Table of Content:

Articles
Per Wisselgren: From Utopian One-worldism to Geopolitical Intergovernmentalism: UNESCO’s Department of Social Sciences as an International Boundary Organization, 1946-1955 148-182
Mikhail Sokolov: Famous and Forgotten: Soviet Sociology and the Nature of Intellectual Achievement under Totalitarianism 138-212
Andreas Kranebitter: “Our classroom methodological prescriptions do not fit easily the problems of studying the SS and their doings”: Elmer Luchterhand and sociological research on Nazi concentration camps 213-236

Forum
Thibaud Boncourt, Robert Adcock, Erkki Berndtson, Emily Hauptmann: Adcock: Liberalism and Political Science 237-256

Book Reviews
Cohen-Cole: The Open Mind reviewed by Christian Daye 257-259
Hess: The Political Theory of Shklar reviewed by Cherry Alice Schrecker 260-261
Larsson/Magdalenič: Sociology in Sweden reviewed by Göran Therborn 262-263
Helmes-Hayes/Santoro (eds.): Everett Hughes reviewed by Marianne Egger de Campo 264-269
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**ARTICLE**

**From Utopian One-worldism to Geopolitical Intergovernmentalism: UNESCO’s Department of Social Sciences as an International Boundary Organization, 1946-1955**

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**Abstract**

As a new coordinating organization in the rapidly expanding international field of post-World War II social science, UNESCO’s Department of Social Sciences (SSD), set up in 1946, played a central role. This article explores the formation of the SSD during its first decade with a special focus on its organizational aspects. By conceptualizing the SSD as an “international boundary organization”, the article analyzes the organizational structuration of agency spaces on different levels – within SSD, in relation to UNESCO and to the UN system at large – as well as over time. As a result, the article discerns four phases, distinguished by organizational changes, under which the SSD was successively transformed from a relatively independent transnational organization, which shared the utopian vision of one-worldism, to an intergovernmental organization considerably more vulnerable to external geopolitical pressures.

**Keywords**

UNESCO, Department of Social Sciences, international boundary organization, organizational structuration, agency space

**INTRODUCTION**

On Saturday morning 7 December 1946, on one of the final days of United Nations Educational, Scientific and Cultural Organization’s (UNESCO) inaugural General Conference in Paris, Dr Julian Huxley proudly declared in his installation speech as the new and very first Director-General: “Unesco is now born.” It was a remarkable symbolic event, unique in its kind, Huxley pointed out:

> never before in the history of the world have there been brought together in one place so many representatives of the arts, science, philosophy and education, of radio, of government, of relief societies and youth organizations, town-planning, and of all the higher activities of the human mind [...] from every region of the world, not merely [...] from China to Peru [...] but from the Arctic Circle to the Equator and from the cradle of our Western Civilisation to the Eastern Mediterranean and the Antipodes.¹

To Huxley the gathering was a great success, marked by hard work and an endless co-operative spirit. This convinced him that the great tasks and ideals which had inspired the founding of UNESCO and its general mission – “to contribute to peace and security by promoting collaboration among the nations through education, science and culture [...] for the peoples of the world” – would be realizable.²

The cosmopolitan internationalism and the hopes for a unified world, expressed by Huxley and which underlay the creation of UNESCO, were not only firmly anchored in the Enlightenment tradition of confidence in the power of knowledge and subsequent nineteenth-century conceptions of evolution. They were also historically situated in, what Glenda Sluga aptly has described as, “that curiously utopian moment bracketed by the end of World War II and the onset of the Cold War” (Sluga 2010: 393). Although the early postwar years witnessed a minor explosion of international organizations, including the creation of the United Nations, none of its other specialized agencies better exemplified the renewed faith in worldwide cooperation than UNESCO (Iriye 2002: 44). As a result of the inaugural conference in Paris and its “utopian one-worldism” a number of departments were set up within UNESCO, one of them being the Department of Social Sciences, or Social Sciences Department (SSD) as it was most often referred to.³

During the decade that followed, UNESCO’s SSD became instrumental for the creation of international associations of political science, sociology, economics, comparative law, psychology and other disciplines, but also of interdisciplinary bodies such as the International Social Science Council, international research institutes, regional social science officers and several major research projects. Furthermore, it systematically worked to improve the infrastructure for the international communication and dissemination of social science by initiating indexing and abstracting services, international inventories, as well as journals, yearbooks, dictionaries and other publications. As one of the central players in the contemporary, increasingly populated, international landscape of social science organizations, SSD is also key to understanding the rapid post-World War II expansion of the social sciences that has been highlighted in a number of recent studies.⁴

Within the broad and steadily growing research on UNESCO⁵, surprisingly few studies have paid more focused attention to the Department of Social Sciences. An early but still useful book is Peter Lengyel’s retrospective “insider’s” account from 1986 which offers a brief overview of SSD’s history, including its “pioneering years” from the inception up to 1961 (Lengyel 1986). More recently historians of science Perrin Selcer (2009, 2011) and Teresa Tomás Rangil (2011, 2013) have contributed with important pieces, enriching our understanding of SSD’s epistemological attempts

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³ Its original name was the “Social Sciences Section”. In 1948 it was changed to “Department of Social Sciences”. The Department existed until 1974, from 1965 as part of the “Social Sciences, Human Sciences and Culture Sector”. This was followed by the “Sector for Social Sciences and their Applications” (1976-1984) and “Social and Human Sciences Sector” (1984-present).
Serendipities are sometimes contested. Individual and collective action, is to introduce the concept of “agency space”. Agency space refers to the situated space which has been made to avoid an interpretation of experts in international policy coordination. The analytical point in this context is that the concept helps us to reformulate the abstract notion of “boundary organizations” – defined as institutions that mediate and stabilize the boundary between science and politics; involve participation of actors from different social worlds; provide space for boundary objects that make collaboration across these worlds possible; and include delegations of authority and integrity between principals and agents (Guston 2000: 6; Guston 1999: 93; Guston 2001: 400-401). In addition to these criteria, my conceptualization of “international boundary organization” has been critically adjusted to the context of this article with regard to, first, the international level of analysis, second, the historical postwar setting, third, the processual rather than the stability-centred aspects and, fourth, the introduction of “agency space” as an empirically investigable domain in-between organizational structures and individual actions.7

By addressing these questions on SSD’s organizational embedding, this article intends to add yet another piece to the body of research referred to by analyzing the organizational structuration of agency spaces on different levels – within SSD, in relation to UNESCO and to the UN system at large – during SSD’s first formative decade. Conceptually, I will do this by interpreting UNESCO’s SSD as an “international boundary organization”. The concept draws on David Guston’s notion of “boundary organizations” – defined as institutions that mediate and stabilize the boundary between science and politics; involve participation of actors from different social worlds; provide space for boundary objects that make collaboration across these worlds possible; and include delegations of authority and integrity between principals and agents (Guston 2000: 6; Guston 1999: 93; Guston 2001: 400-401). In addition to these criteria, my conceptualization of “international boundary organization” has been critically adjusted to the context of this article with regard to, first, the international level of analysis, second, the historical postwar setting, third, the processual rather than the stability-centred aspects and, fourth, the introduction of “agency space” as an empirically investigable domain in-between organizational structures and individual actions.7

6 Selcer (2009) analyzes SSD’s attempts to bring epistemic unity to cultural diversity in the formula of “a view from everywhere”, whereas his dissertation (Selcer 2011) looks more broadly at UNESCO’s strategies for the production of objective global knowledge by navigating bureaucratic rivalries and cold war politics, including a case study of SSD’s “Tensions Project”. Rangil (2011) is empirically focused on SSD’s projects on “Tensions”, “Race” and “Technical Assistance” and discerns a gradual shift from a social-psychologically informed “universalism” to an anthropologically-based “pluralism” around 1950, while Rangil (2013) analyzes the identity-work of SSD’s social scientific co-workers. Besides these explicitly SSD-focused accounts, there are also ongoing projects and relevant studies that have highlighted, for example, SSD’s expert networks, Alva Myrdal’s leadership and approach to developmental issues during her time at the UN, and UNESCO’s role for Latin American social science. See Moesslinger (2014), Ekerwald (2001), Ekerwald & Rodhe (2008), Sluga (2014) and Cutroni (2013).

7 Although Guston’s multidisciplinary STS approach is close to the historical and sociological perspective of this article, the four revisions are critical for the following reasons. The first one concerns the level of analysis and is related to the empirical context of origin of Guston’s concept, namely the history of science policy in twentieth century USA. Although Guston explicitly has argued that the concept is applicable to international cases as well (Guston 1999: 89, 106), other scholars, like Clark Miller, have problematized the crucial differences in dynamics when studying international boundary organizations and the complexity, contingency and contestedness of global politics (Miller 2001: 480). The second and more acute reason for revising the concept is also related to the empirical context of origin of Guston’s concept, and more specifically the historical situatedness of “boundary organizations” as a new kind of institutions, like the Office of Research Integrity (ORI) or Office of Technology Transfer (OTT), which according to Guston’s periodization explicitly were “impossible” before the 1970s (Guston 2000: 12, 139). Therefore, it must be emphasized that my conceptualization is explicitly decontextualized from Guston’s historically situated definition. The third reason is that, without going into too much detail at this stage, it is worth noting that Guston’s main concern is related to the problem of stability (Guston 1999: 88; 2000: 6). Our case will give us reason to problematize this stability-centeredness and instead pay greater attention to the dynamics involved in the formation of “epistemic communities”, i.e. networks of knowledge-based experts in international policy coordination (Haas 1992; Cross 2013), and in processes of de-stabilization (see Leith et al. 2016 for a critique of stability as a defining criterion of successful boundary organizations). The fourth and final revision, which has been made to avoid an interpretation that over-emphasizes the organizational structures in relation to individual and collective action, is to introduce the concept of “agency space”. Agency space refers to the situated – and sometimes contested – material, legal, social, cultural boundaries which circumscribe and set the limits for what actions are potentially possible. The analytical point in this context is that the concept helps us to reformulate the abstract
With these revisions taken into account, however, I argue that the concept of “international boundary organization” offers a systematic approach with a specific set of tools that heuristically highlight and analytically connect a number of central but seemingly disparate organizational themes within SSD, such as the relationship between science and politics, the problem of collaboration across social worlds, the importance of workable boundary objects, and the organizational structuration of agency spaces. Furthermore, it will help us to discern and analyze four relatively distinct phases during the period, all marked by organizational changes that not only affected the formal conditions for SSD's activities, but also set restrictions for what was possible to initiate and achieve and hence also had an impact on its direction and contents. Taken as a whole, it will be argued that UNESCO's SSD during the period was principally transformed from a relatively independent transnational organization, which shared the optimistic vision of oneworldism, to an intergovernmental organization considerably more open and vulnerable to external geopolitical pressures.

In the following sections, SSD's development during the four phases – labeled “visionary creation” (1946), “organizational problems” (1947–1949), “revitalization and consolidation” (1950–1952) and “geopolitical re-organization” (1953–1955) – will be characterized and analysed. The paper ends with a concluding section which summarizes the most important changes with regard to the identified organizational structuration of agency spaces and discusses some theoretical implications when analysing SSD as an international boundary organization.

THE VISIONARY CREATION, 1946

The birth of the SSD at UNESCO's first General Conference in Paris in 1946 might give the impression that its character as an international boundary organization that mediated and stabilized the boundary between science and politics was more or less given from the very beginning. This was however far from the case. As this section will show, both UNESCO and its SSD emerged out of a primarily political initiative, where the “scientific” component – the “S” in UNESCO – was not included until late in the process. And if the presence and position of the natural sciences were insecure for a long time, this was even more true for the social sciences. A second point to be emphasized during this founding phase is the importance of complementing Guston's stability-centered concept with a perspective that is more sensitive to the formation of epistemic networks to better understand the dynamics involved in the creation of SSD.

The multifaceted pre-history of UNESCO can of course be narrated in several ways, with emphases on the dynamics of the broader geopolitical context or on different sets of actors, intellectual traditions and sources of origin. In this article, with its focus on the organizational aspects, the retrospective perspective will be restricted to the formative importance of the first Conference of the Allied Ministers of Education (CAME) which took place in London 16 November–5 December 1942. The red thread connecting this conference initiative with four subsequent meetings – the United Nations Conference on International Organization (UNCIO) in San Francisco in April 1945; the UNESCO Founding Conference in London in November 1945; the creation of UNESCO’s Preparatory Commission, also in London, directly after the Founding Conference; and finally,
UNESCO’s inaugural General Conference in Paris – has been analyzed in detail in earlier accounts (F.R. Cowell 1966; Krill De Capello 1970; Sewell 1975). To this series of conferences we can add a number of complementary organizational initiatives, like the pre-existing Commission for International Intellectual Cooperation, founded in 1922 and a few years later transformed into League of Nations’ International Institute of Intellectual Co-operation, the non-governmental International Bureau of Education in Geneva, as well as the Social Relations of Science movement, which were, so to say, woven into the main thread along the way (see Lengyel 1986: 4-5; Elzinga 1996a: 3-19; Toye & Toye 2010: 315; Petitjean 2008).

The main point in this context is that the organizational creation of UNESCO, with its origin in CAME as an intergovernmental forum based on bilateral agreements between the allied ministers of education, was explicitly inscribed in a particular geopolitical setting – where the initiative in the protracted negotiations was shuttling back and forth between the leading delegations of the United Kingdom, United States and France – and that the area in explicit focus from the beginning was education (Graham 2006: 235ff; Krill De Capello 1970: 2, 25-6). The idea of an international organization based on multilateral agreements, encompassing education as well as science and culture, did not appear until later during the process. By the start of the Founding Conference in London on 1 November 1945, “science” had still not found its place in the plans, as revealed by the full name of the meeting, “Conference of the United Nations for the Establishment of an International Organization for Education and Culture” (Krill De Capello 1970: 9; Sewell 1975: 12; Lengyel 1986: 16). Instead it was during the two-week long conference that “science” was added with reference to its universal character, its international mode of collaborating across national borders and because, as the Preparatory Commission’s Report on the Programme expressed it, “its application constitutes by far the most important means of improving human welfare.”8 In other words, it was first at this late stage that it is possible to speak about UNESCO as an “international boundary organization” in its most basic sense, that is, as an institution situated in the borderlands of politics and science.

It was also at this stage, at the Founding Conference in London, in the direct aftermath of the end of the war, that the visionary ideas of a unified world were spelled out in their most optimistic, almost utopian, articulations, including UNESCO’s famous preamble: “Since wars begin in the minds of men, it is in the minds of men that the defences of peace must be constructed”.9 The gathering brought together mid-century internationalists of all sorts, from moderate proponents of international understanding to radical advocates of world government, filling the air with expressions about “intellectual cooperation”, “international understanding” and the “present and future system of supranational cooperation”, as well as more far-reaching hopes about “the solidarity of all peoples”, “universal peace” and “the world [...] as a single unit”, where science and society would be harmoniously co-produced with the help of UNESCO, almost filling the function of a “world parliament” and hence contribute to “a new world order to be created”.10

These optimistic visions colored not only the debates, but also the concrete organizational proposals. These included an annual general conference open to both National Commissions and international non-governmental organizations, as well as the cosmopolitan principles that the

Executive Board, the Directorship and the Secretariat posts should be occupied by persons in an unofficial capacity and based on their individual intellectual merits. These proposals were embraced not least by Huxley, who, like several other leading names in the new organization, wanted to incline UNESCO away from governments in favor of strong-minded individuals and NGOs (Sewell 1975: 109; Petitjean 2006: 31). In that sense, UNESCO can be described as a “hybrid organization” that heralded principles of universalism and non-governmentalism inside an intergovernmental structure (Elzinga 1996b: 169). On the last day of the conference, the UNESCO constitution was signed, which has been described as “the last great manifesto of the eighteenth-century Enlightenment, a utopian document reflecting fervid belief in [...] reform through education, science and reason” (quoted from Lengyel 1986: 5).

Consequently, it is within the organizational context of UNESCO’s formation with its optimistic and almost utopian internationalism, that the creation of SSD as an international boundary organization is to be seen. However, at this point in the analysis, we also need, as already mentioned, to complement Guston’s stability-centered approach (see Guston 2000: 3) with a perspective that is more sensitive to the central group of actors and their formation as an “epistemic community” (Haas 1992, Cross 2013), as well as to how this network was positioned hierarchically within the organization and in the program-making process (Courpasson et al. 2012). This will draw our attention to the small and relatively anonymous group of scholars set up during the spring of 1946 which constituted the so-called “Social Sciences Section” of the Preparatory Commission Secretariat, then located in Belgrave Square in London. The group was headed by Mohamed Bey Awad, an Egyptian social geographer trained in London and Liverpool, who acted as Senior Counsellor. By his side Awad had two Counsellers, the British economist Percival W. Martin, with a background from the International Labor Organization (ILO), and the Norwegian sociologist Arvid Brodersen, who had a PhD from Berlin and experience as a Rockefeller scholar in the USA.

Although Awad was the Senior Counsellor, the available records suggest that Martin and Brodersen played no less important roles in the initial phase. At least it was Martin who in April 1946 received the initial instructions from UNESCO’s Deputy Executive Secretary Howard E. Wilson, Julian Huxley’s right hand man in the Preparatory Commission. The instructions included a detailed time plan, month by month, for the preparations of the social science activities, together with a suggestion on how the section could be organized. One of the very first tasks was to produce a “discussion paper” to be pre-circulated before and discussed at the General Conference in Paris. In early June, this nine-page paper, entitled “The Social Sciences in Modern Society”, was finished. In it several programmatic arguments appeared that would be recurrent in the subsequent discussions, including the central role of SSD for UNESCO at large:


The social sciences have a peculiarly close relation to the total program of UNESCO. [...] It is impossible to develop a sound and realistic program in the social sciences for UNESCO in separation from the total UNESCO program. In one sense UNESCO is itself a phenomenon in the field of the Social Sciences. [...] It is a responsibility of UNESCO not only to serve the established disciplines, fundamental as that is but also to aid in formulating new syntheses of social analysis based on the human experience and problems, hopes and fears involved in living in “one world”.15

The discussion paper was presented at a meeting of the Social Sciences Committee of the Preparatory Commission in London on 13–14 June 1946. Attached as an Appendix to the paper was a three-page list of proposals from a number of governmental advisory bodies, social science organizations, individual experts and other interested people and groups, who had been invited to submit suggestions regarding the coming work of the social science section.16 Those present at the meeting were, apart from the three main authors Awad, Brodersen and Martin, the four leading members of the provisional UNESCO Secretariat – the Executive Secretary Julian Huxley, Deputy Executive Secretaries Jean Thomas and Howard Wilson, and Alfred Zimmern as Adviser – as well as 23 delegates from 18 countries, including Paolo de Berredo Carneiro from Brazil who chaired the meeting.17

In the next step the social science program was included in the draft “Report of the Preparatory Commission on the Programme of UNESCO”, which was delivered in September 1946, in preparation of the coming General Conference. By then, however, the social sciences had been grouped together with philosophy and humanistic studies and integrated under the chapter heading of “The Human Sciences”.18 This was a significant change. In the printed version of the Preparatory Commission’s Report on the Programme of the UNESCO (1946) the heading “Human Sciences” was motivated by the critical difference between the social sciences and the natural sciences. Even if the social sciences aimed to be as objective, systematic, and scientific as the natural sciences, it was argued, “here the matter is complicated by the need for taking account of values as well as ‘neutral’ facts”. This required collaboration with the humanities and philosophy “in the endeavor to work out a scale of values adapted to the modern world and to its continued and progressive development”.19

As a consequence of the Report of the Preparatory Commission’s Programme Committee, the social sciences were by the time of the Paris General Conference grouped together with philosophy and humanistic studies in the programme, although not under the heading of “Human Science”, but in the sessions of the “Sub-Commission on Social Sciences, Philosophy and Humanistic Studies”. The very first session of the Sub-Committee on Thursday morning, 28 November 1946, was introduced by an explicit note from the General Committee of the Conference that it “very strongly recommends that the programmed sub-commissions should not set up new sub-committees”.

15 Ibid: 2.
Ironically, the very question about the relation between social sciences, philosophy and humanistic studies immediately became the topic of lengthy discussions. The winding debate concerned whether the three areas should be organizationally kept together or divided into two or maybe three separate sections. Some delegates suggested that philosophy and humanities should constitute a separate section, others that social science could be grouped together with the natural sciences under the heading of “science”. A third viewpoint emphasized the affinity between philosophy and social science, whereas a fourth proposal spoke in favor of a broad conceptualization of science, in accordance with German terminology, which included the exact as well as the social and humanistic sciences. Yet another delegate suggested that the whole issue of classification and division should be postponed and that UNESCO, once it had commenced its work, could bring it up anew in one year. At this stage of the discussion, Julian Huxley in his capacity as Executive Secretary resolutely stepped in and proposed: “To sum up, what we are doing is, for purely administrative and practical reasons and to satisfy the requirements of administrative logic, to separate the social sciences section from the section on human philosophy.” And so it was decided. A vote was taken and the resolution was adopted by 30 votes to 1.20 When the Sub-Commission had made its vote, the recommendation to separate social sciences from philosophy and humanities was passed on for adoption by the General Conference Assembly.21

It might seem strange that Huxley both went against the explicit recommendations of the General Committee and chose to intervene so directly in the discussions about the separation between, on the one hand, the social sciences and, on the other, philosophy and the humanities, in spite of the number of different alternatives and options that had been presented. It is, however, worth observing that this very delineation was in perfect harmony with the categorizations made in Huxley’s own, personal and programmatic, booklet *Unesco, Its Purpose and Its Philosophy* (1946), published just before the General Conference. There Huxley spoke in favor of the social sciences in general, and in particular “the importance of psychology” and social psychology as “indispensable as a basis for any truly scientific sociology as well as for the successful application of the findings of social science” (Huxley 1946: 45).

What can be discerned from the above is how UNESCO’s SSD during this initial phase was constituted as an international boundary organization, and how social science was delineated and demarcated as an object of common concern. In this process we have identified an epistemic community consisting of a core group in the Preparatory Secretariat – including Awad, Martin and Brodersen as well as Wilson and, not least, Huxley – that was backed up by the Sub-Commission on Social Science, and a third enlarged circle of delegates at the General Conference, as well as organizations, experts and other individuals who were able to have their say by giving input in relation to the first draft of the discussion paper. This agenda-setting process developed, by and large, in accordance with the formal power structures and the organizational instructions for delegation of authority as formulated and adopted by the General Conference Assembly in Paris. According to these instructions, the General Conference was “the highest authority in the Organization”, whereas the Executive Board, consisting of individual members selected on their intellectual merits, should be “responsible to the General Conference for the preparation and execution of the program”, and the Director-General “responsible for developing an efficient Organization and for adapting it to changing programs and needs”. Furthermore, which we will

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21 UNESCO/C/30 [Records from GC Paris]: 233. See also UNESCO Archives, X07.55, US Delegation statement on SS Program 461128.
have reason to go into in more detail in the next section, the Heads or Program Directors of the different departments “were to be responsible directly to the Director-General” and “be assigned in his field the functions of research, stimulation of services, liaison and operation”.  

ORGANIZATIONAL PROBLEMS, 1947–1950

Once the organizational structure of UNESCO had been settled – initially with eight different program sections: Education; Natural Sciences; Philosophy & Humanistic Studies; Museums; Libraries; Social Sciences; Arts & Letters; Mass Communication – it was time to translate UNESCO’s visionary constitution into practice and to start organizing the internal program work of the individual sections. For that purpose directly after the General Conference Julian Huxley called for a first Heads of Sections meeting on 15 January 1947, at which SSD, then formally the Social Sciences Section, was represented by Awad. However, to launch a large and completely new organization was easier said than done. The delicate task, as described by Léon Blum at the General Conference, was to “put into operation a very complicated administrative system” and to remain “true to the great ideas and ideals which inspired its creation”, while at the same time avoiding the risk, pointed out by the Preparatory Commission, of the UNESCO Secretariat becoming “an isolated bureaucracy.

On the departmental level the task was not much easier. There SSD’s two most pressing questions were, according to Brodersen (1956: 401), “how to translate the general ideas and principles of the constitution into specific objectives in the field of social sciences; and how to design in line with these policy objectives, concrete projects according to priorities of urgency and importance, at the same time adjusting them realistically to existing conditions of implementation.” Added to this came the general challenge of fostering cooperation in spite of the many heterogeneous participants involved, that is, to manage collective action across social worlds and to achieve enough agreement to get work done, or to speak with Guston’s terminology, to provide a space for the creation of workable boundary objects (Guston 1999: 93, 2000: 109; c.f. Star & Griesemer 1989: 387; Fujimura 1992: 168). During this second phase, as we will see, UNESCO’s SSD confronted several practical problems due, among other things, to organizational instability, institutional overlapping and inadequate boundary objects.

The organizational instability – in terms of rapid growth and unsteady leadership – applied to both UNESCO at large and SSD, although the emphases of the problem differed slightly on the two

22 UNESCO/C/30 [Records from GC Paris] “Annex III: Report on Organisation of the Secretariat”: 254-5. In terms of recruitment, the Director-General was the only post elected by the General Conference on the recommendation of the Executive Board, while all other positions, the programme directors included, were formally employed by the Director-General. Se UNESCO Preparatory Commission, Report on Programme, 1946: 17. C.f. Ascher 1950, 1951; Hoggart 1978: 20.


levels. On the general UNESCO level, Julian Huxley was the one who led the practical construction work during this early phase. The way he set his mark on the organization with his visionary one-worldism – summarized in his own words as “a world scientific humanism, global in extent and evolutionary in background”, with its grounding in contemporary scientism, materialism and universalism – and his energetic and inexhaustible style of leading UNESCO’s attempts “to make more real the idea of a world society”, have been analyzed by several scholars. But since his thoughts – especially his materialism – were controversial, Huxley’s mandate had been restricted to only two years instead of the constitutional six (Sewell 1975: 106-7, 127; Toye & Toye 2010: 239).

Hence, already at the General Conference in Beirut in December 1948 Huxley was succeeded by the Mexican author and former foreign and education minister Jaime Torres Bodet, who was elected for six years with an overwhelming majority of votes (Sewell 1975: 128). Although Huxley and Torres Bodet shared many visions, for instance, on the role of education, and their pioneering spirits diffused into the whole organization, there were also significant changes marking UNESCO’s three first years of practical work (see Sewell 1975: 132; Brodersen 1982: 258). After only one year in the office, Torres Bodet reported the acute situation caused by the rapid expansion of the Secretariat. In the last six months alone, September 1949 to March 1950, almost 100 new staff members had been recruited (marking an increase from 717 to 810). This meant that more than half of the budget (56%) went directly to wages and had caused “signs of overstrain” among the personnel, Torres Bodet complained and summarized: “We have been so occupied with reporting on the past and preparing for the future that we have scarcely had time to do anything in the present”.27

On the departmental level, lack of steady leadership caused an even greater problem. During SSD’s first four years there was a succession of no less than four different Heads. Mohamed Bey Awad, who had led the work in the preparatory Social Sciences Secretariat as Senior Counsellor left SSD only a few weeks after the General Conference in Paris.28 The transition to Arvid Brodersen was however a smooth one, since he also, as mentioned, had been in the preparatory Secretariat. Brodersen stayed for two and a half years, from early 1947 until August 1949, when he moved to take up a post as Professor of Sociology at the New School for Social Research in New York (Brodersen 1982: 258). Brodersen was replaced by the Brazilian anthropologist Arturo Ramos. However, only three months later, in late October, Ramos suddenly died. In that situation the American sociologist Robert Cooley Angell, who was currently directing SSD’s “Tensions Project”, volunteered as Acting Head for SSD as a whole.29


27 UNESCO, 5C/3, Report of the Director General, October 1949-March 1950: 15. The expansion of the staff was underblown by the increasing number of member states which more than doubled (from 28 to 60) during 1946-1950, and would almost triple to 74 in 1955 – and yet the most significant influx of new member states occurred during the subsequent decade with a first wave of East European countries after Stalin’s death in 1953 and then a total of 27 newly independent African states joining the Organization (TheCourier, January 1953: 3; DG Report 1955: 185; Elzinga 1996b: 188; Cutroni 2013: 50; Duedahl 2016: 51; Sluga 2013:106).

28 According to Sewell (1975: 100) Awad left due to failure to receive others’ encouragement of his view points on social insurance, wages and collective bargaining.

29 Angell later held positions as President of ASA (1951), ISA (1953-56) and the U.S. National Commission for UNESCO (1951-1956) (see Platt 1998).
Like UNESCO at large, SSD too expanded during the decade. However, as one of the smallest departments throughout the period in terms of numbers and budget, this did not cause a problem in the same way as on the general UNESCO level.\footnote{Lengyel 1986: 2. The more exact numbers depend on which staff categories are included. Rangil (2011: 7-8) focuses on the permanent staff and counts less than 12 in 1951, 24 members in 1952, 48 members in 1955-56, and 53 in 1959-60. According to Lengyel (1966: 569), the budget expanded from $286,500 in 1949, $540,600 in 1953, $761,400 in 1956, and just over $1 million in 1959.} The basic organizational principle, on the departmental level as well as on the general UNESCO level, was a project-based structure. As Huxley had explained at the initial Heads of Section meeting, “the organization would gradually grow out of the proposed projects”.\footnote{UNESCO Archives, H.S./9/1947, 470115. C.f. Cutroni (2013: 55) on the “basic programme” under Torres Bodet and “operational activities” under Evans.} In the beginning SSD was too small – with only a handful of people – to motivate an internal structure with separate divisions, but as it set out to realize its prioritized program, the department was soon to be organized accordingly. By Mid-November 1948, the SSD staff was organized into four divisions – with a Head’s Office (1 Acting Head + 3 administrators), “Tensions Affecting International Understanding” (1 Head of Project + 2 Program Specialists + 1 Program Assistant + 1 Junior Analyst + 2 Secretaries), “Study of International Collaboration” (1 Head of Project + 1 Program Specialist + 1 Program Assistant + 2 administrators) and “Methods in Political Science” (2 Program Specialists + 2 administrators) – mirroring not least the major undertaking during the period, the so-called Tensions Project.\footnote{UNESCO Archives, H.S./9/1947, 470115. The SSD staff was organized into four divisions – with a Head’s Office (1 Acting Head + 3 administrators), “Tensions Affecting International Understanding” (1 Head of Project + 2 Program Specialists + 1 Program Assistant + 1 Junior Analyst + 2 Secretaries), “Study of International Collaboration” (1 Head of Project + 1 Program Specialist + 1 Program Assistant + 2 administrators) and “Methods in Political Science” (2 Program Specialists + 2 administrators) – mirroring not least the major undertaking during the period, the so-called Tensions Project.}

Another crucial and – as it would turn out – recurrent problem, emerging from the complicated UN system with its different levels, was concerned with organizational overlappings, that is, “what scope and role was to be assigned to UNESCO generally, and to its Department of Social Science [sic!] in particular, within the United Nations group” (Lengyel 1986: 17). Although UNESCO’s constitution strongly encouraged organizational collaboration with the UN as well as other intergovernmental and international non-governmental organizations “whose interests and activities are related to its purpose”, problems of overlap with other special agencies such as ILO and WHO as well as the Economic and Social Council (ECOSOC) surfaced at an early stage.\footnote{Brodersen (1956: 405) mentions one example in jurisprudence and another one on town and community planning. C.f. Lengyel (1986: 3-4, 17, 87-95, 113) on the problem of UNESCO’s “competing functionalist polycentrism” and “double hybridization”.} In 1947, for instance, UN’s Social Department planned to set up a whole Educational, Scientific and Cultural Division – which would have completely duplicated UNESCO’s existing scope – which urged UNESCO to remind that UN should have an exclusively coordinating interagency function and not a program-implementation role (Boel 2016: 155). For SSD these organizational overlappings meant that early pilot projects were sometimes abandoned in order to avoid duplicating similar initiatives under consideration by other UN agencies.\footnote{UNESCO 2004: 19, Article XI. See also UNESCO, The Programme of Unesco in 1948 (1947): 24-25.}
An even more central and fundamental problem concerned the object of SSD itself. “It rapidly became evident that the very expression ‘social science’ meant widely different things in different countries”, the editorial to the very first issue of *International Social Science Bulletin* explained when summarizing SSD’s work during its first eighteen months (ISSB 1949: 9). Already at the meetings of the Social Sciences Committee of the UNESCO Preparatory Commission there had been repeated comments about the “wide national variations in the definitions and conceptual structure of the social sciences”, the “flexible character of the social sciences themselves”, and “the vagueness of the term ‘social sciences’”. SSD staff members during this phase – like Hadley Cantril and Marie-Anne de Franz – have in similar ways testified “that the term ‘social science’ meant quite different things to the French, the British, and the Americans” (Cantril 1967: 125) and that “[m]eticulous spirits often requested Unesco in those early days to proceed to a ‘definition’ of the social sciences” (Franz 1969: 406). Brodersen (1956: 401) explained in more detail the latent conflict between different traditions and conceptualizations: “The French, for instance, tended to give it the wider meaning of human sciences, including philosophy and the liberal arts, whereas English-speaking people usually defined it in a more restricted sense.” The conceptualization of social science was in this respect not only a terminological issue, but also a principal question about “negotiations with a view to delineating the boundaries of scientific disciplines” as well as an organizational question with practical implications for the division of labor between the departments and how their respective unifying objects should be defined.\(^{36}\)

Interpreted with David Guston, the SSD’s organizational problems in general and the conceptual disagreements in particular during this early phase, I argue, may well be understood as a lack of necessary boundary objects, that is, common objectives plastic enough to offer shared reference frames for the heterogeneous participants and different traditions involved, and robust enough to make successful collective action possible (Guston 1999: 93, 2000: 109; Star & Griesemer 1989: 387; Fujimura 1992: 168). Hence, when the first issue of *International Social Science Bulletin* was launched in early 1949, the editorial admitted that “the social sciences of Unesco found considerable difficulty in getting under way” (ISSB 1949: 9).

In spite of these problems, several activities were initiated during this phase – although Brodersen admits that the projects often were “rather loosely coordinated” and initiated from a pragmatic “shot-gun” approach, covering vast ground by minor attacks in many different directions” (Brodersen 1956: 403, 407). Among these projects were first and foremost the mentioned “Tensions Project”, in 1950 described as the “oldest and largest undertaking of the Social Science Department” investigating “the factors in the human mind and in cultures and societies which positively or negatively affect international understanding and peace” (Angell 1950: 282-283). Originally named “Tensions Conducive to War”, the project was renamed several times over the years – from “Tensions Crucial to Peace”, through “Tensions Dangerous to Peace” and “Tensions Affecting International Understanding” to “Studies of Social Tensions” – in a way that reveals its successively displaced focus from being centered on the psychological causes of war, to questions about how to foster peace and then to more general questions about international understanding.


\(^{36}\) Brodersen 1956: 401. C.f. Petitjean 2006: 48; Lengyl 1986: 11 on the “persistent tension between the focused, relatively concrete and, if possible, quantifiable lines favoured by the English-speaking countries, the Scandinavians, the Dutch and a number of others, and the synthesizing and moralising Latin tradition with its emphasis on long-term endeavors and tolerance for intangible outcomes.”
(Rangil 2011: 8n10). However, as Brodersen (1956: 405) clarifies, “this was never a single project, but a cluster of at least half a dozen”. A list of its most important participants over the years – Edward A. Shils (Chicago and London), Nathan Leites (Yale), Henry V. Dick (Tavistock, London), Hadley Cantril (Princeton), Otto Klineberg (Columbia) and Robert C. Angell (Michigan), the three latter formally titled heads of the project – illustrates its firm anchorage in American social psychology.37 The Tensions Project was institutionalized as a separate division of SSD from 1948, until it merged with and became a part of the division of “Applied Social Science” in 1952 (Brodersen 1956: 405-6).

In a similar way a second project on “International Cooperation” was institutionalized as a separate division from 1948 under the leadership of the American political scientist Walter Sharp (Yale). The project aimed at studying collaboration in modern large-scale international organizations and included several meta-studies on international collaborations. It became an integral part of Unesco's social science program in the early years and resulted among other things in a special issue of the International Social Science Bulletin on the “The Technique of International Conferences” and a book on Program-Making in Unesco 1946–1951 by the American professor of public administration Charles S. Ascher (Brooklyn).38

Relatively soon, however, it became clear to Brodersen and the SSD Secretariat that the most robust way “to help the social scientists of all countries develop ways and means by which they could best co-operate with each other so as to increase the scientific strength on a world-wide scale” would be to establish comprehensive networks of what they referred to as “single-disciplined bodies”, that is, separate international associations for each discipline. Such cooperation would be “both easier of achievement as a permanent feature, and also in some respects more productive than that involving scholars from different disciplines”, Brodersen argued. As professionals in a common field they would per se be more “familiar with each other's problems and language” and united by “bonds between them before they ever meet” (Brodersen 1956: 402-3). The three first associations – the International Political Science Association (IPSA), International Sociological Association (ISA) and International Economic Association (IEA) – were all set up in 1949, whereas their counterparts in comparative law and psychology followed in the two coming years.39

Brodersen in retrospect self-critically summarized SSD’s activities during his term as “ad hoc pieces of research” and as incidental “projects of the ‘fire-fighting’ kind”. Of these several were interrupted while still in their infancy and no single project “was probably more productive in terms of results in the field”. But there were also other, less visible foundations being laid down, he argued:

The relatively most important staff activities at this stage were perhaps not those which figured most conspicuously in the budget as project proposals, but rather those devoted to the quiet and patient study of the situation in the social sciences [...] the gradual

37 On the informal impact of the Society for the Psychological Study of Social Issues (SPSSI), see Selcer 2009: 309n1; 2011: 89ff, and UNESCO Archives, 3A01UNG.
39 Platt 1998; Boncourt 2015. The International Union of Anthropological and Ethnological Sciences (IUAES) was founded in 1948, but did not belong organizationally to the SSD but to the Philosophy and Humanistic Studies Section, although it later, from 1952, was represented in the ISSC.
establishment of contacts, by correspondence and face to face, with men and women of the profession wherever they could be reached. (Brodersen 1956: 403-404)

By bringing together prominent and engaged international social scientists and by providing a new transnational platform, collaborations and gradually extended networks, partly institutionalized in new professional associations, UNESCO’s SSD contributed with what one of the staff members called the “international spade-work concerning the infrastructure” (Franz 1969: 407). Although this “essential part of the initial groundwork” for international social science, according to Brodersen (1956: 404), far from followed “a general plan in a long-term and large-scale operation”, it is still worth noting, I hold, that the “infrastructures” being laid down had its central junctions, encouraged a certain kind of communication and directed the intellectual traffic in some directions more than others. The emerging networks of prominent social researchers were with few exceptions centered in the USA, as Selcer (2009: 314, 317) observes, usually with a rotating series of American scholars in the central posts as research leaders or presidents of the international associations, whereas the operational secretary functions often went to Europeans, hence establishing a structural trans-Atlantic beam. A second pattern is that the emerging international social science was built on discipline-based organizational structures, and of the disciplines contemporary American social psychology and public administration in particular served as models (c.f. Backhouse & Fontaine 2010: 207-216). Third, the “international” component of SSD’s enterprise was largely implicitly interpreted in terms of a relatively one-way directed social knowledge transfer across the Atlantic to different countries in Europe and other parts of the world (c.f. Myrdal 1951: 157).

The foundation laid during Brodersen’s term was further refined by Robert Angell during his period as Acting Head of SSD, with an even more marked disciplinary approach, a slight sociological twist, and an even stronger emphasis on American research. In late December 1949, for example, Angell in his double role as Acting Head of SSD and Director of the Tensions Project gave a speech to the American Sociological Society – an association that he would become the President of only one year later – that was published in American Sociological Review – a journal that he had been editing during the previous three years (1946–48). In the speech he did not regard the American dominance within SSD as a problem, but quite the opposite as a risk if his colleagues failed to contribute to UNESCO: “There is always the danger that an international secretariat will become isolated from the most dynamic currents of research”.40 Another of UNESCO’s problems, pointed out by Angell, concerned its lack of organizational stability and short planning horizons: “the grouping of studies within the Social Science Department has shifted between 1949 and 1950, and threatens to shift again between 1950 and 1951” (Angell 1950: 282). He probably did not know by then how right he would be about this forecast only a few months later.

REVITALIZATION AND CONSOLIDATION, 1950–1953

From around 1950 a new phase in SSD’s early history is discernible, characterized by both revitalization and organizational consolidation. Although several practical outcomes during this phase emanated from the previous period, there were also a broad and varied range of new

initiatives and activities that expanded and renewed SSD’s scope and status to the degree, I argue, that it is motivated to speak about the SSD as an almost ideal-typical international boundary organization during this third phase. This marked shift happened to co-occur with yet another change of the leadership, as observed by several scholars (Ekerwald & Rodhe 2008: 168; Rangil 2013: 86; Sluga 2015: 64, 66). Selcer, for example, notes that “SSD suffered from disorganization due to lack of steady leadership until the dynamic Swede Alva Myrdal [took] over the department in 1950” (Selcer 2009: 314). Myrdal herself witnessed already in 1952, in a private letter to her husband: “Everybody affirms that I personally set the Department on its feet.”41 As will be argued, these observations will give us reason to pay closer attention to the question about agency space especially on the program director’s level during this phase.

When Alva Myrdal took up the job as head – from this moment formally upgraded to the title of Director – of SSD on 28 August 1950, she actually moved downwards in the UN hierarchy. As Director of the Department of Social Affairs at the UN headquarters in New York, on the “third level from the top”, under Secretary-General Trygve Lie and Assistant Secretary-General Henri Laugier, she had been the highest-ranking woman in the whole UN organization – and remained so as Director of SSD (Ekerwald & Rodhe 2008: 153; Sluga 2015: 51). Although the primary reason for changing office was private and family-related, the tasks that awaited her in Paris were in no way new to her.42 Her commitment to the social sciences, especially education and social psychology, can be traced back to the 1920s. And when she and her husband, the economist and sociologist Gunnar Myrdal, went on a Rockefeller stipend to the USA in 1929–30 her engagement became even more marked. From this moment on they both became ardent advocates of interdisciplinary applied social science.43 Her international career really took off when she moved to the UN headquarters in 1949. But already in 1946 she had attended UNESCO’s inaugural General Conference as observer and actually also been offered a post by Julian Huxley.44 When Torres Bodet asked her anew in March 1950 she was not only already familiar with UNESCO, its mission and early development, but had by then also acquired a superb overview of the entire UN bureaucracy as well as practical experience of working inside it.45 Furthermore, the tasks of UN’s Social Affairs and UNESCO’s SSD were partly similar – some would probably say unsatisfactorily overlapping – an issue that Myrdal had brought up in her discussions with Torres Bodet.46

Hence, when entering the office as SSD Director, Myrdal was well prepared and immediately started to outline the plans for SSD’s programme for the coming years.47 In January 1951 she typed a manuscript entitled “The Cost of National Isolation in the Social Sciences”. In this programmatic

text Myrdal problematized the “immaturity of the social sciences” and the lack of “international pooling”, which according to her view resulted in a heavily imbalanced “system for stimulation between social science developments in different countries”. In this situation, Myrdal envisioned: “Unesco’s role is highly important, as it just consists in bringing into international focus the research that is carried on in disconnected centers over the world”.48 Her social scientific internationalism, as expressed in this early manuscript, basically remained intact during her term as Director – although some minor displacements are discernible during the latter half of the period.49

In comparison with Brodersen and Angell, there are both important similarities and differences. All three were in agreement that contemporary U.S. social research was to be seen as a model. In a lecture held in New York in 1955, for instance, Myrdal suggested that “Its advance in social science might be America’s greatest gift to the art of international social welfare” (Myrdal 1955: 44; italics in original). But in contrast to Brodersen’s “single-disciplined” strategy and Angell’s promotion of U.S. sociology, Alva Myrdal (like her husband) always remained truly interdisciplinary in her problem-oriented approach. In that sense she both followed in the footsteps of Brodersen and Angell and widened and partly redirected the scope of SSD. In practice, the many activities of SSD during this phase form a pattern that mirrors both the similarities and differences between, on the one hand, Brodersen’s and Angell’s discipline-based and U.S.-centered conceptualizations of international social science and, on the other, Myrdal’s U.S.-influenced and pragmatic social scientific internationalism as well as her more interdisciplinary and polycentric ambitions.

Among the initiatives inherited from the previous phase were, as mentioned, the creation of the pioneering international associations of political science, sociology and economics (all set up in 1949). These were accompanied by their counterparts in comparative law (ICLA 1950) and psychology (IUSP 1951) and later also – through affiliations with pre-existing bodies – criminology (ISC) and population studies (IUSSP). More significant though is that this discipline-based institutional infrastructure was complemented in 1952 by a new organization when the International Social Science Council (ISSC) was set up as an interdisciplinary coordinating body which, according to Lengyel (1986: 20), “has done more than most other formal efforts to internationalize the social sciences”.50

In similar ways, SSD’s first major effort from the early years, the loose-knit Tensions Project, bore fruit and resulted in a minor cascade of publications from 1950 and onwards (see Lengyel 1986: 22-23). At the same time these publications partly marked the end of the dominant social psychological paradigm, which during the period was smoothly phased out (Rangil 2011: 41). Significantly, the UNESCO division “Tensions Affecting International Understanding”, was merged and incorporated into the new division “Applied Social Science” in 1952 under Franklin Frazier’s and, from 1954, Otto Klineberg’s leadership.51 These organizational changes were accompanied by


51 UNESCO Archives, X0755 Parts III-IV; Reports of the Director-General, etc.
a displacement of SSD’s focus from questions concerned with the origin of warfare to more general issues on international welfare. From 1950 onwards SSD became involved in the so-called Technical Assistance (TA) program, an enormous UN initiative focused on aid for economic development and coordinated with the U.S. Government’s “Point Four Program” as well as numerous specialized agencies, non-governmental bodies and private funding agencies such as the Ford Foundation. At UNESCO a separate Department of Technical Assistance was set up in 1952, which collaborated closely with the SSD department in particular. The reason was that the TA program assigned the social sciences in general and anthropologists in particular a key role, especially after Margaret Mead’s influential 1953 report on Cultural Patterns and Technical Change which emphasized the “dangers of technical assistance” if it was not combined with a deepened knowledge and understanding of the local cultures in question.52

Another project founded and prepared before Myrdal entered UNESCO, was the project on race and discrimination, initiated by Arturo Ramos and others, which resulted in a UNESCO Conference in 1950 and the book series “The Race Question in Modern Science”. This was accompanied by new anti-discriminatory initiatives by SSD on women’s political role and the United Nations Commission on the Status of Women project 1952–53 – all of which Myrdal advocated behind the scenes – as well as more general population and welfare-oriented projects.53

Before 1950 most of SSD’s activities had been centred along the trans-Atlantic axis connecting US social research and the UNESCO headquarters in Paris. In 1951 a first social science field mission was organized. And during the period social science officers were being attached to the already existing UNESCO Science Cooperation Offices in New Delhi and Cairo set up under Joseph Needham’s pioneering directorship in the Natural Sciences Department (Franz 1969; Elzinga 1996b; Petitjean 2008). Other initiatives aimed at strengthening the international infrastructure of the social sciences, included a country-wise survey of university teaching in the social sciences, documentary services, terminological issues and several journals (Lengyel 1986: 20; Franz 1969: 406). These initiatives were also mirrored in SSD’s internal organization with separate divisions for “Aid to International Scientific Cooperation” and “Science Cooperation offices”, respectively.

Taken as a whole, during Alva Myrdal’s directorship SSD expanded its staff: from around ten people in 1949, to some twenty staff members in 1952, and to over 40 in 1955.54 The budget expanded accordingly, from less than $300,000 in 1949, to somewhat over half a million in 1953, and over three quarters of a million by 1956 (Lengyel 1986: 2; Rangil 2011: 8). To sum up in more qualitative terms, the department developed from a discipline-based organization towards a more interdisciplinary one, with a displaced focus from universal causes of warfare to pluralistic conditions of development, population and international welfare, where the social-psychologically

54 The more exact numbers depend on which staff categories are included. Rangil (2011: 7) focuses on the permanent staff and counts less than 12 in 1951, 24 in 1952; 48 in 1953-56, and 53 in 1959-60. The expansion by 1952, is primarily explained by the inclusion of a whole statistical division, besides the “applied social science” and “international scientific collaboration” divisions.
oriented Tensions Project was replaced by activities related to the more anthropologically-oriented Technical Assistance program as its major undertaking.

Viewed through the lens of Guston’s definition of boundary organizations – as mediating and stabilizing institutions, characterized by different worlds participation, workable boundary objects, and delegation of authority between principals and agents – and my additional revisions as mentioned in the introduction, I argue that SSD during this phase, and in marked contrast to the previous one, stood out as an almost ideal-typical international boundary organization by late 1952. However, as Peat Leith et al. (2015) point out, Guston is not always clear about what it is that makes a boundary organization successful. To answer that question, Leith et al. underline the importance of “exemplary leadership” and the “abilities to navigate controversy and mediate among divergent interests, while maintaining a committed focus on science”, but also that stability should be seen as “a precursor to success rather than a measure of it” (Leith et al. 2015: 376, 392, 395). This may – together with the accounts by Ekerwald and Rodhe, Selcer, Rangil and Sluga referred to – give the impression that it was Myrdal who, in line with her own statement, “put the department on its feet”.

Undoubtedly Myrdal made a difference, and probably a very important one. But still it is worth recognizing that the positive development during this period was not isolated to the Department of Social Sciences but rather part of a more general UNESCO trend. At least Jaime Torres Bodet’s summary in his Director-General’s Report for the period April 1951 to July 1952 was that: “Remarkable progress has been made during these 15 months”. And when James P. Sewell analyzes UNESCO’s political leadership he finds – maybe unfairly – that Myrdal had a “few solid accomplishments […]. But audacious innovation was difficult, particularly at this time” (Sewell 1975: 184). My point here is that these two latter voices should encourage us not to close the door on alternative interpretations and to avoid too simplistic explanations that reduce the question about organizational change to the role of single actors, albeit in a leadership position.

This gives us reason, at this stage of the argument, to expand on the notion of agency space. Its analytical strength, I suggest, is that the notion helps us to avoid both the scylla of structuralist reductionism and charybdis of methodological individualism. Instead “agency space”, as a middle range concept, reformulates the abstract relationship between structure and actor into two empirically investigable research questions. First, how did the organizational changes affect the agency spaces available (in this case on the level of director of SSD)? Second, how did the actor (in this case Myrdal) actually make use of this space? Thus reformulated the first question draws our attention to the relatively wide agency spaces available during UNESCO’s early years, including when Myrdal assumed the post of SSD’s Director. Both Huxley and Torres Bodet were supporters of UNESCO as a relatively autonomous and independent international organization peopled by strong-minded and creative intellectuals with a relatively large freedom to translate UNESCO’s abstract ideas into practical action. Lengyel partly hints at this wide agency space when he describes SSD during the formative years as a relatively flat, informal organization composed of “a small, closely knit managed team”, which established “fruitful relations with widening circles of external collaborators” and further exemplifies: “Much was expedited directly, through personal relations, at very modest cost and with minimal formalities, in the spirit of collegiate adventure” generated by “group dynamics emerging from expert meetings or conferences” and characterized

55 Torres Bodet, DGs Report April 1951 to July 1952, 11.
56 See note 7.
by “a probing flexibility” based on the fact that “Unesco was not yet a highly centralized institution” (Lengyel 1986: 18-19).

The second and quite different question, then, is how the individuals in the organization actually made use of these wide agency spaces. For some, like Brodersen, this freedom was rather seen as a lack of clear and workable guidelines, amplified by a problematic gap between utopian hopes and practical concrete action. For others, like Alva Myrdal, the very same gap probably appeared as a challenging opportunity space. When entering SSD, at a stage when it had suffered from several organizational problems that had been worsened by a lack of firm leadership, it could in a sense almost only get better. That said, Myrdal indeed took the chance to bring in new ideas, new energy and enthusiasm, to make use of her extraordinary qualities, including her witnessed capability to transform visions into practice, and to introduce new working routines. Among the latter were not least, I argue, her cross-organizational collaborative approach and her skills in orchestrating diverse interests and activities in multiple domains – what Miller (2001: 487) calls “hybrid management” – partly based on her experiences from working in similar boundary organizations, both international ones (at the UN) and domestic ones (in Sweden). In a private letter to the Swedish Minister of Social Affairs, for instance, she explicitly referred to her long experiences from a number of domestic Royal Commissions that she had participated in during the interwar period and wrote: “All that I ever learned from commission work in Sweden has now come to use”, including how to mediate among different groups of interests and how to plan and coordinate action in an efficient way. Even more important, though, is that she did not introduce this cross-organizational collaborative way of working only on her own initiative. The frequent correspondence between Myrdal and Torres Bodet that preceded her decision to accept the position, clearly reveals that her unique experience from the UN headquarters was meant to be used constructively, both in order to help coordinate the overlapping and sometimes conflicting interests between SSD and “the ‘social role’ of other UNESCO and ECOSOC activities” and by “making the Department of Social Sciences more of a general service bureau for the whole of UNESCO’s program” as well as to foster more generally the “social applicability” of UNESCO programs. This is also key, I argue, to understanding the relative success with which she managed to anchor and link up the social sciences as a vital component on multiple levels, from the SSD level (Tensions Project) over the UNESCO level (Fundamental Education) and not least to the general UN level (Technical Assistance, Race, Women, Human Rights, Population, Development, International Social Welfare ) – in contrast to her predecessors who were to a larger degree restricted to single disciplines and had a more concentrated focus on the SSD level.

Interpreted in terms of the fourth criterion mentioned by Guston, the principal-agent relation, it could be added that Torres Bodet repeatedly emphasized the role of social science and had great confidence in Myrdal’s capacity and integrity as Director, whereas she had assured herself already when accepting the post that she would “have free access” to Torres Bodet in order to secure “a creative cooperation” free from unnecessary “administrative arrangements”. In that sense the delegation of authority and integrity was based on a stable agreement of mutual trust. Although UNESCO’s SSD as an international boundary organization was characterized by a marked stability

59 ARBARK 405/4/1/7/6, Torres Bodet, “UNESCO and the Social Sciences”, Speech to the University of Ljubljana [1950], UNESCO/DG/146; ARBARK 405/4/1/7/4, A. Myrdal – J. Torres Bodet, 17 April 1950.
in that respect, the following phase will show that the achieved stability was not that long-lasting after all.

**GEOPOLITICAL RE-ORGANIZATION, 1953–1955**

Roughly by the time of Alva Myrdal’s mentioned positive self-assessment in late November 1952, a significant multilevel transformation of UNESCO’s SSD was initiated from above and partly outside the organization. During the following seven months a complicated chain of events evolved in which three of the most significant manifestations were Jaime Torres Bodet’s early resignation as Director-General, the U.S. Government’s introduction of an International Organizations Employees Loyalty Board, and the installation of Luther Evans as new Director-General. Together these changes laid the ground for two discreet and seemingly minor constitutional amendments of the UNESCO Statutes at the Montevideo General Conference in November 1954. The two amendments, no matter how marginal they may appear at first glance, I argue, radically changed not only the relative autonomy of SSD in general and the agency space of its Director in particular, but also the organizational status of UNESCO at large as well as its crucial principal-agent relations.

Jaime Torres Bodet’s declaration of his early resignation as UNESCO’s Director-General at the General Conference in Paris on 22 November 1952, one year before his mandate elapsed, did not come as a total surprise. Although Torres Bodet had had broad and strong support when he succeeded Huxley as Director-General in Beirut in 1948, there had been a growing conflict between Torres Bodet’s energetic visions for UNESCO and some of the most important financial supporting member states.60 Already at the conference in Florence in 1950, Torres Bodet planned to resign with reference to the budgetary restrictions and an emerging critique against his way of leading the organization. At that time, in Florence, he was persuaded to stay on. When the issue about the budgetary needs of UNESCO resurfaced in 1952 – when Torres Bodet had asked for $20 million but was confronted by a cutback of the budget of 7.8 per cent and the introduction of a provisional budget ceiling proposed by the Delegations of USA, United Kingdom and France – he saw no other recourse than to resign his post.61

There were of course two sides of the coin. From Torres Bodet’s point of view he had been recruited to the organization with a long suitable merit list, including a term as Minister of Education in the Mexican government where he had led a successful campaign against illiteracy. He had also been an ardent advocate of both the UN and UNESCO, which he had followed closely at the CAME and Founding conferences, and in them saw “the noblest and most important [initiatives] that men have been able to conceive” (quoted from Sewell 1975: 128-130; cf. Petitjean 2006: 31). When approached as a nominee, he had also spoken in favor of a more concentrated program – a plan which he partly followed with the large “Fundamental Education” program. Nevertheless, he declared, as UNESCO’s work and not least the world had evolved, the budget question was of principle importance since programs had to be expanded if UNESCO was to advance. In this situation, Torres Bodet motivated his resignation: “You had the choice of three possibilities:

60 When elected in 1948, the nomination of Torres Bodet was endorsed by a vote of 30 to 3 (Sewell 1975: 128).
The U.S. Government, represented by the U.S. National Commission, on the other hand, had from the very beginning been one of the most substantial funders of UNESCO. In 1947, for instance, the USA contributed 44 per cent of UNESCO’s total budget – and together with the shares of the United Kingdom and France, amounting to 14 and 7 per cent, respectively, two thirds of the whole budget. Over the years the U.S.’s share successively declined, to 35 per cent in 1950 and to one third in 1951. But from that point of view it was not unreasonable for these National Commissions to expect that their opinions should be paid relative weight. Neither should it be a surprise that those commissions were the ones speaking most eagerly in favor of a more restricted and efficient use of the money, including the recurrent argument that UNESCO’s program should concentrate on a smaller number of major projects rather than be spread out over numerous minor ones (see Düring 1953: 11, 13). Partly because of this both the U.S. and the French, as well as the British, delegations had been skeptical about Julian Huxley’s energetic but also idealistic – and very costly – visions. In that sense, Torres Bodet followed in the footsteps of Huxley (Sewell 1975: 17).

But there were also factors other than the monetary aspects playing a role. On the geopolitical level, Torres Bodet was greatly annoyed by the U.S. request that UNESCO should support the UN military support to Korea in 1950, and took a clear stance at the General Conference in Florence, with the support of India, and refused to act as a “political instrument in the cold war”. After this event the U.S. Government developed a much stricter financial policy towards UNESCO. According to some observers this was deliberately to weaken the organization, whereas others have seen the Florence conference as a turning point for U.S. control over UNESCO (Petitjean 2008: 266-267; c.f. Sewell 1975: 140; Düring 1953: 13).

Even more important in this context than both the budget and the geopolitical events are however, I argue, the fundamental principles that were at stake regarding different forms of internationalisms and UNESCO’s status as an international boundary organization. Like Julian Huxley, Jaime Torres Bodet was a strong proponent of a one-worldism according to which UNESCO was to be seen as a relatively autonomous transnational organization, serving as a kind of world intellectual conscience, with a staff of international officers committed to the general task of contributing to a better world. The idea of institutional self-determination, that is, that UNESCO should not be the object of control by anyone except its participants – in contrast to the UN as a more politicized intergovernmental organization – was not unique among UN’s specialized agencies. It was also to this idea and UNESCO’s original utopian one-worldism that Torres Bodet referred in his farewell speech in Paris in 1952: “May Unesco one day develop its program as we who had the privilege of being present at its birth in London, 1945, dreamed that it might.”

On this point there was a direct confrontation with the U.S. Government and the U.S. Delegation which since the very inception had spoken in favor of an internationalism based on nation-states as the basic units and actors, for which the international organizations were primarily a means for

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62 On the close relation between the U.S. National Commission and the U.S. State Department, see Selcer 2011: 108.
64 Sewell (1975: 72-73, 134) mentions the institutional self-determination of the International Bank for Reconstruction and Development (IBRD) and the International Monetary Fund (IMF); c.f. Petitjean 2006:31 on UNESCO as a hybrid organization; Lengyel 1986: 8.
handling international relations. As the “utopian moment” in the direct aftermath of the end of the war faded away and the cold war geopolitical tensions increased, the inherent and potential contradiction between the two types of internationalism – cosmopolitan one-worldism and intergovernmental realpolitik – became more strained, which in its turn made the status of UNESCO and its staff appear an ever more concrete problem for the U.S. Government. According to President Truman the choice stood between “communism or democracy”.66 And for Assistant Secretary of State for Public Affairs Howland Sargeant, UNESCO was an instrument to strengthen the latter, he explained in a talk to the international conference of U.S. National Commission for UNESCO in January 1952:

We Americans cannot go it alone. We need the other free peoples, even as they need us. Freedom as we know it is being subjected to an assault which has had no parallel in modern history. And we who believe in freedom must meet that assault together.”67

On 9 January 1953, that is, only one month after Torres Bodet’s resignation, President Truman introduced his Executive Order No. 10422, which stipulated that all UN employed American citizens should be investigated by an International Organizations Employees Loyalty Board in order to prove their loyalty towards the American Government – in the wake of the hunt initiated by Senator McCarthy for subversive elements within international organizations.68 When UNESCO was contacted by the U.S. Government and asked for help to distribute the loyalty forms to its staff, this was however directly at odds with several principles in UNESCO’s constitution and staff regulations. The Constitution, for example, proclaimed that the responsibilities of UNESCO’s staff “shall be exclusively international in character” and that they “shall not seek or receive instructions from any Government or from any authority external to the Organization”, whereas the Staff Regulations stipulated that the staff “as international civil servants [...] shall at all time exercise the reserve and tact incumbent upon them by reason of their international responsibilities”.69 UNESCO’s Acting Director-General, the American John Wilkinson Taylor, in consultation with the Executive Board, tried to find a compromise solution and meet the US Government halfway, with the result, however, that already in May one of the American staff members of UNESCO who failed to turn up in front of the Loyalty Board was suspended. UNESCO’s Staff Association reacted directly by formulating a statement of protest in which, among other things, they argued that “[t]he action risks conveying an impression of Unesco having submitted to national pressure”.70

But the external pressure was not isolated to UNESCO’s American staff members. In March 1953, on one of her many visits to the UN headquarters in New York, Alva Myrdal was directly troubled by the effects of the new and stricter U.S. policy towards international civil servants, when she was stopped by the U.S. Immigration Authorities at Idlewild Airport – despite her official UNESCO Travel Order, a UN Laissez-Passer and a non-immigrant visa. The remarkable event immediately generated extensive international media attention, numerous formal as well as informal and

diplomatic contacts on all levels, inside UNESCO, with the U.S. Immigration Office, the Swedish Minister for Foreign Affairs and all the way up to UN’s Secretary-General. For Myrdal, however, who as usual was keen to sort things out and understand the full picture – and also documented the event and the correspondences in detail – the problem was not that the incident had caused her great practical problems that severely affected her tight time schedule as Director but, more importantly, that it concerned the more general principles of the status of “international civil servants” and the “[f]reedom for Unesco staff members to travel”, which meant – even more importantly – that “the integrity of UN and Unesco was at stake” and the way this may “damage the Organization itself”.71

When Luther Evans assumed office as Director-General on 1 July 1953, and replaced Taylor on his six-months interim period as Acting Director-General after Torres Bodet’s resignation, Evans more or less directly had to handle the principal questions regarding UNESCO’s status as an international organization, including the rights of its staff as international civil servants and its relations to the member states. For Evans, however, these issues were far from new. With his background as a political scientist and Chief Librarian of Congress, and more importantly as a former member of the U.S. delegation to the first CAME Conference and later member of U.S. National Commission for UNESCO, where he had held the positions of both Vice-Chairman and Chairman, Evans had followed the birth and growth of UNESCO and knew the organization from within.

In fact Evans was not only familiar with but had also played an active part in the development of the U.S. policy towards UNESCO’s internationalism and the principal issues on UNESCO staff members’ status as international officers versus citizens of their home countries. Already in the U.S. National Delegation’s meetings in late October 1945, in preparation of the UNESCO Founding Conference, Evans participated in the discussions of the draft constitution in which he “thought he saw an expression of a desire to undermine governments” (Evans 1971: 35). As one of UNESCO’s most explicit political realists, Evans never doubted that governments were the ones who made UNESCO’s choices. In line with the same argument he was of the opinion that the members of the Executive Board should represent their respective national governments:

Unesco is definitely an intergovernmental organization, subject to the limitations and procedures inherent in official action, but firmly based on the machinery of government within our Member States including the National Commissions. [...] The fact remains that Unesco works for its Member States, that it works largely through the governments of Member States, and that its success or failure in any Member State is a direct outcome of the degree of understanding and support it enjoys on the part of the government of that State (quoted from Sewell 1975: 166).

And when the U.S. Government in 1950 tried to convince UNESCO about the so-called “containment doctrine”, that is, that international organizations contained subversive elements, Evans in his role as Vice-Chairman of UNESCO’s Executive Board firmly supported the U.S. standpoint that UNESCO should awaken the conscience of the world with regard to security (Sewell 1975: 149; S.E. Graham 2006: 245). A couple of years later, when he had advanced to Chairman of the Board Program Commission in 1952, Evans was in the forefront about “program foci” and a frozen budget, in opposition to Torres Bodet’s expansionist policy – and hence actively

71 ARBARK, 405/4/1/7/7, A. Myrdal, “Memorandum to the Acting Director-General”, p. 7.
contributed to Torres Bodet’s resignation (Sewell 1975: 153). And one of the very last things Luther Evans did in his capacity as Chairman of the Executive Board, before assuming the post as Director-General of UNESCO, was to present a draft resolution in which he proposed that it should be clarified once and for all “that Unesco is an organization of sovereign states” and “that it does not advocate one world government”.

Evans’s views were on the whole in harmony with the U.S. Government’s official policy towards UNESCO. The latter was explicitly expressed in a 34-page report entitled *An Appraisal of the United Nations Educational, Scientific and Cultural Organization*. In it the current Chairman of the U.S. National Commission Irving Salomon and his co-authors explicitly raised the question about UNESCO as a container of communist sympathies. Furthermore it defended the legitimacy of the Loyalty Board, regretted that the Director-General was not empowered to suspend its staff in the same way as in the UN and emphasized that: “It is the view of the U.S. Government that the members of the Board should represent their respective Governments, not themselves”. For these reasons the report underlined the need that “UNESCO’s constitution should be revised” and “that the Executive Board be composed of representatives of Member States, rather than consisting of a group of individuals”. Furthermore, it was argued, in line with the traditional U.S. budget policy that “UNESCO could use its limited resources more wisely” and not try to “cover too many activities”. Instead UNESCO should plan its activities in accordance with a “system of priorities”, preferably the one introduced by the U.S. delegation at the General Conference in 1952. Although most points in this policy were not new, there were now, in view of Evans’s new position, unusually good hopes that they could be realized, Salomon argued:

> It may be anticipated that the new Director-General, who has been a member of the United States delegations to all but one of the Sessions of the General Conference will carry into his job the convictions which he had demonstrated when speaking as United States delegate.

On one point after the other Evans would also, as expected, enforce the mentioned policy. Already in his inaugural statement as the new Director-General he made it clear that he identified himself not as a cosmopolitan intellectual but as a “professional administrator” with well-developed “administrative methods” according to which “arrangements of power” were meant “to avoid [...] confusion of purpose” and that he expected “widespread participation of the staff at all levels in the development of policy”. Furthermore, “[a]s the member of the Executive Board with the longest tenure” Evans also wanted to emphasize, first, the central function of “the Board [as] one of the principal organs of Unesco” and, second, the even more supreme role of the Member States:

> Unesco is an instrument for the increase of collaboration among the Member States. The Secretariat is not, it should not be, an independent power. It should have no goals except your [referring to the present representatives of the Member States] goals.

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74 Ibid: 19.

During the next 18 months Evans systematically and insistently implemented the U.S. policy with regard to the status of international officers and the supreme role of member states, on the local organizational level. Formally, two seemingly discreet but principally important amendments of the UNESCO Statutes at the Montevideo General Conference in late 1954 were what made this possible. The first amendment concerned the Obligations and Rights of Staff Members, where it was stipulated that “The Director-General may [...] terminate the appointment of a staff member [if] the staff member does not meet the highest standards”. The second concerned the composition of the Executive Board, where it was stipulated that each member “shall represent the government of the State of which he is a national”.76

In practice the discreet reformulations of the Statutes, in the first respect, meant that Evans was given the right to suspend the staff members who had refused to witness to the U.S. Loyalty Board, not though with reference to their lack of loyalty as American citizens but to their lack of “integrity” and incapacity to live up to the “highest standards” as expected in their role as UNESCO staff members. The new staff regulations took effect on 10 December 1954 and on the very same day seven staff members were suspended or placed on special leave.77 In the second respect, the implication was that UNESCO’s Executive Board instead of being composed of a group of individual members, more directly represented the governments. In combination with the supreme role of the Board in relation to the UNESCO Secretariat, in contrast to its previous relative autonomy, this meant that UNESCO’s work as a whole, including SSD, became organizationally and formally more dependent on the interests of the member states as represented in the Executive Board, and hence also more open and vulnerable to geopolitical pressures from outside the internal organizational structures.

With Sewell, Elzinga and Graham this constitutional change can be described as the final and crucial step in the transformation of UNESCO’s status from a relatively autonomous hybrid international organization – encompassing international non-governmental organizations as well as governmental actors – to an intergovernmental organization more directly inscribed into the contemporary geopolitical arena.78 Linked up to our conceptualization of UNESCO’s SSD as an international boundary organization, this change furthermore gives us reason to reconnect to Guston’s emphasis on the principal-agent-relation as a central component. Applied to our case, the principal-agent-theory – with its focus on the conflicting interests and the delegation of authority between the principal and its subordinated agents – allows us to highlight the organizational significance of the two amendments by interpreting the revised Statutes, almost literally, as a renegotiated contract of UNESCO’s principal-agent-relations – in a dual sense. An additional point in our case is namely to recognize that this renegotiation included two separate but interlinked parts, two different principal-agent-relations, once again almost in the literal sense, one corresponding to the first amendment (regarding the relation between UNESCO’s Director-General and its staff) and the other to the second (regarding the relation between UNESCO and its member states through their direct representation in the Executive Board). Combined, the two amendments interlinked all three organizational levels and hence fundamentally restructured the formal

76 UNESCO Archives, Records of the General Conference, Eighth session. Montevideo 1954. Resolutions, II.42 Amendments to Regulation 9.1.1, and II.1.2 Amendments to Article V.


delegation of authority and autonomy within the organization. However, whereas Guston’s approach is focused on how principal-agent-relations are used to stabilize the relation between science and politics, I argue, that it is more plausible to interpret the organizational change that took place during this phase as an example of a de-stabilization, which at least potentially meant a structural re-politicization of UNESCO’s SSD and its mission and activities.

From the viewpoint of SSD, this re-organization drastically decreased the agency spaces of both its Director and its staff, whereas the agency spaces of the Director-General, the Executive Board and member states increased. Formulated otherwise, Alva Myrdal and the SSD staff – as well as all other departments – became more directly dependent on the new key role assigned to Luther Evans as mediator and broker in this new linear top-down-structure. However, as already mentioned, the question about structured agency spaces should be carefully distinguished from the question of how these potential agency spaces were actually used. The empirical question is then how Evans chose to use his enlarged agency space in relation to SSD. An option would of course have been to let the practical day-to-day work proceed pretty much as before. As hinted at already in his installation speech, this was not however his intention. Admittedly, new organizational routines had been introduced already with Torres Bodet, but Evans’s leadership brought with them a number of principal changes and a new conception of the Organization’s work, as further clarified in an retrospective article in 1963, which included the introduction of a more direct top-down leadership, a further concentration of projects to a restricted number of “skyscraper projects”, and a redirection of UNESCO’s role from operator to stimulator, from performer to administrator of projects.

In practice this meant, as Sewell (1975: 171) notes, that “Evans acted as judge [...] of innovations advanced by others”. A crucial difference in that respect is that Evans was much less engaged in the social sciences than his predecessors. Huxley had regarded the social sciences, and especially social psychology, as part of his scientific mission, whereas Torres Bodet was not only the one who recruited Myrdal but was also eager to speak about the fundamental importance of the social sciences for UNESCO more generally. Evans, in contrast, had questioned expansionary moves by promoters of social science while still a member of the Executive Board and now as Director-General he inhibited several social science initiatives in their early stages by restricting the executive budget.

For Alva Myrdal as SSD Director the consequence was a drastically increased administrative workload. Under Torres Bodet she had become accustomed to a wide agency space and positive responses to initiatives in need of confirmation. The recurrent task of reporting on the activities of SSD for inclusion in the Director-General’s Report, for example, had been a time-consuming but still smooth bottom-up process. With Evans the reporting of the departmental activities became a much more complicated two-way process, where Myrdal’s early drafts often bounced back or were heavily revised. At other times Myrdal had to remind Evans and the Executive Board about

79 On dual principal-agent relations, where sometimes “agencies are themselves principals”, see Guston 2000: 20.
81 UNESCO/DG/146, Torres Bodet, “Unesco and the social sciences”.
proposals that “disappeared” along the way and – like many other proposals that did not fit the general agenda, according to Sewell (1975: 103) – “were gently laid to rest, quietly forgotten, or left for others”.  

Without being able to offer any more robust empirical support, I would like to suggest in more tentative terms that it is not too bold to set Alva Myrdal’s decision to leave her post in relation to her drastically decreased agency space as SSD Director during this very phase. And she was not alone in doing so. The Director of the Education Department, Lionel Elwin, who like Myrdal had been recruited by Torres Bodet, chose to leave at the same time. Others, like Paolo de Berredo Carneiro and Vladislav Ribnikar, had left the Executive Board already when Torres Bodet resigned in 1952 (Sewell 1975: 122, 154-5). Myrdal stayed on that time. But it is probably no co-incidence that in late December 1954, more or less directly after the constitutional changes had been accepted by the General Conference in Montevideo, with its far-reaching consequences for UNESCO in general and for her work at SSD in particular, she sat down and drafted the very first version of a private letter that only a couple of months thereafter would result in a new job offer. Less than one year later, on 3 December 1955, Alva Myrdal took up the post as Sweden’s first woman envoyé, later Ambassador, at the Swedish Embassy in New Delhi.  

CONCLUDING DISCUSSION

This paper has aimed at analyzing the creation and early formation of UNESCO’s Department of Social Sciences during its first decade with a special focus on its organizational aspects. Interpreted as an “international boundary organization”, that is, as a transnational institution that mediated the relation between science and politics during the early post-World War II period, and with regard to the intra- and inter-organizational structuration of multilayered agency spaces, it has been argued that SSD went through a number of important organizational changes – principal and explicit ones as well as minor and discreet administrative ones – that indirectly but fundamentally affected the direction and character of its activities. More specifically four phases have been discerned.

During the first visionary founding phase, it was pointed out that UNESCO’s SSD emerged out of a geopolitically structured and highly contingent setting, characterized by the optimistic and utopian one-worldism underlying UNESCO’s birth, and that it was only at a late stage of this process that SSD was included and qualified as an international boundary organization. Once set up, however, an epistemic network centered around a core of people within the UNESCO Secretariat, backed up by outer layers, played an important formative role for the intellectual projection of SSD’s work.

However, when it was time to translate the ideas into practice, during the second phase, SSD as well as UNESCO at large confronted several organizational problems in their efforts to establish a “working machinery of cooperation” due, among other things, to the rapid organizational expansion, frequent rotations in the post as Head of SSD, a diffuse aim and strategy as well as growing frictions between different traditions and conceptualizations of social science where the

83 See e.g. UNESCO Archives, 34EX/CP/SR.1-2, Programme Commission meeting, 4 August 1953, p. 3; UNESCO Archives, H.S.4-6, Record of Meeting, 12 April 1954: 3; ARBARK 405/4/1/7/8-9.

day-to-day work, according to one if its Heads, was characterized as an ad hoc approach. It has been argued that these organizational problems can be interpreted in terms of a lack of stability as well as a lack of boundary objects, that is, common goals, workable standards and shared objectives for collaboration across the social worlds represented. In practice, the basic infrastructure in the form of networking was mainly centered around a group of American social psychologists and public administrators, which in one way or the other was also reflected in the practical outcomes by the end of the period. In similar ways the number of international disciplinary organizations that were being set up are typical for the dominant discipline-based way of thinking around SSD’s work. The department remained limited in size and the single most important project during this phase was the Tensions Project.

From around 1950 a revitalization and consolidation of SSD’s activities took place. During this third phase, previous initiatives matured into concrete results, with a “cascade of publications” from the Tensions Project and a number of new more interdisciplinary and collaborative projects with other departments, other specialized agencies and the UN. The ISSC was set up, as well as research institutes and regional social science officers. The large Technical Assistance program on the general UN level started, besides projects on human rights, race and women’s political role. A number of “infrastructure” projects concerned with the communication among international social science were initiated. I have argued that SSD during this phase matured into an almost ideal—typical international boundary organization, and that part of the explanation for this is to be found in Alva Myrdal’s cross-organizational collaborative approach and her way of making use of the available agency space as SSD Program Director.

However, this period of consolidation was soon ended, during the fourth phase, by a series of events around 1953 which in the following year resulted in two constitutional amendments of the UNESCO Statutes, which radically changed not only the official status of SSD’s staff, but also the relative autonomy and integrity of SSD in general and the agency space of its Director in particular, as well as the organizational status of UNESCO at large. These changes have been analyzed in terms of a renegotiated contract between principals and agents on multiple levels.

Whereas earlier studies have tended either to treat the period under scrutiny as a relatively coherent unit, in terms of a pioneering era or as characterized by one major conceptual change in the very middle of the period under scrutiny, that is around 1950, my organizational focus has put greater emphasis on the processual and more fine-grained administrative changes as well as the series of events during the latter half of the period that – on the whole – not only de-stabilized UNESCO’s SSD as an international boundary organization but also fundamentally transformed it from a hybrid organization, which shared the optimistic vision of one-worldism, to an intergovernmental organization considerably more open and vulnerable to external geopolitical pressures.

In terms of the intra- and inter-organizational structuration of agency spaces on different levels of UNESCO during this formative period, the paper has argued that these were relatively wide on all levels of the organization especially during the early phase. Julian Huxley made use of this in his role as Director-General, and so did Jaime Torres Bodet – until he confronted resistance, first in 1950 and then even more so in 1952 when he resigned. Among the Heads and Directors of SSD, both Arvid Brodersen’s and Robert Angell’s leaderships left footprints on the discipline-based activities. But the one who really made use of the wide agency space available was Alva Myrdal – until she started to face problems during the fourth phase and chose to leave SSD and UNESCO in 1955. On the level of project leaders, Edward Shils, Hadley Cantril, Otto Klineberg and Angell set
their marks on the Tensions Project, as did Alfred Metraux on the Race Project and Margaret Mead on the Technical Assistance Project. This also to a certain degree pertains to the general UNESCO staff with their initially relatively independent status as international officers. These observations are also in line with earlier accounts of the relative autonomy of the UNESCO Secretariat during the early period as well as on the leadership of UNESCO (Lengyel 1986: 42-43; Sewell 1975: 18-20).

This pattern was however drastically changed by the series of events culminating in the constitutional amendments in 1954, which instantly decreased the agency spaces of Program Directors, project leaders and international officers, while at the same time increased the agency spaces of the Director-General, the Executive Board as well as the National Commissions and the Member States – the latter in proportion to their relative strength within the UNESCO system. This discreet but fundamental alteration of the administrative structures, I argue, was the single most important change during the period – not only for the formal administrative practices, but also indirectly with its far-reaching consequences for the very scope and contents of SSD’s activities.

The renegotiated multilevel relationship between principals and agents has been interpreted as a new “contract” in David Guston’s terms. The crucial difference is that our case offers an example of how UNESCO’s SSD was de-stabilized as an (international) boundary organization, in contrast to Guston’s case on the formation of U.S. science policy where he focuses on the introduction of boundary organizations as new stabilizing institutions after the 1970s and 1980s crises. As already emphasized in the introductory conceptual discussion, my use of the concept has been explicitly decontextualized from Guston’s historically situated definition and used as an analytical concept. I am also in agreement with Miller’s critique that it is important to be aware of the increased complexity in dynamics when scaling up to an international level of analysis (Miller 2001), as well as Leith et al.’s argument to view “stability” as a means rather than an end and a defining criteria of boundary organizations (Leith et al. 2016). Nevertheless, there are some striking similarities between Guston’s empirical case and ours in that both are concerned with processes of reorganization. But where Guston focuses on the stabilization after the organizational change in question, that is, after the breakdown of the so-called “social contract for science”-era with its prevailing principal-agent relations in terms of self-regulative science and the linear model (Guston 2000: 19, 70, 141), our case has been concerned with the process of destabilization leading up to a major organizational change. One way of turning this around could be to argue that the two cases are concerned with different sides of one and the same phenomenon, namely processes of reorganization. In that sense, my revision of Guston’s concept can be understood as a positive critique, speaking in favour of (international) boundary organizations as an analytic concept with an even broader applicability.

Finally, a general argument in this study has been to highlight the decreased agency spaces on several levels within the organization and its increased vulnerability to geopolitical pressures from the outside. Here it needs to be emphasized that this does not imply that the subsequent development of UNESCO’s SSD can be reduced to the role of a handmaiden of external geopolitical interests (c.f. Solovey 2012: 13-18; Heyck 2015: 15-16). Instead the analytical point of the notion of “agency space” has been to clearly distinguish the empirical question about the potential agency space available from the question about how the actors within these dynamic organizational structures actually made use of these spaces (sometimes in order to change the structures themselves). In that sense the post-1955 development of UNESCO’s SSD as an international boundary organization is, evidently, an open empirical question, though outside the scope of this article.
ACKNOWLEDGMENTS

The research behind this article has been funded by Riksbankens Jubileumsfond (project grant P12-0273). Earlier paper versions were presented to the Uppsala STS seminar, the History of Recent Social Science conference in London, and the Sociology seminar at Södertörn University, Stockholm. I am grateful to the organizers – Ylva Hasselberg and Alexandra Waluszewski; Philippe Fontaine, Jeff Pooley and Craig Calhoun; Stefan Svallfors – for most inspiring conversations at these three occasions, as well as for helpful individual comments from Christian Dayé, Hedvig Ekerwald, Aant Elzinga, E. Stina Lyon, Jennifer Platt and two anonymous reviewers. Thanks also to Adele Torrance at the UNESCO Archives, Paris, and the staff at the Labour Movement Archives and Library, Stockholm, for excellent services.

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Famous and Forgotten: Soviet Sociology and the Nature of Intellectual Achievement under Totalitarianism

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Abstract

For decades Soviet and later post-Soviet sociology was dominated by a cohort of scholars born between 1927–1930 (Grushin, Kon, Levada, Ossipov, Yadov, Zaslavskaya). The origins of their prominence and the character of their recognition offers a puzzle as it seemingly defies conventional ideas about where academic renown comes from. Academic prominence is usually associated with either intellectual leadership or skillful manipulation of the academic power structures. Neither of these stories describes the peculiar pattern of recognition of the giants of Soviet sociology whose fame persisted after they retired from administrative responsibilities and in spite of their ideas from the Soviet era being almost forgotten. The hypothesis developed in this paper holds that this peculiar form of fame emerges from the unique position sociology held in Soviet society. The paper introduces a distinction between natural and intentional secrecy and argues that while most of Western sociology specialized in natural secrecy, Soviet sociology had to deal with intentional secrecy resulting from conscious attempts to conceal the dismal realities of state socialism. The pervasiveness of secrecy during the Soviet era resulted from the central legitimizing myth of Soviet society describing it as built following a scientifically devised plan. This legitimation allowed Soviet sociology to emerge and develop with an unparalleled speed, but, at the same time, it explains why sociology was seen as having considerable subversive potential and faced periodic repressions. This political environment accounts for Soviet sociology’s unique intellectual style as well as for the fact that its central figures remained in the disciplinary memory as heroic role models, rather than as authors of exemplary texts.

Keywords

Soviet sociology, history of sociology, sociology of social sciences, sociology of secrecy, legitimacy

The history of Soviet sociology unearths several puzzles that cannot be solved easily with reference to commonsense views on how the sciences, including the social sciences, work. Among others, this history challenges our views on the origins of success and fame in the academic world. Social studies of science equate the success of an intellectual movement with the degree of acceptance its knowledge claims achieve in the academic world (Latour and Woolgar 1979; Collins and Pinch 1998). Critical of scientists in all other senses, sociologists of scientific knowledge such as Bruno
Latour are incredibly idealistic in their belief that it is only by putting forward groundbreaking ideas that a group of scholars (or in the specific case of Latour’s theorizing, a network uniting academic actors with non-human actants) can come to dominate the intellectual scene. This prominence will hold as long as these ideas continue to be accepted. Other sociologists in the academic world, particularly those studying social sciences (Bourdieu 1988; Clark 1977; Wiley 1979), tried to allow for sources of academic power other than the ability to mobilize intellectual support. Thus, in Pierre Bourdieu’s pessimistic vision of the field of social sciences in France, academic power residing in the control over others’ professional trajectories dominates over purely intellectual influence.

The careers of the more prominent figures of Soviet sociology do not, however, fit easily into either of these two stereotypes—intellectual leader or institutional manipulator. While many of them served in important administrative posts, they remained revered (or even worshipped) figures even after they ceased to have any control over others’ academic careers. At the same time, their works from the Soviet era—during which they won their lasting recognition—are scarcely remembered.

This article will proceed in the following way. First, I will provide evidence substantiating the claim that the recognition won by the Soviet sociologists is of a very intriguing nature to those studying academia. Then I will formulate the major research hypothesis matching types of sociological work to the types of secrecy a researcher has to deal with. Then follows a very short historical overview of the development of sociology in the USSR. Then I will show how the Soviet sociologists’ having to deal with Soviet secrecy explains the nature of the major problems they had to solve, the achievements they valued, the dominant styles of their work, and the peculiar character of their fame. I will conclude by discussing some implications of these arguments for our understanding of “recognition” in the social sciences.

THE PUZZLE: A STRANGE WAY OF BEING FAMOUS

In the 1960s, a group of scholars established themselves as the leaders of Soviet sociology and retained their centrality within the discipline until the first decade of the new millennium. This most prominent members of these group were Boris Grushin (1929–2007), Igor Kon (1928–2011), Yuri Levada (1930–2006), Gennady Ossipov (b.1929), Ovsej Shkaratan (b.1931), Vladimir Yadov (1929–2015), Tatiana Zaslavskaya (1927–2013), and Andrei Zdravomyslov (1928–2009). In the new millennium, their lifelong achievements were celebrated by accolades of various honors, including medals of disciplinary associations, invitations to give plenary talks, and memorial editions. A few leaders of Soviet sociology wrote autobiographies (Kon 2008; Zaslavskaya 2007), complemented by various biographical and historical materials (interviews, collections of historical documents) prepared by their former pupils, relatives, and colleagues.

There still exists something short of resembling a “personality cult” around them. To give only one illustration, to commemorate Yadov’s eightieth birthday, the Moscow-based Institute of Sociology at the Academy of Sciences published a book of memoirs by his colleagues named “c” (Gorshkov 2009). Its chapters had revealing titles such as “Sociology starts with the letter “Y” (B. Doktorov), “Yadov: great, unique and inimitable” (V. Bakirov), “Yadov: The family jewel of Russian sociology”

1 One can find more detailed histories in Zemtsov (1985); Shlapentokh (1987); Weinberg (2004); Firsov (2012); Zdravomyslova and Titarenko (2017).
2 Doktorov 2005; Doktorov 2012; Moskvichev 1997; Levada T.V. 2011; Ossipov and Moskvichev 2008; Firsov 2012; Radaev and Starcev 2008
(A. Bulynina, S. Sedyakhina). While there was probably a degree of self-irony in naming the pieces in such a fashion, this was obviously not intended to be read as sarcasm. To complement the picture of this apotheosis, they remain among the most cited authors in Russian sociology.³

This apparently points in the direction of “intellectual leaders,” an interpretation that suggests that Yadov, as well as other Soviet sociologists, were men and women who established influential schools of thought early in their careers. The existence of these schools could explain the fame they continue to enjoy. This text will argue, however, that this explanation is not completely satisfactory. We will see that, surprisingly, the intellectual achievements of Soviet sociology were nearly forgotten during the very period in which its founding fathers received their highest honors. No abstract intellectual constructions associated with their names that could be considered “a theory” currently enjoys any wide currency in Russia. The works of these Soviet scholars that they defined as their most important intellectual contributions, and which were published at the period they rose to prominence, are scarcely cited, and a few of such texts were never reprinted, despite the fact that many of them are completely unavailable. At the period when the leaders of Soviet sociology were at the heights of acclaim and power, their intellectual legacies from the Soviet past fell into oblivion. Few people remembered and cited their work published before 1991. As an illustration of this, Table 1 gives an indication of the amount of citations for the published work of the six key figures of Soviet sociology in the Russian Scientific Citation Index (RSCI).⁴ The RSCI covers Russian periodicals from 2004. The figures give some idea of which work of the giants of Soviet sociology is most visible now.⁵

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³ As of September, 2017, Yadov with 11,495 citations is the most cited of 5,490 authors that the Russian Index for Scientific Citing (RISC—see below) classifies as sociologists. Zaslavskaya is No.8 with 8,763 citations, Zdravomyslov No.10 (6,501). Igor Kon, who is counted as a psychologist, rather than a sociologist, got 21,213 citations and became the most cited of above 7,000 authors in this category.

⁴ The six who received above 3,000 citations in the RSCI were chosen for analysis.

⁵ The data were taken from the RSCI webpages on 1 September, 2017. RISC does not produce compact disc editions, and as the database it being constantly updated, the figures are not exactly reproducible. Nevertheless, the results proved stable after several recalculations.
<table>
<thead>
<tr>
<th>Name</th>
<th>Total citations</th>
<th>Year text receiving median citation published</th>
<th>Three most cited texts with years published and number of citations in parenthesis*</th>
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<td>Social Identification in a Crisis Society, 1994 (320)</td>
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<td>Strategies of Sociological Research: Description, Explanation, Understanding of Social Reality, 2007 (316)</td>
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<tr>
<td>Zaslavskaya</td>
<td>7254</td>
<td>1999</td>
<td>Contemporary Russian Society: A Mechanism of Social Transformation, 2004 (759)</td>
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<td></td>
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<td>Social Structure of Contemporary Russian Society, 1997 (206)</td>
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<td>How All-Russian Center for Public Opinion Study was Born, 1998 (191)</td>
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<td>Kon</td>
<td>23144</td>
<td>1988</td>
<td>Sociology of Personality, 1967 (1260)</td>
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<td>Discovery of the Self, 1978 (892)</td>
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<td>Child and Society, 2003 (479)</td>
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<td>Zdravomyslov</td>
<td>6813</td>
<td>1995</td>
<td>Needs, Interests, Values, 1986 (1068)</td>
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<td>The Sociology of Conflict, 1995 (388)</td>
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<td>Man and his Work, 2003 [1967] (with V.Yadov) (246)</td>
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<td>The Sociology of Inequality. Theory and Reality, 2012 (133)</td>
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<td>Social Stratification in Russia and Eastern Europe, 2006 (with V. Ilyin) (100)</td>
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<td>The Problem of Elite in Contemporary Russia, 2007 (with L.Gudkov and B.Dubin) (131)</td>
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**Control group**

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<thead>
<tr>
<th>Name</th>
<th>Total citations</th>
<th>Year text receiving median citation published</th>
<th>Three most cited texts with years published and number of citations in parenthesis*</th>
</tr>
</thead>
<tbody>
<tr>
<td>Gurevich</td>
<td>8669</td>
<td>1989</td>
<td>Categories of Medieval Culture, 1972-1984* (2167)</td>
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<td></td>
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<td>Medieval World: The Culture of the Silent Majority, 1990 (507)</td>
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<td>Historical Synthesis and the Annales School, 1993 (466)</td>
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<td>Ivanov</td>
<td>6783</td>
<td>1983</td>
<td>A Study of Slavic Antiquities. Lexical and Phraseological Issues of Text Reconstruction (with V.Toporov), 1974 (543)</td>
</tr>
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<td></td>
<td></td>
<td></td>
<td>Indo-European Languages and Indo-Europeans, 1984 (with T.Gamkrelidze), (496)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>Slavic Languages Semiotic Systems, 1965 (with V.Toporov) (493)</td>
</tr>
</tbody>
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Table 1. Bibliometric characters of citation of work by leading Soviet sociologists in the Russian Index for Scientific Citing, in comparison with scholars from other disciplines

* Titles provided are English translations of Russian-language originals.
* Net citations for two identical editions
* Net citation for two editions.
The first impression is one of the high immediacy of their work: the texts of the Soviet sociologists receiving most citations are relatively recent ones published after the start of Perestroika in the mid-1980s. The major exception is Igor Kon who was a key figure in importing whole fields of Western social sciences, such as (sociological) social psychology and sexology. His earlier texts introducing these fields to the Soviet reader remain widely read and cited, while the popularity of his later books was probably undermined by his interest in gay and lesbian studies, which had a lesser appeal to the majority of morally conservative Russian academics. With the exception of Kon, we find only two Soviet books on the list, both listing Zdravomyslov among their authors: his 1986 treatise on Needs, Interests, and Values from the Perestroika years and the second edition of his and Yadov’s Man and His Work, which appeared in 2003. The newer edition, however, had a totally new section offering a reinterpretation of the older results, contained a previously unpublished account of a comparative study of labor values of Soviet and US workers and a replication of the original study after Perestroika, as well as sections with the author’s recollections of the emergence of their masterpiece. With this exception, the texts of the stars of Soviet sociology that are currently most acclaimed appeared after the USSR was gone. As a measure of the immediacy of their reception, a calculation was made for each of the six representative figures the year in which their text receiving the median citation was published (e.g., roughly half of the citations go to earlier and half to later texts). Yadov’s and Zdravomyslov’s median citations fall at their publications of 1995, Shkaratan’s falls in 1998, Zaslavskaya’s 1999, and Levada’s in 2000 (Levada died in 2006, so half of the papers cite works produced in the last six years of his life). Only Kon’s falls when the USSR was still alive; 1988.

This is a highly unusual citation pattern for leaders of an intellectual movement in twentieth-century sociology. While recent research has partly disproved the earlier conviction that science is “a young man’s game,” it nevertheless seems that in the majority of disciplines, including economics, the most influential pieces are still produced by relatively young people in their late thirties or forties (Diamond 1980; Wray 2003; Jones et al. 2014). It seems that this pattern also holds for sociology. A reader could test this proposition by composing a list of books that have most influenced him or her and then find out the authors’ age when the book was written. For this purpose, I used the list of the “Books of the Century” named by members of International Sociological Association as those “which were most influential in their work as sociologists” in 1994. I took seventy-eight books named by five or more people from the 455 surveyed and calculated the age of the author at publication (for those published posthumously, the age of death was taken, although in most cases it obviously lead to an overestimation of age parameters). The mean age of an influential book author was forty-six years; median was even less at forty-five, and the modal value was only thirty-nine. Only three books qualified as published when their author was over sixty, two of them were published posthumously (Mead’s Mind, Self, and Society and Marx’s Capital). In contrast, Yadov’s median publication citations falls when he was sixty-eight years old, thirty-eight years after his publishing career began. For Levada, the median citation was to a paper published when he was seventy years old, forty-two years after the first piece authored by him was published. Zaslavskaya was seventy-two years old, forty-one years had passed since her first piece was published. This discrepancy can be partly explained by the fact that, in sociology, citation measures are not perfectly correlated with subjectively estimated influence (Najmann and

9 Results reported at http://www.isa-sociology.org/books/, retrieved 09 September 2017
Hewitt 2004). Even taking this into account, however, Soviet sociologists look like as a rather odd group of classics.\footnote{10}

Following the “institutional manipulator” interpretation, one might suppose that this pattern of the Soviet sociologists’ recognition is a reflection of their exceptional political skill in manipulating power structures rather than intellectual leadership. Perhaps those dependent on them cited their work to avoid the wrath of the academic bigshots. This hypothesis turns out to be even more wanting, however. The majority of them did occupy important administrative posts at various points in their lives, such as heads of the Academy of Sciences institutes; in fact, many of them were renowned organizational builders who actively participated in creation of these institutes (Ossipov, Firsov, Zaslavskaya). However, some occupied administrative positions for only very short times early in their lives (Igor Kon), and most suffered from long periods of political disfavor during which they lacked the keys to institutional power. What is more, their acclaim lasts long after their retirement from influential administrative posts, and after most of them, sadly, passed away. This influence obviously cannot be fully explained with reference to their control over others’ careers.

Four familiar explanations can be offered to explain the oblivion of the works of leaders of Soviet sociology without resorting to the institutional manipulator hypothesis. First, the tendency to cite recent literature could be attributed to the citing of later editions rather than the originals. However, qualitative analysis demonstrates that when earlier editions were identical to the original one, the latter received the lion’s share of citations. Furthermore, most of the books routinely cited in interviews as the highest quality work of Soviet social sciences were not reprinted for many years after 1991, with some of them available only as rare mimeographed editions (such as Levada’s 1969 Lectures on Sociology until 2011), or not published at all, such as the second volume of Grushin’s Taganrog studies. It was only recently that some of Levada’s, Grushin’s, and Zaslavskaya’s texts were reprinted as parts of commemorative editions that also included their autobiographies and recollections related to them (Grushin 2001; Zaslavskaya 2007; Levada Ju.A. 2011); none of them were printed by a major commercial publisher and most editions were made available electronically immediately after their release, thus demonstrating that the publishers did not hope for any market success. The only example of a later edition gathering considerable citations were the increasingly extended editions of Yadov’s textbook on methods, giving peaks at 1987, 1995, 1998, 2001, and 2003.\footnote{11}

The second explanation of the unusual tendency to credit leading Soviet sociologists for relatively recent findings is that they, in all likelihood, had to keep their most important thoughts to themselves until Soviet censorship vanished. Their best books could be published only after the fall of Communism, which explains the unusual citation pattern. This claim is hard to disprove bibliometrically, but if it is subjected to a more qualitative analysis, this explanation appears questionable at best. First, if this was the case we would expect the most influential books to be published right after the fall of Communism and to use Soviet empirical material. However, among sociologists, only Levada published a totally new and highly influential book in late eighties or early nineties: The Simple Soviet Man, which largely comprised his reflections on the “Soviet personality.” However, even this book was nearly exclusively based on results of surveys from the\footnote{10 As the RISC stores information on citations to books and edited volumes, not only on journal articles, this pattern could not be explained by the greater retrievability of more immediate journal citations Clemens et al. (1995). \footnote{11 The 2003 edition was three times as long as that of 1987.}

Comparison with other Soviet social-scientific disciplines may be revealing here. I added to the table two figures who shared much with the first-generation Soviet sociologists: historian Aron Gurevich (1924–2006) and linguist and semiotician Vyacheslav Ivanov (b.1929). Gurevich and Ivanov were of approximately the same age as our heroes, they lived and continued their publication career well into the twentieth century, and they also developed a kind of scholarship that was bluntly non-Marxist and ideologically suspicious (Gurevich was a prominent student of Medieval mentality in the Annales school tradition; Ivanov, in addition to developing formal semiotics, was connected to political dissident circles). In spite of this, they managed to publish their still most cited books in the Soviet era. Gurevich, as one might expect, exploded with publications in the early 1990s, taking one manuscript after another from his desk. Later periods saw a considerable decline in their publication activity and an even greater decrease in the number of citations to that publications.

The third explanation refers to the arrival of new theoretical approaches, methodological standards, and rhetorical conventions that could have made writings of the classics of Soviet sociology look obsolete. But while such processes are arguably occurring, their spread in Russia was too slow to explain the fall of Soviet sociological literatures from grace by the 2000s. Indeed, analysis shows that there were no considerable changes in the methodological rigor and statistical argumentation among the authors of the top Russian-language sociological journal since the Soviet era (Sokolov and Kincharova 2015). Even in disciplines in which such shifts arguably did occur, the fall of Soviet-era literature into disregard did not necessarily follow. Comparison of sociology with other disciplines, such as psychology, is instructive here. The classics of Soviet psychology (such as books by Boris Ananyev, Alexey Leontiev, and Alexander Luria) are still widely cited, in spite of psychology being in many senses more globalized than sociology.

The fourth and perhaps the most convincing explanation points to the changes in society itself. The work Soviet sociologists were doing, and possibly the work they have continued to do after the fall of the USSR, was empirical, not theoretical, in nature. An extension of this explanation—and one used by revisionist historians of Soviet sociology trying to draw a less celebratory picture of it (Bikbov and Gavrilenko 2002; Filippov 2013)—was that, in addition to being empirical, it was fundamentally applied in character, aimed at solving the problems faced by the regime. With radical change occurring in society, their findings lost much of their relevance. However, the same people who emerged as the leaders in the Soviet period also proved capable of leading the discipline in the new era. This explanation is probably simultaneously true and insufficient. Soviet

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12 Alternatively, sociologists feeling that the totalitarian regime obstructed publication of their most important findings could have published their reflections in the West and withstood the political consequences. In fact, while quite a few Soviet social thinkers followed this path, none belonging to the core of the sociological movement did. Possibly the closest to being a sociologist was Alexander Zinoviev, who had to emigrate after his “sociological novel” The Yawning Heights was published in the West. Zinoviev was a member of the same intellectual circle as Levada, although he was officially employed as a philosopher, never as a sociologist. Other borderline cases are younger members of Novosibirsk school led by Zaslavskaya and Ryvkina, such as Simon Kordonskii, Olga Bessonova, or Svetlana Kirdina, who obviously did employ their Soviet experience in developing what is possibly the most interesting variety of Soviet indigenous sociology. Their publication careers, however, began after the fall of the USSR, and Kordonskii, the most widely renowned of them, is marginal to sociology.
sociology was (mostly) empirical. It was also (mostly) applied, or at least it universally used the rhetorical forms of applied research, and the topics of this research are arguably obsolete now (e.g., workforce turnover at Soviet plants—a subject of numerous studies during the Soviet era—is hardly a topic of wide interest anymore).

Nevertheless, the claim that leaders of Soviet sociology never invented a “theory,” understood as a set of relatively context-free propositions, would be untrue, although they were obviously less attracted to theorizing than their US or European counterparts. The “disposition conception” of Yadov and his group—a social-psychological model of attitude structure reminding one of Henri Murrey, Gordon Allport, and Milton Rokeach (Yadov et al. 1979), and Grushin’s theory of mass consciousness (Grushin et al. 1980)—a Soviet version of mass society thesis, were theorizing attempts. Moreover, even to the degree to which the answer pointing to the empirical nature of the research in the Soviet sociology holds, it gives rise to new questions. Why did Soviet sociologists abstain from theoretical generalizations? This theoretical muteness cannot be attributed to pressure from official Marxism as such. In history, psychology, or literary studies, in spite of pressure from official Marxism, Soviet scholars produced a few viable theoretical outputs, from Lev Gumilev’s “ethonogenesis” theory to Yuri Lotman’s Tartu breed of semiotics. While some of them would possibly qualify today as a form of conservative mythology rather than scholarship (ethnogenesis), they were definitely attempts at grand theorizing, even when misguided. Their relations with official Marxism varied: some suffered certain degrees of discomfort (Gumilev), although by the end of Soviet rule the influence of Marxist philosophers was limited, and purely non-Marxist ideas were freely developed in academia (in addition to structuralism and cybernetics, which flourished in the seventies and eighties, one can mention economic game theory, studies of Medieval mentalities by Gurevich, or personality testing based on C.G. Jung by Ausra Augustinavičiūtė). Rather than the regime’s repressiveness, it seems that the shortage of theorizing resulted from the lack of its appreciation, both inside the sociological movement as well as among its audience. The same lack of appreciation is evident now: neither the disposition conception, nor any other theoretical construction by the leaders of Soviet sociology has received much attention in recent years. Yadov attempted to revive interest in his work on dispositions in the mid-2000s, but without much success. His and his colleagues’ theorizing attempts are forgotten nearly as fully as their applied, but, paradoxically, that does not subtract too much from their fame.

Finally, evidence of repressions contradicts the belief held by the revisionist historians of Soviet sociology that Soviet sociology associated itself with regime causes and voluntary limited itself to the role of handmaiden of the Communist Party. This does not explain either its troubled history, or its present reputation. By late-Soviet standards, the Soviet sociologists encountered an unusual level of ideological repressiveness. Chroniclers of Soviet sociology, recruited from its ranks, tell its history as one of repeated political repression and pogroms (Firsov 2012; Doktorov 2013). As one of the founding fathers of Soviet sociology has put it, “None of the foreign sociologies has such a history. This is a drama, a tragedy” (Ossipov 2013). While one may criticize this vision as exaggerated, in the light of what happened to sociology in China and some other countries (Tse Tang and Yeo Chi 1978), in all likelihood sociology did experience a pressure unheard of in other social-scientific disciplines at that time. Indeed, some of these disciplines were treated as ideological outcasts (e.g., political science or theology) and were never institutionalized to any significant degree. Those there were institutionalized, such as psychology, however, managed to work out a modus vivendi with the Communist regime by the 1960s and successfully avoided troublesome situations. While it is difficult to propose a measure of political repressiveness toward a given discipline, it seems that none has experienced anything like the pogrom at the Institute of
the Academy of Sciences in the early 1970s following the “Levada affair.” During that episode, the majority of department heads—who were officially responsible for guiding whole branches of sociology in the Soviet academic hierarchy (see Appendix 1)—were fired and many had to pick up jobs at institutes unrelated to sociology, which usually meant that they would radically change their field of study to correspond to the profile of their own employer. An instructive point of comparison is purely ideological disciplines, such as the political economy of socialism or scientific communism, major authorities in which are now firmly forgotten. Were the Soviet sociologists just collaborators of the regime, the same would happen to them as well. In the next section I will develop a hypothesis that simultaneously explains Soviet sociology’s peculiar status in Soviet academia, the reasons for its oblivion and its lasting glory.

SOCIAL SCIENCES AND THE TYPES OF SECRECY: A RESEARCH HYPOTHESIS

All scholarship deals with secrets. The secrets disclosed by research could be divided, however, into two broad classes: natural and intentional. Intentional secrets emerge from conscious attempts to avoid the spread of certain information (Simmel 1906; Goffman 1969; Gibson 2014). Natural secrets are not protected through human conscious efforts. They can be divided into secrets of distance and secrets of perspective. The former arise from difficulty in observing an object, while the latter emerge from difficulty in synthesizing observations and putting forward a hypothesis placing the available observations into a meaningful pattern. In Western, as well as many non-Western societies, empirical sociological research mostly praises itself for disclosing secrets of distance. Thus, in quantitative research, statistics allow a researcher to grasp regularities in social life invisible for those who are immediately involved in it, while qualitative research allows him or her to get in contact with social groups that those middle-class publics who mostly consume sociological findings have little contact with. Sociological theory that dominates present-day textbooks presents itself as disclosing secrets of perspective. Theoreticians such as Georg Simmel or Erving Goffman are credited for shedding new light on what was considered trivial and obvious.

Neither one of these types of natural secrets were the principal aim of Soviet sociologists. This paper posits that Soviet sociology specialized in disclosing intentional, not natural, secrets about society because the latter were much more widespread and salient in the Soviet Union, than they were in the USSR’s Western competitors. Both Soviet sociology’s research styles and peculiar organization of disciplinary memory could be accounted for by this fact. Probably all human societies are familiar with both natural and intentional secrecy. However, a few observers have commented on the all-encompassing pervasiveness of secrecy in societies of state socialism (Harrison, 2013). Indeed, the USSR was unique in the rage of information classified in it (it included not only all statistics, basic demographic statistics notwithstanding, and detailed maps of the country, but even the most innocuous of books, such as a cafeteria’s chief cookbook that was

13 Among other episodes of this kind, after a blunder occurred, Yadov and Firsov were fired from the Institute for Social and Economic problems (ISEP) and had to find asylum at the more tolerant Institute for History, Science and Technology, which was a stronghold of naukovevedy—scientific studies (Yadov), and the Institute for Anthropology and Ethnography (Firsov). Both were demoted from their positions as department heads to senior researchers. Yadov had to put aside his study of industrial relations and do a study in the sociology of science; Firsov’s obligation at his new place of work was preparation of publication of the archive of Prince Tenishev, one of the social surveyors of the nineteenth century—something very far from his own field of mass communications (Firsov 2012).
marked “for restricted use” [dlia služebnogo pol’zovaniya]; the Soviet people were not meant to know in too much detail what the public catering system had to offer).

An explanation one could offer for this fact is that secrecy was the other side of the Soviet central legitimation myth, according to which socialist societies developed along a scientifically devised, all-embracing plan of movement toward communism. In the official Soviet vision, societies unaware of the laws of history discovered by Marx and Engels, or not ready to embrace them, existed in the kingdom of historical necessity. They were governed by forces outside the scope of their control and, consequently, suffered from regular crises. Soviet society, in contrast, existed in the realm of historical freedom because it was based on a recognition of these laws, allowing Lenin and his comrades to work out a blueprint according to which the first socialist state developed.

This legitimation myth had several empirical implications, a falsification of any of which would put the whole myth in doubt. It followed from the myth that:

1. The development of Soviet society follows a pre-scheduled path;
2. The implementation of this plan assures the superiority of socialist society over its capitalist rivals;
3. The Soviet people more or less unanimously support the movement into the direction chosen by the Party;
4. A few rather specific predictions would come true, such as the gradual disappearance of the divide between manual and intellectual labor and, thus, working class and intelligentsia, in both living standards and lifestyle.

The legitimacy of the Communist Party’s rule depended on how well Soviet society approximated these predictions. Multiple grand discrepancies would inevitably raise doubts in either theory, practice, or both, and would ultimately undermine the Party’s right to rule. Unluckily for the Soviet elites, many of these predictions were quite precise and could thus be quickly (and easily) disproved, as occurred with the ill-fated 1937 census resulting in the infamous “Statisticians’ Affair.”

To make things even more complicated for the Party, the fact that one of the central predictions was that Soviet society would be governed according to an all-embracing plan inevitably produced a sort of a vicious circle: the necessity to legitimize any new action through reference to a plan lead to proliferation of progressively more precise and, thus, more collapsible predictions covering every aspect of the USSR’s development.

Overall, due to its definition as a fully rationalized state, the Soviet Union was unique as a state in the sheer scope of responsibility for its social problems that its rulers had to accept. The rulers of a totalitarian regime are by definition responsible for everything happening with its citizens. This “everything” ranged from the persistence of religious beliefs to lack of shoes of a particular size in a particular village store. The same legitimizing myth of all-encompassing responsibility that gave the regime license to intrude into every corner of citizens’ lives made it extremely vulnerable to criticism when it failed to fulfill its promises. Moving to the subject of our study, this meant that an

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"The results of the 1937 census contradicted the projected outcomes of the Second Five-Year Plan; the population had increased much less than expected, more than half of adults confessed as religious, and one third remained illiterate. These results were never published and statisticians responsible for the census were accused of sabotage with the most gruesome consequences."
analysis of any aspect of social life had repercussions as an evaluation of the legitimacy of the Soviet regime as a whole.

An ever-increasing amount of effort by the Soviet regime was dedicated to coping with such failures. A variety of explanations or excuses were available for the ruling elites. Some problems could be attributed to natural or historical causes, such as the cultural legacy of the Tsarist period, and such legacies were regularly referred to, even late in the Soviet period. This defense, however, was a relatively weak one as it raised doubts about the Party’s predictive abilities. Another, and more efficient, defense was reinterpreting the plan and the expectations to which it gave rise, and the constant work of many ideological commentators was reconciling reality with Lenin’s visions through redefining what Lenin and other classics had intended to say. Still another way of coping was localizing the blame, preferably at a lower level of hierarchy, and defining it as a personal failure. The localization of those responsible was determined by the scope of the problem. Problems at the level of a particular organization were, in all likelihood, the result of shortcomings at the level of the directorate and, if the organization was large enough, the Party and/or Komsomol (Communist youth organization) cell. Problems at the regional level were the responsibility of the municipality and rajkom (local Party committee), gorkom (city Party committee), or okkom (regional committee – see Attachment at the end of this text), depending on how large the affected territory had been. Finally, nationwide problems were the responsibility of the Council of Ministers and the Politburo. Unless the problem was experienced nationwide, the legitimacy of the regime as a whole could be safeguarded by making some personnel changes.

The fourth and final defense mechanism was suppressing the evidence of a social problem. Such evidence could not be allowed to become public, or even to appear. This explains the all-embracing secrecy that was one of the most pronounced traits of the Soviet regime. Together, the last two defenses give us the tendency to limit the scope of any activities that could generate discrediting information and to prevent such information from becoming publicly available. And here come the sociologists who, by the very nature of their trade, were divulgars of secrets.

A VERY BRIEF OVERVIEW OF THE HISTORY OF SOVIET SOCIOLOGY

Statistical, positivist, and Marxian movements reached Russia soon after they emerged in Western Europe (Golosenko and Kozlovsky 1995). The further development of sociology in what later became the USSR was also largely synchronous with that of other countries. After WWI, the initial blooming of sociology was followed by a widespread decline (Shils 1970; Turner and Turner 1990). In Russia, however, this downturn ran far deeper than in most other countries as the even the word “sociology” disappeared from the official lexicon. Lenin used it inconsistently, both as a specific name for Comtean positivism (an anathema for a Marxist), and as a generic name of any social theorizing (he occasionally referred to Marx as the father of scientific sociology – a citation widely used by Soviet sociologists later). Unluckily for the term, Bukharin employed it as a generic name publishing “A textbook in Marxist sociology”. After his downfall, the word also fell into disgrace and was reserved as derogatory term to describe “bourgeois teachings” (burzhuaznyje uchenija). Eventually, a group of professional critics of such teachings emerged at the Institute of Philosophy of the Soviet Academy of Sciences in the late 1940s, but its personnel never referred to themselves as sociologists (Batygin 1991). Historical-materialist philosophy was officially the only theory applicable to historical process.
Social surveys continued in the 20s and, to a lesser degree, the early 30s (Batygin 1998), but following the consolidation of Stalin’s rule they virtually ceased. The turning point was, arguably, the “Statisticians’ Affair” of 1937-1938. Although some of those involved in the surveys of the 1920s survived the repressions, all traces of their activities disappeared, and a new generation of empirical researchers started their work without any knowledge of their early Soviet predecessors. A revival of sociology began after Stalin’s death and, in many senses, was a prototypical example of the homogenizing pressure of the “world-society” (Scott and Meyer 1994; Meyer and Schofer 2005). Its history began when the Academy of Sciences – a network of institutes in which most basic research was located (see Appendix for details on how the Soviet Union was organized) – received invitations from organizers of the World sociological congresses. The invitation for the Liege congress of 1953 was ignored, but the next one, for the Amsterdam congress of 1956, found a warmer welcome. In correspondence with their Party curators, academics argued that absence of a Soviet delegation at such an event could be regarded as a sign of Soviet intellectual weakness; they made a special point of insisting that leaving participants from Third-world countries exposed to advocates of capitalism from the US and Western Europe was dangerous (Moskvichev 1997). The academics had found the right arguments needed to persuade the Party officials. A delegation consisting of twelve philosophers attended the 1956 Congress, and Soviet representatives were present at them from that point on. Moreover, the USSR expended a great amount of effort to send the largest delegation as this was perceived to be a matter of national prestige. For many Soviet sociologists, this offered the only real chance to see the outside world.

However, Soviet participation in such events had far-reaching consequences. First, according to the provisions of the International Sociological Association that gave a country the option of sending a delegation, a national sociological association had to be set up. This was the principal reason for organizing the Soviet Sociological Association in 1958 under the leadership of Yuri Frantsev, one of the central members of academic establishment of his time. He was the editor-in-chief of the main Party newspaper “Pravda,” the rector of the Academy of the Social Sciences attached to the Party’s Central committee, and one of the founding father of the MGIMO (the Moscow Institute for International Relations) – the elite Moscow diplomatic academy. It is safe to say that from that moment on, sociology officially existed in the USSR.

The creation of the Association was not enough by itself, however. Soviet philosophers appeared somewhat amateurish at sociological meetings and their ability to resist alleged assaults of the advocates of capitalism were feeble. More suitable candidates were needed for such a challenge and Frantsev soon transferred the Association to one of his students from MGIMO, Gennady Ossipov (1959), who, from then on, became one of the central figures of the Russian sociological establishment for more than 60 years. Ossipov, and other students of Frantsev from the diplomatic school (Semyonov, Zamoshkin), formed one of the first groups in the USSR that read Western sociologists extensively and with a more positive attitude than their predecessors, who functioned as professional denouncers of capitalist heresies. Other groups emerged at approximately the same time at faculties of philosophy at Leningrad University (Yadov, Zdravomyslov, later Firsov), and Sverdlovsk University (Rutkevich, Kogan).

These early comers and the future leaders of the sociological movement formed a remarkably homogenous group. They belonged to the generation of 1926-1930 and were thus peers of Bourdie, Habermas, Foucault, Luhmann, Tilly, and Howard Becker. Most of them were children
of intelligentsia — professors of humanities, teachers, doctors, or prominent Party officials. Many were active in Komsomol, the Communist Youth Union, which was not surprising given that the philosophy faculties were widely regarded as a source of cadres for the Party apparatus. In their later reflections, they would define themselves as Communist true believers at the outset, incorporating the vision of socialist society as a rational project (see interviews in Batygin 1999). After Stalin’s death, however, there was a general feeling that the Soviet project had suffered a great distortion under the rule of a tyrant, and was in need of a radical renovation. These renovation attempts shaped the last period of enormous social creativity in the Soviet history, known as “the Thaw” (1955-1968). For many, including the young philosophers, that meant revitalizing the technocratic imagery of social engineering with the latest achievements of science. In this they were supported by powerful Party philosophers, like Frantsev, who were also looking for a new creed. The first Soviet sociologists can be regarded as the younger generation in their academic patronage networks and worked under the following arrangement: the bosses would provide them with political and administrative cover, while their dependents would produce innovative work for which the bosses could take credit. Such bosses included Lovchuk (patron of the Sverdlovsk group), Rozhin (patron of the Leningrad group), Rumyantsev, Fedoseev and Konstantinov in Moscow, and later the political economist Prudensky in Novosibirsk. The motives of the bosses who fostered sociology still remained subject to conflicting interpretations; the subsequent narratives on them fall typically into two groups according to the connections of the narrator to a particular figure. For example, the former clients of Frantsev describe him as a dedicated reformer and a secret critic of Communism, while others believed that he was an unprincipled opportunist interested in distancing himself from excesses of Stalinism.

Whatever the motives, sociology promised to bring new life to the legitimizing myths of Soviet society as a totally rational organization. That made it attractive and not only for the academic bosses. Making a career in the Party required demonstrating initiative that would be noticed by one’s superiors. For a provincial Party official that meant starting a campaign creatively invoking one of the grand themes of the Soviet ideology, ideally making its way into federal press or television. Risks were involved, however; the initiative could be considered un-socialist or something could go wrong in the process. Nevertheless, there was always the chance to get ahead, and for many there was no other way. Given that American empirical sociology promised to make social engineering “truly scientific,” a phrase that was central to Soviet legitimizing myth, it was inevitable that sociology should spread and expand in the USSR. Sociology became a source of several Union-wide campaigns such as “scientific organization of labor” or “social planning”.

Social planning was probably the single most important political achievement of Soviet sociologists. From its early days, almost all legal economic activities in the USSR were engineered according to an all-encompassing plan developed by Gosplan. A group of sociologists based in

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15 Few of the first-generation Soviet sociologists were exceptions to this rule. Among such exceptions were Mikhail Rutkevich from Sverdlovsk (born in 1917 and a WWII veteran) and Vasilii Elmeev from Leningrad (born in 1929, but, in contrast to the rest, in a distant Mordovian village). Characteristically, both were politically much more orthodox than the rest of the movement. In Communist times, they served as nemesis to some of the more liberal and reformist figures; in post-soviet times, they became outcasts excluded from the pantheon of Soviet sociology.

16 One of those close to the MGIMO group described him as an “originally a promising Egyptologist” who “in his own words, sold his soul to Bolsheviks”. This was probably too strong a thing for a leading Party philosopher to say (Grushin 1999: 147).

17 This is nicely described in what is probable the best sociological account of the spread of Soviet sociology, a paper of two Soviet émigrés who worked at Yadov’s group for a considerable time (Beliaev and Butorin 1982)
Leningrad advocated the inclusion of a set of social parameters into these blueprints, so that they regulated not only production and consumption, but also the rise of educational levels or stability of marriages. Such plans existed at national level as well as at the level of particular enterprises, which were responsible for social development for their personnel. A request to develop “plans of social development” was included in the new Constitution adopted in 1977.

When, in the 1970s, the Soviet Sociological Association published a directory of its members (Ossipov 1970), it listed 1426 individuals and 231 organizations, which made it the second largest national association at the time (after the American Sociological Association). This list of organizations gives an idea of the niches Soviet sociologists occupied. 91, or 39%, of them were university departments (kafedry) and the laboratories attached to institutions of higher learning. It is worth noting that none of these departments had the word “sociology” in their name. 40 (17%) of the organizations were laboratories of the scientific organization of labor or social planning at industrial enterprises, 38 (16%) were institutes or divisions of institutes at the Academies of Sciences (Soviet and republican academies), 31 (13%) were centers of applied research attached to the profile ministries, 15 (6%), were centers attached to the Party and Komsomol divisions and the remaining 16 (7%) were attached to various organizations such as mass media, trade unions and artistic societies (see Appendix 1 for a very brief introduction to the Soviet governance).

Soon after rebirth of their discipline in the late 1950s and early 60s, Soviet sociologists reached the heights of public acclaim. Newspapers with the widest circulation sought to publish the latest results of their surveys, and their lectures translating the wisdom of their Western colleagues gathered crowds. Igor Kon recalled that his course on “the sociology of personality” (largely consisting of American interactionist social psychology) in Leningrad University was attended by over 1000 people. The Big university hall proved to be unable to accommodate this amount safely, and Kon had to sign a paper embracing full responsibility for any possible consequences (Kon 1999). One wonders if Talcott Parsons, or any other American sociologist, ever gathered such an audience.

This development did not come problem free. First, as one might expect, it encountered opposition from some Marxist-Leninist philosophers, who regarded the rise of sociology as an encroachment on their territory. During the 1950s and 60s the spread of sociology met sporadic resistance from those criticizing, for example, any statistical analysis of survey data as an expression of “bourgeois positivism.” This kind of opposition was silenced, however, by the 1970s. A symbolic turning point was seen in 1971 with the publication of an article in the official flagship journal of Party ideology The Communist, which was authored by leading Soviet philosopher Grigorii Glezerman, the Party curator of academic philosophy Nikolai Pilipenko, and philosopher and sociologist of science Vladislav Kelle (Glezerman et al. 1971). This article, characteristically titled “Historical materialism – theory and method for scientific research and for revolutionary action” formulated the division of spheres of influence between the disciplines. Historical materialism was proclaimed the only true theory of historical development. Sociology was responsible for “concrete” empirical research instrumental in order to help solve the social problems of the Soviet society but ultimately demonstrating the correctness of the grand theory. Sociology thus became a sub-discipline of historical materialism, to use Abbott’s (1986) phrase. After that, no objections were raised against

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18 As representatives of a sub-discipline, sociologists suffered from many minor humiliations; they were denied the right to call their journal “Sociological issues” (Voprosy sotsiologii), which was customary for a fully-fledged discipline, and had to call it “Sociological research” (Sotsiologicheskie issledovaniya).
the empirical research, but macro-sociological theorizing, especially as far as comparison of socialist and capitalist societies was involved, fell mostly beyond the purview of Soviet sociologists.  

Thus, Glezerman participated in the campaign against Yuri Levada who carried out bold comparisons between socialist and capitalist societies in his lectures at the Moscow University. Levada was dismissed from the university, and subsequently the Academy’s Institute for Applied (Konkretnykh) Sociological Research, where Levada headed a department, became a victim of a political pogrom. An anonymous letter accused the institute of a loss of political vigilance, as was demonstrated by Levada’s lectures, a fall in publication productivity, and a “one-sided ethnic composition” (which can be translated from the idiom of the day as an employment of a significant number of Jews). This letter later reached the desk of the main Party ideologist Suslov (Batygin 1999: 445-475). As a result of Party investigation, the hardliner Rutkevich replaced the former philosopher-patron of sociologists Rumyantsev, and a few leading figures, including Levada, had to leave the institute. This was the best-known, but in no way the only, case of a “purge.” Similar campaigns followed what was called “prokoly” (political blunders), which occurred in Tartu, Leningrad and Novosibirsk in late 1970s and early 80s (Firsov 2012; Zdravomyslova and Titarenko 2017).

Apart from individual repression, sociologists experienced certain restrictions of a less direct nature. Graduate schools (aspirantura) and PhD-level degrees (candidate of sciences) in “applied sociology” existed, but were extremely few. Undergraduate education in sociology was unavailable until the beginning of Perestroika. What probably depressed Soviet sociologists most was a ban on launching a research center that could carry out nationwide surveys. Their studies were confined to singular enterprises, or to the audience of a newspaper, or, less frequently, to communities like a village or middle-range town (for example Grushin’s study of reception of mass communication in Taganrog) but were never at the level of a larger territorial or administrative unit. Finally, only during Tchernenko’s brief administration in 1984 were principal decisions made to launch undergraduate education at Moscow and Leningrad universities and to create an All-Russian research center for public opinion studies (WCIOM), neither appeared until the beginning of Perestroika.

Perestroika totally altered the landscape for Soviet sociology. This general political liberalization allowed those who had been repressed and dismissed from their posts to return in triumph their previous positions and to head institutes and the governing bodies of the Association. Their former nemeses took their place as outcasts. Sociology faculties flourished in universities, especially after 1991 as a result of the conversion of former historical materialism chairs. Yadov headed the Moscow Institute for Sociology, the descendant of the Institute for Applied Sociological Research and a national survey center was created with Zaskavskaya as the first director, soon replaced by

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19 There were some exceptions to this rule. One of them was comparative studies of science, which were carried out in a relatively free fashion even when the comparisons turned out to be unfavorable to the Soviet side. Interestingly, however, such studies were symbolically isolated from the rest of sociology as a specific discipline named “Science studies” or “Naukovedenie.” Naukovedy had a few licenses sociologists were denied, which allowed them, for example, to publish Foucault’s “The Order of Things” in 1977 as a treatise in history of science. However, the subject of their studies was nearly exclusively natural sciences, which were, at that time, officially viewed as being above class conflicts and apolitical.

20 Arguably, Russia pioneered the use of various research performance metrics the usage of which in Russian universities could be traced to early 19th century. The Soviet Academy of Sciences paid tribute to this obsession.

21 Late-Soviet Anti-Semitism fueled by the Six-days war was on the rise at this moment.
Levada. Zaslavskaya also headed the Soviet Sociological Association. Their authority won in the previous period secured them leadership in the new times.

THE ACHIEVEMENTS AND THE TROUBLES OF SOVIET SOCIOLOGY

As one can easily see from even from this brief reconstruction, the entire development of Soviet sociology occurred in the shadow of the major legitimizing narrative of Soviet society. This narrative and the necessity to maintain the belief in it explains its emergence, the repressions it suffered from, and the recognition its leaders received. We have already seen how the legitimacy themes surfaced in the story of the Soviet sociological revival. Soviet sociology as a whole emerged from the necessity to demonstrate that the Soviet Union was eager to implement the newest techniques of governance rationalization as well as that it was a leading player in the global intellectual scene. It also emerged from the immanent necessity to expand the scope of spheres to which the planning procedures applied.

Following the path chosen by Lenin required much ongoing planning activity, such as the development of subsequent five-year economic plans. In Soviet Marxism, the economic base determined the development of the social and cultural superstructure, which meant that economic plans were considered the most important types of blueprints in the engineering of Communist society. Nevertheless, the ideology of a fully rationalized state also required expanding planning into new spheres. To explain some of its failures, the Soviet leadership had to recognize that cultural and social rudiments of capitalist society, if not dealt with in a rational manner, may impede the development of socialist economics and defer the coming of communism to the indefinite future. That meant that cultural and social spheres had to be rationally managed as well. Ideally, the plan should have covered everything, from the output of potassium to the transformation of family values. Soviet sociology’s mission was to assist in what, to use Coleman’s phrase (1983), was the rational reconstruction of Soviet society.

In the first stages of development at least, support of the sociological movement in the upper echelons of political elites were met with initiatives from below. At a lower level of the administrative hierarchy, a broad spread of applied sociological research was facilitated by the necessity to demonstrate that each individual organization was scientifically managed. As such demonstrations also contributed to confirming the legitimacy of the whole Soviet project, they were highly valued; providing them was the road to a career in the Party (Beliav and Butorin, 1982). The need to demonstrate the intellectual superiority of Soviet society over the outside world, as well as attempts to resolve its internal tensions, explain why the institutionalization of Soviet sociology was so rapid and successful.

The legitimacy needs of the Soviet regime explain, however, not only sociology’s successes, but also its hardships. Any advance in the institutionalization of sociology led sociologists into potential problems. Most obviously, sociological studies could raise doubts about the presuppositions Soviet politics relied on. For understandable reasons, research that directly tested propositions of official ideology were under particularly intensive control. Nevertheless, some studies followed in this vein. Yadov’s and Zdravomyslov’s masterpiece, Man and His Work (1970), was directly aimed at testing the proposition that, as Soviet society moves toward communism, post-material incentives (such as having an interesting job) replace material ones (meaning salary). Yadov surveyed employees at a large plant in Leningrad and discovered that engineers were much less likely to be materialistic than manual workers. That could be interpreted as a proof of the maxims of official ideology. It was believed that highly qualified labor will replace unqualified labor in the course of the new scientific
revolution, and, if the character of labor is responsible for degree of materialism, new generations of workers are likely to become more post-materialist than earlier ones. Yadov and his colleagues, however, walked on thin ice as they showed that, first, important cultural divisions existed between classes, and, second, working class members were further than the intelligentsia from the Communist ideals.

The episode ended well for Yadov and his colleagues, although not everybody was so lucky. An instructive example was the “Golofast affair” of 1983–85 in Leningrad (Bozhkov and Protasenko 2005). Valerij Golofast, a Leningrad sociologist and a younger colleague of Yadov, had prepared a book on the sociology of family that made relatively free comparisons between Soviet and US studies. Given the fact that these studies demonstrated similar dynamics (such as a decrease in the number of births per family couple), Golofast concluded there were processes common to all industrial societies. That blatantly contradicted the official position of Marxist ideologists, who insisted that the USSR and its capitalist rivals should not be put under a common more general category and that their paths diverged. The reviewers at Nauka, the Academy of Sciences publisher who were responsible for ideological quality control, duly pointed out Golofast’s mistakes. The criticisms were relatively mild, merely requiring the revision of several paragraphs, but Golofast ignored them, concealed the whole episode from colleagues at his institute, and attempted to get the manuscript printed without alterations. When the truth surfaced, he was subjected to a detailed investigation and his expulsion from the Party was discussed. Ultimately, the punishment turned out to be not so severe—Golofast received an official reprimand (vygovor), which meant that he was unlikely to be promoted or allowed to go to conferences abroad. His book was excluded from the publisher’s schedule.

Independently of how well sociologists’ findings or theorizing fitted in with the Party line, an important element was what intellectual sources sociologists relied upon in developing their reasoning. An overly intensive and uncritical reliance on “capitalist” sources in social sciences (except psychology, which by the 1970s attained the status of a natural science) was suspicious and possibly signaled that an individual did not recognize the superiority of Soviet science with its Marxist-Leninist foundations. Citing Parsons without ritually condemning the capitalist bias in his reasoning was a risky thing, and Levada suffered partly because he was not cautious enough.

The risk of coming into conflict with the guardians of Soviet ideology was the greatest in the case of researchers working at the Academy of Sciences who were responsible for basic research and for translating Western literature into Russian (knowledge of foreign languages and access to foreign books were severely limited). The next source of political troubles for Soviet sociology was equally important for all belonging to it, not only to those working at the elite research institutions. As an inevitable consequence of sociology’s legitimization through its usefulness in bringing about the Communist society, sociologists were primarily experts on problems. As such, they were interested in the proliferation of problems to specialize on, were keen to ensure that these problems would remain active in the public consciousness, and were inclined to find potential threats in whatever subject they studied. The success of sociology was intertwined with shedding light on the dismal realities of Soviet socialism and this made sociologists an inevitable danger to the legitimacy of those who were responsible for the realities they studied.

For those at the bottom of the Soviet administrative hierarchy, sociology’s potential as an opportunity was to be balanced with its dangers. Initiating a campaign that promised scientifically based improvement could lead to one being noticed at the top. However, the research could also reveal problems that would then be attributed to its initiator. Active heads of local Komsomol at a
plant could launch a series of surveys of young workers to find out how political propaganda among them could be improved. In itself, such an initiative was highly regarded. But, if the surveys revealed that workers were completely indifferent to propaganda, and no improvement occurred after new measures were implemented, that would put the activists in significant danger (Rusalinova 2008); the lack of political consciousness could be attributed to their own failings as political agitators. This was generally the safest way for their superiors to interpret the findings. Sociological research was thus a threat for those responsible for the setting studied. The latter, knowing that they had no chance to refuse their superiors the information on their performance after it appeared, often chose to prevent this information from emerging at all. The infamous Soviet secrecy often consisted not of classifying information which was publicly available in other countries, but of gathering none. Thus, after the damaging 1937 census, a new census was carried out in 1939 that demonstrated results closer to what the Politburo wanted to announce. Following this, however, no further censuses were carried out for 20 years, until 1959.

Carrying out sociological research in a given setting required collaboration at different levels; collaboration between the subjects researched, between those in authority in the research setting and those above them in the authority chain as well as those above the researchers. Research could be blocked by a refusal to cooperate at any of these levels. The higher levels of a bureaucracy could overrule the decisions of lower levels when they suspected them of trying to avoid their control. In practice, however, they were often convinced to do otherwise by the lower levels, disguising their own secrets as being secrets of the regime in general. If such arguments failed, the research could still be blocked, through either a tacit lack of cooperation or a counterattack.

An episode that occurred in Leningrad at approximately the same time as the Golofast affair is instructive. Boris Firsov, a friend and colleague of Yadov, was charged with the task of developing an “information system” at Leningrad obkom. Among other things, this system applied content-analysis procedures to accumulate information on complaints. The task gave Firsov an office at obkom and a direct telephone connection to the higher Party officials (vertushka), a symbol of highest Party trust in an individual. At some point, however, a secretary of Yuri Andropov, the head of Politburo, requested data from Firsov on complaints from Leningrad inhabitants about the state of the public health system. Having provided the data, Firsov’s group was removed from the “information system” project the following day and the project closed. The fact that Georgii Romanov, the powerful head of the Leningrad obkom, had punished sociologists for making potentially damaging information on the state of healthcare system in his territory available to the Secretary General was, however, never mentioned. Instead, Firsov’s carelessness in dealing with sensitive information in a totally different case was given as a pretext of his fall from grace (Firsov 2012). The institute’s director, unhappy about Firsov’s direct contacts at obkom, used the blunder to force Firsov to leave for another institute.

The craft of sociological research in such a setting is indistinguishable from the art of political intrigue. One had to build coalitions consisting of agents belonging to different hierarchical levels who needed to be convinced that the benefits of a given piece of research (its value in bringing in real improvements or increasing international prestige, the chance to gain recognitions from one’s superiors) outweighed its risks. The list of specific settings researched by Soviet sociologists reflects the opportunities that existed to build such coalitions. The chances were best at the bottom of political hierarchy; the higher the level, the less political support they found.

Overall, the policy adopted by the Communist Party toward sociological research could be formulated in the following way. Sociologists were allowed to do their research at particular local
cases, as anything happening at a level of a particular plant could be dismissed as a “singular shortcoming” (edinichnyj nedostatok), probably together with the plant’s director or head of local Party organization, so not discrediting the Soviet project in general. But the wider the scope of the research, the more general were its implications, as it meant that a higher level of the political hierarchy would be deemed responsible. The higher the level, the less incentives there were to initiate a research campaign. While a local obkom secretary for propaganda could see it as a chance for promotion, a Politburo member had no upward promotion aims. This explains why the institutionalization of sociology stalled at the local level, and why establishing a national survey center proved to be such a formidable task.

The stagnation of sociology in the 1970s and early 1980s, which a few observers noticed (Shlapentokh 1987), was probably the result of its reaching the limits of expansion. On one hand, the list of problems the Soviet regime was ready to recognize as existing had been exhausted and new groups within the discipline found no subjects to study. On the other hand, sociology was unable to institutionalize at the level that would allow it to be regarded as a fully fledged discipline by the powers that be, the public, or sociologists themselves. As the mission of Soviet sociology was to assist the Soviet state in developing plans and evaluating performance, its structure was correlated with the structure of the state. Each research center was attached to a decision-making unit of a certain level in the administrative hierarchy, with the research center’s status and profile corresponding to that of the respective unit. The scope of settings the center analyzed also coincided with the scope of authority of the unit to which it was attached. Thus, the institutes within the Academy of Sciences, connected to the Union ministries and to regional Party branches (obkomy), stood at the top of this hierarchy. They could study at the level of whole regions or industries. Even they, however, could not research such subjects as the class structure of Soviet society or public opinion of the Soviet people at national level. Without such divisions, the institutionalization of sociology was deemed incomplete. Sociologists striving to boost the importance of their discipline inevitably endeavor to enlarge the territorial scope of their research to the national level. That meant, however, conducting research for which the results could not be interpreted as characterizing “singular shortcomings.”

Unable to expand upward, Soviet sociology also faced increasing resistance at the lower levels. As sociological work became more familiar, the attitudes held toward it by Soviet administrators became increasingly less enthusiastic. For latecomers, who did not initiate campaigns to introduce innovative methods of scientific governance but merely joined them following orders from above, there were no possible gains, only risks. Sociological studies were not stopped, but at most enterprises they were reduced to a certain number of safe, and often purely decorative, forms.

While the obstacles and risks in developing sociology in the USSR were enormous, so was the perceived importance of research. Precisely the same vulnerability that made the regime respond so violently to alleged sociological misdoings was the reason why sociologists could see themselves as a part of subversive social action of great importance. In the USSR there could be no study of industrial relations at a plant that would not, at the same time, be a study of the performance of particular officials in the bureaucratic hierarchy, of Soviet policies in the industrial sphere, and, ultimately, which would not be a test of the intellectual foundations of the Soviet regime’s credibility, something sociological censors were always eager to remind sociologists of. But while this status was a source of much trouble for Soviet sociologists, it also gave them the feeling of possessing enormous influence that was unheard of in other contexts.
This understanding of the significance of their research created a unique intellectual style, not fully intelligible for scholars living in another system of relevances (an example of such gross misunderstanding is Greenfeld, 1988). As elements of Western sociology travelled East, they were put to rhetorical usages not intended by their originators. One example will suffice here. A trait of Soviet sociology that may surprise an international observer is the central role public opinion studies played within it. Rather than an industry at the periphery of the profession, public opinion polls were (and, to a certain degree, still are) widely regarded as perhaps the most important sociological practice. The stars of Soviet sociology strove more vigorously to become the heads of national public opinion research centers (Grushin, Levada, Zaslavskaya) than to receive professorships at Russia’s most prestigious universities or to be elected a member of the Academy of Sciences. “The sociology of public opinion” is still a necessary course in sociologists’ undergraduate curriculum and George Gallup may be mentioned in the same breath as idols of sociology such as Robert Merton and Paul Lazarsfeld. There are still no established terms in the Russian language to distinguish Gallup’s polls from academic sociology.

The relevance of public opinion studies to the Russian sociological context was, in all probability, due to the fact that they disclosed one of the most sacred secrets of Soviet society. An essential part of the Soviet regime’s self-description was its self-declared democratic nature, which was buttressed by the claim it enjoyed the unanimous support of all Soviet people (with the exception of a pitiful band of renegades, dissidents, and class enemies). Together with scientific authority, this democratic support was one of the regime’s two major sources of legitimization. Public opinion studies, however, revealed this picture of unanimous support to be illusionary. At times they even showed that the majority disagreed with the course chosen by the Party. In a society deprived of any real elections that, nonetheless, witnessed constant references to the “popular will” as the ultimate source of authority, public opinion polls emerged to play the role as a substitute for plebiscites. As such, they tore apart the most politically important veil of secrecy. For those who saw this as the major aim of sociological enterprise, pollsters naturally gained the stature of sociological giants.

This understanding of the role of sociologists was shared by international observers familiar with Soviet society. One of the final Soviet-era scandals broke out in Novosibirsk in 1983 after the text of a memo, which was intended for a closed seminar, was leaked. The memo, which recognized the existence of latent class conflict within the USSR and called for the wider introduction of quasi-market mechanisms to the planned economy, emerged in the West, where it was published and widely discussed. Its author, Tatyana Zaslavskaya, found herself famous overnight. What made the conference paper important was not that it contained any ideas or evidence that was totally unfamiliar to Sovietologists. The remarkable thing about it was rather, from the perspective of the Western press, that its existence proved that such views could be expressed by a member of the Soviet establishment.
Soviet academic establishment who was officially entitled to define realities of Soviet societies. For outside observers, as well as from the perspective of social sociologists themselves, the very fact of its emergence and relatively free circulation along administrative channels signaled important political changes.

The importance of sociology was also recognized among the Soviet educated public, which we have so far omitted from this discussion. Soviet people were surrounded by information screens but they were constantly anxious to know what lay behind them. One of the central slogans of “Solidarność,” the Polish trade union that became a central force in overthrowing Communist rule, was “To tell the truth about the real situation in the country.” This also expressed the feelings of many Soviet citizens. The initial burst of enthusiasm over Soviet sociologists, which made their leaders little short of media stars, was followed by a period of indifference during which sociologists were left studying local problems. This was, in turn, followed by a new wave of acclaim during the Perestroika years when national research became possible and the list of forbidden topics rapidly shrank. After decades of struggle, Soviet sociologists ultimately prevailed. Soviet sociology ended in a burst of enthusiasm, not in a whimper of subservience.

**DISCUSSION AND CONCLUSION**

This returns us to the original question of this paper: Why has the fame of Soviet sociologists persisted, while the specific fruit of their intellectual labors has almost been forgotten? The answer to this question consists of two parts, the first part pointing to the ambiguity of intellectual achievement, the second to the traits of Soviet society that favored certain forms of achievement over others. First, there is a growing literature that questions the accuracy of the conventional opposition between “moral” and “purely intellectual” qualities of a researcher and the belief that great discoveries are predominantly a product of the latter (Shapin 1995; Lamont 2009). All involved in research know that any groundbreaking study is no less as a demonstration of stamina and courage, as it is of imagination and breadth of vision. Some settings, however, tax character qualities particularly harshly, and they are particularly likely to produce figures whose standing as heroes fully eclipses their contribution as providers of facts or ideas. An example here are polar explorers: few people remember what (if any) were Scott’s unfortunate expedition discoveries, but this does not detract much from Scott’s fame.

The settings in which investigators have to deal with strong intentional secrecy are similar to polar expeditions in the sense that they are another area in which moral qualities become a dominant element of academic achievement. The discoveries of sociologists struggling with intentional secrecy are akin to the revelations of investigative journalists: as with journalism, the major obstacle in disclosing intentional secrets are usually organizational or political, rather than intellectual. Facts are difficult to construct because of the need to overcome the active resistance of those who would like to suppress their construction. To deal with such secrets one needs courage, patience, diplomatic skills, sometimes even cunning, and other qualities usually qualified as moral. The success of the founding fathers of the Soviet sociology lay in their possession of these qualities and this, more than anything else, was what they were and are still admired for.

The second part of the explanation points to the specific characters of Soviet society, which was unique in the extensiveness of secrecy present in it, and which thus particularly favored moral components of academic achievement over the cognitive. I argued above that this preoccupation
with suppressing information open to all in most other countries was a by-product of its self-definition as a single rational project. The pervasiveness of secrecy engineered to sustain this self-definition created numerous obstacles Soviet sociologists had to overcome. It also gave their work importance unheard of in other contexts. The central legitimizing myth of Soviet society as scientifically planned gave certified scientists’ voices enormous weight as they could not be ignored without jeopardizing the whole of this myth. Ironically, they were regularly reminded of this fact precisely by those Party scions who impeded their studies. The reason why they were so repressed was precisely because what they said was considered so important. This sets the USSR apart from many other twentieth- and twenty-first-century authoritarian regimes. While all non-democratic governments are likely to produce some kinds of enforced secrecy, for most, the scope of such secrecy is limited to highly specific issues, such as the levels of a dictator’s popularity. Moreover, most do not feel that disregarding advice by academics delegitimizes them. The subsequent, post-Soviet, regime refused to take the blame, even for corruption and currency devaluation, and happily ignored sociologists who tried to draw attention to Russia’s mounting problems. Even in the Soviet era, sociologists could probably avoid repression by carefully choosing their research topics, but this came at the cost of rendering their work less important in their own eyes than it could otherwise be. Escaping repression meant shirking the serious challenge of revealing really far-reaching truths able to influence the legitimacy of the Soviet regime and possibly change the country’s course overnight. The research taxing moral qualities most heavily was also the one perceived by sociologists and their publics as far more important, than safer topics.

The problem with studies overcoming intentional secrecy is that they, as with work by investigative journalists, are more likely to produce stand-alone scandalous reports, attracting the widest attention in a short time-span, than lasting intellectual legacies. Settings rich in intentional secrecy produce role models with greater ease than canonical texts, and moral examples for young beginners with greater ease than theoretical generalizations. For those who associate true success and deserved fame in the social sciences with authoring texts and ideas outliving their authors, it would seem that Soviet sociologists were victims of the “resource curse” that has played a prominent role in Russian history in general. They were seduced by the ease with which they could obtain an audience and a sense of self-importance, and neglected to undertake work that would leave more solid landmarks behind. Their efforts at breaking the veil of secrecy were unlikely to outlive the political regime creating this veil. After an enormous change in the dominant forms of political legitimation, the very nature of the Soviet sociologists’ achievements became unintelligible.

This paper emerged from a talk given at a panel of the World Sociological Congress in Yokohama in 2014 named “Failed Sociologists and Dead Ends in the History of Sociology.” Soviet sociology can quite obviously be considered a dead end. Its heroes outlived the memory of their research and there are few people today who would claim to develop their theoretical legacies or who make use of their empirical findings. If, however, we measure success by a feeling of importance that the researchers themselves and their immediate audiences share, then Soviet sociology probably represents one of the peaks of sociological history.

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23 My gratitude is to Christian Fleck, to whom the exciting idea of this session belongs.

24 Yuri Levada is an exception as he had at least two highly visible younger colleagues who claimed developing his ideas on the Soviet personality, Boris Dubin (1946-2014) and Lev Gudkov (b.1946).
ACKNOWLEDGEMENTS

My first and greatest gratitude is to Boris Maximovich Firsov, with whom I taught a graduate seminar on the history of Soviet sociology for six years at the European University at St Petersburg, and to all those who attended this seminar (I wish to specially mention my indebtedness to Katerina Guba, Anastasya Kincharova, and Veronika Kostenko). I owe much to discussions with Darya Dimke, Dmitrii Kurakine, Alexandra Makeeva, and Dmitrii Shalin. Suggestions from Matthew Blackburn and Kirsty Kay made the whole text much more readable than it would otherwise be. In 2010–2011, this work was supported by a small grant from the American Council of Learned Societies (project ‘Re-Assembling the Social Science: Soviet Sociology as a Paradigm’).
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APPENDIX 1: HOW THE SOVIET UNION WAS ORGANIZED

Organizationally, the USSR can be regarded as an extremely complex matrix structure consisting of a multiplicity of independent functional hierarchies. These hierarchies, however, were largely arranged according to the same set of principles, and intertwined at each successive level. The following is, of necessity, a very simplified picture that mentions only the agents appearing in our story.

The basic dualism in this structure was between hierarchies belonging to the state apparatus on the one hand, and to the Party and Komsomol, the Communist youth league – on the other hand. Nearly all organizations providing employment, including industrial enterprises, universities, and research institutes were integrated into the state apparatus. The Party and Komsomol primary cells were created as organizations-employers. As such, Party members from among university professors had to attend meetings of the Party primary cell and to pay dues at the universities where they worked. The higher levels of the Party organization, however, were organized following territorial, rather than institutional, principles. All primary cells were subordinates to the district (rajon) Party committee (rajkom), and the latter to the regional (oblast') committee (obkom) (sometimes an intermediate level of city committee (gorkom) existed). A strict hierarchy among the territorial units of the same class was present, with Moscow obkom being unquestionably the first in Russia, and Leningrad obkom the second for example. Komsomol was structured in the same fashion, and subordinated to the Party at all levels. The role of the Party was described in the Constitution as that of the “leading and directing force of society” (rukovodiaschaya I napravliajuschaya sila obschestva). While any organization was immediately responsible to a respective ministry, it also reported to the local Party committee and could take orders from it. In a way, this system reproduced in institutional form the old Western dualism of mind and soul, with Party playing the role of the soul, and ministries of the mind. The local Party bosses (secretari obkoma) took the credit for any success at in their territory; they were also blamed for any major failures. The boss had subordinates responsible for functional sectors. Thus, in greater academic centers like Leningrad, one of them was responsible for science. This subordinate reported to the regional party boss as well as to the party boss responsible for science across the whole country (this was the head of the respective department of the Party Central committee in Moscow).

Research activities in this system were distributed between several functionally separated hierarchies. Basic research was located at the system of the Academy of Sciences that had the rights and privileges of a separate ministry. The Academy was organized according to strictly bureaucratic principles. It consisted of institutes that were responsible for a given field of research. The institutes were divided into divisions (otdely) and departments (sektora) each responsible for progressively lesser fields. A young employee started as an assistant, then proceeded to a junior researcher, then to senior researcher, and then to a department head (sektora). It was assumed that all those below department heads would work under their close supervision.

Institutions of higher education belonged to the system of the Ministry of Education or some other ministry (e.g. humanities institutes belonged to the province of Ministry of Culture). They were created as administrative centers of territorial units of a certain scale and importance. Their own status was derived from the status of these territorial units. At the very bottom of this pecking order there were provincial teacher’s training institutes existing in every town. At the top were

Still the best description of this structure is probably (Hough and Fainsod 1979).
three types of higher education institutions. The first of them was institutes immediately connected
to the ministries, such as the MGIMO that was connected to the Ministry for Foreign Affairs. The
second type was institutes connected to the Party central organs, such as the Academy for Social
Sciences that was Party’s graduate school. Finally, there were major universities that represented
the capital cities and received students from them, including children of the elites in Moscow,
Leningrad, Sverdlovsk, or Novosibirsk. In addition to these two branches of the academic system
there was the applied research system that was affiliated with the ministries (upper strata) or
particular enterprises (lower strata).
ARTICLE

“Our classroom methodological prescriptions do not fit easily the problems of studying the SS and their doings”: Elmer Luchterhand and sociological research on Nazi concentration camps

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Abstract (English)
In the on-going debate about sociology and National Socialism, publications focused on the history of sociology in National Socialism. Sociologists who dealt with National Socialism have not received as much attention. Based on archival material, the work of sociologist Elmer Luchterhand (1911–1996), who took part in the liberation of Hersbruck concentration camp as an US Army officer, will be portrayed and put into the context of other early research. Even though Luchterhand dedicated much of his professional career to the study of Nazi concentration camps, his work remained largely unpublished. It will be argued that the difficulties he faced were substantially related to methodological concerns. Aware ‘classroom methodological prescriptions’ provided little instruction, Luchterhand grappled constantly with an unrewarded search for the ’right’ way to research genocide.

Abstract (Deutsch)

Keywords
National Socialism, Concentration Camps, History of Sociology, Elmer Luchterhand, Methodological Challenges in Studying Genocide
INTRODUCTION

In the last few years, the relationship between sociology and National Socialism has again given rise to debates in Germany and Austria (see Christ and Suderland 2014 for a recent overview). Since many authors have questioned post-war sociology’s dubious silence in confronting National Socialism and the Holocaust for decades, this is a surprisingly recurrent debate, but it is not new (Kranebitter and Horvath 2015). In the German-speaking world, most debaters have focused on the question of whether sociology existed in Nazi Germany and Austria at all and on the “contaminated” past of certain sociologists (see e.g. Rammstedt 1986; Klingemann 2009), as well as on the reactions of contemporary sociologists to fascism and National Socialism (Käsler and Turner 1992). In this way, publications focus on the history of sociology in National Socialism. The other side—i.e. sociologists who dealt with National Socialism in their research—has not received as much attention, however. The consensus seems to be that there were not many sociologists whose research was dedicated to topics related to National Socialism or the Holocaust, and that the few works that were, as Zygmunt Bauman famously put it in 1988,

show beyond reasonable doubt that the Holocaust has more to say about the state of sociology than sociology in its present shape is able to add to our knowledge of the Holocaust; and that this alarming fact has not yet been faced (much less responded to) by the sociologists. (Bauman 1988: 471).

Even though much has been published since 1988, many authors still agree with that statement. Agreement is not only in Germany and Austria, but also in the US, where it has been stated that sociologists did not consider fascism, National Socialism or the Holocaust to be potential topics for sociological research (Bannister 1992; Berger 1995, 2012; Gerson and Wolf 2007; Halpert 2007). However, the main problem with this view is that those few early studies that did exist have not been studied thoroughly. The reasons for remaining unpublished, remaining incomplete or, most importantly, having been ignored, have not been considered adequately. Yet there are more hints about unfinished projects on this topic than on any other.

The starting point for this paper is that there were sociologists dealing with Nazi concentration camps who deserve to be dealt with for various—not least methodological—reasons but who were hardly ever read or taken notice of in the ongoing debate, perhaps because of the narrative that there was no sociology of National Socialism whatsoever. Based on archival sources, this article will outline the research of the American sociologist Elmer Luchterhand (1911–1996), one of the most forgotten, yet probably most productive researchers. It will contrast it with other early research on concentration camps and focus on research in US sociology. Existing, but still neglected studies—in particular by Polish authors (e.g. Pawelczyńska 1979; Jagoda et al. 1994)—are beyond the scope of this paper.

THE CONTEXT: SURVIVORS’ REPORTS AND PSYCHOLOGICAL WARFARE

It is easy to identify the first studies on concentration camps by trained or future social scientists. Some of these were even published before 1945: Paul Martin Neurath (2004 [1943]); Bruno Bettelheim (1943); Eugen Kogon (1946); Viktor Frankl (2008 [1946]); Benedikt Kautsky (1948 [1946]); Ernst Federn (2012 [1946]); Hans G. Adler (1960); and the aforementioned Anna Pawelczyńska (1979), to name but a few of the most cited authors. They were concentration camp survivors, and social scientists. Their accounts were meant to be more than “survivor’s reports.” Instead, they had research objectives, even though this is precisely what was disputed by their
academic colleagues, the general public and even their fellow survivors. Bruno Bettelheim, for example, was asked if he had obtained permission to publish his observations from SS guards and fellow prisoners (Fleck and Müller 2006: 189), and Paul Neurath not only had difficulties in getting his book acknowledged as a PhD thesis at Columbia University, but was also told by a friend that: “... just because one repeatedly uses the word ‘society’ and speaks of a rule of game one is not a sociologist.”1

Most of the survivors mentioned had to convince many people that their books were more than “literature” and were of general scientific relevance and importance, but at least they were heard. Others working in the background were not. Kogon’s book might serve as an example in this regard. Whereas his book has had 43 editions with a circulation of more than 500,000 copies in Germany alone, and might thus be regarded as one of the best-known books of German-speaking sociology in general, the men behind this study fell into oblivion. In fact, the book was based on a study compiled by the Psychological Warfare Division (PWD) within the British-American Supreme Headquarters Allied Expeditionary Force (SHAEF) (Hackett 1995). It was the Göttingen-born Jewish German Albert G. Rosenberg, later to become Professor of Sociology at the University of Texas in El Paso, and his dedicated team called the Kampfgruppe Rosenberg [Rosenberg Task Force] within the PWD/SHAEF who were responsible for the basic Buchenwald Report study. It was they who acquired and adapted social research methods during their military training in the field (Kranebitter 2016), by interrogating German Prisoners of War with teams of reliable anti-fascists and by using questionnaires developed by sociologists like Edward Shils (Lerner 1971: 109–110). What they did was no different to what Shils and Janowitz (1948) later called “the sociological and psychological analysis which the propagandist must make if he is to obtain maximal response to his communications” in ‘Cohesion and Disintegration in the Wehrmacht in World War II’ (ibid: 280). Rosenberg’s team applied standard research methods to the situation at the Buchenwald concentration camp for the purpose of the German Wehrmacht’s imminent surrender and re-education. After the war, Rosenberg became professor of sociology and social work at the University of Texas in El Paso (UTEP). Now it seems clear that he and his team were obliged to ‘step back’ for political reasons and because of the secrecy of intelligence reports. In their place, Eugen Kogon—a left-wing catholic who studied sociology in Vienna with the fascist sociologist Othmar Spann—was given full credit for this unique study of the “SS state” (Kogon 1946), not the intelligence team of Austrian and German Jewish emigres.

The Buchenwald Report was the first sociological study of concentration camps and took one of the most innovative approaches, but it was not the only one. From 1945 to 1951, an interdisciplinary team made up of the psychologist Jacob Goldstein, the historian Herbert A. Strauss and the sociologist Irving F. Lukoff analyzed 507 interviews with 728 Hungarian Jewish survivors taken

1 In German: „Weil man society 10 mal wiederholt und [von] einem rule of game spricht, ist man noch kein Soziologe. Du verzeihst die Bosheit...” (Letter by Felix Reichmann to Paul Martin Neurath, 12 April 1943. Paul Martin Neurath Papers, Paul F. Lazarsfeld-Archives, Department of Sociology, University of Vienna, see Fleck et al. 2004 in general). In the same letter Reichmann wrote: “Even if 100 ‘Dachauers’ sent you critical material, it would never be reliable sociological data. A patient is not able to describe his condition in the same way a doctor can. Not only because he does not see the connections, this would not be true in your case, but simply because the doctor feels no pain.” [In German: „Auch wenn hunderte alte Dachauer Dir kritisches Material brachten, es werden nie reliable sociologische data draus. Der Patient kann seinen Zustand nie so beschreiben wie der Arzt. Nicht bloss weil er die Zusammenhänge nicht sieht, das würde in Deinem Fall nicht zutreffen, sondern einfach weil der Arzt keine Schmerzen hat.”]
from 14,000 interviews by the Jewish Agency in Budapest (Goldstein et al. 1991). Like Rosenberg, there were some who had been present at the liberation of the camps as members of the US Army: Herbert A. Bloch, who interviewed survivors in Buchenwald and in the Mauthausen subcamps at Ebensee and Lenzing where he studied the resocialization of 547 women prisoners (Bloch 1947); and Elmer Luchterhand, whose work I will present in this paper in some detail.²

Although individual articles resulting from these research projects were published in the immediate post-war period, it can be said that none of these studies became well known in either sociology or research on Nazi concentration camps. They were either terminated, remained unpublished or were never started. In order to ask why this happened, we have to examine these studies in detail.

**ELMER LUCHTERHAND: WORKS AND FINDINGS**

Elmer Luchterhand was born into a family of farmers of German descent in 1911 in the small township of Colby, Wisconsin. As a young man, he worked as a journalist. According to his 1946 US Army “Separation Qualification Record”, he “[w]orked as a farm editor and also did legislative and general news writing.”³ His main concern, however, seems to have been as a political activist, campaigning against what he called the “Friends of New Germany” in the cities around Lake Michigan. As late as the 1940 census, he still declared his occupation to be an “organizer” in the “industry” of “political work.”⁴ On June 8, 1932, Luchterhand found fame. Under the headline “Police Win Battle With Church Wall Orator,” The Wisconsin State Journal reported on its front page:

Ex-U. W. Student Loses Shirt When Pulled from Rostrum; Two Others Seized / Elmer Luchterhand, youthful and gangling former University of Wisconsin student and another alleged communist were arrested by police after a struggle today when he tried at 2 p.m. to lead a protest meeting of unemployed in front of St. Raphael’s church, opposite the Dane county courthouse.⁵

While what followed may tell us more about the harshness of state policy in dealing with social protests than about Luchterhand’s biography, it should not be ignored. After his arrest, Luchterhand was transferred from the Dane County Jail to the “Wisconsin State Hospital for the Insane,” with the alleged mental condition “Psychopathy”.⁶ He was not released until 18th June

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³ Army of the United States, Separation Qualification Record, undated [1946], provided by Erika Luchterhand.
⁵ The Wisconsin State Journal, Vol. 140, No. 68, Madison, June 8, 1932, page 1. According to the Jail Register Dane County (Wisconsin Historical Society Archives, Dane County Series 4, Sheriff, Jail Register, 1872-1931, January – 1933, November, Vol. No. 9), Elmer Luchterhand and Edward Pollock were arrested on June 8, 1932, for the reason “Disorderly and viol. city traf. ord.,” and sentenced to a bail of 200 dollars, which they paid. According to the Register, which lacks more detailed information, Luchterhand was then sent “To Mendota for observation”, which meant the “Wisconsin State Hospital for the Insane”.
1932 by an “Order of Court.” His “condition when discharged” is given as “Improvement.”

The newspapers from that period point to the influence of a broad political protest movement that eventually succeeded in getting him released.

In March 1943, Luchterhand was drafted into the US Army. In his position as Sergeant and Public Relations Writer of the 261st Infantry, 65th Infantry Division, he was among the troops who liberated the Hersbruck concentration camp—a subcamp of Flossenbürg concentration camp—on April 20, 1945. His exact duties are unknown, but it seems reasonable to assume that he was given a kind of special army assignment not dissimilar to Rosenberg’s regarding re-education. In a “Summary of Military Occupations”, his Separation Qualification Record states:

Public Relations Writer: Worked with a battalion intelligence section, interrogating German Prisoners of War. Handled civil affairs in a number of towns before arrival of military government. Assisted regimental information and education officer. Edited regimental newspaper and supervised the work of 20 company public relations representatives. Served as chief of public relations section, XX Corps.

In this context, from April to November 1945 Luchterhand visited eight different concentration and labour camps upon their liberation, interviewing some 75 survivors and bystanders—whom he preferred to call co-presents, (see Luchterhand 1979: 3)—in Ohrdruf, Buchenwald, Hersbruck/Happurg, Gusen, Mauthausen, Dachau, Wanfried, and Feldafing, as well as in the hamlets that the death marches in Austria had passed through (Luchterhand 1949: vii).

After his return to the US, Luchterhand completed his BA, MA and a PhD at the University of Wisconsin. His research was already focusing on survival and resistance in Nazi concentration camps. His MA thesis (Luchterhand 1949) was based on the informal interviews mentioned above and on dozens of existing survivors’ accounts published shortly before and after 1945. His PhD thesis was based on interviews with 52 concentration camp survivors (among them survivors with training in the social sciences such as Paul Neurath and Ernst Federn) in the United States in 1950 and 51 (Luchterhand 1952). His thesis was supervised by two eminent experts on Germany and Nazism, Hans Gerth and Howard P. Becker. Both were not only well known as experts on Max Weber, but had also written papers on National Socialism. Gerth—after fleeing Nazi Germany at

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7 “Discharges for the Month of June 1932” (Wisconsin Historical Society Archives, Mendota Mental Health Institute, Admissions and Discharge Registers, 1881-1960, Series 2166, Volume 10, p. 159)

8 On June 16, there are reports of a meeting of 300 people, including Communist Party members as well as staff members of the University of Wisconsin, which protested for Luchterhand’s release (The Wisconsin State Journal, Vol. 140, No. 76, Madison, June 16, 1932, page 1). On June 18, a medical doctor is quoted as stating that Luchterhand was a “psychopathic person without psychosis,” but was eventually to be released as “now sane” (The Wisconsin State Journal, Vol. 140, No. 78, Madison, June 18, 1932, page 1).

9 Army of the United States, Separation Qualification Record, undated [1946]. He described his presence at the liberation of Hersbruck concentration camp in an introduction to the memoirs of a priest who served as an SS secretary in Hersbruck concentration camp (Lenz 1982; due to disagreements with Lenz the intended introduction was not published in this volume): “On Hitler’s birthday, April 20-1945, while his true believers tuned their radios for what was to be the last live broadcast of his voice, units of an American infantry regiment – mine – moved painlessly through the market town of Hersbruck. Just outside it, they passed without recognizing what it was, and almost without seeing it, a small concentration camp. There were only twenty barracks and several auxiliary buildings” (Elmer Luchterhand: An Introduction, February 1981, Elmer G. Luchterhand Papers, Brooklyn College Archives and Special Collections, Accession #2001-005 [from now on ‘Elmer G. Luchterhand Papers’], Sub-Group II, Series 5, Box 9). In a CV, he stated only to have been an infantry officer in the Rhineland and Central European campaigns from 1943 to 1946 (Elmer G. Luchterhand Papers, Sub-Group V/VI, Series 14/15/16/17, Box 19).

10 Army of the United States, Separation Qualification Record, undated [1946].
the last possible moment in 1939—started his career in the US with a widely-read paper on ‘The Nazi Party: Its Leadership and Composition’ (Gerth 1940, see also Gerth 2002: 87–94). Becker, who had worked for the Office of Strategic Services (OSS) in Britain, France, Luxembourg, Germany and Austria from 1944 to 1948, wrote extensively about the relationship between Nazism and German youth movements (see Becker 1946).

In 1970 and 71, Luchterhand repeated his interviews with 43 of his earlier respondents from 1950 and 1951. He interviewed them about readjustments in the post-war era and stress factors, representing a rare longitudinal study on concentration camp survivors.

<table>
<thead>
<tr>
<th>Social Characteristics of 52 Veterans of Nazi Concentration Camps</th>
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<tbody>
<tr>
<td>Category</td>
</tr>
<tr>
<td>---------------------------------</td>
</tr>
<tr>
<td><strong>Age at induction</strong></td>
</tr>
<tr>
<td>16-20</td>
</tr>
<tr>
<td>21-30</td>
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<tr>
<td>31-40</td>
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<tr>
<td>41-50</td>
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<td>58</td>
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<td><strong>Sex</strong></td>
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<tr>
<td>Male</td>
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<tr>
<td>Female</td>
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<tr>
<td><strong>Marital status at induction</strong></td>
</tr>
<tr>
<td>Single</td>
</tr>
<tr>
<td>Married</td>
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<tr>
<td>Married with children</td>
</tr>
<tr>
<td><strong>Occupation at induction</strong></td>
</tr>
<tr>
<td>Business manager</td>
</tr>
<tr>
<td>Artist</td>
</tr>
<tr>
<td>Dress designer</td>
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<tr>
<td>Farm laborer</td>
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<tr>
<td>Farm operator</td>
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<table>
<thead>
<tr>
<th>Occupation</th>
<th>Count</th>
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<tbody>
<tr>
<td>Housewife</td>
<td>5</td>
</tr>
<tr>
<td>Journalist and writer</td>
<td>1</td>
</tr>
<tr>
<td>Musician</td>
<td>1</td>
</tr>
<tr>
<td>Office worker</td>
<td>5</td>
</tr>
<tr>
<td>Political party functionary</td>
<td>1</td>
</tr>
<tr>
<td>Salesman</td>
<td>5</td>
</tr>
<tr>
<td>Social worker</td>
<td>2</td>
</tr>
<tr>
<td>Student</td>
<td>5</td>
</tr>
<tr>
<td>Teacher (university, college)</td>
<td>3</td>
</tr>
<tr>
<td>Beautician</td>
<td>1</td>
</tr>
<tr>
<td>Butcher workman</td>
<td>1</td>
</tr>
<tr>
<td>Electrician</td>
<td>2</td>
</tr>
<tr>
<td>Factory supply clerk</td>
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<tr>
<td>Mechanic</td>
<td>1</td>
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<tr>
<td>Plumber</td>
<td>1</td>
</tr>
<tr>
<td>Sewing machine operator</td>
<td>2</td>
</tr>
<tr>
<td>Shoe worker (skilled)</td>
<td>2</td>
</tr>
<tr>
<td>Tool and die maker</td>
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Approximate class position at induction

<table>
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<th>Count</th>
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<td>Upper class</td>
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<td>Middle classes</td>
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<td>Upper middle</td>
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<tr>
<td>Middle</td>
<td>18</td>
</tr>
<tr>
<td>Lower middle</td>
<td>15</td>
</tr>
<tr>
<td>Working class (skilled)</td>
<td>12</td>
</tr>
</tbody>
</table>

Country of longest residence before induction

<table>
<thead>
<tr>
<th>Country</th>
<th>Count</th>
<th>Percentage</th>
</tr>
</thead>
<tbody>
<tr>
<td>Germany</td>
<td>31</td>
<td>60 %</td>
</tr>
<tr>
<td>Others</td>
<td></td>
<td>40 %</td>
</tr>
<tr>
<td>Poland</td>
<td>9</td>
<td></td>
</tr>
<tr>
<td>Austria</td>
<td>7</td>
<td></td>
</tr>
<tr>
<td>Czechoslovakia</td>
<td>3</td>
<td></td>
</tr>
<tr>
<td>Netherlands</td>
<td>1</td>
<td></td>
</tr>
<tr>
<td>Hungary</td>
<td>1</td>
<td></td>
</tr>
</tbody>
</table>

Places of longest residence before induction

<table>
<thead>
<tr>
<th>Place</th>
<th>Count</th>
</tr>
</thead>
<tbody>
<tr>
<td>Cities over 100,000</td>
<td></td>
</tr>
<tr>
<td>Berlin</td>
<td>11</td>
</tr>
<tr>
<td>Vienna</td>
<td>6</td>
</tr>
<tr>
<td>Breslau</td>
<td>3</td>
</tr>
<tr>
<td>Frankfurt on the Main</td>
<td>3</td>
</tr>
<tr>
<td>Düsseldorf</td>
<td>2</td>
</tr>
<tr>
<td>Krakow</td>
<td>2</td>
</tr>
</tbody>
</table>
Table 1: Social Characteristics of 52 Veterans of Nazi Concentration Camps (from Luchterhand 1952: 9–11). For a discussion of “representativeness,” compared to the “prisoner’s society” of the Mauthausen concentration camp, see Kranebitter 2014: 80–83.

In 1967, Luchterhand became Assistant Professor of Sociology at Brooklyn College and in 1971 was elected Full Professor. The sociology department included sociologists such as its long-time chair Alfred McClung Lee, Charles Radford Lawrence and the German emigre Hilda Weiss. Charles Lawrence in particular played an active role in Luchterhand’s ‘recruitment’ to the college. From fall 1966 the department was “concerned with covering courses”\(^{12}\), openly looking for potential candidates. In March 1967, after the faculty “had interviewed Elmer Luchterhand at lunch on Monday, February 20 and now considered his qualifications for an appointment to a university associate professorship,”\(^{13}\) he was unanimously recommended for appointment as Associate Professor.\(^{14}\) In the summer of 1967, Lawrence finally introduced Luchterhand to his departmental colleagues in a letter:

Elmer Luchterhand, University Associate Professor, is a product of the University of Wisconsin, having taken his undergraduate and doctoral work in Madison. He comes to us from New Haven where he has been Director of Research for Community Progress, Inc. and Research Associate at Yale. Elmer has a special interest in the sociology of groups under severe stress, a rubric under which he has studied survivors of Hitler’s concentration camps, inner-city youth, and new industrial communities in

\(^{12}\) Minutes of the Appointments Committee, Monday, December 19, 1966 (The Office of the President, Brooklyn College Archives and Special Collections, Accession #91-032, Sub-Group XXI, Series 1, Box 330).

\(^{13}\) Minutes of the Committee on Appointments, February 27, 1967 (The Office of the President, Brooklyn College Archives and Special Collections, Accession #91-032, Sub-Group XXI, Series 1, Box 330).

\(^{14}\) Minutes of the Departmental Appointments Committee, March 1, 1967.
It is noteworthy that many members of departmental staff, soon including Luchterhand, took an active part in the social protests of the time, which was indicative of the college’s general intellectual climate. Between them, they funded the Martin Luther King Jr. Lectures given by prominent leftists like Howard Zinn.\(^{10}\) They discussed and supported protests—such as a strike by the United Federation of Teachers in 1968—and encouraged research on the faculty’s surrounding neighbourhoods in Brooklyn.\(^{17}\) They also actively demanded that the “summary expulsion of students involved in the May 18, 1968 sit-in be deplored on the grounds that such action was in violation of the spirit of due process”\(^{18}\) and that criminal charges against them be dropped.

Whatever effects the turbulent college context may have had on Luchterhand’s teaching and faculty activities, he nevertheless started working on a book he called *Doggerwerk* in the early 1970s, and throughout the following years conducted 73 interviews not only with survivors and co-presents, but also with perpetrators. Luchterhand worked on this book until his retirement in 1981. He died on one of his research visits to Germany in 1996 with *Doggerwerk* unfinished. In short, despite the fact that Elmer Luchterhand worked and published on stress in natural and man-made disasters, as well as in the fields of youth sociology and the sociology of work, throughout his life his main concern was the study of concentration camps. Symptomatically, in one of his draft introductions to his *Doggerwerk* study he states (although later deleting the sentence): “It was around the 20\(^{th}\) of April, 1945 that this inquiry had its unofficial beginning.”\(^{19}\)

\(^{15}\) Letter of Charles R. Lawrence to the Colleagues in Sociology Department, August 1, 1967 (The Office of the President, Brooklyn College Archives and Special Collections, Accession #91-032, Sub-Group XXI, Series 1, Box 330).

\(^{16}\) News, Office of College Relations, Brooklyn College, March 20, 1969, For Immediate Release (The Office of the President, Brooklyn College Archives and Special Collections, Accession #91-032, Sub-Group XXI, Series 1, Box 330). The lectures were organized by the sociologist Hylan G. Lewis.

\(^{17}\) According to the meeting minutes, “Professor Luchterhand presented a 25-minute paper, setting forth some of the sociological implications he perceived in the Ocean Hill–Brownsville – UTF-Board of Education controversy. A lively discussion followed” (Minutes of the Special Meeting, October 21, 1968, Brooklyn College Archives and Special Collections, Accession #91-032, Sub-Group XXI, Series 1, Box 330). After discussions on the “continuing and ongoing struggle over the character of public school control and personnel in the City”, the department called the President to “appoint and organize an academic task force of appropriate specialists to investigate both the short-term and long-term aspects of this struggle in the context of the changing ethnic percentages, aspirations, and leadership in the City’s neighborhoods, [...]” (ibid.). In his answer, President Harold C. Syrett urged to department to do the work itself: “I enthusiastically agree with the substance of the department’s resolution, but I disagree on how this resolution should be implemented. I have more than once suggested to you that in my opinion sociologists should be collecting and interpreting the information of the kind called for by the resolution. In short, this is the work of professionals, and your department by its very nature has the professional competence needed. The fact is that Brooklyn College knows very little about the sociology of either the Borough of Brooklyn or the City of New York. We have neither the facts nor the theories that might grow out of such facts. No presidential task force could possibly be as well equipped for this work as the Department of Sociology. I am, therefore, proposing that the Sociology Department undertake the task outlined in the resolution and that it draw on any help from other departments that it may require. Sincerely, Harold C. Syrett” (Letter of Harold C. Syrett to Charles R. Lawrence, November 12, 1968, Brooklyn College Archives and Special Collections, Accession #91-032, Sub-Group XXI, Series 1, Box 330).

\(^{18}\) Minutes of Department Meeting, October 16, 1968 (Brooklyn College Archives and Special Collections, Accession #91-032, Sub-Group XXI, Series 1, Box 330).

\(^{19}\) Elmer G. Luchterhand: The Start of Field Work and Method, 8 February 1982, Elmer G. Luchterhand Papers, Sub-Group III, Series 9, Box 15.
The aim of his early studies, on which I want to focus in this paper, was to test the hypotheses formulated by Bruno Bettelheim. Bettelheim had a hard time finding someone willing to publish his article (Fleck and Müller 2006). Yet it was widely read immediately after it appeared in print in 1943. Bettelheim’s core assertion was that the longer prisoners stayed in concentration camps, the more they would identify with the Gestapo or SS, taking on their values. “Old prisoners” would therefore regress to a childlike state, identifying with the aggressor. Survival for other prisoners would then be a matter of staying sane by choosing the right coping strategies of one’s pre-camp personality, by splitting one’s personality into one which was present and one that observed what was going on (“de-personalization”). In other words, isolating oneself from the prisoners’ society, and especially from “old prisoners” by individual means. “It seems easier to resist the pressure of the Gestapo and the Nazis if one functions as an individual,” Bettelheim concluded (1943: 452). Bettelheim’s individualistic thesis had a great impact on concentration camp research. Many of the researchers already mentioned, especially Bloch (1947) and Arendt (1950), referred to it, and well-known books like The Order of Terror by Wolfgang Sofsky (1993) repeated the same argument.

According to this view, the SS had been successful in its aim of atomizing, dissolving, and disaggregating the prisoners’ society, making survival arbitrary and only possible on an individual basis. According to Sofsky, ultimately “the state of aggregation of society approximated dissociation” (Sofsky 1998: 1155, my translation).

Luchterhand questioned this view early on, which he labelled “Hobbesian.” His MA thesis already contained the conclusion that:

> Even in the period of induction [into the camps – AK], to characterize life in KL society as a Hobbesian ‘war of each against all,’ as some observers have done, may sound convincing on the basis of the journalistic reports which rose to a flood in the period of dissolution of the concentrationary universe. The facts are clearly otherwise as presented in this study (Luchterhand 1949: 223).

At that time, however, this view was only based on his informal interviews and close reading of available publications. To further substantiate this thesis, Luchterhand conceptualized interviews with survivors who had immigrated to the US after 1945. Even though it was clear to him that “there is no possibility of obtaining a representative sample of the whole prisoner population” (Luchterhand 1952: 5, see also Luchterhand 1967 for a short description), his research design involved sampling questions. Ultimately, his 52 interviewees showed considerable variation in terms of age, gender, functions and work details in the camps, as well as the duration of their imprisonment in various camps. Using lengthy interview guides, Luchterhand investigated changes in certain behavioural patterns. Applying what were then innovative interview techniques, he was able to gather data on social actions such as theft—which hardly anyone gathered either before or after him—patterns of social relationships, religious beliefs, and highly sensitive issues like sexual behaviour.

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20 Terrence Des Pres (1976, the German version was only published in 2008) would be another example of an individualistic approach close to Bettelheim’s, even though Des Pres stressed major differences to Bettelheim, and was also seen as his opponent (Pollak 1988: 166–172). Whereas Bettelheim (1943) in his psychoanalytic approach stressed the importance of individual (pre-war) autonomy against disintegration, De Pres (1976) stressed the prisoners’ needs to adapt morally to a new situation by developing a kind of survivor’s morale. Even though there is an important and serious contradiction here, it could well be argued that both positions only mark the opposites of individualistic approaches, therefore more strongly resembling rather than contradicting each other – ultimately, whatever the individualistic “reason” for survival in its concrete theorization may be, both highly overestimate the individual freedoms available for social actions in the circumstances of a concentration camp (see Kranebitter 2014: 221).
By reading statements about incidents of theft, for example, to his respondents, nine interviewees admitted to having committed theft in the camps, while five more interviewees admitted to having thought about thieving (Luchterhand 1952: 140). Since most of these incidents occurred when the prisoners had been on transports or had recently been transferred to another camp, Luchterhand concluded that far from increasing in likelihood the longer one was imprisoned, thieving was likely to be abandoned in favour of behaviour like the infamous “organizing.” This denoted the acquisition of food or consumables illegal in terms of SS “norms,” but did not include stealing from other prisoners, which carried harsh sanctions within the prisoner society. Therefore, Luchterhand cautiously proposed the thesis that “the longer one lived among prisoners the more one became acculturated, taking on the norms of the main mass of prisoners” (ibid.: 152), i.e. a prisoner code. Certain behaviours and the change in behavioural patterns in the camps pointed towards the emergence of a social system among prisoners influencing their behaviour to a large extent. He also concluded this from the observation that patterns of interpersonal relations generally changed positively—i.e. resulting in more intense social ties—with longer imprisonment (ibid.: 124–127). This was also based on his respondents’ accounts that suicides mostly happened during early stages of imprisonment (ibid.: 243), on the finding that members of the underground organizations were mostly so-called “old prisoners,” prisoners who had been incarcerated for a long period (ibid.: 212–217); and from the observation that prisoners’ earlier traumatization “tended to adapt in harmony with the prisoner code” (ibid.: 258) during longer imprisonment.

In short: Luchterhand’s findings in his 52 interviews essentially contradict Bettelheim’s hypothesis. It was not by being a “lone wolf”—by isolating oneself from a “prisoners’ society”—that one survived. It was not by being a “Speckjaeger,” as one of his interview partners, a Mauthausen survivor, called them, but through a relationship pattern of sharing that Luchterhand termed “stable pairing.”

From the data of this study, much of the strength of survival—psychic and physical—seems to have come from ‘stable’ pairing. With all of the raging conflicts in the camps, it was in the pairs, repeatedly disrupted by transports and death, and paradoxically restored in general bereavement, that the prisoner kept alive the semblance of humanity (Luchterhand 1967: 259f.).

He categorized only four out of 47 of his interviewees as “lone wolves,” the rest being involved in at least some sort of social relationship involving sharing and mutual aid. Moreover, the mean time of imprisonment of the few “lone wolves” was significantly shorter than those involved in any kind of social association. Contrary to Bettelheim, this led to the conclusion that “old prisoners” were more likely to be involved in sharing relationships than “new prisoners.” Accordingly, the pairs and groups he found were rarely networks formed in the pre-camp era, but rather networks built spontaneously in the camps. Quite often the interviewees could not say why they chose this or that person, sometimes they could not even remember their names. Luchterhand called them “incompatible pairings” (Luchterhand 1952: 100). From all of his findings, he concluded that there was such a thing as a prisoner code; a prisoner social system with an associated system of norms, which developed within the camps and which was crucial for survival.

Later, Luchterhand also considered these numbers to be very low, given situations conducive to theft. In the author’s opinion, this result is unlikely to have been achieved in later interview projects with concentration camp survivors, which is one example that highlights the value of Luchterhand’s research in the light of current research on concentration camps.
Table 2: Number and mean imprisonment time of former concentration camp prisoners per “pattern of association” (from Luchterhand 1967: 252). Since involvement in small and large groups also included involvement in “stable pairs,” Luchterhand stressed the importance of the latter.

Stressing the existence and importance of a prisoner social system for survival was a genuine sociological perspective. Luchterhand continued to take this perspective in his later work, in his longitudinal study, when he interviewed most of the older sample again in 1970 and 1971, for example. His main finding at this time was that since no correlation could be found (except for gender) between stress (measured by a “mental health” or stress index used by Gerald Gurin) and the length or place of imprisonment, post-war stress symptoms were likely to be post-war-products and not necessarily connected to the camp era. On the contrary, and paradoxically, imprisonment, especially longer stays in the camps, appears to have had stress-releasing and de-traumatizing effects as well as going through similar situations more often. Luchterhand differentiated strongly between different types of situations, such as induction, roll calls, transports, and death marches. It seemed that “the stress manifested by our sample members is chiefly a consequence of stressor situations in post-camp life” (Luchterhand 1972: 5). Again, with a seemingly straightforward thesis he questioned central psychological theories such as “survivor guilt,” which manifested differently in the different post-war societies.

Bettelheim’s individualistic and anti-sociological stance—“He writes as though no prisoner social system existed or could exist” (Luchterhand 1967: 248, E.L.’s emphasis)—implicitly remained Luchterhand’s core point of attack. When writing a proposal for the National Endowment for the Humanities in November 1973, he outlined the motivations for his research on concentration camps. In contrast to Bettelheim who in effect (consciously or unconsciously), proved “the efficiency of Nazi terror” in his research, Luchterhand wanted to show the limits of Nazi politics.23

22 Elmer G. Luchterhand: The Resocialization of Nazi Camp Survivors (unpublished manuscript, New York 1972), p. 5 (Elmer G. Luchterhand Papers, Sub-Group II, Series 5, Box 9). Interestingly, in this — as well as in many other points, like the importance of small groups for survival in general — sociologist Michael Pollak (1988) came to similar conclusions, without knowing Luchterhand’s work. I want to thank Christian Fleck for this hint.

23 The paper, dated 17 November 1973 and addressed to the National Endowment for the Humanities, Washington D.C., is titled ‘Purpose’ (Elmer G. Luchterhand Papers, Sub-Group V/VI, Series 14/15/16/17, Box 19, Request for Grant Money: Different Applications, Research Proposals).
It is worth noting that Bruno Bettelheim seems not to have recognized Luchterhand’s criticism, or rather refused to answer it. There was only one psychoanalyst who responded to his research, the Austrian Ernst Federn, who corresponded with Luchterhand while he worked on his dissertation and who was a close ally of Bettelheim during his time in the camps. In a letter dated April 1969, Federn contradicted Bettelheim’s overemphasis of the “identification with the aggressor,” while simultaneously rejecting Luchterhand’s sociological approach in general:

I would like to start with your primary thesis that the ‘human group and social system emerge as the most fruitful foci for social scientific analysis’. This is a fair statement to make for a social scientist and in fact his job. Of course I disagree because I fail to see how a strictly sociological approach can throw sufficient light on psychological problems.24

“MOREOVER, THE SERIES DOES NOT INCLUDE BOOKS ON METHOD.” EXPLORING MARGINALIZATION AND NON-PUBLICATION

Elmer Luchterhand did publish some papers on his studies, albeit not the books he planned. However, they are almost never cited in recent sociological studies on Nazi concentration camps. Most notably they are ignored by Wolfgang Sofsky (1993) and Maja Suderland (2009). It is not an exaggeration to say that sociology always responded with silence. When Luchterhand presented a paper on his Doggerwerk study to an Ad Hoc Group at the 9th World Congress of the International Sociological Association in Uppsala in 1978 (Luchterhand and Wieland 1978), he was asked to repeat his presentation the following day to the Research Committee on the Sociology of Mental Health.25 After this second presentation a colleague, Thomas J. Scheff, asked the audience “to observe a minute of silence in recognition of my [E.L.’s – AK] research undertaking.”26 Although scientific interactions at conferences might constitute a theme (Collins 2015), a minute of silence can hardly be considered “normal” at such events. In a letter, Luchterhand interpreted it as a symptom of scientific curiosity, but one that might be connected to a general silence regarding reactions and responses to National Socialism:

The prolonged silences of research people in sociology and psychology in this research area are something to which I have long had to adjust; this special silence came as a stunning surprise. I mention this reception for the paper only to affirm [...] that interest in studying the peculiar destructiveness of the German variant of fascism is far from over.27

Sociologists’ reaction of silence might have been a sign of curiosity. Yet it resulted in the non-publication of his later research by making it “outstanding” in every sense of the word. The episode is symptomatic: sociological research on concentration camps did not provoke a scientific response, but was regarded as being something special and exceptional, something beyond classroom sociology. It was methodology that was stated as the problem. When Luchterhand asked

24 Letter by Ernst Federn to Elmer G. Luchterhand, 9 April 1969, Elmer G. Luchterhand Papers, Sub-Group I, Series 1, Box 3.
26 Letter by Elmer G. Luchterhand to George L. Mosse, 28 August 1978, Elmer G. Luchterhand Papers, Sub-Group IV, Series 11/12/13, Box 18.
27 Letter by Elmer G. Luchterhand to Vice-President and Provost of Brooklyn College Donald R. Reich, 31 August 1978, Elmer G. Luchterhand Papers, Sub-Group IV, Series 11/12/13, Box 18.
the same colleague who had called for the minute’s silence at the conference in Uppsala for his opinion on his research, the colleague advised him not to submit his proposal to two institutions due to methodological concerns:

“They are both quite likely to turn them down, on the grounds that your design is not sufficiently systematic. I do not sympathize with such a view, but I don’t know any way of getting around it.”

Thomas Scheff was not the only one to express methodological concerns.

It is worth quoting a letter by the well-known historian George Mosse, who reviewed some parts of Luchterhand’s Doggerwerk manuscript for SAGE publications, one of the reactions to Luchterhand’s research, at length:

Thank you very much for your letter of August 28. I have read your paper with interest. It seems to be a very fruitful line of approach, though it is difficult to see where it would lead you or what will hold it together eventually. I take it that it will be a study of this local concentration camp and of this minister who worked in it. I do not know whether you are going to stress message in the final manuscript or not. Interesting though your project is, I do not think it is suitable for the Sage monographs which Walter Laqueur and I are editing. We are looking for greater synthesis than such a local study would provide, though a local study could be fraught with more general implications. Moreover, the series does not include books on method. From what I could gather from your outline, therefore, your work would not be suitable for us. We also have too many manuscripts on Germany at the moment, and our priorities are historical manuscripts dealing with other nations. I therefore do not think that our Sage series is suitable for the publication of this work. However, I wish you and your collaborator much luck. Local studies are very badly needed, and I hope you will persevere with your project.

Unfortunately, it is not clear which parts Luchterhand sent to Mosse, either from Luchterhand’s papers or from the George Mosse papers at the Leo Baeck Institute in New York. However, it seems reasonable to assume that there is an obvious misunderstanding in Mosse’s reaction. Luchterhand had not stated that he intended to write a “local study,” but rather a study of the nucleus of National Socialism in a small German town from a “multiperspective.” Research on the system of subcamps was practically non-existent before the 1980s (see, e.g., Orth 2007), and there were definitely no projects interviewing those who “ran the camps.” Comments such as those contained in Mosse’s letter may have deliberately misunderstood the author’s intentions. Everett C. Hughes seemed to have experienced similar misunderstandings (see Fleck 2015a), so it is probably fair to assume that publishers, of whatever profession, were not particularly eager to learn about sociological studies conceptualized in an unorthodox way.

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28 Letter by Thomas J, Scheff to Elmer G. Luchterhand, 5 October 1978, Elmer G. Luchterhand Papers, Sub-Group IV, Series 11/12/13, Box 18.

29 Letter by George Mosse to Elmer G. Luchterhand, 18 September 1978, Elmer G. Luchterhand Papers, Sub-Group IV, Series 11/12/13, Box 18 (emphasis by EGL). Another publisher, Michael Meller of the Bertelsmann Publishing Group, advised Luchterhand to write his book in the way William S. Allen had written The Nazi Seizure of Power (Allen 1965) – thus suggesting that there was such a thing as an ideal solution for presenting research findings on Nazi genocide: “Maybe you are familiar with it, if not, I would like to ask you to have a look at it as this is the ideal way to present such material and is somewhat how I would eventually envisage your book to be written. The more I think about it, the more I am convinced that you will need an experienced and good American editor to shape the book and develop its full potential (which is very considerable).” (Letter by Michael Meller to Elmer Luchterhand, 30 September 1978, Elmer G. Luchterhand Papers, Sub-Group IV, Series 11/12/13, Box 18).

30 Research in the George Mosse papers was undertaken at the Leo Baeck Institute in New York in April 2017 with the permission of the donors of the Mosse papers. I want to thank the staff at the Leo Baeck Institute for their help.
It seems, however, that Luchterhand expected methodological criticism like Mosse’s. Various manuscripts titled “On Methods” point to the fact that he had been thinking about methodology for a while and had grappled with finding the “right” way to both research and describe genocide. Starting with lengthy standardized interview guides in the 1950s, he ended with interviews based in a phenomenological approach (Luchterhand 1979: 17). While always using highly developed techniques, he made flexible adjustments to them according to the interview situation—not least in his interviews with SS men from the early 1970s—and may thus not have been regarded as being “consistent” in his research design. After teaching research on genocide for several years, he concluded: “These classroom efforts seem to satisfy my students, but I personally regarded them as unsatisfactory and not useful for the development of a theory of genocide” and wrote about the “dilemma in the presentation of material itself – an editorial dilemma but one that is made more difficult by the inherently repulsive nature of all discussions of genocide.” While in general he was convinced that the “pandemic questions” which the Nazi genocide threw up could only be answered by “highly experienced journalists, as well as social scientists who are willing to venture beyond text book research methodology,” he was always sceptical regarding application of the “usual methods” and not only anticipated, but also provoked, criticism.

Methodologically this may seem questionable to those whose research stays strictly within the conventions of method as I suppose some of us have taught it in our academic lives. But our classroom methodological prescriptions do not fit easily the problems of studying the SS and their doings.

To summarise, one could formulate the following argument. Luchterhand permanently grappled with forms of researching genocide and presenting research findings, since to him, “[i]n terms of method, then, there is no royal road.” This methodological grappling, however, never satisfied either him or his supervisors and colleagues in different fields. Beginning his research on holocaust survivors using standardized (yet not very rigorously applied) questionnaires, he analysed them using quantitative methods (Luchterhand 1952, 1967 and 1980). For his Doggerwerk study, however, his interviews were far less standardized and designed for hermeneutical analyses (Luchterhand 1979; Luchterhand and Wieland 1978, 1981). It is interesting to note a certain anachronism in these methodological decisions. Using quantitative methods made sense given their promotion by the US Army and their growing domination as part of a general trend towards positivism (Steinmetz 2005a: 16–17; Steinmetz 2005b: 280–281). But it was an odd decision given that his supervisors—Howard P. Becker and Hans Gerth—were among the few sociologists to openly resist the positivist turn (Steinmetz 2005b: 308). Apart from this, the limits of quantification in Luchterhand’s research were obvious. There was no way of achieving a representative sample of concentration camp survivors in his research design (Kranebitter 2014: 83–84), and quantification frequently hid contradictory evidence, a point stressed quite harshly by Becker in an earlier review of the thesis:

By way of general comment, it may be noted that there are a few apparent discrepancies in the interviews, and that those that are referred to are promptly explained away. Empirical evidence does

32 Untitled manuscript, 28 February 1984, Elmer G. Luchterhand Papers, Sub-Group I, Series 2, Box 4.
33 Untitled manuscript, undated, Elmer G. Luchterhand Papers, Sub-Group I, Series 2, Box 4.
34 Manuscript titled “Method”, undated, Elmer G. Luchterhand Papers, Sub-Group I, Series 2, Box 4.
35 Here, Steinmetz lists only seven “sociological critics of methodological positivism” (Steinmetz 2005b, 308) for the American 1950s and 1960s.
not always come in such neat packages. [...] Summarizing, the reader regards the dissertation as falling distinctly short of standards of scientific detachment and accuracy. This judgement, be it noted, is not based on conviction that scientific accuracy and elaborate quantification are synonymous. In addition, the dissertation is poorly organized; it does not give an impression of coherence and unity, and the reader is of the impression that some parts coming late in the sequence were written earlier. He may be wrong in this inference, but the impression still remains. It is therefore recommended that the writer of the dissertation seriously consider the elimination of some of the more glaring defects before turning in a final draft.\(^{36}\)

Becker’s critique might well have led Luchterhand to give up quantification, as well as to eventually not realize his intention to publish his dissertation, even though he long planned to do so.\(^{37}\) It is important, however, not to understand methodological decisions and their perception by colleagues as individual shortcomings. Becker’s critique might have been motivated in part by his anti-positivist view, and Luchterhand’s own “hermeneutic turn” in the 1970s might well have been perceived as ‘unscientific’ by his quantitatively-trained, positivist fellow sociologists: “Indeed, nonpositivist positions began to seem unscientific, unprofessional, or nonsociological partly as result of such [quantitative – AK] training” (Steinmetz 2005b: 308)\(^{38}\). At the same time, paradoxically, giving up quantification in favour of life histories and hermeneutics not only meant contradicting mainstream positivism in sociology, but was also anachronistic given social history’s embrace of quantification after 1968 (Sewell 2005). The historian George Mosse’s rejection of Luchterhand’s *Doggerwerk* manuscript might well be connected to this part of the story, even though it must be said that George Mosse did not consider himself to be especially prone to quantitative methods.\(^{39}\) However, Luchterhand’s personal decisions regarding methodology were

\(^{36}\) Howard P. Becker: Memorandum to Hans Gerth, 22 October 1952 (University of Wisconsin-Madison, Division of Archives, College of Letters and Science, Department of Sociology, Howard Becker Files, Series No. 7/33/6-1, General Correspondence, Box 1, General Correspondence, Folder 2 (1937-1953, H-Q)). It should be noted that since this is the only letter by Becker regarding Luchterhand’s dissertation, it is not clear which version Becker was referring to and what Luchterhand changed in response to Becker’s critique. Unfortunately, nothing is known about Gerth’s relationship with Luchterhand. In his biography, written by his second wife (Gerth 2002), there is no mentioning of Luchterhand. Yet since Gerth did not normally supervise dissertations directly (Gerth 2002), there might have been a close relationship between Gerth and Luchterhand. This can also be assumed given that both stayed in contact, corresponding with each other and meeting in Germany, long after Gerth had left Madison for Frankfurt (there are three letters by Luchterhand to Gerth to find in the Deutsche Nationalbibliothek, Deutsches Exilarchiv 1933–1945, Frankfurt am Main, Nachlass Hans H. Gerth).

\(^{37}\) In a *Curriculum Vitae* from the early 1970s, Luchterhand wrote: “IN PREPARATION: A book on the longitudinal study of 52 survivors of the Nazi camps.” (CV, untitled, Erika Luchterhand collection).

\(^{38}\) Due to the limited scope of this article Luchterhand’s relationship to his colleagues at Brooklyn College is not touched upon here. One incident might be treated here as symptomatic: The department’s publication of a small brochure called “Sociology for Career training pamphlet” (The Office of the President, Brooklyn College Archives and Special Collections, Accession #91-032, Sub-Group XXI, Series 1, Box 331), written by Paul Montagna, proudly features the diversity of research done by faculty members, but fails to mention historical sociology or the history of sociology among many different “lecture topics” given. It may be concluded from this that for Luchterhand there was no discursive context, no possibility for discussion in his everyday activities besides the conferences he attended, and little possibility for a “condensation” of the research on Nazi concentration camps in his teaching activities.

\(^{39}\) In his autobiography, Mosse is critical of the quantification trend brought to history by social historians: “New kinds of history were pioneered, such as the use of statistics in historical research, and an attempt was made to build bridges between history and other social sciences. The old order was definitely dead. Yet I was once again the insider as outsider: I took part in departmental business and made close friends with many of the new arrivals, but once Bill Aydelotte had pioneered the statistical approach to history, I stood aside from the scholarly discussions. I had no training in mathematics and was not enthusiastic about the nature of this research, especially as its practitioners seemed to find it difficult to arrive at coherent, publishable, analyses.” (Mosse 2000: 135).
certainly connected to the pre- and post-1968 methodological debates in sociology as well as historiography, both of which tended to view his research and material as arbitrary and preliminary; in short ‘unscientific’, leaving Luchterhand ‘caught between two stools’.

To further complicate the story, one must look beyond science itself. In a research note dating from 1988, Luchterhand assumed a connection between the social scientists’ inability to do proper research on genocide and an unwillingness to listen on the part of a wider audience:

The paucity of empirically-based interpretation had important consequences. General readers of accounts by former prisoners soon felt that they had had enough – all they could bear – of what there was to know. They tended to fill the interpretative gaps with simplistic psychologizing and sociologizing. Social scientists and psychiatrists, who also had ‘had enough’ of these accounts, set out to fill the interpretative gaps with their higher-grade speculations. At the same time they defended [sic] themselves with the general assumption that there was little one could do with field-study approaches.40

CONCLUSION

Raising the question of why something did not happen can always be labelled as speculative. It is nearly impossible to find the reasons why sociologists did not deal extensively with aspects of National Socialism. In questioning why National Socialism and the Holocaust have not attracted more research in post-war sociology, many German speaking authors have stressed the contaminated past of the discipline itself in the early phase of its establishment (regarding Austria e.g. Reinprecht 2014), while American authors have pointed to the wide spread anti-Semitism among predominantly Christian sociologists (Halpert 2007, Berger 2012). Beyond biographical or institutional reasons, authors such as Zygmunt Bauman or Michaela Christ have recently identified epistemological problems in dealing with National Socialism within hegemonic sociological paradigms. Given the supposed linear development towards ‘modernity’ that modernization theory suggests, National Socialism can only be perceived as its accident; violence cannot be treated as ‘deviant’ behaviour in a society where it is ‘normal’ and state-sponsored, and the paradigm of rationality is unlikely to be able to explain directionless violence at all (Christ 2011).

Based on the case study dealing with Elmer Luchterhand’s research on concentration camps, I would like to add two points. Firstly, there is a political reason. As mentioned by Paul Foreman—one of the few sociologists who referred to Luchterhand—Bettelheim’s individualistic approach soon became quite influential, if not hegemonic, accompanied by an anti-sociological stance: “[...] vulnerable theorizing in early American discussions about the camps are conforming and tend to seal off major sociological interests” (Foreman 1959: 289).41 In this sense, Luchterhand’s research contradicted the zeitgeist, Stressing solidarity and group behaviour in a time that favoured individualistic perspectives like those of Bettelheim may simply not have been a fruitful “publication strategy.” Moreover, as the collectivist approaches to concentration camp survival had been “left” to the eastern world (Foreman 1959)—i.e. the Soviet bloc—such studies from the 1950s were generally under suspicion of being “communist”. Although more research is necessary, this

40 Elmer G. Luchterhand: Planning the Study, 18 December 1988, Elmer G. Luchterhand Papers, Sub-Group I, Series 2, Box 4.
41 For a general discussion of anti-sociological stances in theorizing about concentration camps, see Suderland 2014.
may well have played a role in the reception of Luchterhand’s work. Because of his history as a labour organizer in the 1930s, the beginning of the Cold War and McCarthyism forced Luchterhand and his family to move to Canada from 1953 to 1958.42

Besides personal political affiliations and attitudes, the fate of Luchterhand’s research resembles that of the few other sociologists researching concentration camps, chief among them Albert Rosenberg, whose Buchenwald Report was soon to quite literally vanish in the Army archives (Kranebitter 2016). Soon after 1945, the Army’s original focus of using concentration camp research for denazification and re-education purposes among the German and Austrian public was dropped. Thus, the institutional motive behind both Rosenberg’s and Luchterhand’s research soon fell away.

Secondly, in what is the principal conclusion of this paper, there is a methodological reason. Methodological problems, such as how to survey and analyse Nazi concentration camps in a technologically sophisticated, yet morally sensitive, way, have not been proposed so far.43 As can be concluded from the dozens of papers and manuscripts entitled “On Methods” in his estate, Luchterhand highlighted the methodological problems of researching the Nazi genocide in various papers and much of his correspondence. These questions relate to the collecting of data, its presentation, and even sociological terminology. In several interviews he had disputes over terms like ‘role conflict’ and ‘responsibility,’ as well as over the importance of certain social situations (quarrelling, for example, with Neurath about the meaning of situations such as forced and prolonged prisoners’ roll calls).

There has always been a discussion about the limits of ‘normal’ sociological methodology when confronted with critical and exceptional events and ‘research objects’ like the concentration camps. Aside of her major works, shortly after World War II Hannah Arendt wrote a paper called ‘Social Science Techniques and the Study of Concentration Camps’ (Arendt 1950, see also Arendt 1948), in which she reflected on the impossibility of applying ‘normal’ research methods to the ‘senselessness’ of the concentration camps. Downright furious with social scientists’ inability to understand phenomena like National Socialism or Stalinism, from the 1940s onwards Arendt, as Peter Baehr puts it,

> loathed the social sciences in general and sociology in particular [...]. The earlier spirit of engagement with sociology is replaced by tempestuous root-and-branch dismissal of it (Baehr 2010: 3).44

42 Interview with Erika Luchterhand, New York, April 18, 2017.
43 Interestingly, the methodological question of how to produce knowledge of any genocidal event within the social sciences, i.e. beyond survivors’ memoirs and legal reconditioning and their implicit limitations for a broader elicitation, seems to be one that has not been elaborated on in the social sciences. Certainly, the “National Socialism and Sociology” debate, which for the most part adheres unconsciously to the thesis of the singularity of National Socialism and the Holocaust, does not focus at all on this side of the problem. Since genocide on a macro level and camps as social systems have not disappeared since, and since the question of how to deal methodologically with sites of mass violence is not a banal question at all, much could be learned from those grappling with these problems in the first instance.
44 From the many examples that could be cited here I only want to point to the Spanish Buchenwald survivor and novelist Jorge Semprún, whose book “Literature or Life” is a general plea for a literary description of events in contrast to a scientific (especially sociological) one – another recurring theme in the history of sociology (see Lepenies 2006). Franz Kafka’s descriptions of bureaucracy, for example, to Semprún seemed timeless, because they were written “in his own register, one of literature, not of sociological analysis” (Semprún 1994: 154; my translation – see also Kranebitter 2016).
Contradicting this view, some sociologists have proposed methodological positions that explicitly refuse the “temptation of methodological exceptionalism” (Dobry 2009), calling instead for the application of ‘normal’ methodological and conceptual instruments (ibid: 74).

Luchterhand’s work is an example of a dedicated search for a research strategy beyond the false dichotomy of an ‘exceptional’ vs. ‘normal’ approach in sociological analysis. His empirical material can be regarded as a unique collection of interview material, which differs from the first published memoirs on the concentration camps by including behaviours like theft, sexual feelings, sharing patterns, and changes in religious belief. Like Rosenberg and his team, he utilized regular methodologies, adjusting them flexibly in his research design as the situation demanded but applying them nonetheless, albeit carefully in respect of quantitative methods. Ultimately, however, Luchterhand’s methodological ‘grappling’ with how to survey mass violence was not successful, at least in terms of publication output; it convinced neither sociologists nor historians. ‘Departing’ from quantitative social research in the late 1940s, and eventually working with hermeneutical interview techniques in the mid-1970s, Luchterhand was paradoxically ‘sandwiched’ between sociology’s hegemonic positivism, which did not view this ‘departure’ favourably (Steinmetz 2005a and 2005b), and social history’s reverse embracing of quantification (Sewell 2005: 176-179). In short, it could be argued that ‘grappling alone’ with finding the right methodology for researching concentration camps, besides generating fascinating findings worth reading in their own right in light of current research on those camps, did not satisfy Luchterhand’s own standards of publication. Neither was it rewarded by his fellow sociologists, who applauded but silenced him, or by historians, who distanced, and perhaps envied, him. Moreover, in addition to the fact that within academia there were too few who, in a political climate unfavourable to ambivalent meaning, tried to carry out concentration camp research, there was another external factor: soon after 1945, there were too few who were prepared to listen.

ACKNOWLEDGEMENTS

I want to thank Christian Fleck, Christoph Reinprecht and the two anonymous reviewers of this paper for various remarks. For their general help and for their help in tracing personal and archival documents I want to thank Erika Luchterhand, Marianne Labatto and her whole team (Brooklyn College Library), Lee Grady (Wisconsin Historical Society), David G. Null (University of Wisconsin, Madison, Division of Archives), Katja Seybold (Bergen-Belsen Memorial), Henning Borggräfe and Bianka Geißler (International Tracing Service, Bad Arolsen), as well as Sylvia Asmus and Regina Elzner (Deutsches Exilarchiv 1933-1945, Frankfurt am Main).
References


FORUM: BOOK REVIEW SYMPOSIUM

Adcock: Liberalism and Political Science

Comments by Emily Hauptmann, Erkki Berndtson and Thibaud Boncourt
Response by Robert Adcock

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312 pp.
Price: £ 39,99

Introduction to the Symposium by Thibaud Boncourt

The following contributions stem from a roundtable held in Poznan, Poland in July 2016 at the International Political Science Association’s Congress. The roundtable was put together by IPSA’s Research Committee 33, which focuses on the history of political science as a discipline. Contributors produced a stimulating debate on Robert Adcock’s award winning Liberalism and the Emergence of Political Science: A Transatlantic Tale.

As its title suggests, this important book weaves two narratives together. First, it sheds a new light on the history of liberalism, by highlighting the way in which liberal political thought changed between the early 19th century and the early 20th century. Adcock highlights the transition from “democratised classical liberalism” to alternative conceptions of the liberal tradition such as “progressive liberalism” and “disenchanted classical liberalism”. Second, the book documents the history of the emergence of American political science. By studying the pioneers of this discipline, Adcock analyses the progressive spread and institutionalisation of political science in America until the landmark creation of the American Political Science Association (APSA, 1903).

These two stories are interesting in themselves but what makes Adcock’s argument more so is that he weaves these two trends together. He shows convincingly how the development of one of the biggest political discourses and the institutionalisation of political science fuelled one another. By linking the history of the discipline to that of political power, Adcock’s impressive study resonates with other great work on the subject such as those of Sonja Amadae (2003) and Nicolas Guilhot (2005).

Another of the book’s strengths lies in the choice of adopting a transatlantic perspective. In line with recent literature, Adcock goes beyond narratives of intellectual history as shaped by “national
traditions” to emphasise the transnational exchanges that shape the history of political thought. Thus, Adcock first traces the history of liberalism in Europe, before analysing how it was imported in American academia. The book tells the story of the “Americanisation” of liberalism, understood as the way in which European liberal beliefs were adapted by American scholars to address American political and economic realities.

In order to discuss several aspects of this stimulating, multi-layered book, the roundtable gathered specialists in the history of political thought, history of political science as a discipline, and the internationalisation of the social sciences. The following three contributions raise some of the topics that were discussed at the roundtable, as well as new ones. Robert Adcock’s rejoinder discusses these topics by expanding on the book’s argument. (All references in the text refer to Adcock 2014a, if not stated otherwise.)

Comments by Emily Hauptmann

Each time I have revisited Adcock’s *Liberalism and the Emergence of Political Science*, I have been drawn to different themes and questions. This is a multi-layered book; re-readings reveal different aspects of it rather than reinforcing one central notion of what the book is about. I say this to explain why I’m offering a substantially different set of remarks in this forum than I did at our roundtable last summer. At that meeting, I had raised questions about Adcock’s historiographical reasons for attributing various liberalisms to his central cast of not-yet-self-identified-liberal 19th c. characters and for declining to make “American exceptionalism” a prominent feature in his account of their early political science. Because I think Adcock answered my questions fully last summer, I felt it might be better to ask a few new ones. So I returned to his book once more. When I did so, I realized that I had not fully engaged with the book as a narrative about political science nor with Adcock’s account of the part political scientists played in the intellectual and political life of the U.S.

In that spirit, I have recast my comments to prompt Adcock to expand on the implications of his argument that 19th c. U.S. political scientists were crucial agents in the development of liberalism. If I understand Adcock’s argument correctly, he claims that although Lieber, Sumner, Lowell and others did not call themselves liberals, they should nonetheless be seen as the first “agents” in the “Americanization” of liberalism (2, 281). As Adcock shows, when European liberals articulated what liberalism meant to them, they did so in part by offering assessments of American democracy. This wasn’t an exclusively European conversation, however; American political scientists also took part in it, developing their own ideas about the relation between liberalism and democracy. So while liberalism didn’t come into full political currency in the U.S. until the early 20th c., Adcock urges us to read 19th c. political scientists’ transatlantic exchanges with European liberals as a preface to its later emergence.

This account casts 19th c. political scientists in a role crucial to the development of a 20th c. liberal American political culture. Not only does Adcock portray political scientists as the first importers and adapters of European liberalism in the U.S.; he also suggests that the varieties of liberalism they cultivated and hybridized in the academy “prefigure[d]” (275) the forms it would later take in American political discourse. The importation and adaptation part of this story is told in meticulous detail throughout the book. Readers learn a great deal not only about the views of
Lieber, Sumner, Lowell and Wilson but also about the European liberalisms with which they engaged.

In my view, however, the second part of the story—how the liberalisms cultivated by U.S. political scientists prepared the ground for the development of the liberal political culture of the early 20th c.—isn’t told nearly as fully as the first. This is, I acknowledge, partly because the book’s principal historical narrative ends with the founding of the American Political Science Association in 1903. Still, to stress, as Adcock does, that 19th c. political scientists should be seen as the “lead agents of the Americanization of liberalism,” implies that these academics set a discourse in motion that was later taken up by a wider array of people in public life. At least that’s what highlighting the agency of these 19th c. political scientists suggests to me.

Perhaps, however, this overstates the role Adcock believes 19th c. political scientists played in setting the terms of 20th c. U.S. political discourse. In other passages in his conclusion, Adcock portrays the relation between political scientists’ incipient liberalisms and what came into wider political currency in the first decades of the 20th c. much more tentatively, saying that their adaptations of European liberalism merely “prefigure[d]” or lent themselves to being “mapped onto” (275, 276) what liberalism came to mean in 20th c. U.S. politics. So were early political scientists “lead agents of the Americanization of liberalism”? That is, did they help format the shape of later U.S. political discourse? Or are the similarities Adcock notices between them too faint to register as influence?

I’m trying to do more here than split hairs. Even if answering these questions fully would pull the narrative further into the 20th c. than intended, I think these formulations invite some further questions about how Adcock understands the relation of early political scientists to public life. On the one hand, as Adcock notes, many 19th c. U.S. political scientists sought public office; only the prominence of the offices Woodrow Wilson held, therefore, was unusual, not his pursuit of them... Moreover, the political involvement of its members was affirmed and encouraged by the American Political Science Association at its founding. But on the other hand, the professionalization of the social sciences that began in the late 19th c. is often read as a deliberate retreat from the public engagement of earlier generations, an attempt to win legitimacy as experts on social and political life by speaking from a space above the partisan political fray. And this more guarded view of how early political scientists engaged in public life is one I believe Adcock also affirms (cf. 272–274). These two somewhat incompatible claims about political scientists’ political involvement lead me to extend one of my earlier questions about how Adcock understands the relation between 19th c. political scientists and 20th c. U.S. political liberalism: If political scientists were indeed the “lead agents” of the liberalism that so profoundly shaped American political life, in what venues and by what means did they exercise that agency?

I am aware that my questions focus on a claim advanced most fully in the book’s conclusion rather than one developed in its core. In my defence, I would cite the depth and completeness with which Adcock treats his principal themes as my license to ask such “what happened next?” questions. Those interested in the history of political science have much to learn from this book—and from what Adcock chooses to do next.
Comments by Erkki Berndtson

Liberalism and the Emergence of American Political Science is one of the most important studies on the history of social sciences in recent years. Its focus on the relationship between liberalism and the emergence of political science as a discipline in the 19th century United States is not itself new, as this relationship has been discussed in earlier studies (e.g. Crick 1959; Ceaser 1990). However, Adcock’s book differs from earlier studies in two ways. First, he frames his analysis using a transatlantic and comparative perspective, looking at how American scholars interacted with British, German and French scholars and how they learned from each other. Secondly, he analyses the relationship between liberalism and political science through varieties of liberalism, offering “a history structured in terms of, ..., multiple liberalisms” (3). The early 19th century study of politics was a product of democratized classical liberalism, but political scientists of the late 19th century were mostly progressive liberals or disenchanted classical liberals. In this sense, the book is a critique of Crick’s classic study of American political science, as Crick understood liberalism as a monolithic ideology. Adcock presents “pioneering political scientists as helping to import ‘the liberal tradition’ into American thought and Americanize it, whereas for Crick ‘the liberal tradition’ was a ‘pre-existent consensus’” (5-6).

The book also contains many other new insights for the study of the history of political science, which make reading the book rewarding. For instance, Adcock’s “central” argument that the study of politics and the transformation of liberalism were framed by changing political and economic realities, “the spread of democratic belief in popular sovereignty and the political capacity of the common man”, and secondly, “the growth of large-scale industry, which altered the structure and power of social classes while confronting governments with novel policy demands” (3), allows him to analyze the study of politics in a wider perspective. It is essential to notice how important historical research and political economy were to the study of politics, as the discipline was transformed from a wide political science of the early 19th century to a narrow political science of the late 19th century. As Adcock argues, paying attention especially to the role of political economy in the history of the discipline is a novel move (10).

Adcock’s book is a rich and detailed study and it contains many interesting things to discuss. In the following, however, I will pay attention only to two major questions which need more critical scrutiny: liberalism and the transatlantic perspective, and secondly, the origins of a narrow political science with reference to other disciplines.

Liberalism and the transatlantic perspective. If Crick understood American liberalism as a monolithic ideology, Adcock, on the other hand seems to downplay the importance of the liberal heritage in the United States. It must be remembered that political thinkers who laid the foundation for liberalism (e.g. Grotius, Locke, Montesquieu) were an important part of teaching already at colonial colleges (see Haddow 1939). And, of course, The Federalist Papers and the U.S. Constitution are a mixture of liberal political thinking and republican political theory (e.g. Lienesch 1988). In that sense, there was a liberal political tradition in the United States already before Adcock’s “transatlantic tale”.

I agree with Adcock that the interaction between American and European scholars was important for the development of political science as a discipline. However, the problem is that what was liberal and what was conservative political thinking and action during the 19th century it is a matter
of interpretation. Political ideologies and political systems, in the sense we know them today, were only coming into existence. From today’s perspective, the arguments in the first chapter of the book (The “Political” in Political Science. The Liberal Debate about Democracy) are easy to understand and accept, as the chapter discusses, the ideas of, e.g. Francois Guizot, Benjamin Constant, Alexis de Tocqueville and John Stuart Mill.

The second chapter (The “Science” in Political Science. The Historicist Debate about Method) is more problematic. In the chapter, Adcock discusses the methodological tradition he labels as developmental historicism. Later in the book, he shows how historicism was an important part of the American study of politics. However, when he attempts to weave together the “science” and the “political” in European thinking, he runs into difficulties. Using Johann Bluntschli, Henry Maine and Edward Freeman as examples, Adcock argues that, “[t]ogether these three Europeans exemplified both political variety within liberalism and methodological variety within developmental historicism, relative to which I will subsequently analyze American figures” (43).

However, of these three, only Freeman can be said to have been a liberal, as he was a historian and a liberal politician. The ideas of Henry Maine, on the other hand, have usually been interpreted as having a conservative rather than a liberal bent. In addition, Johan Bluntchli had represented a moderate conservative party in Switzerland in his youth, and although he had liberal nationalist sympathies later in Germany, he can also be described as a racist and anti-feminist (which, of course, applied to many other European and American scholars as well).

In that sense, conservatism and liberalism mixed with each other in many ways. Germany is a good example, not only because the majority of American scholars went there to study in the late 19th century. German political liberalism became divided in the 1860s into “progressive liberalism” and “national liberalism”, the latter group accepting many of Chancellor Bismarck’s economic policies during the 1870s. However, when Bismarck adopted protectionist policies after 1879, the relations between him and liberals waned. After that, the National Liberal Party’s left wing merged with the Progressive Party and the remaining party members approached the Conservatives. The question now is, how did American political scientists understand these varieties of German liberalism when studying in Germany and after returning home? Most German liberals favored free trade, as did American scholars, but it was Bismarck as a Chancellor who developed social legislation which many American progressive liberals also favored. The question is, what kind of liberals were the main characters in Adcock’s tale, such as, Francis Lieber, William Graham Sumner, John W. Burgess, A. Lawrence Lowell, and even Woodrow Wilson? Could the title of Adcock’s book have been Liberalism, Conservatism and the Emergence of American Political Science? The development of political science can also be analyzed as a struggle between liberalism and conservatism (with radicalism in the background).

From wide to narrow political science. Analyzing the emergence of political science as an academic discipline, Adcock pays attention to the study of history and political economy which were important parts of the study of politics (wide political science) before political science developed into a distinct narrow academic discipline. In many ways, as Adcock is well aware of, the development from wide political science to a narrow one was due to specialization. Before the Civil War colleges were small. One scholar specializing in politics hardly made a discipline. It was no wonder that the study of politics was grouped together with other subjects, in many cases, political economy, history and/or law. On the other hand, the scholars themselves were interested in various topics and did not identify themselves solely as historians, economists, legal scholars or
political scientists. The growth of universities led to specialization. It was not only political scientists who wanted to become specialists. As Anna Haddow has shown,

“Moral philosophy, the old vehicle for teaching politics, was changing into ethics in the modern sense and was losing most of its former great interest in political forms and obligations. The study of law was being separated from its early connections with the study of philosophy and politics, and becoming a technical and analytical study of American law, designed for the budding practitioner rather than the educated citizen. Political economy was coming forward rapidly as a distinct subject, later to be called “economics”, with a primary interest in the production, distribution, and consumption of economic goods. History was progressing more slowly, and its attention was being given mainly to political and constitutional developments. The teachers of philosophy, law and political economy were, therefore, deserting the field of politics to concentrate upon their special interests, while the teachers of history were beginning to encourage political and constitutional studies. Later, when the historians turned more to cultural, social, and economic history, the field of political studies was left more definitely to the specialized group of political scientists (Haddow 1939: 167).

In spite of this, law was still an important framework for political scientists when the American Political Science Association was founded in 1903. The legal emphasis in the study of politics was natural as many political scientists at that time had a legal education. It is odd that Adcock does not pay any attention to this. As Westel Woodbury Willoughby, the first Secretary of the American Political Science Association wrote in his 1904 Report of the Secretary, “[i]n order to cover effectively the whole field of Political Science, the Association will distribute its work among sections, devoted respectively to such topics as International Law and Diplomacy, Comparative Legislation, Historical and Comparative Jurisprudence, Constitutional Law, Administration, Politics, and Political Theory” (Willoughby 1904: 27).

In the same manner, Frank J. Goodnow, the first President of the American Political Science Association, stated in his 1904 Presidential address that until the formation of the Association there had existed no other association which had assembled “on a common ground those persons whose main interests were connected with the scientific study of the organization and functions of the state” and “one of the most important objects of the association is just this study of the public law. For it is only by a study of law, sometimes a most detailed study, that we can arrive at an accurate idea of the form and methods of a governmental system. Indeed, it is very doubtful whether one can be a political scientist in any sense without a knowledge of the law governing the systems subject to study” (Goodnow 1904: 42).

To understand the legal emphasis of early political scientists is important for two reasons. First, when a narrow political science was formed, the most pressing political problems were constitutional and administrative (which Adcock acknowledges through his analysis of the writings of Woodrow Wilson and Frank J. Goodnow). In that sense, the third changing political reality influencing the study of politics was the functioning of political institutions at the time when a “New American State” was built (Skowronek 1982).

The second reason for the need to understand the role of public law in the history of political science is that it gave a systematic framework to the study of political systems. This argument differs from Adcock’s interpretation that it was James Bryce, whose “The American
Commonwealth deserves to be hailed, or harangue, as the grounding work of this new kind of political science” (235). However, as Bernard Crick already argued, Bryce’s use of facts was non-theoretical and his influence on American political scientists was ambiguous. Bryce was skeptical about general ideas in the study of politics and his advice to ‘stay close to the facts’, probably meant something different to Americans than it meant to him, as Americans were becoming increasingly possessed by a general idea of politics (Crick 1959: 113-117). In that sense, it was public law and legal reasoning that gave political scientists theoretical means to focus on politics systematically.

In conclusion. There is still one open question which needs to be asked. If the emergence of American political science was due to the transatlantic interaction between American and European liberal scholars, why did the narrow political science not develop in Britain, Germany or France in the 19th century? Maybe Crick was right after all when he argued that political science was an American idea, rooted in American culture. It was born out of a peculiar relationship between a common sense of science, the idea of citizenship training, the habits of American democracy, and embracing all these, the belief in an inevitable progress or manifest destiny for American society (Crick 1959: xv). This may well at least be part of the explanation for the question, why a distinct discipline of political science began to develop in the United States during the 19th and early 20th centuries and not in Europe. The notion of American “exceptionalism” still has its merits in understanding intellectual developments in the United States.

Comments by Thibaud Boncourt

I will not go back to the many qualities of Robert Adcock’s Liberalism and the Emergence of Political Science, raised both during the roundtable and in the introduction to this symposium. Like all stimulating studies, Adcock’s book answers as many questions as it opens new avenues for research. In the following, I would like to prompt Adcock to reflect on some of these avenues and elaborate on some aspects of his argument.

(1) When reading the book, I first reflected on Adcock’s use of the term “liberalism”. When using such categories, authors tend to chose one of two options. Some start from their own understanding of liberalism and project it unto the past, so that they are the ones who decide what belongs to liberalism and what does not. Others reject this a priori approach to let actors themselves define what they are and, in particular, whether they situate themselves within a given intellectual tradition (in this case, liberalism) or not—and, correlatively, whether they are relevant to the story told.

Unconventionally, Adcock choses to do both. He first “introduces liberalism with reference to what the words ‘liberalism’ and ‘liberal’ meant as they first entered into political use in early- to mid-nineteenth-century Europe”. He then “[carries] the [European] language of ‘liberalism’ with [him]” as he moves across the Atlantic to tell the American part of his story, even though “‘liberal’ and ‘liberalism’ lacked resonance in nineteenth-century American politics” (7). These choices contrast with those he makes in the case of the label “political science”. When tracing the origins of the discipline in America, Adcock looks for the institutional uses of the term “political science”, i.e. in “the naming, first of academic chairs, then of schools and departments, and finally the APSA” (10-11).
This leads me to prompt Adcock to reflect on the consequences of these choices for his narrative or, in other words, on the alternative book he could have written had he fully embraced an approach focused on the uses by actors themselves of the labels he is interested in. This is tantamount to asking why the term “liberalism” took time to gain currency in American political debates and who were those who first fully embraced it, while some of their contemporaries did not.

(2) A second, related point is the way in which the book approaches intellectual history. Adcock pays tribute to the “conceptual approach” notably promoted by James Farr and John Gunnell, which approaches the history of political science through that of the concepts used by political scientists. While Adcock’s book contains rich conceptual discussions, it also veers away from this approach by studying the institutionalisation of the discipline into academic departments, chairs, and professional associations, and by tracing the sociological connections between some of the actors of his history. For example, in a fascinating Chapter 3, Adcock shows that intellectual exchanges between Francis Lieber, the earliest occupant of a chair of “political science” in America’s academy, and Alexis de Tocqueville, were fuelled by an enduring friendship forged during Tocqueville’s tour of America. Intellectual exchanges were, thus, supported by the tangible circulation of authors across the Atlantic and their interactions.

This sociological picture, however, is sometimes incomplete. While the book provides an impressive picture of the intellectual debates of the time (what Pierre Bourdieu called the “space of positions-takings”), the sociological connections between relevant actors (the “field of positions”) and the concrete channels through which ideas circulate are not always made explicit. When Chapter 2 describes, for example, the intellectual proximities between Benjamin Constant on the European side and James Kent on the American side, the discussion stays at the conceptual level. Can we identify key translations, interactions, or forums through which ideas circulated? Did some of these scholars form tangible transnational networks?

(3) Key to Adcock’s argument is the idea that early political scientists played an instrumental role in the emergence of an American blend of liberalism that profoundly shaped US political life in the twentieth century. However, while the “Americanisation” of liberalism in the late nineteenth and early twentieth century is described in detail, the impact and legacy of this process is less clear. In her comments, Emily Hauptmann asks how early political scientists became “lead agents” of liberalism. In addition to this, I wonder how the processes that Adcock describes relate to later interactions between American political science and democracy. Stimulating research, by Adcock and others, on the development of behavioralist political science between the 1940s and 1970s, has highlighted the close connections between this paradigm and the defence of the American model of pluralist liberal democracy. Did Lieber, Lowell and the other protagonists of Adcock’s history prefigure these developments? How do later dominant conceptions of liberalism differ from the blends identified in the book? In other words, how structuring are the processes described in the book for the later histories of American political science and liberalism, and their interactions?

Some of these comments go, of course, way beyond the main topic of Adcock’s study. I hope that they can still be part of a discussion that does justice to the quality, depth, and innovativeness of a book that is already a landmark for historians of political science and political thought.
Response by Robert Adcock
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It is a privilege to have this opportunity to respond to these three excellent commentaries on my Liberalism and the Emergence of American Political Science: A Transatlantic Tale (2014a). Let me begin by expressing my great gratitude to Thibaud Boncourt for organizing the International Political Science Association roundtable on the book and now this successor symposium, and to Emily Hauptmann and Erkki Berndtson for joining Boncourt in engaging with my work so very thoughtfully in person and now print. In beginning her commentary Hauptmann generously describes the book as “multi-layered” in the array of themes and questions that it touches on. Turning her description into a metaphor, I worry that this array is all too akin to a display of artifacts from an archaeological dig presented without information about what layer of the dig they came from. I will hence use this response in part as an opportunity to provide some background information on how my approach and themes shifted during the course of the almost a decade and a half of research and writing that culminated in the book. First, I discuss a dimension of the book that I remain especially happy with: my approach to political science. Second, I revisit my transatlantic perspective, where the commentaries spur me to articulate some broader conjectures implicit at best in the book itself. Finally, I address the methodological and substantive questions raised by the commentaries regarding my treatment of liberalism. Framing the book around such a fraught and variously interpreted ideological label has always made me anxious. I welcome the chance to reflect here on the choices underpinning my approach to liberalism, and to speculate upon issues raised by my all-too-preliminary concluding comments on the Americanization of liberalism.

**POLITICAL SCIENCE WITH ADJECTIVES: “WIDE,” “NARROW,” AND “NEW” POLITICAL SCIENCE**

Any historian of political science who looks back before the institutional watershed of the 1903 founding of the American Political Science Association (APSA) faces challenges regarding: 1) which figures fall within their remit, and, 2) what aggregate category to use when formulating more general claims. I started my research under the influence of three approaches. Revisiting political science’s pre-APSA pioneers has been part of the American discipline’s discourse about itself since Charles Merriam first told a disciplinary history almost a century ago. Figures he attended to, such as Francis Lieber and John Burgess, subsequently retained their prominence through generations of disciplinary histories down into the works of John Gunnell and James Farr, who inspired me as the latest most methodologically sophisticated practitioners of the genre I aimed to contribute to. I was also, however, informed by intellectual historian Dorothy Ross’s use of “historico-politics” as an aggregate grouping that situated Lieber and Burgess in a broader nineteenth-century field of knowledge only divided between political science and history in the era of APSA’s formation (Adcock 2003). Ross called my attention to figures like Andrew D. White (the first president of the American Historical Association, AHA) not usually accorded a major role in political science’s disciplinary histories. Finally, I was further inspired by Bernard Crick’s examination of sociologists William Graham Sumner and Lester Frank Ward in his study of what he called “the American...
science of politics”. Blending these influences, my dissertation identified its overall remit as the “American science of politics” (seen to potentially include any American writing on politics with a scientific self-identity). Within that all-too-amorphous remit I more specifically studied and compared: 1) Crick’s sociologists with 2) traditional disciplinary history figures grouped alongside other figures of Ross’s historico-politics.

I formulated the institutional approach to political science Boncourt highlights only as I rethought my project post-dissertation. The immediate spur came from finding that Sumner had been hired in the early 1870s as Yale’s “Chair of Political and Social Science,” and his introductory lecture had differentiated between political science in “its narrower and its wider significance” (Adcock 2014a: 113-14). Its wider sense, whose scope was similar to what would come to be called the “social sciences,” was also formally institutionalized in the naming of the “School of Political Science” opened at Columbia in 1880. Focusing on these formal institutional uses, I dropped the “American science of politics” (as well as Ross’s “historico-politics”) as aggregate categories. I reframed my work instead in terms of an older “wide political science” and a narrower offspring of it institutionalized with APSA’s founding. Attending to institutions and their naming enabled me to identify my remit more precisely as American academics employed in a chair, department, or school with “political science” as part of its title (even if the later differentiation of the social sciences has led some figures to be seen retrospectively as historians, economists, or sociologists, rather than political scientists). The institutional approach I settled on led to a major exclusion—I dropped Ward for my book—as well as a pivotal inclusion—I added Richard T. Ely and via him the attention to political economy that Berndtson generously highlights as a novel move of mine.

I was and remain satisfied with my institutional approach. Attaching the adjective “wide” before political science provides a category under which to group pre-APSA figures that is compatible with Gunnell and Farr’s methodological call to attend to the historical use of language, especially when that usage differs from our own. But as sensitive to past use as I hoped to be, it should also be stressed that in choosing my category and a boundary criterion for it I consciously constructed a particular perspective on the past. That perspective pointed my research in some directions (for example, toward political economy), but also—as all perspectives must—cast other facets of the past into shadow. Berndtson’s observation that I overlook the role of public law in the emergence of American political science compellingly shines a light on one such area of shadow. Law schools began to be founded in the American academy (for example, Harvard’s Law School in 1817 and Yale’s in 1824) several decades before chairs, department, or schools with “political science” in their titles. In focusing on the latter, I looked away from academic training in law. While a reader of my book would, I hope, be unsurprised that the young APSA repeatedly gave its presidency to figures who served at other times as AHA presidents, nothing in the book would prepare readers for how many early APSA presidents had an advanced degree from a law school and what this says about the character of political science at that time.

I have no regrets about having consciously written a history of American political science from a perspective, but Berndtson’s comments make me wish I had been more explicit about decisions built into that perspective. Alongside my institutional approach, my approach to political science also incorporated a further decision that I left implicit. During my research I came to believe that the disciplinary history genre has focused too much on the Columbia School of Political Science and its founder John Burgess at the expense of other institutions and individuals. I suspect this is due to the genre carrying forward focuses first articulated by Charles Merriam, who saw his field’s emergence through a lens shaped by his PhD training at Columbia. As a revisionary corrective I
made moves in my book to lower the relative profile of the Columbia School and its founder. Cornell’s White and Yale’s Sumner got central billing in Chapter 4, with Columbia’s slightly later School of Political Science not introduced until mid-chapter, and unmentioned in its title. Chapter 5, in turn, jumped ahead to 1880s developments at Hopkins and led by its faculty. My revisionary moves contributed to downplaying public law because ties between the law school and political science at Columbia were especially (I think exceptionally, Berndtson might disagree) close at Columbia. Public law was notably prominent in the first two books of the “Systematic Series edited by the University Faculty of Political Science”: Burgess’s *Political Science and Comparative Constitutional Law* (1891) and Frank Goodnow’s *Comparative Administrative Law* (1893). I discuss both books in Chapter 8, but in a way that advances my revisionary push. I argue there that Burgess’s book was poorly received and notably less influential than Bryce’s *American Commonwealth*, and that Goodnow’s approach shifted between his 1893 book and his *Politics and Administration* (1900) in ways that specifically illuminate Bryce’s influence.

My revisionist moves regarding Columbia and Burgess thus shaped two features of the book that Berndtson questions: the relative absence of public law and relative prominence of Bryce. I hope our exchange helps readers to consider my Chapter 8 with a fuller awareness of what I aimed at. But I trust that the chapter’s documenting of changes in works by Harvard’s A. Lawrence Lowell and Goodnow as each began to employ Bryce’s “political system” approach provides solid evidence of Bryce’s influence in American political science (on my reading it was no accident that Bryce, like Goodnow and Lowell, served as an early APSA president, while Burgess played no role). I remain convinced there was much right in Lowell’s crediting of Bryce as “the master and guide of all students of modern political systems” (Adcock 2014a: 267). Judging the relative influence of late-19th century figures matters, in turn, for how we plot the subsequent trajectory of political science in America. Consider the subtitle of Somit and Tanenhaus’s (1967) *The Development of American Political Science: From Burgess to Behavioralism*. If we instead used the transatlantic figure of Bryce to set up the trajectory “From Bryce to Behavioralism” we would see significant continuities—not least the concept of the “political system”—that question the characterization of behavioralism as a revolution wielded by its proponents and later its critics (Adcock 2007).

While disagreeing with Berndtson over Bryce’s influence, his challenge has made me all too aware of conceptual ambiguity in my addition of adjectives to political science. When adding adjectives intended to flag a contrast with “wide political science,” I switch between “new” and “narrow” in discussing political science during the period of the APSA’s founding. This may obscure the fact that, while the association’s remit was narrower than “wide political science” had been, it carried forward strands of scholarship from its precursor—such as public law and institutional history—that were far from new. What I meant to flag with the phrase “new political science” in Chapter 8 was only the new strand of work on modern political systems I credit Bryce with inaugurating. I believe this differed significantly from older strands of public law and institutional history. But all found a home together in the APSA. So, to clarify my adjectives: “new” political science was only one strand of “narrow” political science as institutionalized in the APSA.

**A Transatlantic Tale: Fleshing out My Perspective with Conjectures**

I subtitled the book “A Transatlantic Tale” with the hope of flagging two transatlantic concerns in a single phrase. First, as all three commentaries discuss, I was concerned to situate pioneering American political scientists in transatlantic intellectual *exchanges* with British, French, and/or German figures. Second, I sought to stress the role played in the work of these political scientists by
transatlantic comparisons that located America relative to developments in European nations. My introduction explicitly tied the book to the rising trend of historians supplementing, or even supplanting, national with transnational narratives. But reading the commentaries makes me fear that in hitching myself to the transnational turn in history, I directed readers’ expectations toward exchanges at the expense of comparisons. In revisiting my transatlantic perspective here, I first take up Boncourt’s question about how I treat transatlantic ties, and then Berndtson’s question about APSA’s founding, which he crisply poses using a transatlantic comparison. Both questions have pushed me to think about issues I did not engage in the book, and in responding I try out some conjectures more sweeping than anything my own research alone could substantiate.

Beside conceptual and institutional approaches, Boncourt also sees a sociological approach in my work, specifically when I trace transatlantic connections between actors. He observes, however, that I do this to differing extent with different figures. Contrasting Chapter 3’s tracing of Lieber-Tocqueville ties to my earlier noting of Kent-Constant parallels without associated transatlantic links, Boncourt asks whether there would be such links. The contrast here arises from my chapter structure, in which Chapter 1 and Chapter 2 give broad brush framing portraits of “liberalism” and “historicism,” with specific figures invoked intermittently for illustrative purposes. Only in Chapter 3 with Lieber—the first figure that my institutional approach categorizes as a pioneering American political scientist—did I first pursue the research needed to supplement summaries of arguments with details about transatlantic ties. Pondering Boncourt’s question now has, however, pushed me to step back from my (over) embrace of the transatlantic turn. Rather than assuming significant transatlantic ties waiting to be found for Kent-Constant, I would instead assume that the extent of transatlantic links evolved historically in connection with technological changes and war/peace, and on this basis I would conjecture that transatlantic links were sparser in the era of Constant and Kent than they became through the course of the rest of the nineteenth century.

Let me motivate (without claiming to substantiate) this conjecture. Looking back now across the chapters (3 to 8) in which I situate pioneering American political scientists in transatlantic links, I am struck by how much more extensive links were in later chapters. For example, a network diagram of the ties with multiple American scholars cultivated by Bryce over his 1870, 1881, and 1883-84 trips to America, and his later service as British ambassador (1907-1913), would be much larger than a diagram of Tocqueville’s ties from his single 1831-32 visit. Without dwelling on this in the book itself, I choose for my cover a map of transatlantic steamship and telegraph routes. These technologies transformed cross-oceanic communication between Tocqueville’s era and Bryce’s (the timing of this communications revolution and its influence on British liberal thought about international relations is explored in Bell 2007 and 2016). Alongside technological change, I would also expect that war vs. peace influenced the extent of transatlantic exchanges. My narrative picked up at the end of the Napoleonic Wars, which also saw the end of the related War of 1812 between the US and Britain. Considering these with the succession of transatlantic tensions associated with the wars of revolutionary France, and the prior disruption of the War of Independence, I would expect that American academics of Kent’s generation (Kent graduated from Yale in 1781) tended to have fewer transatlantic links that the generation that came before or those that came after. My detailed narrative ended in the opening years of the 20th century, shortly before World War One, which would, of course, also massively reshape transatlantic intellectual links. So assuming impacts from wars and technology, I would conjecture that the decades my book’s narrative covered were generally characterized by a step-by-step growth in transatlantic intellectual ties, sandwiched between disruptions before and after.
Turning from exchanges to a transatlantic comparison, let me now take up Berndtson’s concluding question: “Why did the narrow political science not develop in Britain, Germany, or France”? He ponders if Crick (1959) was right to interpret this development as rooted in American culture. But I would look more to institutional than cultural factors. The development of narrow political science involved more than the idea of political science as just one among many social sciences (an idea that could have, and probably did, occur to figures in Europe too). It required academic conditions conducive for the successful institutionalization of that idea. I would conjecture that a key condition in which America was exceptional at the time of APSA’s creation was the rate of expansion and institutional innovation in its academy. As the twentieth century began America was several decades into a process of academic transformation that proliferated new institutions of multiple kinds at multiple levels. Entirely new universities—such as Cornell (1868), Johns Hopkins (1876), the University of Chicago (1890), and Stanford (1900)—had been opening at the same time as former colleges—such as Harvard and Columbia—underwent major reform and expansion. Entrepreneurial scholars based in the new and/or reforming universities led the way in establishing national level associations that promoted the social sciences and their advancing differentiation. My book focuses specifically on the American Historical Association (1884) and American Economics Association (1885), and a generation later, the APSA (1903). These can be situated, moreover, as part of a broader dynamic that also included the founding of the American Psychological Association (1892), American Anthropological Association (1902), and American Sociological Society (1905). Recognizing the broader condition of rapid academic expansion and institutional innovation helps make sense of how the idea of a narrow political science embodied in APSA would be successfully institutionalized via the subsequent proliferation of freestanding departments of narrow political science such that these became the academic norm by the 1920s or so. The parallel story of sociology’s relative success in America further illustrates the import of broader academic conditions. While the idea of sociology was thoroughly European in origin, institutional academic space was created for it sooner, more rapidly, and more widely in America than Europe. If this conjecture about academic conditions being favorable to institutionalization of narrow political science is plausible for America, we may then extend it by asking if the post-WWII decades during which narrow political science later gained institutional traction in Britain, Germany, and France was an era of major academic expansion and reform. It was in Britain, but Boncourt and Berndtson would know better if this conjecture fits continental European cases or not.

As important as broad conditions of rapid vs. slow vs. no expansion in the academy might be for promoting or inhibiting the institutionalization of new ways of organizing knowledge production, certainly further factors must be considered in explaining any particular successful founding. For example, when considering the founding of national associations for narrow political science in France in 1949, Britain in 1950, and West Germany in 1951, we would also stress international factors. Did America’s post-WWII power and prestige encourage European scholars to pay more attention and respect to narrow political science as first institutionalized there? How did the geopolitical context of the late 1940s shape the effort of the new UNESCO to promote international political science, which spurred the creation of the International Political Science Association in 1949? When situated in a transatlantic comparison, the pioneering American creation of APSA is indeed exceptional, in no small part because it lacks the prominent role for international factors that appear important for the subsequent diffusion of narrow political science to other countries.

In combination with my conjecture about exceptional academic conditions in turn of the century America, a second domestic factor I would stress in explaining the development there of narrow
political science is also institutional. Specifically, I would spotlight the state building that Berndtson flags earlier in his commentary. In early-twentieth-century America government was expanding its role at the same time as civil service reform remade the paths to public employment at local, state, and federal levels. I was struck when researching APSA’s founding by the role played by the chairman of the Interstate Commerce Committee and other public servants (Adcock 2014b: 274). While later decades would see the creation of schools of public administration/policy that split the training of future civil servants and policy advice off from narrow political science in America, in its early decades public administration was the new field’s largest subfield, and the APSA provided a vibrant nexus between public service and the academy. Hauptmann notes, however, potential ambiguities in my conclusion’s paean to this lost age of relevance. To clarify I would stress more how civil service reform conceptually and institutionally remade the very character of “public service.” Ever since Lieber, pioneering American political scientists had avowed their commitment both to serve a public purpose and be non-partisan. But when a scholar like Sumner pursued a public role by writing in a vigorous style for non-academic audiences he was charged (notably by other academics) with being “partisan.” As civil service reform went from an ideal promoted by political scientists to an institutional reality, political scientists gained new ways to serve a public purpose—such as training future professional civil servants, doing policy research for professionalized audiences, and being appointed themselves to government posts on account of their professional knowledge and skills. Rather than a retreat, I see here the rise of new modes of public service premised on the conceptual/institutional division between political/partisan actors and professional service. If some political scientists—most famously, Woodrow Wilson—successfully pursued a public role in the former mode, others who tried to do so failed, and it was the newer professionalized modes of non-partisan government service through which more could and did serve a public purpose during the heyday of political science’s relevance.

**Liberalism: Methodological and Substantive Issues**

The terrain of liberalism is a contested one I did not plan to enter when I began researching the history of American political science. I set out to study uses of cross-national comparison and to situate these in the inheritance of methodological traditions from Europe: a naturalistic tradition among sociologists, and a historicist tradition in Ross’s historico-politics. But as I researched the substance of the comparisons actually made by Americans what especially struck me was a late-nineteenth century cleavage cutting across the methodological traditions I had expected to focus on. The cleavage was between transatlantic comparisons situating America as: 1) an exceptional nation that ought to be wary of European examples, vs. 2) a laggard with lessons to learn from one or another European country. Interwoven with skeptical vs. optimistic attitudes toward the expansion of government’s roles in an industrializing society, this cleavage suggested that what political scientists saw when making comparisons was shaped more by political visions than by methodology. In reframing my dissertation to spotlight this cleavage, I built upon an advisor’s suggestion that the political visions I was finding were varieties of liberalism, and interpreted the cleavage as one between “disillusioned classical liberals” vs. “progressive liberals.” Reframing my research in terms of liberalism led into a series of challenges well illuminated by questions raised in the commentaries. I welcome their impetus to revisit methodological and substantive choices I made in relating pioneering American political scientists to the history of liberalism.

Boncourt spotlights the methodological difference between my approaches to “liberalism” and to “political science.” For the latter, as discussed earlier, I ended up relying on the way American scholars situated themselves, especially as formalized in the naming of chairs, departments, and
schools. By contrast, when interpreting these scholars as varieties of “liberals” I projected onto them a label they did not apply to themselves. Boncourt asks how my narrative would differ if I had fully embraced an actor-centric approach. I think that doing so would make it hard or even impossible to present a narrative pivoting on the evolving political visions of political scientists. Most American political scientists, from Lieber down to today, have believed in an ideal of non-partisan scholarship. We might see their not labeling themselves as “liberals” as of a piece with them not labeling themselves “Democrats,” “Whigs,” or “Republicans.” Boncourt’s query makes clear that I could have better introduced my methodological choices by explicitly noting that, in narrating the substance of political science as shaped by political visions, I pursued a project that interprets the scholarship of political scientists in a way that sits in some tension with the self-understanding many of them have regarding what they try to achieve in their scholarship.

If some element of projection is necessary to map political labels onto political scientists, the key methodological issue becomes not if, but what, and how to project. My choice to project “liberal” brought my study of the history of political science into the orbit of the huge heated literature on the history of liberalism. As Boncourt notes, a common move in this literature is to project one’s own understanding of liberalism onto the past. A recent, methodologically self-conscious, and substantively excellent example is Edmund Fawcett’s Liberalism: The Life of an Idea. Explicitly disavowing actual usage of the word “liberalism” as a poor guide, Fawcett (2014: 6, 10) projects his own clearly stipulated view of the ideas constituting liberalism. The narrative he proceeds to unfold parallels mine in key respects. It picks up at the end of the Napoleonic wars and presents Americans alongside figures from France, Britain, and Germany. But Fawcett connects his array of figures by interpreting them through the common lens of the ideas he has stipulated. I instead sought as much as possible to connect figures by documenting historical links between them. In focusing especially on transatlantic ties, I sought a bridge over which to project historical uses of “liberal” and “liberalism” from nineteenth-century Europe onto American scholars of the period. By projecting within a transatlantic historical context, I aimed (perhaps quixotically) to reconcile projection with the methodological preference I drew from Farr and Gunnell for taking seriously the language of the past. Reflecting back on my choices I wish I had also attended to the way that pioneering American political scientists, although they did not call themselves liberal, did use the word. For example, Lieber wrote a “Liberal” entry for the Encyclopedia Americana he edited, and Wilson published paeans to the British Liberal party leaders Gladstone, Cobden, and Bright. Documenting the substance (and positive valence) of “liberal” in such pieces might make clearer that, while projecting this label onto American political scientists, I sought also to be sensitive to language usage within the transatlantic historical context I situated them in, with this usage itself including the way American political scientists wrote about liberals in Europe.

Berndtson questions, however, if some of the Europeans I discuss are really liberals. He perceptively points here to a challenge I skated over in the book. While my efforts to project “liberal” within a transatlantic context focused on documenting ties to bridge the Atlantic, I was
sketchy at best in documenting how liberalism was evolving in various European nations in the decades covered. If Bluntschli should be labeled liberal relative to the shifting politics of Germanic Europe is a great question I would have to punt to those better versed in the relevant political history. But Berndtson’s accompanying question about Maine raises a challenge I did consider with more care. My book interprets Maine beside Spencer as British examples of a “disenchanted classical liberalism” that Sumner and Lowell exemplify for me in America. This transatlantic grouping is based on a web of ties I am confident in. But my label for the group is contentious. While I worked on my book several colleagues suggested labeling it conservative—advice that would have led to Liberalism, Conservatism and the Emergence of American Political Science, just as Berndtson envisions. But I limited my re-labeling to a tweak: the “disillusioned classical liberals” of my dissertation became “disenchanted classical liberals” in the book. Why persist in calling them liberals? First, because the liberal vs. conservative issue was forcefully addressed by Spencer, who saw his views as true liberalism while criticizing Britain’s Liberal Party for becoming a “New Toryism” as it came to favor new roles for the state (1882/1884). To call Spencer’s views conservative would be to take sides and assert by default that the “new liberalism” developing in late-century Britain (and the parallel progressive liberalism in America) had better claim to the liberal label. Second, because a similar debate over what constitutes true “liberalism” subsequently occurred in America in the 1920s and 1930s until FDR’s use combined with his political dominance to settle the American sense of “liberal” as meaning support for greater state roles. Third, because recent decades have seen, in contrast to this now longstanding American sense of liberalism, use of “neo-liberalism” to identify state-skeptical thinkers and politicians. I sought to narrate liberalism’s past in such a way that readers who connect my history to debates in the present could see why both strands of thought could justifiably be identified as liberal. To sum up, I believe that applying “liberal” to both sides of the late-nineteenth century cleavage I presented positions us today to see that this cleavage was never really transcended, with the subsequent history of liberalism in no small part a story of the ebb and flow of sub-traditions descending from each side of this critical cleavage.

Speculating about twentieth-century liberalisms bring us to Hauptmann’s questions about the extent to which American political scientists influenced subsequent developments in American liberalism. Her focal point here is the claim I make when opening and closing my book (2014a: 2, 280) that political scientists were “agents” in the “Americanization of liberalism.” My intent when initially formulating this claim was less ambitious that I now realize it reads. I chose to talk of “agency” to make a point about how the political scientists I studied related to the influences on them. I did not want to replace Crick’s (1959) portrait of American political scientists as mouthpieces of an already liberal culture with a portrait of them as passively importing European liberal beliefs. My aim was to persuade readers that pioneering political scientists did “more than just embody and express liberal beliefs.” They were agents because they “adapted liberal arguments to address American challenges and audiences” and thereby were “active participants in the transatlantic transformation of the liberal tradition” (2014a: 2). Hauptmann’s questions make clear, however, that when I conclude my book with a sketch of scenes from the “Americanization of liberalism” that leads to a closing sentence re-stating my agency claim (now with the adjective “lead” added to make political scientists “lead agents”), I imply a claim, not only about how political scientists stood in relative to influence upon them, but also about their influence in turn.

So let me close by speculating on how I might flesh out the agency of political scientists in the sense Hauptmann brings to the fore. Agency in my limited sense of active adaptation of influences is something I would claim for all the scholars I studied, but agency in the sense of influence upon
American political life would be more varied. I could see claiming anywhere from no/minimal to major influence depending on the figure. I would also distinguish between influence on political visions and influence specifically on the use of “liberal” to talk about those visions. An exemplar from my book of the first kind of influence is Richard Ely. My account of Ely suggests he had an exceptional ability to spur new ways of thinking in others. I presented him as leading his patron Andrew Dickson White to leave behind an earlier embrace of laissez-faire, and transforming the way his Hopkins’ students Albert Shaw and Woodrow Wilson thought about economics. These were more than intra-academic influences. They speak also to the cross-party political impact of Ely’s progressive vision. White was a Republican, former state legislator, and leading American diplomat; Shaw another Republican who would edit for decades a mass-circulation progressive magazine; and Wilson a transformative Democratic president. A fuller study of Ely would also chart pathways of influence through PhD students that he taught after moving to the University of Wisconsin (such as John R. Commons), through his role in the social gospel movement, and the impact of his textbooks on undergraduates. Ely has a strong claim to be the most important intellectual influence on the formation of progressivism as an American political vision. This is far from a novel claim, and the novelty in my own treatment of Ely is to make this much studied figure part of the history of political science through my conception of wide political science.

The narrow political science intertwined with the founding of the APSA does not stand out with regard to the political visions I charted, but does so with regard to the entry of “liberal” into American politics. Progressivism could have developed as a political vision in America without becoming attached to the language of liberalism. Indeed, if elaborating my conclusion’s sketch of how this attachment developed in 1913-1916, I might speculate that it was highly contingent and that it was promoted by the political-public-academic connections mediated by the APSA in its early years. The critical juncture here is Woodrow Wilson’s defeat of Theodore Roosevelt in the election of 1912. If Roosevelt had won the presidency as the Progressive Party candidate the word “liberal” might never have entered American political life. It is specifically in the aftermath of this defeat that we find intellectuals favoring the active state championed by the Progressive Party shift from “progressive” to “liberal” to re-brand their political vision. The first such use I have found of “liberal” is in the 1913 speech of future APSA president William F. Willoughy printed in the American Political Science Review in early 1914. In my conclusion I move from this initial use to the subsequent better-known uses in the New Republic. But to avoid being too speculative, I avoided pointing out there that the New Republic’s lead editor Herbert Croly was an APSA member, and indeed served on its executive council during 1912-14. Moreover, Walter Weyl of the New Republic was also an APSA member in this period, and had given a paper at the 1912 APSA conference shortly after Wilson’s presidential victory. These associational ties hint at the tantalizing possibility that conversations at the APSA might have been a crucial venue in which the new American use of “liberal” was tried out and spread to the New Republic figures who then gave it a broader circulation. What we do know from existing research is that the New Republic’s use of “liberal” in 1916 was soon thereafter followed by Wilson making the word part of his own presidential rhetoric in his famous “Peace without Victory” speech to the US Senate in January 1917. Wilson (1971: 538-39) provocatively declared: “I hope and believe that I am in effect speaking for liberals and friends of humanity in every nation and of every programme of liberty. ... These are American Principles, American policies. We could stand for no others. And they are also the principles of and policies of forward looking men and women everywhere, of every modern nation, of every enlightened community. They are the principles of mankind and must prevail.” I would conjecture that Wilson’s charged equating of “liberals” with “American Principles” catapulted the word into a broad orbit and motivated competing visions in American political life to lay claim to it
in the 1920s and 1930s. Indeed, it is probably no accident that the contest to define the term is most famously captured in the dueling uses of Herbert Hoover and FDR, both of whom had served in Wilson’s administration.

If Wilson had not won election in 1912, and “liberal” had never entered American political life, the development and cleavage of political visions charted in my book would have been the same. But there would be much less of a motivation for me to project “liberal” onto those visions. Both my book, and many other histories of liberalism, would look profoundly different if not for the contingent entry of “liberal” into American politics. If political science did indeed mediate this crucial linguistic turn, then the history of political science matters for the history of liberalism in a fundamental way.
References


A recurrent narrative in scientific studies of the social and human sciences in the United States during the Cold War relies on the motif of an intellectual narrowing. The impending danger of nuclear war, it is argued, added further fuel to the attempts to move these branches into a sphere of scientific rigor comparable to those of the natural sciences. Thereby, a broad range of forms of knowledge production was deemed non-scientific and virtually excluded from the first lines of the research front. In some instances, this motif is even over-stretched to fit into a regression narrative that accuses the coeval scholars of being too preoccupied with following the bandwagon to realize the stupidity, flaws, and pitfalls of their own research. “Issues that could not be measured [by the Cold War behavioral scientist] were either ignored or trivialized in order to fit the paradigm,” claims historian Ron Robin (2001: 7). His colleague Bruce Kuklick (2006: 14) diagnoses a “lack of complication in the imagination of [Cold War defense intellectuals]” with regard to theorizing Soviet behavior. Finally, Hamilton Cravens’ claim that “social scientists [in this period] largely embraced a powerful nationalism along with an equally virile belief in scientific truth—in positivism, in other words” (Cravens 2012: 132), aims into the same direction.

While adding to the currently blossoming literature on Cold War social science, the book under review takes a different perspective than the one sketched above. It traces the history of open-mindedness, a notion central to Cold War political, scientific, and socio-cultural discourses. As a catchword, the open mind served as a crystallization point for the hopes of American liberals. Personality traits related to an open mind were autonomous thinking, creativity, and the use of reason in deciding.

In the context of the emerging bilateral opposition between the Soviet Union and the US, this notion clearly had a political dimension. The message was that societies, depending on the degree of freedom they offered their members, either damped or fostered the development of an open mind; that the Soviet Union were too illiberal to allow for open minds; and that the US should take measures to foster the development of open minds in order to prevail in the struggle of systems. By fostering the open-mindedness of the American people, liberal elites in the US hoped to achieve a society that was at once more liberal, more rational, and more prosperous.
The book follows the idea of the open mind through three inter-related discourses and explores these in consecutive parts. The first part (chapters 1 and 2) analyzes the concerns of educators seeking for means to increase the open-mindedness of students of various ages. This concern received support from a specific form of social scientific studies en vogue at that time: studies of national characters that used psychological (or psychoanalytical) techniques to explain cultural semantics. As an influential instance of this form of study, Cohen-Cole discusses *The Authoritarian Personality* (Adorno et al. 1950) and shows how the fears permeating this 1000-pages study match the liberal hopes attached to the open mind (pp. 40-54).

The book’s second part turns to the “role” of the open mind in academic discourses. Chapter 3 convincingly argues that the coeval rise of interdisciplinarity as a “valued mode of research” (p. 8) or even a virtue (p. 68) can be explained by reference to the ideal of the open mind: it is a mind that is not restricted by seemingly artificial boundaries and is able to transcend them in search for truth. Based on virtues like interdisciplinarity, members of the academia were certain that they could come up with remedies against social fragmentation and disintegration. They were also convinced that their community itself, the organized communication it engendered, could function as a model for society at large. “Properly conducted and nurtured,” Cohen-Cole concludes, “the community of leading intellectuals would heal rifts in the academy and would also provide the model for how modern society could be healed as well. They themselves and their community were what America needed” (p. 137).

The third part (chapters 5, 6, and 7) turns to the then-nascent field of cognitive science and investigates how this field propagated the virtues, both epistemic and social, of the open mind. After reconstructing the theoretical debates that led, according to Cohen-Cole, to an adoption of the virtues upheld by academic elite circles as basic human characteristics, the book describes the installation and early years of Harvard University’s Center for Cognitive Studies (CCS). Founded in 1960 by Jerome Bruner and George Miller, the Center was a very important institutional home for this young branch of psychological research. Initially designed as a place for interdisciplinarity, around 1965 the center turned into a place where multidisciplinarity—various lines of disciplinary research on the same topic that run in parallel and are mutually informing—dominated. The driver of this transformation was specialization. While in the beginning, interdisciplinarity had allowed for trying various concepts and techniques available across the scientific fields, once the adequate tools had been selected, the need for interdisciplinary openness vanished. The decline of interest in the weekly seminar of the Center, once a highly appreciated element of the Center’s culture and a place to discuss books from a wide spectrum of scholarly activity—among them Ernst Gombrich’s *Art and Illusion*—indicates this. As evinced by personal reminiscences, the Center’s younger members “now found the weekly seminars a time sink and superfluous to their own research activities” (p. 181). Yet, uninfluenced by this change of culture at the Center, Bruner had developed an elementary studies curriculum that embodied the virtues of the open mind. This curriculum, called “Man: A Course of Study” (MACOS), and its history are described in chapter 7.

In concluding, Cohen-Cole points out how the open mind later went on to become an important point of reference for social movements, among them the feminist movement. He argues that “what held together the political, the academic, and the scientific visions of open-mindedness was that these aspects were not defined through a set of abstract or logical descriptions but by reference to real people whose psychological profiles served as exemplars for the category”. In striking contrast to those processes of rationalization that so heavily shaped other branches of Cold War culture (cf. Erickson et al. 2013), the discourses on the open mind upheld the humanistic intellectual and
scholar as a model for a good life. It is this balancing effect of Cohen-Coles book that has left a long-lasting impression on me and that makes it an important contribution to the history of the social sciences during the Cold War.

References


BOOK REVIEW

Hess: The Political Theory of Shklar

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235 pp.
Price: $110,00 (Hardcover)

Judith Nisse Shklar was born as Judith Nisse in Riga in 1928, she died in Cambridge Massachusetts at the age of 63. During her early years she was marked by the anti-Semitism which prevailed in Latvia, and affected both her and her family. Shortly before the Russian occupation of Latvia, to be closely followed by the Nazi invasion, she was exiled with her family for the first time, when they moved to Sweden. But an invasion of Sweden seemed imminent and, after a long drawn-out flight via Russia and America, Shklar and her family arrived in Montreal.

Throughout this intellectual biography Andreas Hess emphasizes the link between Shklar’s private life and her intellectual production, arguing, for example, that this double exile, the discrimination which affected Shklar during her early years and the ambiguous attitude towards her and her family encountered on arrival in Canada brought her to consider herself as an “outsider”. This outsider-status, he suggests, was considered by Shklar as an advantage; forwarding her intellectual development and founding an attitude of self-reliance as she went on to become an independent and free thinker. It was to bring a unique flavor to her intellectual contribution: “Her conscious turn to political theory and the way she saw political theory as a way of making sense of her own refugee experience is the most important hint we have that from early onwards Shklar began to develop a habit of thought which distinguished her way of thinking from that of others” (p. 36).

Not only did exile affect Shklar’s intellectual development, but her status—as a Jew exiled from Europe—also limited the institutional opportunities open to her throughout her academic career. In 1950, with her newly wed husband, Gerry Shklar, Judith moved to Cambridge, Massachusetts to study in the graduate school in Harvard. She was later engaged in that same university to teach political sciences. As a teacher she is remembered as extremely conscientious working out during her teaching programs the ideas that were later to be published in books and articles. But career progress was slow, and the staid anti-intellectualism of Harvard, particularly during the McCarthy era, did not leave much place for those whose approach was original or different. The difficulties encountered were compounded by the fact that she was a woman, though Shklar never became a feminist and did not make an ideological issue of the setbacks she encountered (p. 64). Indeed, Hess affirms that, though hard to accept on a personal level, they were in some sense stimulating intellectually.
In her extensive intellectual production, Shklar discussed the political ideas of philosophers such as Rousseau, Hegel, Montagne, Montesquieu or Tocqueville among others, bringing their theories to bear in a reflection on American politics. She developed a theoretical analysis of the modern state and the type of citizenship necessary to maintain it. Hess describes this progression in detail. As from her earliest writings on Rousseau, for example, Shklar combines the development of her own “intellectual agenda” (p. 85) with a discussion of the contribution brought to political thought by the author under study. Over time, her own voice came to the fore as she developed her own personal style and reinforced her original contribution to political theory. This remained based on a thorough investigation of the works of the author she was discussing and of the historical context in which the ideas emerged. Her reflection on citizenship involves discussion on the relationship between democracy, liberty, justice and individual rights. In particular, she developed an analysis of victimhood, particularly relevant in view of her own experience.

Despite the belated official recognition extended to Shklar, in the form of her nomination as John Cowles Professor of Government in 1980, her election as President of the American Society for Political and Legal Philosophy in 1982, and, in 1983, as Vice-President of the American Political Science Association, she did not achieve a success equivalent to that of political scientists such as Hannah Arendt, Isaiah Berlin or John Rawls. Indeed, Hess suggests that her ideas were more often cited by others in support of their own arguments than subjected to analysis in their own right. In particular, she seems to have remained in the shadow of Arendt. Hess points out that on several occasions Shklar engaged in a discussion with Arendt, feeling, though she admired her, that something was missing in her work. Though the two women met on several occasions, this discussion, at least as it appears in print, does not seem to have been reciprocal. Shklar, who emphasizes certain liberal democratic trends in American politics, vehemently criticizes Arendt, the staunch republican, affirming that she remained turned towards Europe and never firmly got to grips with America and American ideals.

In the last pages of his book, Hess resumes some of the post-humus reflections and comments on Shklar’s work, many of which put the accent on the originality of Shklar’s position. He argues that as an ““exile from exile”” she acquired a unique vantage point (p. 201). It is this point that Hess has described, drawing our attention to an intellectual who has until now remained relatively far from the public eye.
BOOK REVIEW

Larsson/Magdalenić: Sociology in Sweden

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This is a book in the Palgrave Macmillan series of short national histories of sociology, by two members of the academic millennial generation whose sociological formation was largely centered on studying the discipline. After a very brief sketch of its pre-history – including a mention of Maxim Kovalevsky’s (the key founder of Russian sociology) sixteen lectures in Stockholm in 1888 and the dead-end professorship (1903) in economics and sociology of Strindberg’s former assistant Gustaf Steffen – the authors follow institutional Swedish sociology from its foundation in 1947 into the twenty-first century. They have adopted three perspectives as their guiding principles. 1. Scientific boundaries and their making; 2. The issue of gender; 3. The interconnection between the Swedish welfare state and the social sciences and scientists.

The founding history is the best part, outlining the domestic and international political context of the establishment of a peculiar current of American sociology, obsolete and dead in the US soon after it was enshrined in Sweden, where it remained central textbook literature until the mid-1960s. That was the positivism of George Lundberg. Possible directions therewith discarded, various European traditions, Durkheim, Pareto, Weber, excavated by Talcott Parsons in the 1930s, and ethnological research, are hinted at, although the nomination procedures excluding their protagonists are not brought to light. The founding history rightly puts Torgny Segerstedt Jr, the incumbent of the first sustainable chair of sociology, as the overtowering, dominating figure and gives a spare outline of his basic theoretical position, coming out of philosophy itself more sophisticated than Lundberg’s.

After outlining the founding moment, well covered in previous Swedish literature, the authors deploy their particular approach, focusing on “how the ideas and rhetoric of sociology were implemented as institutional and organizational practices”. The authors’ explicit choice and their faithful pursuit of it put into relief the two poles of writing a history of an academic discipline.

At one pole, you write a history of science, or an intellectual history, focusing on the significant works produced, their production, their main findings/arguments, their reception, and the discussions and new departures they give rise to. At the other you write a history of a profession,
concentrating on its organization, its boundary-drawing, its membership, and their self-identity. In principle both approaches could be used by the same author(s) in the same book, but the small Palgrave format promotes a clear preference for one or the other.

Larsson and Magdalenic give us a vivid story of organizational practices in Swedish sociology, in particular glimpses of the reorientation around 1968, e.g., at the department in Stockholm, where the syllabus suddenly changed from the local professor’s nine books to include Marx, Lenin, Mao, and Lin Biao’s Long Live the Victory in the People’s War, as well as an extended narrative of women’s slow but decisive march upwards through the departments and the association of sociology. They further include a story of the academic women’s band Busy Woman, who performed at the conferences of the Swedish Sociological Association, and among whom six members later became university professors.

Scientific output is treated by the authors as events, such as the two largest research projects and the public debate around them, the longitudinal “Project Metropolitan” on the Stockholm cohort of 1953, the Level of Living reports of the 1970s with press headlines of the time, and the methodological debate in mid-1960s on “soft” (qualitative) and “hard” (quantitative) data.

In several ways this is a good book for its genre and format. However, it comes with an intellectual price. There is little attention to and no discussion of scientific or intellectual achievement. This is also underlined in the references, which do not list a single major work by major Swedish sociologists after Segerstedt. Many of the latter do appear in the references, but only with circumstantial discussion pieces. The most distinguished sociologist of the post-1968 generation, Peter Hedström, is not even mentioned anywhere.

The authors neglect many opportunities for raising intellectual questions with respect to their own guidelines. For instance, the evolving post-foundation boundaries and non-boundary interfaces with economics, political science, ethnology, epidemiology and social medicine, ethnology, social work, and philosophy of science each important to weighty groups of Swedish sociology practitioners. The welfare state connections are taken note of, but hardly during the neoliberal turn of the latest twenty-five years. Much attention is given to gender relations within sociology, little to sociological gender analyses. No curiosity in the authors is awakened by the impression many people have, that in spite of its early international profile through Alva Myrdal, and allowing for the Norwegian recruitment of a significant practitioner, Karin Widerberg, to a chair in Oslo, Swedish Feminist sociology, and Swedish Feminism in general, have been less influential internationally than, say, Danish and Norwegian Feminism.

This is a history of sociology as a profession, like a profession of social workers or of accountants, restricted to academia, which is the Swedish sociology profession’s self-identity, in contrast to a wider conception in other Nordic countries. Sociology as a profession has a legitimate right to a history, and this is not a bad one. But those of us who are interested in sociology mainly as an intellectual adventure, we have to look elsewhere.
BOOK REVIEW

Helmes-Hayes/Santoro (eds.): Everett Hughes

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243 pp.
ISBN: 978-0-85728-178-4
Price: $ 115 / £ 70 (Hardcover)

The series of Anthem Companions of Sociology claims to offer “authoritative and comprehensive assessments of major figures in the development of sociology from the last two centuries” (publisher’s website). This Anthem Companion to Everett Hughes (1897–1983) edited by a Canadian-Italian duo compiles appraising, interpretative, and critical contributions by a distinctly international community of knowledgeable Hughes scholars. Their views on Hughes’s (hereafter ECH) somewhat scattered works and legacy of teaching sociology also reflect the ECH reception that has taken place since the 1990s in European sociology.


ECH’s contribution to sociology is said to be underestimated, because "he did not produce either an elaborate, complex and highly abstract system of conceptual categories or a philosophically inclined critical theory“ (Helmes-Hayes and Santoro 2016: 17). Still his capacity of conceptualization and his fine, detached and sometimes ironic style is typical for him, and for his students, among them Erving Goffman and Howard S. Becker. The latter has contrary to the former frequently pointed to this lineage. On the other hand, however, ECH "made no effort to build a coterie of followers“ (Helmes-Hayes and Santoro 2016: 2). So readers who enjoy Goffman’s or Becker’s accurate reports of the sometimes weird facets of social life that contain a good amount of humor should definitely consider to read ECH’s Sociological Eye (Hughes 1984).

As opposed to the "high-strung indifference" of the merely apparently free true believers (cf. Hughes 1984: 350) ECH seemed to enjoy the gaze of “self contained indifference to the opinions of ‘others’ which one sometimes observes, perhaps more often in women and cats than in men and dogs; ... detachment, amused and even bemused” (Hughes 1984: 350). This quotidian practice of
distancing oneself as observer from the studied situation is not a bad start for the professional scientific study of the social world.

Fieldwork was one of the main concerns and threads of Hughesian sociology and this ethnographic perspective has provided a bridge between the two Chicago Schools (the founders W.I. Thomas and Robert Park of the First Chicago School and the Symbolic Interactionists H.S. Becker, Erving Goffman, Anselm Strauss and others), as Chapoulie argues in his chapter, a reprint of a 1996 article.

"Cultivate a way of thinking where everything is interesting“ (H.S. Becker, personal communication, Feb, 12, 1998 UCSB)—I bet that Howie Becker’s advice given in a graduate seminar at UCSB has been inspired by ECH. "Every detail of social life had some meaning if you were good and patient enough to look for it“ (Helmes-Hayes and Santoro 2016: 11). This approach requires modesty towards one’s own discoveries and will lead to reluctance towards authoring big tomes of grand theory. Instead, this approach encourages to write precise and concise conceptualizations of a great variety of observations, conceptualizations that always stay in touch with the macro concepts of sociology. Helmes-Hayes coins the term "Interpretive Institutional Ecology“ for this approach that makes use of anthropological functionalism, human ecology, and Simmel’s as well as Weber’s work (Helmes-Hayes 2016: 72).

An example for ECH’s reference to "macro-variables“ such as social change in even trivial observations of everyday life is his concise and accurate conclusion that burnout is a consequence of the free choice of career: "[ECH] mused that occupational burnout—then a hot topic—had been around as long as occupations were chosen rather than assigned. This phenomenon was particularly troubling when occupations required faith or belief in a larger purpose“ (Harper 2016: 134).

This puts in a nutshell what commentators of the woes and worries of “late modernity“ spread out with many more words but far less brilliancy.

ECH practiced free association (Helmes-Hayes 2016: 77) at its best, which is able to illustrate the Sociological Imagination (Mills 1959) to novices to Sociology. It’s no coincidence that the volume Lewis Coser edited in 1994 was titled Everett C. Hughes on Work, Race and the Sociological Imagination.

In “Teaching as Fieldwork” ECH gives many examples for this craft of free association, among them the observation of one of his students whose father was a salesman for men’s suits. These salesmen busy themselves in convincing the customer of the perfect fit by stroking neck, shoulders and back of the suit. This laying on of hands creates a somewhat ambiguous and uncomfortable situation for both customer and salesman. "This case helps one to understand why designers of dresses for women are, if they are, male homosexuals“ (Hughes 1984: 570). Inspired by ECH one is tempted to push the argument further and asks, what about male hair stylists, male nurses, or dancers, and flight attendants. But wait; am I reproducing a common stereotype? However, ECH explains concisely that "stereotyping ... is evidence of lack of communication and understanding“ (Hughes 1984: 575). A diligent fieldworker, such as ECH, may be immune to the fallacy of stereotyping because her/his curiosity will lead to communication and eventually to understanding.
Hughesian-style free association does not lead to a huddle of unconnected observations. “Rather, he has an extremely strong conceptual mind which operates with the materials of concrete reality, which functions by relating apparently disparate observations, presenting them in new perspectives, producing frameworks and concepts for organizing and integrating them” (Becker et al. 1968: xi).

The chapters in the Anthem companion not only offer insights and analyses of Hughesian sociology and its reception but also biographical information related to the work. These references to his life are, however, redundantly appearing in many chapters and could have been better integrated. Both Helmes-Hayes and Vienne give a detailed and nevertheless interesting account of ECH’s life—very deserving is that Helmes-Hayes also deals with the merits of Helen MacGill Hughes, thus recognizing her as a scholar and a congenial life partner.

We owe to Helen MacGill Hughes the explanation of the biographical fundament of ECH’s conviction that a sociologist ought to be a marginal man, because marginality is the precondition to the craft of accurate dispassionate observation of the social world:

"The sociological investigator cracks the secrecy, but buries the secrets, one by one, in a tomb of silence—as do all the professionals which deal with the problems of people. This means, of course, that the student of human groups must remain willingly and firmly a marginal man in relation to those he studies;“ (Hughes 1984: 436, emphasis in original).

She traces this attitude and ability to his upbringing as a PK—a preacher’s kid. “In the small Ohio town where Everett grew up, the PK was always something of an outsider. From childhood Everett was disposed to look on people analytically, dispassionately" (MacGill Hughes in Vienne 2016: 96). ECH’s curiosity about people and his interest for the even minuscule details of daily life are therefore deeply rooted in his youth and family history.

ECH is a typical representative of the emancipated provincial coming to the big city and travelling to foreign countries and thus encountering foreign cultures, this again with his innate curiosity for people. When in 1917 he worked as a night school teacher for blue collar workers of the steel mill he noted, “Out in the steel mills I saw industry for the first time; the heat, the roar, the white-hot iron pouring from the blast furnaces, ... It was thrilling after the life in Ohio villages, and a quiet college town” (ECH Papers in Vienne 2016: 97).

ECH stresses that emancipation must not come with alienation of one’s roots, rather each new experience adds a new perspective and yet another layer of the analytical gaze to the things so familiar to the observer. Emancipation is achieved by sociology thus engraining sociological insights into one’s personal development. I suspect that many a student of sociology will share this view and will therefore appreciate ECH as a virtual companion in research. Now that the Anthem Companion ennobles this approach as originating from a master of the discipline students are safe in defending it against Ph.D. committees or reviewers who consider fieldwork conducted in one’s quotidian surroundings not worthy to be called “data” or “evidence”.

The manifold observations of ECH’s Sociological Eye make good reading because we become immersed in the world of the provincial intellectual—this sounds like an oxymoron but probably represents the emancipated and yet not alienated sociologist from the country side who shows sympathy and yet detachment in his reports about the things so familiar to him—and us.
It almost reminds me of the popular fictitious community of Lake Wobegone by Garrison Keillor who portrays the characters of the small Midwestern town from the perspective of the returning and now intellectual provincial. The humor and sentimentality of Keillor’s fiction are related to a Hughesian approach to fieldwork.

ECH’s merit for sociology of the 20th century (and through this Anthem Companion Reader hopefully beyond) is his inspiring impact on scholars outstripping him in fame. Just like parents support their children’s development best when they keep their own egos in the background, good mentors and teachers are those who give their apprentices room to unfold their own ideas and evolve their talents. Interestingly enough, ECH himself metaphorically called one of his students the grand-child of his own teacher and mentor Park (cf. endnote 20 in Vienne 2016: 112).

One can draw a parallel to the sociology of professions – a centerpiece of ECH’s work: “For a sponsor, a protegé (1) eases the transition to retirement (Hall 1948; Hughes 1945); (2) gives him a sense of continuity of his work, and (3) gives some assurance that his intellectual offspring will build on his work” (Epstein 1970: 969).

Hardly is any research finding so highly self referential to the personal life of the scholar than as in the case of ECH and his impact on his students. Transition to retirement, however, was very late in ECH’s life. Evidence for this offers the beautifully composed chapter by Douglas Harper who found in ECH his Ph.D. advisor at Brandeis. Harper pictures in a very personal and moving manner his own development and the impact of his teacher and mentor and surmounts his text with a photograph he had taken of ECH at one of his last visits (Harper 2016). Harper’s praise would be appropriate for a festschrift, yet the editors Helmes-Hayes and Santoro declare that they deliberately wanted the book not to be another festschrift and thus included a critical view on ECH, too.

In contrast to the rest of the book, this chapter on ECH’s view on race relations (McLaughlin and Steinberg 2016) lacks the profound knowledge of the work, life and archival material about ECH. (Obviously the archives in Chicago provide an exceptionally rich gold mine for scholars since ECH kept notes about almost any incident in his life and amply corresponded with his students and mentees.)

McLaughlin and Steinberg’s critique that ECH were “an erudite professor, accustomed to the cloistered university that was deliberately walled off from the noise and distraction of the world outside” (McLaughlin and Steinberg 2016: 211) simply contradicts the facts well documented in ECH’s fieldwork which again is reflected in the other chapters of the book. The authors nevertheless progress as true believers of the now so fashionable Marxist view on the civil rights movement and claim that ECH, by using terms such as race relations (instead of “subjugation and exploitation”), were ignorant of the causes and effects of the “civil rights revolution” (McLaughlin and Steinberg 2016: 223) which allegedly prevented him and the whole discipline of sociology from foreseeing the popularity of the civil rights movement. Therefore, while ECH delivered his ASA Presidential address in L.A. in August 1963, Martin Luther King marched to D.C. and held his famous “I have a Dream” speech.

The claim that oppression of the black (as well as of the native) Americans had been “the elephant in the room that is studiously ignored in the Chicago paradigm” (McLaughlin and Steinberg 2016: 224) blatantly contradicts the evidence e.g. presented in Fleck’s chapter citing ECH’s notes on a discussion he had with a student in Germany who related German anti-Semitism to the fate of the
American “Indians”: “So I answered that we had found the Indians not willing to get out of our way, so we had killed a lot of them and shut the others up in concentration camps ... 'you probably wait for me to disown those people who did the dirty work. But I cannot do it because my own family passed on the legend of one predecessor who guilefully killed the last Indian in Gallia County, Ohio”’ (ECH Papers in Fleck 2016: 153, emphasis in original).

If only McLaughlin and Steinberg had subjected Herbert Marcuse’s Essay on Liberation a similar critique (cf. Marcuse 1969)! Moreover, does Marx’s term ‘relations of production’ prevent us from recognizing their exploitative character?

Anyway, the critics accuse ECH of following the “doctrine of value-free sociology” (McLaughlin and Steinberg 2016: 217)—thereby showing their lack of familiarity with the differentiated Weberian term of ‘Werturteilsfreiheit’ (cf. e.g. KAESLER 2003: 234–51). Their only evidence for ECH being “blinded by a white frame” is a critical book review he wrote about Cox’s Caste, Class and Race, another book review (Frazier’s Black Bourgeoisie) that contained a praise but was allegedly not fully appreciating “Frazier’s theoretical acumen and ethnography” (McLaughlin and Steinberg 2016: 229). And finally an introduction he wrote for Black Metropolis where ECH did “not seriously engage [its literary verve]” (McLaughlin and Steinberg 2016: 229) while still lauding it.

"Of course, none of this would be worth talking about ... “ (McLaughlin and Steinberg 2016: 230)—I couldn’t have put it any more accurately! One very interesting and informative chapter demonstrates how ECH’s concept of ‘master status’ was received and changed from the context of race and sex to a wide range of interpretations. It became finally absorbed by the discipline so that eventually scholars use it today without even referencing it to its creator ECH (van den Scott and van den Hoonard 2016)

All of this shows that ECH’s ideas are timeless and most probably will inspire many more intellectual offsprings in the future of sociology. His minimalistic, almost aphoristic style liberates the student from the heavy load of grand theory or dogma. Free minds—and only free minds—are able to create innovation.

References


