences (mainly theoretical physics and chemistry) in public affairs in the British-American context. Building on findings of extant materials in the scholarship, I concluded that their technical brilliance was on a par with the degree at which they exalted the virtues and utilities of their profession and sought to defend the autonomy of science. And in doing so, they all saw different things at stake.

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