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ARTICLE

The Polish Career of *The American Soldier*

From the Model to the Legend[1]

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Abstract

The study conducted by Samuel Stouffer and his team in the US Army during World War II is generally considered to be a founding study in quantitative empirical sociology. The book *The American Soldier* (1949-1950) played an important role in the development and institutionalization of empirical social research. Joseph Ryan's monograph *Samuel Stouffer and the GI Survey* (2013) analyzes the history and reception of the research and book in the United States. This paper investigates the reception and impact of the book far from the United States: in Poland.

Keywords

The American Soldier; Samuel Stouffer; empirical social research; Polish sociology

Introduction

In the spring of 1945, divisions of the victorious American army halted at the Elbe, in the center of Germany. This border was to become the demarcation line between communist countries and the "free world." Many Poles regretted that the American soldiers had not gone further east. However, a soldier of sorts did venture further; *The American Soldier*, the work of the Research Branch created by Samuel Stouffer. This is the story of its career.

The research on the American military by Stouffer's branch during World War II was intended to provide reliable information to the leadership about soldiers' attitudes in order to aid decisions regarding the guidance and management of the armed forces. Generally speaking, the research helped the military—in war conditions—to move from an authoritarian model to a managerial one, replacing an emphasis on obedience with one on morale (Ryan 2013). Engineering and science are mutually inspiring. Published in the years 1949-1950, the four volumes of the *Studies in Social Psychology in World War II* are today acknowledged to be not only the fundamental study in military sociology,

[1] This is a paper presented at the International Sociological Association RCHS Interim Conference 'Monuments, Relics and Revivals', The University of Warsaw, 6-8 July 2016. The comments made by the two anonymous reviewers are greatly appreciated.

but also one of the founding works in quantitative sociology, playing an important role in the development and institutionalization of empirical social research (Converse 1987; Platt 1996; Schwebber 2002).

Strictly speaking, *The American Soldier* is the title of only the first two volumes of the *Studies* (Stouffer et al. 1949a,b), but in common parlance this is the name given to the entire work by Stouffer's team, which also includes *Experiments on Mass Communication* (Hovland et al. 1949) and *Measurement and Prediction* (Stouffer et al. 1950). Joseph Ryan's recently published (2013) and pithy monograph *Samuel Stouffer and the GI Survey* reveals the history and reception of the research and book in the United States. This article explores their reception and influence far from America, in Poland.

From the war to the post-Stalinist thaw

Before World War II, sociology in Poland was among the most highly developed in Europe and was based on a tradition reaching back to the nineteenth century (Bucholc 2016). Polish sociology originated as a native version of positivist sociology and later diversified into historical sociology, Marxism, humanistic sociology, the Durkheimian school, sociography, and other orientations. In the interwar period, sociology in Poland departed from theories of a speculative nature, entered the universities, and turned, as Thomas Kuhn would say, into a "normal science." Ludwik Krzywicki (1859-1941), Leon Petrażycki (1867-1931), Stefan Czarnowski (1979-1937) and Florian Znaniecki (1882-1958) are commonly recognized as the founding fathers and leading figures of early and prewar Polish sociology. Stanisław Ossowski (1997-1963), and Maria Ossowska (1896-1974), the scholars from the Lvov-Warsaw School of Philosophy, and Józef Chałasiński (1904-1979), a student of Znaniecki, all started their brilliant academic careers in the 1930s. Polish sociologists began to travel to universities abroad; a number of books on American society were written by those who had visited the United States. In 1932, Stanisław Rychliński published the first modern handbook on methods of social research, *Badanie środowiska społecznego* [*Studying Social Milieus*], (Rychliński 1932), which was based mainly on American sociological literature, particularly on the achievements of the Chicago School.

World War II interrupted this development. Many sociologists were killed, and some left before, during, or after the war, but afterwards the discipline revived. New sociological institutions were created and some prewar institutions were reactivated. The University of Warsaw, along with other academic centers in Poland, started teaching sociology and conducting empirical social research. Just as before the war, case studies involving fieldwork and biographical research were most common. Polish sociology cemented its first contacts with world sociology in 1949 in Oslo, when Stanisław Ossowski signed the founding declaration of the International Sociological Association (ISA). Then, with the consolidation of communist power at the turn of end of the 1940s and beginning of the 1950s, social research and the teaching of sociology at universities was interrupted because sociology was considered a bourgeois discipline and was supposed to be replaced by historical materialism.

In this brief period, news of the most recent developments in American sociology did not manage to reach Poland let alone become widespread. The first postwar works on survey methodology could not even mention Stouffer's research; their authors based themselves on American textbooks from before 1945 (Mirek 1948) and on the experiences of the Gallup Institute and the Czechoslovakian

Public Opinion Research Center (Matejko 1948). The first postwar manual on methods of sociological research was written by Jan Szczepański (1951)—a student of Znaniecki and a future president of the ISA—and was based on his lectures at the University of Lodz. In the manual, he pointed to standardized surveys as a method that plays an important role in sociology. He thus based his work on American books, but did not refer to them explicitly. In any case, his readers did not learn about Stouffer's study of the American military from this book either, although they may have heard about the work during their classes.

In March 1949, when the "cold war" period had already begun, Stanisław Ossowski, then head of the Sociology Department at the University of Warsaw, took part in the Scientific and Cultural Conference for World Peace in New York as a member of the official Polish delegation. After the conference he visited Harvard, where he met Pitirim Sorokin and toured the Laboratory of Social Relations directed by Samuel Stouffer. On April 9, he noted in his diary (Ossowski in press):

> Stouffer. He did not come to the party yesterday; today he slipped away at once. He is probably afraid to have contact with a delegate to a peace conference.[2] He receives large subsidies for research on the military. (The state can not subsidize science, because science is free. The military can.)

It is hard to call this episode a meeting even. From the entry it is also hard to determine whether Ossowski had heard about Stouffer's research during the war or about his book, which was to appear a couple of weeks after this entry. Perhaps he had heard about it from Theodore Abel or Robert Lynd, with whom he met earlier at Columbia. Abel, who came from Poland and had been a student of Florian Znaniecki, sketched for him a collective portrait of American sociologists, and Lynd, who captivated him by his "progressive" views and interest in the social changes in Poland, published a notable criticism of Stouffer's research a couple of months later (Lynd 1949).

The sociology departments of Polish universities were formally closed in 1952, but until then academic libraries could receive and import books from abroad. Ossowski's Sociology Division in the Humanities Department of the University of Warsaw managed to buy all four volumes of *The American Soldier*. Ossowski knew about the appearance of the work from advertisements and reviews in the *American Sociological Review*. When his division was eliminated in 1952, the book collection – including the set of *The American Soldier* – was sent to the newly established library of the Philosophy Department. These were the last contemporary American books to arrive there. The political blockade on purchases lasted only for a short time, however. At the end of 1954 the library bought a second set of *The American Soldier*, and it was obvious that there were thoughts of reinstating sociology.

After being prevented from teaching sociology at the University of Warsaw, Stanisław Ossowski conducted private seminars in his own home for his students and colleagues—legally, though he was harassed by young communist activists. The attendees reported on and discussed contemporary American books. Hanna Malewska-Peyre (2003), a participant, remembered of these books Ernest Greenwood's (1945) *Experimental Sociology* and "some methodological tome with those large studies into the American army"—that is, obviously, *Measurement and Prediction*. Both books came from the former library of Ossowski's department. Among newer books they read *The Language of*

[2] Ossowski's surmise is not necessarily correct. In a letter to me, Stouffer's biographer, Joseph Ryan, commented that "It isn't difficult for me to believe that Stouffer ducked out of a party early. While not exactly shy, he preferred smaller gatherings or the quiet of his office" (9 IX 2016).

Social Research by Paul Lazarsfeld and Morris Rosenberg (1955) and Stouffer's (1955) *Communism, Conformity, and Civil Liberties*. From them they learned social research methods: "just theoretically for the moment, but with the hope of application." They were right, because before long the post-Stalinist thaw began. In 1955, the Polish Academy of Sciences's Institute of Philosophy and Sociology emerged and the Sociological Research Unit, directed by the Marxist Julian Hochfeld (1911-1966), began to study the working class. Marxists quickly abandoned their distaste for the methods of American sociology and in 1957 sociology returned to the university as a field of study and area for research. Stanisław Ossowski returned to academia at the same time. In sociology, a new generation of sociologists had appeared alongside the scholars trained before the war.

Many leading Polish sociologists traveled to America and other countries on Ford Foundation grants. (Sulek 2010; Kilias 2017). The strategic goal of this program was to weaken Marxism and other ideological traditions, to reinforce empirical and rational thinking, and consequently to strengthen political pragmatism in Communist countries. The primary mission of Paul F. Lazarsfeld, who was the driving force of the program (he visited Poland twice in 1958), was to propagate his own conception of sociology in Europe as "empirical social research" (Pollak 1980; Sulek 1998a). Polish sociologists travelling to the United States brought back the latest sociological knowledge, acquaintance with contemporary methodology, personal contacts with foreign sociologists, and the newest books.

The *American Soldier* in the Polish army

After the political turning point of 1956, major changes also took place in the army. These were symbolized by the return to Moscow of the Soviet marshal Konstantin Rokossovsky, who had been minister of defense since 1949. The changes encompassed military training, the political apparatus, and methods of political propaganda. The Political-Military Academy, an institution training officers to conduct political and educational work in the armed forces, was reformed. The new commander of the Academy, General Adam Uziembło wrote firmly in an article in the officers' magazine *Wojsko Ludowe* [*The People's Army*] that political work in the military should rest on scientific foundations. "In the field of sociology and sociological studies," he wrote, "we have a lot of catching up to do in order to acquire a real understanding—no worse than in, for instance, the United States—of the moods and social attitudes of the working class, the masses, and in the army—of the mass of soldiers" (Uziembło 1957). That making up for lost time began quickly.

As early as 1957, the Military-Political Academy opened a sociology department, which was briefly headed by Jan Szczepański. In the same year, an extensive presentation by Zygmunt Gostkowski (1957) of the research on the American military in wartime appeared in *Wojsko Ludowe* along with Uziembło's article. Before the breakthrough of 1956, Gostkowski had been interested in the shaping and researching of public opinion in the United States and before long was to become one of the most innovative survey researchers in Poland. In his article, Gostkowski discussed the organization of research by Stouffer's team: the format of questionnaires; the idea of a representative sample, with the manner of its selection for research on the military; the experimentally proven effectiveness of film propaganda; and how the material was used. Gostkowski presented the research so illustratively, and in such detail, that it was possible to follow in the footsteps of Stouffer's team without having read the source material. He also presented American research into soldiers and enemy propaganda, and research on the military in peace time.

Before long, in 1958, the chairmanship of the Sociology Department in the Military-Political Academy was taken by Jerzy Wiatr of the University of Warsaw, a student of Hochfeld and an "engaged"

Marxist, but also a proponent of modernizing Marx through empirical research and the assimilation of sociological achievements. For example, in 1959 and on his initiative, the two first volumes of *The American Soldier* were translated on the basis of the copy in the University of Warsaw's sociological library. The following year the book appeared in four volumes (*Studia* 1960). These were published by the Board of Political Propaganda of the Main Political Board of the Polish Armed Forces as issues in the *Biuletyn Informacyjny* [*Information Bulletin*] in the "Psychology and Military Pedagogics Series." The book was intended "for the exclusive use of generals and officers of the Polish Armed Forces," but it was available, without restrictions, in several copies in the library of the Philosophy and Sociology Department at the University of Warsaw. It is not known whether other translations of the book were made anywhere in the world.

In the foreword to the translation, Wiatr made a partly formal, partly honest critique of the book from the Marxist standpoint; among other things, he pointed to the author's lack of class analysis of the American military. Such introductions were added to many translations of western sociological books in Poland. More importantly, Wiatr argued for the value of this work for the Polish military. First, he wrote, the book showed the American armed forces as a social institution and environment. Second, it revealed how sociological research was conducted and utilized in the American military. Third, it taught the methodology of sociological research in military conditions and for the practical uses of the armed forces. Wiatr also wrote favorably about *The American Soldier* in his numerous works on military sociology—starting with an introduction to military sociology, *Armia i społeczeństwo* (1960) [*The Military and Society*], where he reported on some of Stouffer's findings and generalized them for other armies (for instance, the findings concerning the hierarchical structure of the military). It is worth adding that Wiatr's department also issued a duplicated translation of Morris Janowitz's work (1962), including fragments of the book *The Professional Soldier*. These supplemented reports of research on the American military with a theoretical view of the military as a social institution and added information about military professionals to the information about conscripted soldiers.

It can be considered that, thanks to the translation and discussions of *The American Soldier*, the book was known to the educated members of the military, or at least many of them would have heard about a large and interesting sociological study of the American military during the war. I am unable to say how the findings of the American sociologists on shaping soldiers' attitudes affected practices in the Polish military in regard to training and leadership, the adaptation of soldiers to army conditions, evaluations of their morale, and so forth. However, this book undoubtedly influenced the development of military sociology in Poland. The Sociology Department, changed into the Department of Sociology of the Military, began systematic survey research into various aspects of military life and the relation between the military and civilian society. Research into the military, which was first undertaken by the Military-Political Academy, was continued by other research institutions. After the elimination of the Academy in 1990, there was even a separate Military Institute for Sociological Research that existed for several years. Naturally, that research is not currently shaped by studies of the US Army during World War II, but the first drives toward it came from America and are part of the tradition that *The American Soldier* created. The book itself is quoted in the works of contemporary military sociologists. For the most part, these are ritual and generalized references, serving to give an impression of the authors' erudition and to underline their connection with that praiseworthy tradition.

In the university world

From its rebirth (in the middle of the 1950s) to the end of the 1960s, sociology was taught and practiced at three universities in Poland: Warsaw, Lodz and Krakow. It was at the University of Warsaw that *The American Soldier* was most prominent, due mainly to Stefan Nowak, who became an "intervening variable" in the influence of American methodology on sociology in Poland. But also to Stanisław Ossowski and Zygmunt Bauman, who referred to Stouffer's works in their discussions and polemics on the sociology of the time, both in Poland and abroad. At the time, Ossowski and Bauman were emblematic figures for two streams and two generations of Poland's renewed discipline of sociology. Ossowski, who was born in 1897, was considered to be a classic exponent of Polish humanist sociology, while Zygmunt Bauman, born in 1925, was the rising star of modern Marxist sociology.

Shortly after the rebirth of sociology, Stefan Nowak, a student of Stanisław Ossowski, became the leading figure in the field of sociological methodology and also the embodiment of a new model of empirical sociology. In 1958, he conducted the first academic survey research in Poland ("Warsaw Students"), which was described on the front page of the *New York Times* (6 October 1958) as the first opinion survey to be conducted beyond the "iron curtain." Nowak spent the academic year of 1958/9 on a Ford Foundation fellowship with Paul Lazarsfeld at Columbia, and on returning gave popular seminars and lectures propagating the new research techniques. Nowak had a significant impact on the empirical sociological model practiced in Poland after 1956 (Sułek 1998b). Although during his stay at Columbia, Nowak most probably did not meet Stouffer personally (Stouffer was then at Harvard), he studied *The American Soldier* closely, both directly and from other books that referred to it: *Studies in the Scope and Method of "The American Soldier,"* edited by Robert Merton and Lazarsfeld (1950), Herbert Hyman's book *Survey Design and Analysis* (1955), and the well-known essay on *The American Soldier* written by Lazarsfeld (1949) for *Public Opinion Quarterly*.

For Nowak, the research by Stouffer's team was a source of persuasive and memorable examples to be used in his university teaching. He quoted findings from *The American Soldier* in order to show how deceptive a reliance on common knowledge can be. It was generally considered obvious that better educated soldiers and soldiers from the city would have a harder time dealing with the difficulties of military service and the stress of battle than less educated soldiers and those from rural areas: farm people and people with little education were considered to be less psychologically sensitive and more accustomed to hardships. The research showed the opposite: better educated soldiers and those from the city managed more easily. In order to show how a mediating variable can elucidate a surprising original relation, Nowak (1968) cited the explanation of an *additional* relation between the educational level of recruits and their willingness to do military service: less educated soldiers more often had acquaintances who, on account of work in arms factories, were exempt from service, and thus being drafted appeared to them an injustice on the part of fate. From Stouffer's research, Nowak also drew models of valid indicators. One such example for him was the question of whether a soldier wore his uniform or civilian clothing while on leave; this behavior was a very accurate indicator of identification with the military. Nowak's former students also remember a curious indicator of the caste nature of the American military—non-commissioned officers who were promoted into the officer ranks were released from service for a day before being called into the officer corps!

In empirical social research *The American Soldier* functioned as a manual, or rather as a tool box. The first manual of research methods appeared in Poland only in 1965, when censorship allowed the printing of Nowak's reader *Metody badań socjologicznych* [*Methods of Sociological Research*]. It was composed of translated chapters of an American manual of research methods from the first part

of the 1950s, and many of them referred to examples, experiments, or innovations in Stouffer's research (Nowak 1965). These included the chapters presenting—or examples illustrating—the selection of indicators (Patricia Kendall), control and interpretation of statistical relationships (Marie Jahoda et al.), experimental designs (William Goode and Paul Hatt, Carl Hovland et al.), Lazarsfeld's latent structure analysis, and Guttman scalogram (Stouffer). The book had a print run of 5,000 copies, which was many times higher than the number of all the sociologists and sociology students in Poland, and for many years was the basic manual of social research in Poland, not only for sociologists, but also for pedagogues, psychologists, and other researchers. It was a major conveyor of the methods and findings of *The American Soldier*.

Examples from Stouffer's team's research also appeared in other popular books of this time. Aleksander Matejko (1962), in a monograph on the sociology of industry in America, referred to findings concerning the key importance of "small groups" in the American military and expanded them to other armies (the Wehrmacht) and other institutions (factories). Hovland's experiments were discussed by Stefan Szostkiewicz (1961) in the textbook *Procedury i techniki badań socjologicznych* [*Procedures and Techniques of Sociological Research*], and generalizations concerning group conformism were formulated in Andrzej Malewski's well-known book (1965, 1967) *O zastosowaniach teorii zachowania* [*Applications of Behavior Theory*].

The case of the Guttman scale is particularly interesting as an example of the migration of a methodological idea. This type of scale, which was developed during the research into American soldiers, appears in Nowak's studies of Warsaw students. In 1958 the students were asked if a person "should risk his life in defense of"—the truth, human dignity, family, religion, the fatherland, friends, a social ideal, or human life. It is not possible to determine, today, whether it was earlier surmised or unexpectedly discovered that the students' answers formed a cumulative Guttman-type order with a high "coefficient of reproducibility": human life, family, the fatherland, human dignity, friends, truth, religion, a social ideal. Witold Jedlicki, who discovered it, drew inspiration directly from *Measurement and Prediction* in acknowledging that the scale did not measure an outward preparedness to "die for" things but is a scale of "ritualism" in regard to values. Three years later, the repetition of the question revealed a "distinct decline in the popularity of the model whereby a Pole is ready to stick his neck out and risk his life for everything, or at least to value that readiness in others" (Pawełczyńska and Nowak, 1962). Unfortunately, the cumulative nature of this scale was not used in the analysis; the respondent was not given *one* score on that scale, defined by the highest chosen value. Furthermore, Jedlicki's analysis itself remained in typescript form, and later existed only in the memories of the quite small number of people who had read it.

Over time, ideas from Stouffer's team's research became part of the practice of social research in Poland. Their connection with *The American Soldier* became blurred, and it became possible to turn to Polish studies for examples. For Nowak himself, *The American Soldier* was replaced by *Warsaw Students*, and the paradigmatic question of whether a soldier wore his uniform or civilian clothes on leave was replaced by the question "Would you want the world to move in the direction of some form of socialism?" On the other hand, the theoretical implications of the research into the American military gained in significance. This was favored by the fact that in 1962 translations of Hans Speier's article "*The American Soldier* and the Sociology of Military Organization" and of Robert Merton and Alice Kitt's essay "Contributions to the Theory of Reference Group Behavior" appeared in a popular anthology, *Zagadnienia psychologii społecznej* (Malewski 1962) [*Issues in Social Psychology*]. Both articles were from Merton and Lazarsfeld's (1950) book *Continuities in Social Research: Studies in*

the Scope and Method of "The American Soldier." The second article also appeared as a chapter in Merton's *Social Theory and Social Structure* when it was published in Polish (1982).

Among the theoretical innovations of *The American Soldier*, the best known is the idea of relative deprivation. In connection with another important concept—the reference group—it quickly entered circulation, first in academic sociology, later in public sociology, and finally in social journalism. Today, it is no longer elucidated by the example of promoted but dissatisfied air force soldiers and unpromoted but satisfied MPs in the American military.[3] The idea can be understood without illustration and, if it needs exemplification, then in Poland's period of systemic transformation and the population's striking inequality in profiting from it there are sufficient examples of relative deprivation—among those who *came out ahead* in the transformation—to not need to reference Stouffer. The longevity of this concept is attested by a newspaper excerpt of an interview with an expert in regard to the 2016 terrorist attacks in Europe (Sasnal 2016). In rebutting the opinion that acts of terror are the blood-soaked harvest of a policy of multiculturalism, the expert explained that

> Millions of French Muslims are quite well integrated with the rest of society. But at the same time, many at the lower levels of society are suffering so-called relative deprivation. They aren't hungry, as in Syria; the bombs aren't dropping on their heads; they aren't subject to the terror of the rules introduced by the Islamists. But it is enough for them to turn on the television to be convinced that they have less than others, that they live worse, are poor, and are condemned to failure in life. Such a feeling grows, and thus social and individual frustrations accumulate. And this breeds radicalism, which in the case of Arab society, can be Islamized.

Stouffer's research and book also appeared in meta-sociological discussions in Poland in the early 1960s.

Stanisław Ossowski then wrote the important book *O osobliwościach nauk społecznych* (1962, 1973) [*On Peculiarities of the Social Sciences*], which contained a critical analysis of "modern empirical sociology" and its fascination with the natural sciences model. In Poland empirical sociology—because that is what it was called—was also the model for engaging in sociology, and thus Ossowski countered it with the model of sociology as a humanist discipline. The book is full of the imprint of his meetings, lectures, and experiences during a long stay in America in 1958, supported by the Ford Foundation. Stouffer's research into the American military appears in Ossowski's book as a model representation of empirical sociology, both in terms of research technology and epistemological limitations: Stouffer's team developed a refined quantitative methodology but used it to research problems that concerned specific collectives and were moreover narrowly practical.

Ossowski expressed his opinion of Stouffer's studies using quotations from reviews by their American critics. He referred with approbation to Sorokin (1956: 146), who wrote in *Fads and Foibles in Modern Sociology* that Leo Tolstoy's *War and Peace* gives "not only a more vivid, more significant, but also more accurate picture of the motivations studied." Sorokin referred to Tolstoy because Stouffer opened his work with a quote from that author—"in warfare the force of armies is the product of the mass multiplied by something else, an unknown *x*," and that *x* factor is "the spirit of the army," or its morale, which Stouffer had decided to study with modern scientific methods. Ossowski

[3] Although promotions in the air force were decidedly more frequent than in the military police, the air force soldiers had a much worse opinion of their system of promotion than the MPs had, because the soldiers in each force compared themselves with their colleagues and not with the soldiers in the other force; for those airmen who were promoted, the promotion did not seem out of the ordinary, but for those who were not, the failure was more painful than for the MPs (Stouffer et al. 1949a: 251-253).

considered that contemporary "research of the Stouffer-studies type" was morally ambivalent—"they aim to perfect the technique of effectively managing human masses according to aims decreed by those at the top" (p. 310). He suggested here, probably due to reading too much into Lynd's opinion, that the true aim of Stouffer's research was to "find a successful method of subordinating hundreds of thousands of young men to the will of the American leadership, in order most efficiently to incline them to risk their necks for matters that are foreign to them." This was supposed to be an example of how sociological research, encouraged by the holders of power and capital, enhances the power of those who already possess it.

In *Wizje ludzkiego świata* [*Visions of a Human World*], another much-read book of the period—and relying on the opinions of those same two American sociologists—Zygmunt Bauman (1964) appraised Stouffer's research in a similar spirit. Criticizing the ease with which the sociologists treat the survey answers as information about psycho-social reality, Bauman (1964: 274) quotes Sorokin's words from *Fads and Foibles*:

> In their study of the correlation between the educational level of samples from the U.S. armed forces and their combat performances—they [the group of scientists headed by Stouffer] neither directly observed nor scientifically tested the combat performance of their samples, nor evaluated it themselves on the basis of any objective, verifiable data. Instead, they simply took the opinion of some army authorities without the slightest checking of its correctness" (1958: 38).

In criticizing what he considered to be the manipulative nature of neo-positivist sociology, Bauman (pp. 258-259) relied on Lynd's opinion of *The American Soldier*, cited by Mills (1959):

> These volumes depict science being used with great skill to sort out and to control men for purposes not of their own willing. […] With such socially extraneous purposes controlling the use of social science, each advance in its use tends to make it an instrument of mass control, and threat to democracy.

It is noteworthy that neither Ossowski nor Bauman informed their Polish readers about the contents and character of *The American Soldier*; they wrote about the book as about things that were then known from hearsay. Neither Ossowski nor Bauman attempted an independent interpretation of the work, and it is certain that at least Ossowski knew the book firsthand. In the case of both authors, it was basically not about criticism of Stouffer's research and work; it was only an argument in the critique of a certain model or style of engaging in sociology; *The American Soldier* appeared as an "empirical type" of the style, something like the Church of the Gesù in Rome for baroque sacral architecture. We might call it the humanist critique, because the connotations of the term "leftist criticism" are rather too strong and too unequivocally political. Both Ossowski and Bauman, it would seem, were speaking less of American sociology and more about sociology in Poland. They did not criticize it directly, but very indirectly: using quotations from American sociologists, they criticized its American prototype. It was as if people who did not like the baroque churches built in Poland criticized them not by pointing out that their ornamentation offends harmony and moderation, but by quoting the opinions of Italian critics of the Roman prototype. In Poland at the time, Stouffer's name (like Lazarsfeld's) symbolized empirical sociology, which for some meant "modernity" and for others "the invasion of sociological Americanism." The name was given to a kind of research and appeared in the unpleasant plural "Lazarsfelds and Stouffers." In sociology, world theories, methods, and names are tools in local struggles. This was precisely such a case.

Conclusion

In the case of *The American Soldier*, as in the case of many other works, the Mertonian principle of incorporation by obliteration operates. The methodology and general findings of this work have grown into the fabric of sociological knowledge, and for Polish sociologists, particularly the younger generation, the general awareness that Guttman scalogram and relative deprivation are connected with Stouffer's research on the American military in World War II has worn away. Although only the smallest number of currently active sociologists have ever had *The American Soldier* in their hands; the ideas, methods, and conclusions of this book reached them indirectly, by means of a multistage process of academic communication. The same happens with many classic sociological works, which are more often "known" than read; that is, they "are known" without being read.

When empirical sociology formed in Poland at the turn of the 1950s to 1960s, *The American Soldier* was presented not only as a great achievement but also as persuasive proof of sociology's vast potential. In his lectures on methodology at the beginning of the 1960s, Stefan Nowak reminded his audience that, thanks to this research, the American leadership was able to learn what soldiers preferred for the coming time of peace, and the government was thus able to prepare university places for millions of former soldiers wanting to study. This made an impression on the students. In the literature, it is pointed out that Stouffer's research helped plan and conduct, with as little conflict as possible, the great operation of demobilizing a couple million soldiers. Stouffer worked out a point system that made it possible for a soldier to be discharged on the basis of four criteria: length of service in the army, length of service abroad, participation in battle, and number of children (waiting for their father's return). It turned out that among the soldiers who were not released in first order, the decided majority considered the system to be just (Stouffer et al. 1949b: 520-548). This is one of the most well-known examples of a point system as a method for distributing indivisible goods (Lissowski 2012). Today in Poland, after the outburst of Solidarity and the implosion of the communist system, we know that sociology cannot manage to foresee systemic discontinuations as well as it can predict mass individual decisions (Sułek 2009). We also know how difficult it is to design a method for distributing goods that will also be accepted by those who do not receive them, or receive them in diminished quantity. Nevertheless, Stouffer's research and work became part of the scientific legend, a symbol of an innovative and successful social study.

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ARTICLE

Communalism and Internationalism

Publication Norms and Structures in International Social Science

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Abstract

This article presents a historical-sociological case-study that addresses the "enactment" of the ideals of communalism and internationalism in the social sciences. It focuses on the transformations in/of two journals, *Isis* and *International Sociology*, which deliberately attempt to enhance international social science. Our analyses of the publication practices in these journals point to the skewed global orientation in/of these journals, despite their outspoken internationalist ideals. Internationalization looks more like Americanization, when we compare the publication practices in international social science journals with their own ideal of balanced national representation.

Keywords

Merton's CUDOS; sociology of science; internationalization; Americanization; journals; publication pattern

Introduction

In a well-known paper on the normative structure of science, originally published in 1942, Robert Merton put forward "four sets of institutional imperatives," which "are taken to comprise the ethos of modern science" (1973: 270). These sets of institutional imperatives were: communalism; universalism; disinterestedness; and organized scepticism (CUDOS). Communalism referred to the institutional imperative for the public communication of research findings. "Secrecy is the antithesis of this norm; full and open communication its enactment." (1973: 274)[1]

Merton's view on the normative structure of science has often been criticized, in part while it has given way to various a-historical interpretations. Merton himself maintained that these norms and imperatives were functional, that they could provide for "the fullest measure of development" of the scientific system (1973: 270). But the institutionalization of these norms and imperatives may also

[1] In the original, pre-Cold War formulation, Merton spoke of the imperative of "communism." In later versions and reprints of his article, he introduced the term "communalism." But he kept distinguishing this approach towards knowledge from the focus on "private property" in capitalist economies.

be historicized. We may also inquire into the historical transformations of different aspects of the communication practices established within particular scholarly contexts.

Internationalism is an important implication of the imperative of "full and open communication." A variety of institutional arrangements, such as international journals, international conferences, and international associations, aspired and aspire to a global role. In significant ways, such institutional arrangements shaped and shape the circulation and reception of scholarly work at the global level (Schofer 1999; Heilbron 2014). They have also come to define what internationalism means in the current academic world. In this paper, we present a historical-sociological case study that addresses several aspects of the "enactment" of communalism and internationalism in the social sciences.

Our case study focuses on the transformations in/of two journals, which deliberately attempt(-ed) to enhance international social science. The first one is *Isis*, a well-established journal that is now mostly associated with 'history of science,' but that had a much broader orientation in the first half of the twentieth century. The second one is *International Sociology*, an official journal of the International Sociological Association, which since 1986 has explicitly intended to publish work of authors from diverse regions of the world. On the basis of analyses of the publication practices in/of these journals, we intend to discuss internationalism in/of the social sciences.

We will introduce some theoretical and methodological reflections that have guided our historical-sociological analyses followed by a brief presentation and discussion of relevant changes in the communication practices in the journals *Isis* and *International Sociology*. In our discussion, we will pay particular attention to changes in language of publication, institutional affiliation, and internationalism of the citation environment. For a period of about one century—from the early twentieth to early twenty-first centuries—the following analyses seek to discern the norms and structures that shaped and shape predominant publication practices in international social science.

International communication

Before analysing the historical dynamics of international communication in the social sciences via a case study, it is useful to present some general observations and reflections on the establishment of an international infrastructure for the communication of research findings.

Historically, it is useful to pay attention to different 'phases' in international scholarly commitments. Although the 'take-off' of the internationalization of the social sciences is mostly situated after the Second World War, internationally oriented scholarly institutions—such as congresses and journals—had already emerged in the latter half of the nineteenth century. Within the field of social statistics, for example, international congresses were held throughout the third quarter of the nineteenth century, the first being organized and convened in 1853 in Brussels by the Belgian "social physicist" Adolphe Quetelet. On a bi- or tri-annual basis, these congresses brought together hundreds of scholars and state employees to discuss the technical, scientific and organizational progress of their work.

The development of sociology took place at a somewhat later date, but its international infrastructure was created almost simultaneously with the first national or local sociological institutions. In 1893, the first international association for sociology, the France-based *Institut international de Sociologie*, was founded by René Worms. Shortly before founding the IIS, Worms had also launched the explicitly internationalist *Revue International de Sociologie*. After that, he launched a related book

series, the *Bibliothèque International de Sociologie*. On behalf of the IIS, he also founded the *Annales de l'Institut International* and organized a series of international congresses, the first five of which were held in Paris between 1894 and 1903 (Wils and Rasmussen 2012: 1275).

Sociology's international ambitions were not unique, although an important difference between it and other disciplines needs to be noted. At the end of the nineteenth century, several disciplines had already gained prominence in particular national contexts and university systems. Building upon such national and local settings, international networking was used to widen the geographical reach. In other disciplines, which were less established and institutionalized at the time, including sociology, it worked the other way around; internationalization preceded academic recognition. It was a way to acquire scientific legitimacy at the national level within national university systems.[2]

Overall, the twentieth-century expansion and institutionalization of the social sciences was closely entangled with the expansion of nation states. In many ways, both institutionally and intellectually, the social sciences became institutionalized along national lines. The prevalence of methodological nationalism in the social sciences might be seen to ensue from this historical relationship (Chernilo 2008). Most contemporary histories of sociology are also written as national disciplinary histories, as histories of American sociology (Calhoun 2008; Turner 2014), Austrian sociology (Fleck 2016), Belgian sociology (Vanderstraeten and Louckx 2018), Danish sociology (Kropp 2016), French sociology (Heilbron 2015), Irish sociology (Fanning and Hess 2015), Polish sociology (Bucholc 2016), and so on.

On this national basis, international exchange expanded rapidly in more recent decades. Several scholarly institutions, including organizations such as the *International Sociological Association* (ISA), were set up to bridge the gaps between national disciplinary communities and to facilitate global cooperation. At present, the social sciences might be seen as an increasingly global system, not only because they have come to include scholars from virtually all regions of the world, but also because global exchanges have become organized through several closely related institutional forms, such as international conferences, international associations, and international journals (Heilbron 2014). Hence, we may also analyse how these institutional forms have come to channel and control publication and communication practices within international social science.

As well as conferences, scholarly journals have often been used as a mode of communication and circulation of knowledge on an extended scale. But these journals do not just enable or facilitate "full and open communication" between the members of the scientific community. Importantly, these journals and their editorial boards also allow for 'boundary work.' They allow separating a small body of 'legitimate' scholarly work from other enterprises making it possible to maintain a distinction between academically legitimate forms of scholarly work and merely popular or wholly 'unscientific' undertakings (Heilbron 2015). Scholarly journals are often entitled to claim a monopoly on defining the legitimate forms of scholarly work in a particular discipline.

Seen in this light, these journals and the articles therein 'control' the formation of scientific disciplines or specializations. The journals and their editorial boards put up a barrier, but also grant a minimal form of academic recognition to the published research findings. They both secure the

[2] But an international outlook was not a sufficient condition for academic success. As is well known, Worms' initiatives did not receive much academic recognition in France (Heilbron 2015: 93-98). In terms of acquiring national visibility and institutional support, Durkheim's school of sociology was more successful.

shared values of a scientific community and endorse what the scientific community takes to be certified knowledge. And while the authors of articles typically accept the specialization chosen by the journal to which they submit their work, they also continually modify this specialization by the cumulative effect of their published findings (Stichweh 1984; Abbott 1999; Vanderstraeten 2010).

It may be added that journals influence the temporal structure of academic work. The periodicity of appearance presses scholars to publish at regular intervals; 'publish or perish'. The institutionalized publication imperative even discredits research that has not yet produced this kind of output. As long as no results are published (in peer-reviewed or refereed journals), it is difficult—both institutionally and psychologically—to close off particular research projects. Researchers only gain freedom to do something else, to move to new research projects, once they have been able to communicate the results of previous commitments to their peers via 'appropriate' venues.

In this sense, scholarly journals specify Merton's communication or publication imperative. They carry, channel, and give shape to the communication processes within scientific disciplines. They do so in ways that pre-structure who and how one can contribute to the development of particular lines of research (Bazerman 1988; Grafton 1997). 'Scientometric' instruments that have been developed in the past decades—such as the *Journal Citation Reports* and *Journal Performance Indicators*—have moreover strengthened the relevance of 'high-ranked' journals in ongoing scientific communication processes.

It may be added that in recent decades instruments, such as *Web of Science* (WoS) or *Scopus*, have become important tools for the evaluation of the quality of scientific research. They are used to monitor and control the publication practices of researchers from a broad range of disciplines; their statistics are used to discuss the success, impact, and visibility of research conducted in various national and/or local settings (Schofer 1999; Espeland and Sauder 2007). For scholars and science administrators alike, publications and citations included in these databases have become the difference that makes a difference. Currently statistics, such as impact factors and rankings, not only provide a powerful vision of what 'international' has come to mean in research, they have also been incorporated into the everyday 'world' of a variety of academic systems. Because of their significance for this communication process, we may also use the history of journals included in these databases to shed light on the historical enactments of Merton's imperative.

In what follows, we will present empirical analyses of the changing forms of internationalism in the communication processes in the social sciences. This includes a case study of two scholarly journals, one founded before the First World War (*Isis*) and one long after the Second World War (*International Sociology*), which explicitly attempt to enhance global cooperation and international social science. Both journals are also indexed by most bibliometric and scientometric tools. Our empirical analyses will allow us to explore the relation between communalism and internationalism and discuss the tensions between the local and national level, on the one hand, and global horizons of scholarly communication on the other. While we deal with both journals as source material, we will quote from their publications by referring to the journal, publication year and page numbers.

Isis

The journal *Isis* is now associated with history of science, but its disciplinary orientation was initially much broader. When *Isis* was founded in 1913 by the Belgian-born George Sarton, its *Comité de Patronage* included prominent figures with diverse disciplinary orientations and affiliations, such

as Émile Durkheim, Karl Lamprecht, Henri Poincaré, and Arnold van Gennep. In *Isis'* programmatic opening essay, Sarton put forward his view on the identity of a yet-to-be-established field of study. He defined it as a "psycho-sociological investigation" into the history of science (1913: 36-37).

At the beginning of the twentieth century, this scholarly interest followed from the expansion and increasing specialization within science. Several other periodicals devoted to the history of science also appeared at that time, including *Janus: Archives Internationales pour l'Histoire de la Me´decine et pour la Ge´ographie Me´dicale* (1896-1990), *Mitteilungen zur Geschichte der Medizin und der Naturwissenschaften* (1902-1942), *Archiv für die Geschichte der Naturwissenschaften und der Technik* (1909-1922), and *Archeion: Archivio di storia della scienza* (1919-1934). For various reasons, however, most of these periodicals did not survive. It was, at least in part, due to the entrepreneurial skills of Sarton that *Isis* became the flagship journal in its field.[3]

The first issues of *Isis* were published in Sarton's place of residence in Belgium (Wondelgem-lez-Gand). Almost immediately, however, the First World War interrupted its publication. After the German invasion of Belgium, Sarton emigrated via England to the United States. The second issue of the second volume of his journal could only be published in 1919. Its new subtitle also specified its broad remit: *An International Review Devoted to the History of Science and Civilization*. For Sarton, studies on the history of civilization could serve to shed light on the social benefits of the diffusion of scientific principles and scientific findings (see Pyenson 2007: 186-191).

A few years after the First World War, Sarton cofounded the *History of Science Society*. HSS was closely tied to the journal *Isis*. Its primary purpose was "to promote the study of the History of Science, and more particularly to support the publication of *Isis*, which has become its official organ." It had to "aid in maintaining and in assuring the future of a journal that is recognized at home and abroad as a powerful factor in stimulating the study of the history of science" (*Isis* 1924: 4, 6). In 1938, Sarton also started the publication of *Osiris*, a periodical companion to *Isis*, in which he included longer (at times book-length) manuscripts on the history and sociology of science.

Sarton ended up at Harvard University. At Harvard, he and Talcott Parsons jointly supervised the Ph.D. dissertation of Robert Merton (titled *Science, Technology and Society in Seventeenth Century England*, and first published as volume 4 in Sarton's *Osiris*). Merton also became Associate Editor of *Isis* in the late 1930s, first responsible for what was called "the social aspects of science" and, as of 1942, for "sociology" (see also Merton 1985, 1988). Sarton remained the chief editor of *Isis* for four decades, until 1952. *Isis'* subtitle was subsequently changed to *An International Review Devoted to the History of Science and its Cultural Influences*, which it remains so today. The institutional ties between *Isis* and HSS also remained; subscriptions to *Isis* are still concurrent with membership in HSS. At present, HSS counts some 900 institutional and 2300 individual members.

From the outset, Sarton tried to address an international audience with *Isis* (Pyenson and Verbruggen 2009). In 1913, his journal appeared as a multi-lingual quarterly review with contributions in French (Sarton's own native language), German, Italian and English. In 1919, however, when the publication of *Isis* was resumed in the New World, Sarton made a plea for one lingua franca in sci-

[3] A good indication of the central role of Sarton and *Isis* in this field of study is the establishment, in 1955, of the George Sarton Medal, a lifetime achievement award, which is presented as "the most prestigious award of the History of Science Society" (http://www.hssonline.org/about/society_sarton.html). This "most prestigious award" is not only an explicit tribute to George Sarton and his accomplishments; it is at the same time an award that reaffirms the leading position of Sarton's brainchild *Isis* in the field.

ence. After a short French language "Avant-Propos," he reiterated in an English text *Isis'* commitment to the formation of a globalized community devoted to the history of science, but also communicated his intention to henceforth "restrict its publication to one language instead of four" (*Isis* 1919: 321). For Sarton, *Isis'* "poly-glottism" had been "a serious and unnecessary obstacle to its circulation and consequently to the diffusion of the history of science" (*Isis* 1919: 321). He concluded his text by stating that he himself would from now on only write and publish in English.

Sarton continued to publish in English until the end of his life. Throughout his editorship, he also continued to defend the choice for one language in order to support the international character of (the history and sociology of) science. Shortly after the Second World War, in a comment revealingly entitled "The Tower of Babel," he observed that "during the last decades, the number of languages employed for scientific purposes has considerably increased" (*Isis* 1948: 14). But he immediately added his own point of view: "In the field of science the excessive multiplicity of languages is not only objectionable, but stupid and wicked. The scientific needs of mankind are served best by the monopoly or quasi-monopoly of a few languages." (*Isis* 1948: 14)[4] The material published in *Isis* allows us to shed light on the transformations of *Isis'* internationalist aspirations.

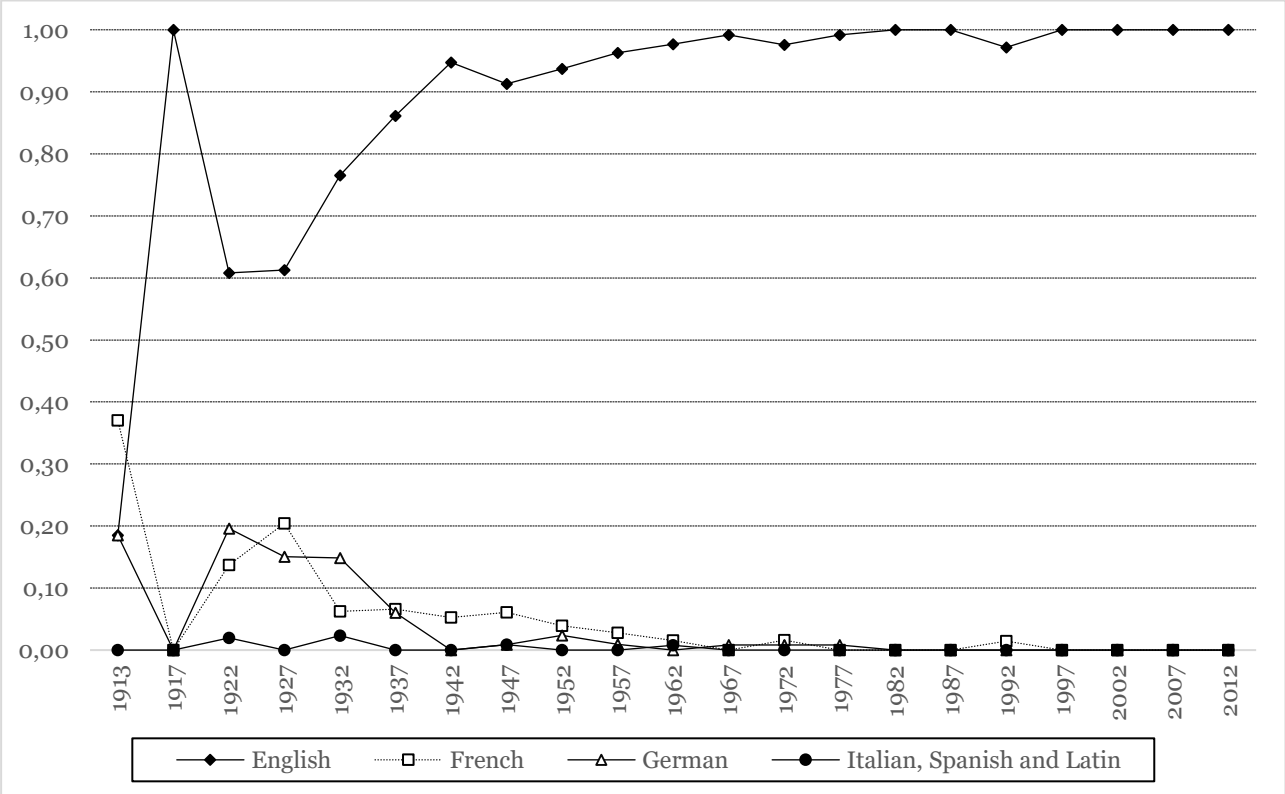

Figure 1: Language of the articles published in Isis, 1913-2013

Figure 1 provides an overview of the language of all articles published in *Isis* since its foundation. As this figure shows, *Isis* would continue to publish a limited number of contributions in other European languages for quite some years after the Great War. The explanation for this multi-lingual trajectory was probably rather prosaic. In the interwar period, the journal often had little or no backlog

[4] Not all members of Sarton's editorial team shared his point of view. Merton, for example, had a quite different view on Sarton's 'language policy' and the 'gatekeeping role' of the editorial team (see Merton 1973, 1985). Until the period of the Second World War, however, *Isis* was very much Sarton's journal.

of articles. Sarton indeed often had to actively solicit submissions within his personal network (which in part was still situated in Europe). Altogether, contributions in six – not four – "international" languages were published. There appeared one Latin text in *Isis*: a reprint of a fourteenth-century treatise on trigonometric methods (*Isis* 1923: 99-115). Sarton also included a few publications in Italian or Spanish. In the 1920s, 1 out of 5 published articles could be written in either French or German. There was nevertheless a relatively sharp increase in the number of articles written in English during the interwar period. The last non-English language article was included in 1974; it was a French language contribution by a Québec-based historian of science (*Isis* 1974: 212-228). During the last four decades, *Isis* has been an English-language journal.

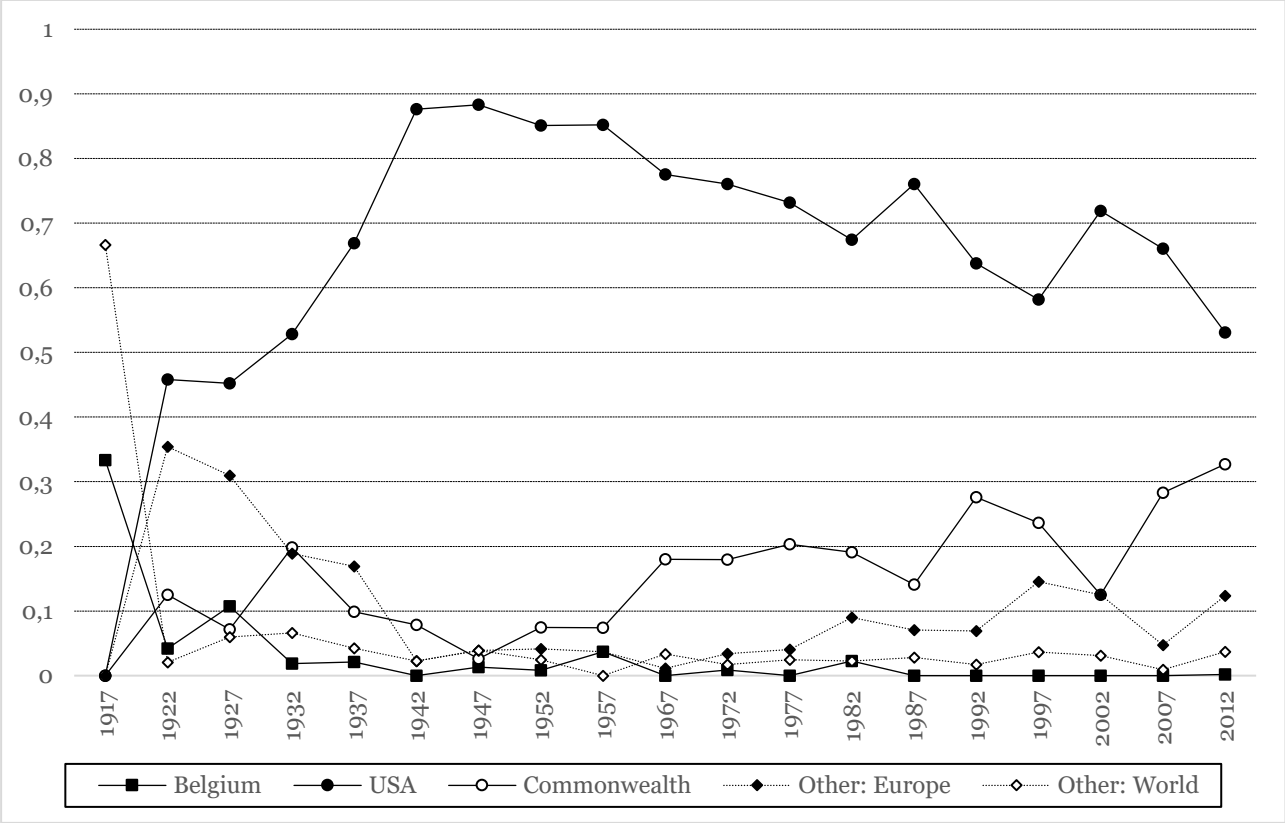

Figure 2: Country of institutional affiliation of the first author of the articles published in Isis, 1913-2013

Figure 2 should be read in conjunction with Figure 1. Figure 2 provides an overview of the countries of institutional affiliation of the first author of the articles published in *Isis*. It displays changes in the geographical distribution of the members of the scientific community who have been able to publish in *Isis*. After *Isis* had left Europe for the United States (in World War I), the number of US American contributions increased strongly. Around the middle of the twentieth century, almost 90% of the authors were affiliated with US American institutions. Of course, this shift also reflected practical difficulties caused by World War II, such as the problems of obtaining publishable material from the occupied territories. But after World War II, the US American dominance decreased only gradually. At present, two thirds of the authors list institutional addresses within the United States. Concomitant with the rise of the number of US contributions, there was a sharp decrease in the number of European contributions in the first decades after *Isis'* foundation. Only from the 1970s onwards have European authors again become more visible on the pages of *Isis*. But from the middle of the twentieth century onwards, most non-US contributions have come from authors from other English-

speaking countries, such as Canada, England and Australia. Seen in this light, *Isis* has not only become an English-language journal, but also a journal of the English-language world.[5]

In the field of history and sociology of science, *Isis* is, arguably, the oldest journal that still appears. Its leading role in the field has never been disputed. According to its official websites, it remains "the widest circulation journal in the history of science."[6] As the analyses show, however, *Isis* is also a journal that heavily features research conducted at US American universities and research institutions. The post-war expansion of this field of study has also reinforced the scientific authority of communication media and individuals with US American credentials. For Sarton, who himself moved from Belgium to the USA (Harvard University), "the scientific needs of mankind are served best by the monopoly or quasi-monopoly of a few languages" (*Isis* 1948: 14). However, our analyses also indicate that not all of mankind is able to actively participate in the disciplinary communication in an English-language *Isis* to the same degree. Thus, despite good intentions, the norm of communalism does not always go hand in hand with internationalism.

International Sociology

The First World War had a negative impact on many experiments of internationalization. In spite of some new initiatives, such as the creation of the League of Nations (1920), the interwar years are mostly characterized as a period of national closure and mounting international hostilities. The early history of *Isis* provides an illustration of this period of 'involution'. A renewed expansion of international scholarly associations only occurred after the Second World War.

In the years after the Second World War, UNESCO, an intergovernmental organization founded in November 1945, began to play an important role in "developing the international mind" (UNESCO 1946: 14, cited in Rangil 2013: 67). It initiated several international disciplinary associations, including the International Economic Association (IEA), the International Sociological Association (ISA), and the International Political Science Association (IPSA) (see Platt 1998; Coakley and Trent 2000). In 1949, UNESCO also started the publication of its *International Social Science Bulletin*.

UNESCO and its international associations mimicked the general UN model of representation. ISA and its sister associations initially made use of a system of national association membership, in which all nations were treated equally. The international associations aimed to incorporate all nations into the international social science community; their legitimacy was thought to rest on both national diversity and equality of representation. At that time, UNESCO's hope was that this model of internationalization would advance social science just as much as internationalized social science would advance the international community (Selcer 2009; Rangil 2013; Duedahl 2016).

To assure a balanced national representation in these international associations, efforts were undertaken to widen membership. In the period around 1950, UNESCO stimulated the establishment of national disciplinary societies. For instance, the *Österreichische Gesellschaft für Soziologie,* the *Sociedad Mexicana de Sociologia* and the *Société Belge de Sociologie* were founded in 1950, while the *British Sociological Association* was set up in 1951. All of these national associations soon joined the ISA (de Bie, 1986; Platt 1998: 17), thus there was a strong interdependence between the national and

[5] Recently the editorship of *Isis* has returned to the Old Continent; the Dutch historian of science Floris Cohen assumed office in 2015. We might therefore expect an increase in the share of European (and especially of Dutch) publications in *Isis* in the near future.

[6] See http://www.journals.uchicago.edu/journals/isis/about.

international level. International organizations and associations, such as UNESCO and the ISA, are both the product of nation states and serve to justify the existence of the national level.

In the ISA, general individual membership was introduced in 1970. This organizational change was explicitly intended to further the mission of internationalization. Interestingly, early opposition to the introduction of individual membership also referred to the ideal of internationalism; it rested largely on opposition to the 'skew' that would inevitably come given the size of the US intellectual community (Platt 1998). As compensation for practical inequalities, differential membership subscriptions and conference attendance fees have since been provided. Along the same lines, much emphasis is still placed on the development of a representative, international sociology. According to the current mission statement on its website, the "goal of the ISA is to represent sociologists everywhere, regardless of their school of thought, scientific approaches or ideological opinion, and to advance sociological knowledge throughout the world".[7] It adds that its members, over 5000 in total, presently come from 126 countries. But how does it enact the norm of a balanced national representation in its communication and publication practices?

To stimulate communication within the international community of sociologists, the ISA now publishes two widely distributed journals: *Current Sociology* (*CS*) and *International Sociology* (*IS*). *CS* was first published in 1952. It was initially a bibliographic journal that contained overviews of sociological publications from all over the world. It later also published trend reports, analyses of particular topics or of the state of sociology in particular nation states or regions, and papers from ISA conferences. Only after the ISA World Congress of 1998 in Montreal did it adopt a submission-driven peer-reviewed format (instead of the older invitation-only system).

The first issue of *IS* was published in March 1986. Fernando Cardoso, the then-president of the ISA, emphasized in a programmatic essay in the first issue that the journal would focus on "international sociological analysis in a specific sense; made by sociologists from diverse cultural traditions and national origins." By launching the new journal, he added, the ISA wanted "to create a new possibility for sociologists across the world to be better acquainted with each other's work" thereby "increasing our knowledge about contemporary societies and sociologies." Further, this should "be done by maintaining a balanced editorial policy and thus publish authors from diverse regions" (*IS* 1986: 2). His view thus echoes and specifies the broader UNESCO model of a balanced national representation in this international publication forum.

On several occasions the editors of IS also discussed international representation. Martin Albrow, *IS*'s first editor-in-chief, for example, defended an editorial policy of "positive discrimination" with regard to "underrepresented groups" in order to achieve "worldwide accessibility." "Country of origin, age, gender, and to that one might add, region, language, type of institutional affiliation, are relevant background factors in editorial decision-making." He was also proud to be able to say that the papers submitted within the journal's first year came from 35 different countries, while those published came from 13 countries (*IS* 1987: 4-6; see also *CS* 1991: 101-118). At present, the journal still has the goal or mandate to maintain "high scientific standards, while reaching out to all corners of outstanding scholarship around the globe" (*IS* 2015: 342). Ensuring a balanced national and geographical representation remains a crucial ambition of *IS*'s editors. As gatekeepers, they look for ways to deal with the tension between spreading a disciplined way of knowing (and its "high scientific standards") and representing the diversity of views from "around the globe."

[7] See http://www.isa-sociology.org/.

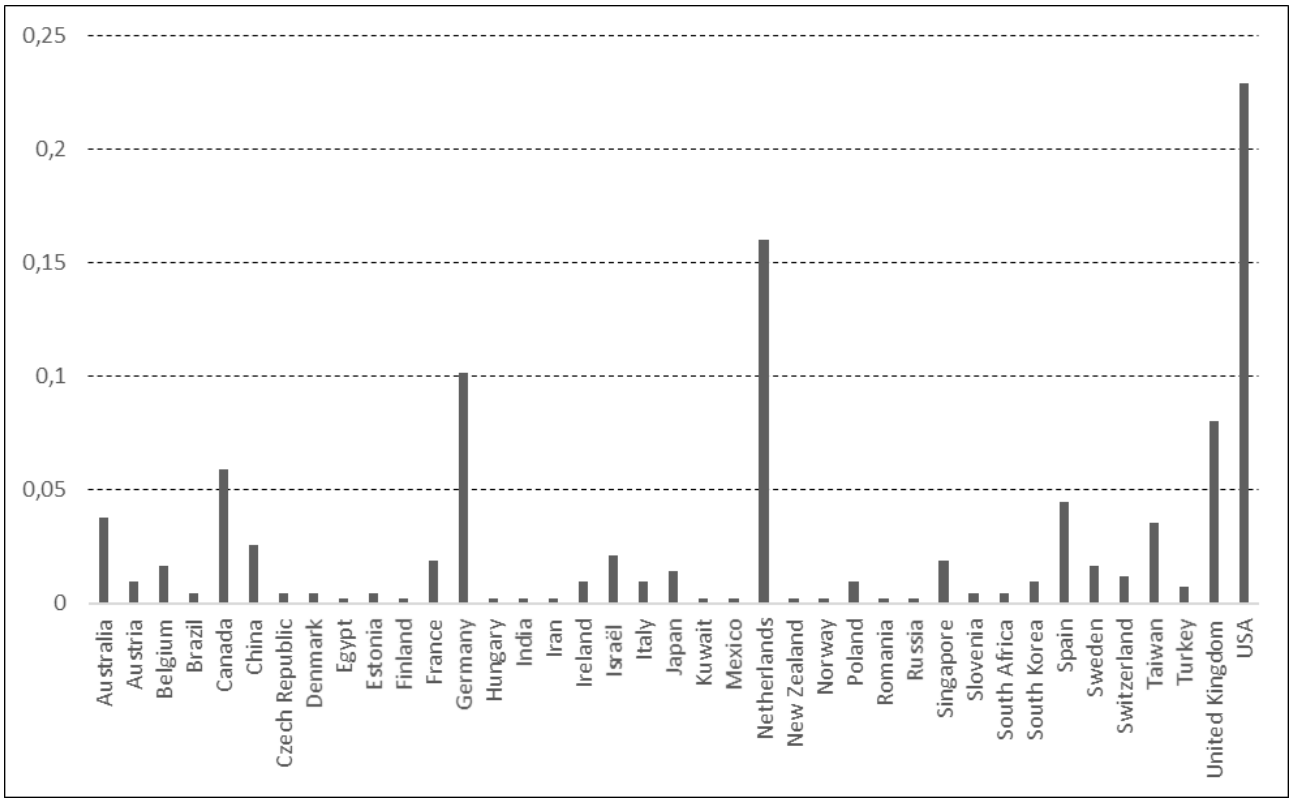

Figure 3: Country of institutional affiliation of the authors who published in *International Sociology*, 2003-2013

IS has always only published English articles – although submissions are possible in other languages and abstracts are translated into French and Spanish (as the two other official languages of the ISA). Figure 3 displays the geographical distribution of the authors whose work was published between 2003 and 2015 in *IS*: almost 23% of the authors worked in an US American institution, 16% worked in the Netherlands, 10% in Germany, 8% in the United Kingdom, but less than 1% in countries such as Brazil, India, Iran, Poland, Russia, Romania, etc. These figures need to be interpreted carefully, as there is no available list of the total number of individuals by nation state who might be able to publish in journals such as *IS*. In terms of the ideal of equal representation, however, an imbalance can easily be observed. While some progress has been made since Martin Albrow published his overview, it is evident that the geographical distribution of authorship remains far from equal.[8]

To contextualize these data, it may also be added that the disproportionally high participation of Dutch authors occurred in a period in which *IS* was edited in Amsterdam (2004-2010). Between 1996 and 2003, five articles were (co-)authored by Dutch scholars. Between 2004 and 2012, however, 37 articles were (co-)written by authors who worked at a Dutch institution. Editorial teams may bring their own national agendas, too!

[8] For a recent discussion of the inequality in attendance at the ISA conferences, see Dubrow et al. (2018). To make further sense of the internationalization of *IS*, it might also be helpful to take the geography of authorship of the journals indexed by Web of Science into account. Of all the sociology articles included in the 2016 edition of Web of Science, 3.3% of the authors was based in Australia, 0.8% in Belgium, 0.3% in Brazil, 4.9% in Canada, 0.1% in Chile, 0.9% in China, 2.9% in Germany, 0.4% in India, 1.9% in the Netherlands, 1.1% in Spain, 10.0% in the UK, 34.3% in the USA, 0.02 % in Vietnam, and so on.

To obtain a complementary, citation-oriented view of the internationality of international sociology, figure 4 visualizes the ties of *IS* to other journals. To draw this network, the relatedness data of the Social Sciences Edition of the Journal Citation Reports (Web of Science) were used.[9] Both cited and citing data (in-degrees and out-degrees) were considered. To level out annual fluctuations, the average scores for all available years were calculated. Journals that happened to have, on average, less than one relation to *IS* per year were excluded from the analysis.

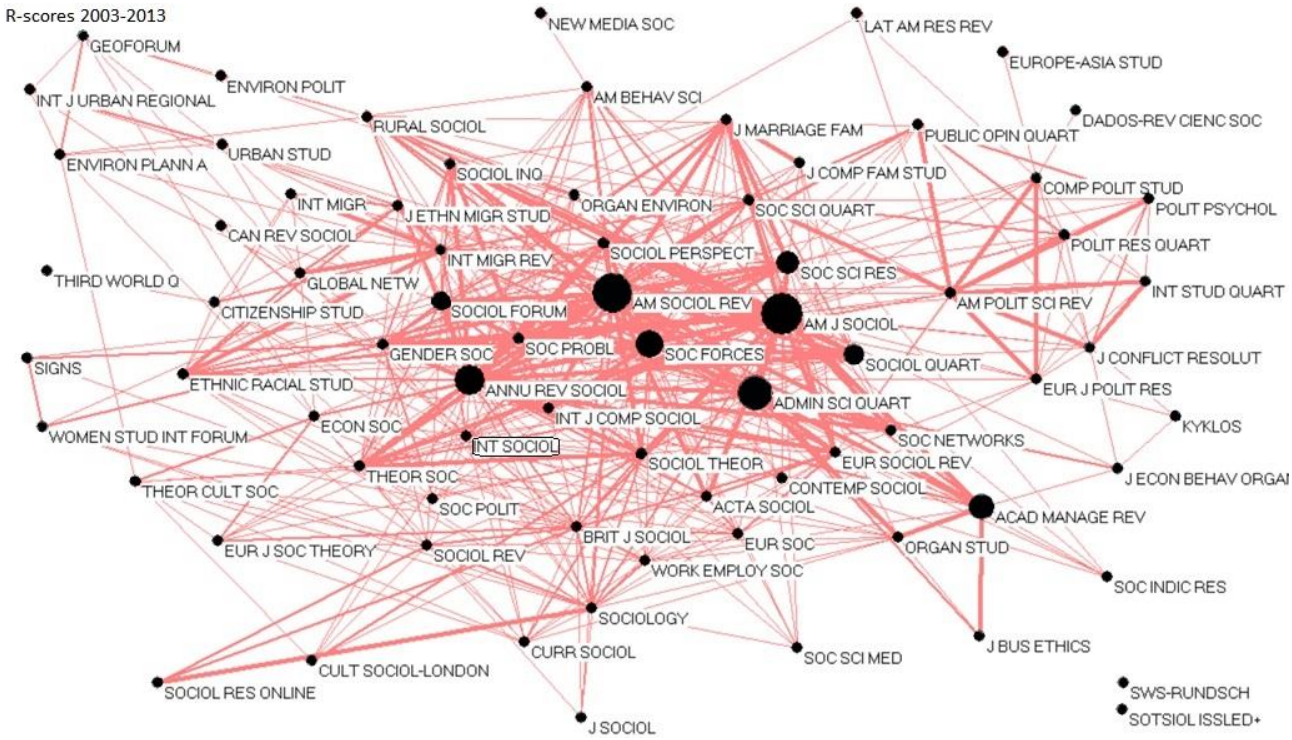

Figure 4: The citation network of International Sociology, 2003-2013

In figure 4, the size of the dots indicates the importance (or centrality) of the journals in the citation network. The thicker lines in these graphs stand for stronger connections. With 74 journals, *IS'* network is relatively large (see Vandermoere and Vanderstraeten 2012; Vanderstraeten, Vandermoere and Hermans 2016). Considering the journals to which *IS* is strongly connected, in the field of sociology we mainly see America-based journals, such as *American Sociological Review*, *American Journal of Sociology*, *Social Forces*, *Sociological Quarterly*, *Annual Review of Sociology*, *Sociological Perspectives*, *Sociological Theory*, and *Theory and Society*. Together with the Australian *Journal of Sociology* and the *Canadian Review of Sociology*, the main British journals in the field appear in the margins of the network: *British Journal of Sociology*, *The Sociological Review*, *Sociology*, and *Work, Employment & Society*. There are also some other European journals, such as *Acta Soci-*

[9] The relatedness data express the relationship R between two journals x and y by: $R_{x>y} = C_{x>y} * 10^6 / (P_y * Rf_x)$ where $C_{x>y}$ refers to the number of citations from the citing journal x to the cited journal y, P_y refers to the total number of papers published in journal y, and Rf_x refers to the number of references cited in journal x. For an exploratory discussion of the use of this dataset, see Vandermoere and Vanderstraeten (2012). In the field of scientometrics, pleas for the development of this kind of topographies of scientific disciplines are age-old (e.g. de Solla Price 1963: 515).

ological, *European Societies*, *European Journal of Social Theory*, and *European Sociological Review*. Most of the other journals in *IS'* network focus on sub-disciplinary specializations, including political, environmental, urban, and ethnic issues. Also remarkable is that the other ISA-journal, *Current Sociology*, is only modestly connected with *IS*.

Thus the citation environment of *IS* is rather diffuse. But it is also characterized by global patterns of domination. While *IS* may well have achieved some success in publishing articles from authors from all over the world, this broader geographical basis goes along with a highly limited knowledge base. US American journals clearly dominate *IS'* network. In this sense, *IS* is important for the diffusion of knowledge from the centre to the peripheries.[10] Or stated more generally: international institutions, such as the ISA and its journals, have contributed to more regular transnational links and exchanges, but they simultaneously also contribute to the formation of an international disciplinary canon and an international hierarchy, dominated by scholars and scholarship from the USA.

Of course, it can be argued that this conclusion is an artefact of the data and the database used. Our database only includes part of the scientific literature, viz. articles in journals included in the Journal Citation Reports of WoS. Biases in this database – WoS favours journals over books, and English language journals over journals in other languages – have thus been reproduced in our analyses. But it should not be overlooked that publications in journals included in this database have become the canonical form of scholarly communication in a wide range of countries and a wide variety of disciplinary specializations, including social-scientific research. Publications in these journals have become the yardstick with which scholarly reputation is commonly measured. The journals indexed by WoS – which are time and again presented as "the world's leading journals" – provide a powerful vision of what internationally accepted 'quality' has come to mean in research. In this sense, we would like to argue that the foregoing network visualizations and analyses shed light on what is considered to be the 'relevant' citation environment in contemporary international social science.

Conclusion

We do not want to blame the editors of international social-scientific journals for the observed inequalities. To a large degree, most academic journals remain supply-driven. Editors are dependent on submissions and have limited possibilities to intervene in the production process. There might well be significant "positive discrimination" with regard to "underrepresented groups." What we would like to point out, however, are some of the relevant differences between scholarly norms and their "enactments." Our analyses of the publication practices in *Isis* and *International Sociology* point to the skewed global orientation in/of these journals, despite their outspoken internationalist ideals and ambitions.

Some of the ensuing legitimacy problems for international social science are directly connected with the differences between the norms and their enactments. The organizational model of international scholarly associations, such as the International Sociological Association, is predicated on the spread and strengthening of national associations. These associations claimed and claim to represent diverse national points of view, hence their legitimacy seems to rest on national diversity and on equality of representation – even when the degree of support for, and institutionalization of, the social

[10] It might be added that the average out-degree of the journal is almost twice as high as the average in-degree in the period under study (20.83 vs. 10.88). In other words, publications in IS have been cited more often in other WoS-indexed journals than the other way around during our timeframe.

sciences differs markedly at the international level. Although scholarly associations and their journals frequently look for ways to compensate for these practical and institutional inequalities, internationalization in the social sciences clearly looks more like Americanization when we compare the publication practices in international social science journals with their own ideal of equal or balanced national representation.

At the same time, however, the foregoing analyses also shed light on the structural patterns underlying the forms of inequality in international social science. The norm or imperative of communalism has been institutionalized in particular ways. Communalism has been identified with visibility in international journals, with publications and citations in WoS-indexed journals. The specification of this imperative ('publish or perish') has changed the everyday world of scholars in most disciplines and most nation states. Our analyses of the citation environment of *International Sociology* suggest that current publication imperatives lead (potential) authors to 'play it safe.' The hierarchical rankings of journals have become a reality in their own right. Databases such as WoS are no longer only useful to search for information and conduct bibliographic studies; they have also been successful in diffusing their specific selection criteria. To build their argument, 'international' authors prefer to rely on what is considered certified knowledge; publications in high-ranked, America-based journals. Both with and against Merton, it might be asked whether the current communication imperatives are functional within international social science.

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Serendipities
Journal for the Sociology and History of the Social Sciences

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INTERVIEW

On Being the Editor of AJS

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Abstract

Andrew Abbott, editor of the American Journal of Sociology from 2000 to 2016, explains in this interview the role of an editor, discusses criticisms against flagship journals, relativizes the influence of editors, praises colleagues participating in the peer review process and offers a look at the back-stage of academic publishing.

Keywords

American Journal of Sociology; editor; peer review; metrics; academic publishing

Note

Serendipities approached Andrew Abbott and asked him whether he would be willing to share his experiences as the long-term editor of one of the flagships of sociology, the American Journal of Sociology. Abbott agreed, but suggested to do the interview in written format. The following text are Abbott's answers and the questions in italics are from Christian Fleck.

Introduction

The questions asked range across a variety of areas, and so it is best to start with an overview of the aims and everyday processes of the journal during my tenure. It was written in 2016 and thus reflects my whole experience. It is also in the present tense, although I am no longer editor. That is because putting it into the past might make it seem that "everything has changed," even though I would guess that things are much the same under my successor, Elisabeth Clemens. But the running of the journal is the editor's prerogative, and I cannot speak for her, only for myself.

After this opening section, I will answer whichever of your specific questions has not already been answered.

The AJS as of January 2016

1. The aim of the journal

The AJS has two aims. First, it aims to publish top quality work from across the subcommunities of the discipline: work that brings new theory, new method, or substantial new data, or that makes a decisive intervention in an ongoing debate. Second, it hopes and expects that that work will be writt

written in a way that will have meaning for a more general sociological reader. The journal, like the department that runs it, believes that there *is* a general reader; that sociology is a discipline, for all its diversity, and indeed that part of the discipline's discipline, so to speak, is its ability to embrace and to find interesting many ways of thinking about the social world. The job of the editor is to choreograph and administer a selection process that produces a journal as full as possible of papers that met those two standards - top quality in their subfield and substantial interest for a general sociological reader.

That the journal must cover the range of the discipline means that our standards are not simply an article-by-article matter, but that we must also use a decision process that will produce a journal that contains many different kinds of work and areas of work. This too is something we bear in mind continuously. I often say that the discipline is an archipelago of islands like the Philippines, with big islands like stratification, gender, and medical sociology, medium-sized ones like sociology of religion or sociology of science and little atolls like conversational analysis. The journal should have a couple of papers a year from each of the big islands, one a year from the mid-size ones, and a paper every now and then from the little atolls. The Editorial Board[1] has to bring about that representative sample even though people on the different islands submit at different rates, even though the various islands disagree wildly about the nature of good work, and, most important, even though some of the big islands have their own journals, often as prestigious or more prestigious (in the subfield) than we are: gender has *Signs* and *Gender and Society*, medical sociology has JHSB, organizations has ASQ, demography has *Demography*, and so on. Moreover, we have to make this discipline-wide representativeness happen through a process of individual decisions on papers one-at-a-time. We can't pile up papers for half a year and then sit with a spread sheet and decide which combination of a whole set of possible papers would work best. We simply have to constantly bear representativeness in mind, alongside top quality in subfield and substantial interest to the general reader.

2. The AJS as a social structure

The "we" of the journal involves three major constituencies: submitting authors, reviewers, and readers. Of these three, the readers – both the general readers and the experts – are the most important. If a decision comes down to a zero-sum situation and we must displease one or another of these constituencies, it is always the readers' interest that must predominate. The journal exists for the readers first and foremost.

The reader constraint is a relatively general one, however, and we have the referees to help us estimate it. So in practice we who run the journal are usually more focused on our obligations to the immediate constituencies - authors and referees.

To authors we have an obligation of fair and timely process. And secondarily we have the obligation to teach some of them some things they may have no other way to learn. Not everyone has colleagues near at hand to read his or her work, and so we must take seriously our de facto professional education function. Our referees often write excellent "teaching reviews," and that is an important, if unsung, professional activity. We get a surprising number of thank-you notes from rejected authors who have found the reviews very valuable. Indeed, our role in professional education and collegiality is one reason we hesitate to increase the number of desk rejects.

[1] Usually about five or six total: The editor, two or three other faculty members, and two senior PhD students in their fourth or fifth years of the Chicago sociology program. CF.

To reviewers/referees we have an obligation first to take their views seriously and, second, not to overburden them. (This second obligation to referees obviously conflicts with the professional education obligation to authors, as we know well.) Our attempt to be broadly representative means that we are often selecting different reviewers to handle different aspects of a paper – methods, general interest, theoretical solidity, etc. So we often must make decisions based on combinations of reviewers with different expertises, interests, and agendas. Doing this in a way that is fair to all the reviewers concerned is an important challenge in maintaining the AJS reviewer pool.

3. Process

The AJS process is straightforward. An initial vetting is done by the student manuscript board. We have to check papers for the authors' current and PhD institutions (so we can avoid getting referees who know them and could be biased one way or the other). We have to establish a list of potential referees. Sadly, we have to check for duplication of prior work; we can no longer be sure that all authors will police themselves on this matter.

Once the referees are proposed and accepted or modified by the Managing Editor and myself, the requests for reviews go out. Today, most papers will require multiple new proposed reviewers as they go along. Prior to electronic solicitation, AJS got 3 reviewers for four requests 80% of the time. That figure is now down to 40% of the time, and for 25% of the papers we are eventually asking 7 or more people to get 3 reviews. And reviewers themselves are taking longer. The office spends a lot of time chasing late reviews.

When three reviews are in, the paper comes up for decision. AJS is one of the last major academic journals in existence that has a weekly board meeting – 9AM to noon nearly every Wednesday of the year. The faculty and student Associate Editors sit around the table. I chair the meeting, in which we go over the reviews and the paper. Often we get into elaborate discussions of methodological issues or interpretive questions. Often we use our sense of a reviewer's prior reviews to interpret ambiguities in the current review. If the paper is a revised one, we carefully go over the revision memo to make sure the prior referees' questions were addressed.

Sometimes there are so many questions that we postpone decision: perhaps we need another review – we'll come up with names on the spot. Or perhaps we need to lean on a delinquent referee who is covering a crucial angle. Or perhaps one of us will take the paper home and write yet another review. But if we are ready, I just start calling on people in a random order around the table, asking them to contribute their views. Consensus is usually quick. When you work together week after week, and in some cases year after year, you get a real sense of each other's views. I myself need to cast a deciding vote only rarely. The Board's decisions are indeed Board decisions.

4. Myths and Problems

As this discussion makes clear, my supposed editorial power is a myth. In the first place, the main determinant of what appears in AJS is submission. If papers aren't submitted, they won't be published. For most areas that don't appear much in AJS, the reason is that their practitioners choose to take the best papers elsewhere. Since we in turn do not want to publish less than the best, the result is to make the field underrepresented in AJS. My taste has nothing to do with it.

In the second place, as I have just said, the Board makes the decisions and is surprisingly consensual. Any one of us can and does challenge that consensus from time and time, and sometimes persuades

the rest. But we generally read dissensus as requiring more information and cast about for new referees or adjudicators.

There could still be bias in such a system via preemptive decision (by authors) not to submit because I am editor, on the *belief* that I must exercise some biased authority, or that my previous discussion of the Board's strong consensus is a matter of misrepresentation or bad faith. But the evidence that I do not control the content is easily found in the journal's pages. One of my most cited papers is a critique of what I called "general linear reality," the ontological assumptions underneath standard sociological quantitative methods. Yet dozens of excellent papers in the GLR tradition have appeared in AJS during my editorship, as well they should. Myself, as a scholar, I have reservations about a considerable amount of what is published in the AJS. But it's not the *Abbott Journal of Sociology*, it's the *American Journal of Sociology*. What matters are 1. the views of the experts in the paper's subdiscipline, 2. the issues of bias that can arise in those views (examples of biased reviews are "not in my backyard" reviews, "I said this last year" reviews, "tempest in a teapot" reviews, "I love this because it does the kind of stuff I do" reviews, etc., etc.), and 3. the responses of the Board as it attempts to channel the general sociological reader. My own personal thoughts about the work are irrelevant.

So I myself do not try to read and judge all the papers from scratch. Our job is to pick good referees and then carefully to read the reviews and the paper in the light of the reviews, knowing what we know about the habits of the reviewer, the seeming quality of the paper, the styles of the subcommunity, and so on. As I noted, one of us may take a difficult paper home to read it carefully and write another review for the board. We may have to run some particular statistical worries by a colleague in statistics, or run a political sociology paper by a colleague in political science. The idea is that the paper has to be excellent in the terms of its own subcommunity *and* to have something interesting for the general reader. And our job is to make that happen.

Another myth is that Chicago people have an advantage at AJS. I have looked at the last 8 years' worth of data, both in terms of current location (as faculty or student), and in terms of location of PhD. In terms of current location of authors, the Chicago people are eighth in total submissions in this period and fifth in success percentage, among departments submitting twenty or more papers. In terms of authors' site of PhD or expected PhD, Chicago is fourth in terms of total submissions in this period and fourth in success percentage, among departments submitting at least thirty papers. (The data are not quite as good on PhD institution, since we lack PhD departments for about 20% of authors, so I compare a slightly smaller group. Note also that there is no feasible way to control for the different sizes of the PhD pools from various institutions, which must shape the submission rates to some extent.) On neither success indicator are the "Chicago and above" departments (five in the first case, four in the second) substantially above other departments. A few changed decisions on particular papers could reorder these lists a good deal. In sum, there is no convincing evidence of any Chicago effect, either by present affiliation or by site of PhD. Chicago looks the same as comparable departments, both in terms of submissions and success rate.

So that is the basic story about the AJS. Let me now consider the particular questions you have asked.

> *After Albion Small at the very beginning of AJS your term as editor has been the second longest. Since you did research on the history of AJS in the past, could you give us an idea what have been the major differences you experienced in your role as an editor compared with what you did know from researching the history of AJS?*

The history tells us that the journal has had the same general structure since the great demographic transition of American sociology (and American academia more broadly) in the late 1970s. With the end of American academic expansion, American faculty hiring changed from a sellers' to a buyers' market. So vitas had to look better. The gradual result was a steady increase in publication per person per year. This was not generated by wonderful new techniques, which came at the same rate as before, or by intellectual revolutions, which also came with great regularity. It mostly derived from the market change.

It is true that a variety of forces in the 1960s and 1970s drew a particularly talented generation into the discipline, which might explain some of this increase in publication. But entry to the discipline (and academia in general) fell like a stone in the 1980s, once the news of the newly tight market spread. It is also true that technical changes (the personal computer) made writing articles much easier from the early 1980s onward, as did canned statistics (from mid 1970s), and similar developments. These technical changes reduced barriers to entry and contributed to the publication expansion. But I still think that the main factor was the switch to a buyers' market for faculty. As of the 1950s, the modal US PhD was publishing two or three papers in a lifetime.

We can also infer that the quality of the average submitted paper probably fell, since the pool of people writing articles had already been submitting their best work, and expansion inevitably meant publishing things they wouldn't have bothered to publish under the earlier regime. Only the "strong 1960s generation" factor militated against this decline in quality, while the market pressure and the lowered barriers to entry clearly favored it. All AJS editors since Charles Bidwell in the 1970s have therefore faced the same problems - lots of papers, of uneven quality, and a seeming dearth of excellence.

There are however some problems that have steadily worsened in the period from the 1970s to now (2018): declining disciplinary consensus, increasingly explicit political arguments in submissions, increasing specialization within subfields, competition from more and more journals (particularly, excellent specialty journals), differing international genres for article-writing, and, eventually, a loss of the professional ethics that had guaranteed that we didn't have to check for double publication, self-plagiarism, and the like. It's also true that the sheer size of the published record today means that journals are probably sometimes publishing things that had been done before – ten or twenty years ago – with no one (authors, referees, editors) being aware of that earlier publication.

Some of these worsening problems are things I did not expect. I knew about the overpublication and the quality decline. I knew about specialization, competing journals, and international differences. But I did not expect politicization to become so strong so quickly, and I certainly did not expect authors to try to publish in AJS articles that, in effect, changed only a few variables (and sometimes the theoretical framework) from articles they had already published in ASR or elsewhere.

> *I am sure that in the life of an editor there are episodes of excitement and success but also periods of frustration and anger. Could you let us know what have been your best and worst experiences as editor?*

The best experience was getting notes from authors thanking us for the quality of the reviews and the editorial process. Of course, these were most touching when they came from authors who had been rejected.

Another good moment was saying thanks. For some years, I sent a hand-signed thank-you note to every single reviewer. That was a happy time, too, for it made me realize how wonderful was the

community that supported the journal. And saying that the Journal was a wonderful community gives me another happy opportunity to say thanks. Most of the work of making AJS into a community of editors, students, and referees was not done by me, but by the Managing Editor, Susan Allan. The AJS of the last thirty years is unthinkable without the talent, dedication, and personal warmth of this wonderful human being. She has been the continuity and the glue of AJS. The Journal's excellence over this period owes more to her than to anyone else.

The worst experience? Probably the one or two flame emails I got from people who resented rejections and who didn't wait long enough to cool off. But such emails were very few (less than five). That's because in fact the AJS process is scrupulously fair, and people know that. Out of the thousands of people whose work AJS rejected, there are only two people, to my knowledge, who simply talked speaking to me afterwards. So there weren't a lot of bad moments.

What are the two or three papers published during your tenure you liked most? And why?

This is an invidious question, and I wouldn't answer it even if I had favorites. But in fact, there is no answer. It wasn't my job to like papers or not like them. It was my job to make fair decisions in line with the Journal's aims and its rigorous professionalism. Therefore, Andrew Abbott the scholar and Andrew Abbott the editor had very little to do with each other. The editor was very concerned to keep the scholar out of the editing process completely, because the scholar has strong and quite particular positions about many things in the discipline. There had to be complete separation between the two personae. That meant paying much more attention to referees than to my own thinking, and it meant invoking my own analysis of a paper only when the referees left us in a real muddle. In practice, the only way to shut up my scholarly self was to curtail my own reading to a considerable degree, and I did that.

In the end, the result is that there are many fine papers in AJS, of many different kinds, and I'm happy to have been able to publish them.

In preparation for this interview, I talked to several people from different corners of the world, differently placed in the status hierarchy etc. Surprisingly enough all of them did not know how a journal like AJS functions nowadays. The one thing outsiders probably overestimate most is the degree of influences and direction an editor has. Am I right that it is wrong to suppose that you made decisions about acceptance or decline of papers at any time? But, why then acting as an editor when the chance to make a difference are low?

Yes, as the discussion above says, the editor actually has very little power. And, indeed, by today's professional ethics, it would be morally wrong for an editor to exercise any power that he or she did have. The days of journal editors' "making a difference" by choosing work to suit an agenda are over, particularly given the current generation's near-obsession with fairness. That kind of "making a difference" has moved to edited volumes and special issues.

Why then be a journal editor? Because someone in the department had to do it and most of my colleagues didn't want to. So I edited the journal even though it was a lot of work, a lot of emotional responsibility, and absorbed attention I could and perhaps should have given to scholarship. (I did manage to publish six books while editing the journal, so it can't have done *that* much damage to my productivity.) As I said, I edited the journal because it's a duty that has to be done by someone in my department, and I found it a mode of doing my departmental service that suited my inclinations and talents. It served both the department and me (and, I hope, the discipline) to have a long tenure, which in turn meant steady policies, predictable processes, and so on at AJS.

Could you describe in some detail the routines of AJS? Many papers come in, who is doing what with them? Is anyone of the editors reading all these pages or do you assign reviewers instead? After getting enough reviews back, you and your associates do read them and what's next?

I explained the AJS process above. With about 500 submitted new MS per year, and five members of the sitting editorial board, it is obvious that the board itself does not read all the papers. Given the diversity of the submissions, five people could not in any case cover the possible types of papers. This is what consulting editors are for, at AJS as elsewhere. The problem of such a journal is to maximize the fairness of the decision, given the general aims of the journal, as noted above. It is by no means necessarily the case that this could or would be done by focusing the editors' time on the reading of all papers. Rather, one aims to triage the papers into the obviously strong, the obviously impossible, and the not quickly decidable. One focuses all one's attention on the last category. Truly excellent papers would be a pleasure for us all to read, but in fact they get less attention than those on the edge of decision. Only in a very different submission environment could one focus on improving papers that are already excellent.

All the same, it should be noted that many authors of excellent papers are unwilling to believe that those papers can be improved in any way. Indeed, I often feel that way myself, when I first read suggestions in reviews of my own work. So it's not clear that extra attention to already-excellent papers would be a useful deployment of editorial effort for AJS.

To be sure, however, my own experience persuades me that one's initial reaction ("nothing needs to be improved here") is always wrong. Referees' suggestions have inevitably proved helpful once one has fought down one's egotism and have paid attention to them. They aren't necessarily helpful in the sense that one does what referees want, but they are helpful in that they force one to see what can and should be improved, perhaps in a quite different way. I have a feeling that the authors of many already-excellent AJS submissions ended up behaving this way – not necessarily doing what referees recommended, but improving the paper in other and important ways, indirectly suggested by referee comments.

Some people argue that peer review produces too much mainstreaming of the publications? Do you remember any "heterodox" paper you thought worth to be printed which became declined according to the consensus of the reviewers? Or did it happen that you overruled them?

We published some funky things over the years. But at least in my time, we never published an article that *only* we (that is, only the sitting Editorial Board of five or six) thought was excellent. For us to publish an article, there *had* to be one outsider – one referee – who thought the paper was really excellent.

One therefore provided for funkiness by looking for reviewers who had broad tastes. The late Arthur Stinchcombe, for example, was a man who could find surprising virtues in unusual papers. Often, we would send an unusual paper to him or someone like him - somebody who had a feel for the unusual. Not all unusual things are great, after all; some of them are just wrong. So you need reviewers who can judge such papers. But on such papers, you also still need some mainstream people who will give you the more general expert reader's reaction.

But the bottom line is that we never moved ahead without having one strong outside reviewer who was completely convinced of a paper's importance.

During your term the academic publishing business experiences tremendous changes: digitalization on the one hand, metrics on the other. I guess AJS is still making enough money but most

probably only because of the library subscriptions. However, does it make sense to print journal issues any longer? The alternative to go online would make it possible to expand the size of any journal, so the questions is: would an online only AJS publishing double number of articles losing in quality?

Whether or not to keep the journal in a print version is not my business. Subscriptions, in any case, do not have to be print-based.

Obviously, expanding the journal because it became purely an online document would be more or less costless. The reason against doing that expansion is simple. The journal is basically selling selectivity – its product is "excellent selection," not "the papers in AJS." Its mantra is "if you have time to read only six articles in the next two months, then here are six very good possibilities." If we said, "Here are twelve possibilities" or "here are twenty-four possibilities," then people would go to other journals, because they don't have that much time to read. The whole point of journals and peer review is to save readers' time by hierarchizing the things that could be read.

And indeed, the main problem of the digital environment is the lack of such quality signals, which have disappeared because while the costs of print and distribution used to justify the centrality of strict and intensive peer review, nothing real has replaced that justification. Indeed, now the online environment is full of bogus quality signals – citation level being the worst of them all. By contrast, AJS is about delivering a really careful quality signal based on a model peer review process. To move away from that would be crazy.

Let me come back to all those metrics, like impact factor, altmetrics, download statistics etc. In which way did you care about them during your tenure?

As my comment about citations suggests, I paid no attention to metrics at all. I have worked extensively with citation statistics for more than twenty years. I know what they are good for and what they are not good for. I have, indeed, written a careful analysis of all the articles in one year that cited my own most heavily cited book – an analysis which told me that many of the book's citers have no idea what it actually says. Most important, I know that citations are not a linearly ordered system of quality. Most of "how much something is cited" is determined by its subfield, its ability to rephrase the obvious, its bandwagon status, and so on.

The only statistic I found interesting was the fact that the vast majority of hits on AJS articles in JSTOR result in a print. That is, the move to "digital" is not a move to "digital" at all. It's merely increasing the use of paper and hiding that increase. On the good side, at least the fact of mass printing shows that people recognize that "reading online" is not real reading.

A recent study on coercive citation practices (Wilhite Allen W. and Eric A. Fong, 2012. "Coercive Citation in Academic Publishing", Science, 335: 542-543) did not mention AJS as coercer but many leading journals from different social sciences. What have been your experiences with regard to wishes from reviewer to quote their publications in papers they read for AJS?

"Coercive citation" means two things. First (and worse) it can mean that a journal forces authors to cite articles which have appeared in that journal. The AJS does not do this. There is no pressure to cite AJS articles. It would be unethical to make such pressure. A discipline that allows such things is a discipline that will deservedly degenerate into rubbish. There are journals that are doing this kind of coercive citation, of course. But I will stand by my prediction: any field in which this becomes a widespread practice will die as a serious intellectual enterprise.

Second, coercive citation can mean that reviewers suggest that they themselves be cited. Yes, reviewers do this. It is quite obvious to anyone who reads many reviews. (I read about 17,900 reviews as editor of AJS). At one point near the end of my tenure, I did an analysis of many years of AJS reviews, and found that IF a review contained the string "(19" or "(20", then the chances were about 1 in 3 that the review also contained the reviewer's last name. There are a lot of slippages in such a measure, and one could see them as making it either higher or lower than the "true" figure. But this measure means that it is a reasonable guess that there's a good bit of "suggesting" of such citations going on. It's not overwhelming, but it's there.

I think much of this suggesting follows from the excess of publication. Here's the argument. Most good ideas are quite common – hundreds of people have them. (Merton was underestimating when he talked about "multiples.") In a system where everybody has to publish all the time, more people are going to publish these common ideas, instead of saying (as we did, when wondering whether to publish as graduate students in the 1970s) "Oh, everybody probably knows that." Today, young scholars can't afford to think that.

So I think many referees do genuinely see, in articles they referee, things they have themselves published. It just has to do with the sheer excess of publication, which means that many people in effect publish the same things – commonplace, everyday ideas that are almost truisms. I don't think there's a lot of maliciousness or pushy self-interest here. It's just produced by the death of reading, which means that scholars aren't aware of the hundreds of *other* people who have had "their" idea and, indeed, have published it elsewhere.

The solution to this is of course to stop useless publication. I have no idea how to do that.

> *Could it be that authors submitting to AJS did align their list of references with unneeded quotes from AJS?*

Yes, I suppose people could pad their reference lists with AJS articles. But referees are not going to be misled by seeing a lot of AJS citations. They still read articles critically. If the Board itself were doing all the reading and deciding, there would be more danger from this. But it doesn't do so, so there is less problem than one might expect.

> *By a rough calculation during your term as editors AJS published 600+ research articles. Following your own advice in Digital Paper we sorted them by the frequency of their being quoted by others and the distribution picture is somewhat surprising. Excluding the most recent volumes the range of times cited goes from 0 (5 times) up to 1103 for R.S. Burt's Structural holes and good ideas from 2004. It seems to me that this paper is anything than an original contribution but just a follow up or a personal view back. On the other end there is a paper by two reknowned scholars Rueschemeyer and Mahoney on A neo-utilitarian theory of class? from 2000 which got cited by others only two times. So, do citation say anything in sociology? If so, about what?*

Yes, citations mean a whole bunch of things. The problem is that in any particular case, you don't know which of those things a citation level means.

For example, sometimes citations are high because an article simply gives common sense a clever name. Mark Granovetter's celebrated "embeddedness" article is an example. At the time it was published, many of us couldn't understand why it was published at all. Its content was totally obvious. Any first-year sociology student knew that the economy was embedded in social processes. But the article became a useful "index" to that common idea, and it gets its thousands of citations because of that. I myself have a similar kind of trademark on the word "jurisdiction," it seems. Indeed, you will

find that the most heavily cited articles in sociology are generally articles that are indexed by single words or phrases and are themselves often largely summaries of literatures or works beyond them: embeddedness, toolkit, weak ties, reciprocity, relational sociology, etc.

Or again, sometimes an article is heavily cited because it was the first use of a particular method – Tuma, Hannan, and Groenveld (AJS 1977) on event history methods, for example. Or sometimes an article genuinely does have a kind of new concept – Sampson and Raudenbush's "collective efficacy" article is an example, although there too we see the importance of a good phrase. (All too many articles come to AJS with a phrase in quotes in the abstract; young authors know all about this need to coin a clever phrase.) Or again, sometimes a citation is just used to identify the citing author as a member of this or that school or subgroup. Or an article can be heavily cited simply because it is in a subfield that consists of lots of short articles with many citations. I once did an analysis of all references in sociology in a given year and it turned out that ten per cent of them had appeared in the *Journal of Marriage and the Family*! No wonder family demographers have high citation counts – it happens because of the shape of article they write.

So citations can mean lots and lots of things. In any particular case, however, we don't know which one they do mean. As a result, citations as general measures and as inter-article comparisons are largely meaningless. It would helpful if people would all tell their deans this obvious fact.

> *AJS is one of the flagship journals in sociology worldwide but it is still the American journal of sociology? You explained your policy by calling US sociology an archipelago with some large island and a lot of small once and AJS should mirror this. What's about the international stance of AJS? Do you think people from abroad do have the same chance to get published or why are there differences?*

The problem with internationalizing AJS is that AJS has for the last thirty or so years favored a certain kind or genre of article. This has a rough rhetorical form: 1. big theoretical puzzle, 2. broad review of literature(s) leading to a restructuring or focusing of the theoretical puzzle(s), 3. new data, 4. lengthy analysis (covering lots of details), and 5. attempt to draw general conclusions. Probably two-thirds of AJS articles are like this. It's an American genre – indeed, even in the US, it's just an AJS genre. AJS articles also tend to be large and (to some eyes) pretentious.

The problem is that other countries often favor different genres. The Dutch publish lots of very short, scientistic articles, with very focused and limiting theoretical assumptions of a kind that disappeared thirty years ago even from the quantitative short-article form in the US. By contrast, the French often write articles with multiple methods, aiming to come to a "best understanding" of a given empirical situation, rather than an advance of a (supposed) theoretical trajectory.

I could go on. The fact is that there are (very loose) national styles of article, and "the AJS article" is a style that seems mainly American. During my tenure we published more articles from abroad, but they tended to be cast into the AJS genre.

I don't think there is much to be done about this. There ought to be multiple genres of articles, and it makes more sense for journals to select for a particular genre than for a particular subject matter or methodology or theory or nation. So I feel that the internationalism question is actually hostage to the genre question.

> *Besides having been the editor of one of the leading journals in US sociology over the last couple of years you expressed more than once harsh criticisms against your fellow sociologists in the US.*

Was your role as editor influenced by these opinions and in which way did you try to change the direction of US sociology via AJS?

Yes, Andrew Abbott the scholar and disciplinary figure has made a number of comments about other sociologists and sociological work over the last many years. But that has had nothing to do with what has appeared in AJS. I made no attempt to change US sociology via AJS, but aimed rather to make AJS reflect what was happening in the discipline in terms of areas and methods. The evidence of my success is that AJS is full of things I inveighed against as an individual scholar, but that are common, indeed valued, in the discipline: these are things like (to give them my personal names for them…) unnecessarily complicated methodology, illegitimate ontological assumptions, and internally contradictory theories. And we even published some things that I found repetitive, boring, one-sidedly political, and so on. My job as editor was to provide the discipline with a collection of excellent work in the styles that it – the discipline – chooses to favor. And I did so. My personal preferences as a scholar had next to nothing to do with the journal. Had they done so, it would have looked very different indeed.

Recently German sociologist Wolfgang Streeck commented approvingly on your editorial success when he claimed: "In American sociology we have seen interesting developments taking place in recent years. Many articles in the American Journal of Sociology now have an historical background and understand that the United States of America is not as a matter of course the universal model of modern society. Moreover, the economy, and indeed the political economy, is given an increasing role. Fewer and fewer sociologists today seem to be willing to abide by the peace treaty that Talcott Parsons negotiated with the Harvard economics department, defining the turfs of the two disciplines in such a way that they didn't get into conflict with each other – in effect depriving sociology of some of its most important and most foundational themes." (Interview in Sociologica 2016 (3):14, Doi: 10.2383/85816) Do you agree?

Yes, the journal has a more international flavor than it had forty years ago, and probably even more than fifteen years ago. And that is partly a matter of my policies, and of my predecessors' policies. My own personal contribution to this trend was the Barbara Celarent series (of review articles of old social thought from around the world), which preoccupied me for the last six years that I edited the journal and which tried to open a space for social thought from outside Europe and North America. (Its success is evident in the 120,000 hits a year on the Barbara Celarent papers section of my website.)

But I think most of the new internationalism arises from factors much larger than the journal. First, there is much more international contact among sociologists, simply because of the vast increase in international travel consequent on US airline deregulation, the Schengen system, and a host of other "globalization" factors. As a result, European sociologists come to ASA, PAA, SSHA, and other US sociological venues, and vice versa. Second, EU funding has underwritten a large amount of comparative work, both within Europe and between Europe and the US, and this has produced a new emphasis on comparison that has reduced US isolationism. (This has been particularly noticeable in some of the ISA subgroups, like RC 28.) Third, "American sociology" is now very often not sociology done by "Americans," in the sense of native-born citizens of the United States. American sociology departments are full of East Asians, Europeans, and others who are not native-born citizens. About a quarter of my own department is first generation migrants. If you were to include second and third generation migrants, that figure would be half the department.

In sum, I think internationalization is certainly becoming stronger, but in the main, the Journal's internationalization is just an indicator of larger phenomena. That said, both I and, I am sure, my successor, believe strongly that internationalization is a good thing.

As for the relation with economics, I differ from Streeck's view. Yes, it is true that economic themes have returned to sociology. But this is not really about the ending of any Parsons treaty with the economists. It's really about the peculiar history of economics in the last fifty years.

Economics as of the mid-twentieth century was still a general social science. Figures like Schumpeter and Knight, and, in the following generation, figures like Arrow, Samuelson, and Gerschenkron, were general intellectuals, thinking broadly about all social issues, and viewing economics proper as a way of understanding that part of the social world that was effectively measured by money. But Friedman and Becker – the new Chicago School, as opposed to the Veblen/Knight Chicago School – defined economics not substantively, but purely in terms of method and scientization. Gary Becker didn't really write a treatise about the family as everybody else conceived it, he wrote a treatise about an abstraction that was the family without anything in it that could not be "seen" by economic theory and methods.

Although this move was defined at the time as imperialism (e.g., by Hirschleifer), in practice this "methodization" had the effect of defining economics as a fixed set of techniques, and of defining economists as engineers rather than as intellectuals. It actually left the entire realm of political economy open to sociologists, political scientists, and historians. Becker and company simply didn't care about that area, and their methods had become such that they could – quite literally – no longer see any facts that controverted their view of the world. The result, sadly, has been the intellectual suicide of a noble and brilliant discipline.

But perhaps a better metaphor is that economics has turned into sort of reverse black hole, unable to receive any genuine message from outside itself. Intelligent economists are reduced to reinventing the social psychology of the 1960s and calling it "behavioral economics." Economists win Nobel prizes for "discovering" that people are sometimes irrational, apparently having forgotten that Friedman's classic 1953 paper argued not that everybody was rational, but that absolute rationality, although a nutty assumption, would prove profoundly interesting to make, at least for a while.

As for the sociologists who write about political economy, I myself think they are mired in Marxist analyses that aren't relevant in a world where economies and nations and classes crosscut each other in bewildering new patterns. I agree that great explanatory problems lie ahead. I just don't think that the nineteenth century armamentarium is going to help us much.

If you would do it again, what would you do differently?

At this point, I sometimes regret that I spent so much time editing the journal when I should have been writing my theory book. But on the other hand, editing the journal changed me a good deal. It made me more intellectually tolerant, it forced me to think extensively about the rest of the world (through Barbara), it gave me extensive experience with colleagues. So I wouldn't be the me I am today without having done it. And it is probable that the theory book I would have written - had I had more time to devote to it a decade ago - would not have been as good as the one I can write now, because I am older, which means that I have learned some useful intellectual lessons and that I write under more pressure of time and hence will write a more concise book. Of course, it's also true that I might have wasted the time I spent editing the journal doing other, useless things, so maybe there's nothing to regret at all.

A somewhat bigger regret is that I was unable to publish in the AJS for sixteen years. I write articles that are precisely the kind of article that AJS aims for, and - it should be noted – the kind of article that ASR is generally unwilling to publish (as I know quite well from experience). It undoubtedly hurt my reputation that for over a third of my career I was unable to publish in the sociological journal best suited to my type of work. I have published at least five articles and chapters elsewhere that would have been ideally suited to AJS (although, to be sure, who knows if my colleagues would have published them!), and a lot more people would have seen them in AJS.

But when it's all said and done, I would not have done anything differently. I had a wonderful time editing the journal. I talked to smart colleagues week after week about everything under the sociological sun. I had the friendship and support of a wonderful managing editor, Susan Allan. I got to read the work of hundreds of authors and of thousands of reviewers, nearly all of them thoughtful and admirable. I got to help authors - both those accepted and those rejected - to improve their work. I got to spend time with the dozens of graduate students who worked with the journal. If there was a "privilege" to editing the journal, it was all this, not any mythical ability to "shape the discipline." It was a privilege to have a three hour meeting, every week, dedicated to talking and reflecting with colleagues, both present and present-in-writing, about the nature of the social world and how we know it. It was a magnificent gift, and I am deeply grateful for it.

Interviewer: Christian Fleck.

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BOOK REVIEW

(Post-) Soviet Sociologies

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Czech, Polish, and Russian sociologies are often considered together as a part of the so-called Second World, or semi-peripheries, and still have much in common. First of all, it is their political history, mainly the period of state socialism. According to the totalitarian paradigm—nowadays popular mostly in the region itself—Stalinization reforms, the capacity of universities, and the isolation of the Iron Curtain were shared experiences. Most scholars outside of Eastern European or Slavic Studies know little more about the region than those widespread clichés. Thanks to the "Sociology Transformed" series, international audiences can gain insights into the history of what might be called (post) Soviet sociologies and may establish their own opinions more easily.

All three books discussed in this review were published in the Palgrave Macmillan series edited by John Holmwood and Stephen Turner, along with 14 other volumes so far. Most of the publications represent Western Europe (Austria, Belgium, Denmark, France, Ireland, Italy, Portugal, Sweden) or English-speaking academic circulations (Australia, New Zealand, South Africa), but also include a monograph on Israeli (definitely a part of the core of knowledge production) and Chinese sociology (the fastest growing academic field in the

world). The latter volume could be included as a part of academic systems operating under state socialism; however, the three selected cases have much more in common. Each volume in the series offers a quick read of a very reasonable number of pages (100 to 150) and a basic set of information about national sociologies. Readers may gain a lot when calculated per amount of pages, but not so much per euro spent, especially when considering the poor editing and proofreading as in the Russian volume.

Of course, how national sociologies are defined is highly problematic, and indeed the series' authors answer this question differently. For example, Polish sociology in general is much broader than sociology in Poland. However, all three cases focus on sociologies limited to the usage of national languages, scholars' origins, and institutional boundaries defined by the national state. More information is found on those who emigrated and became prominent scholars abroad than those who came from abroad to research local societies. Furthermore, all three volumes seem to be much closer to the history of sociology than the sociology of sociology. This might not be so much the choice of authors themselves, but rather the general objectives of lesser-known academic circulations. British, American, or German sociologies (however defined) not only are better known to the general reader, but are also far more discussed. Therefore, any new work simply enters an already vibrant debate, while in the case of less well-known national academic fields, authors play the role of gatekeepers who explain national peculiarities to the rest of the world. In the foreword for the Russian volume, G. Therborn stated the uncomfortable truth: "Sociology in the current era of globalization is very much part of this geopolitical divide of ignorance and knowledge, where Russian [and we can add Czech or Polish] sociologists read and cite western European and North American colleagues frequently, while few Westerners know about the former. And even fewer read them" (p. V). Obviously, most of the readers do not have enough insight to verify authors' judgments and interpretations. In consequence, the stances and notions, misjudgments, or bold pronouncements of the authors are difficult to verify. On the one hand, the authors bear more responsibility on their shoulders. On the other hand, they are pushed into the position of an "objective" witness giving an account of a foreign country, a position certified by their national authenticity.

It is worth keeping in mind that the narratives offered are also localized and struggle with how to tackle presenting one's own history. All the narratives inevitably bear traces of the authors' personal involvement: for example, Elena Zdravomyslova is the daughter of Andriei G. Zdravomyslov, a prominent figure of the 1960s generation of Russian sociologists; and Marta Bucholc, trained in Warsaw, tends to center her focus on the capital. Furthermore, these narratives are particular interpretations of the discipline and its history. The authors could offer many parallel narratives: some vary in details but, most interestingly, they vary in the general framework of how they define sociology, its aims, and its role, as well as academia in general. Whereas the account of Czech sociology by Marek Skovajsa and Jan Balon seems to be most revisionist[1] and critical while offering impressive data gathered by the au-

[1] In opposition to the totalitarian paradigm, an opposition introduced in the context of American historians researching Soviet Union.

thors themselves, the Russian narrative by Larissa Titarenko and Elena Zdravomyslova explicitly states a longing for an imagined "normal social sciences" or academic freedom "like in the West" and refers mainly to secondary sources.

All three volumes offer a chronological story of sociology's developments with periodization based in political events, and all the authors focus on generations (with continuation and rapture as main categories), institutions, and common areas of research. Academia is state-driven and state-dependent. However, a more general political, economic and social background is rarely present in these volumes; sociology seems to be an ivory tower negatively influenced by outside pressure, a peril for academic autonomy. Interestingly, despite the overarching Stalinization argument, political trajectories in the three cases differ profoundly and justify national divisions.[2] Actually, the development of sociology under state socialism seems to differ more than post-Soviet trajectories of its development, suggesting that the visible hand of state socialism was not as brutal as the invisible hand of capitalism.

I. Beginnings: Pre-1918 sociology was emerging mainly from philosophy, law, and history as early as the 1860s to the 1880s. It was often seen as competition to those already established disciplines. In discussing this period, all the authors focus on individuals and their biographies and later influence. Obviously, the definitions of who is a proto-sociologist are problematic. In the Polish case, the author focuses on scholars who self-identify as sociologists; the Czech authors narrow their focus to Czech-speaking intellectuals (mainly T. Masaryk); the Russian proto-sociology is impressive with its broad definition of who could be considered as the discipline's founding fathers (apparently, no founding mothers). In all three cases, sociologists are presented as mostly social reformers typically acting in opposition to the government (which in Czech and Poland is additionally seen as a foreign occupant).

II. Interwar: While in the Russian case, the 1917 revolution defined a new era, in the Polish and Czech cases, establishing new independent states in 1918 meant building national academic circulations almost from a scratch. Polish sociology had to cope with three separate post-partition territories. The Czech one dealt with the reminiscences of German intellectual traditions and its two main sociologists' strong engagement in politics. Sociology in the entire region was strongly politicized, thanks to a strong intelligentsia ethos; many sociologists were leftists, especially in Poland and Russia. The interwar years were also a time of sociology's institutionalization and rapid development, early research projects, and methodological and theoretical choices. The generation of the founding fathers became inevitable reference points for future scholars. At the same time, the first debates and conflicts fed the discipline's development, like the Prague-Brno competition between a theoretical and empirical approach (additionally inscribed into the generational shift). Despite the region's geographical closeness to German, French, and British universities, American universities had a strong influence from the beginning and maintained this influence in the decades to come.

[2] Profound differences in postwar academia between GDR, Czechoslovakia, and Poland were interestingly presented by John Connelly (2002), *Captive University: The Sovietization of East German, Czech and Polish Higher Education, 1945-1956*, Chapel Hill: University of North Carolina Press.

III. Postwar: The late 1930s were already increasingly difficult for sociology, especially in Russia, where under communist rule it had been marginalized and seen as a bourgeoisie science since 1929. It had to wait for a revival as late as the 1960s. In the other two cases, rising authoritarianism, antisemitism, and governmental control over academia created problems. The Second World War was especially devastating for Polish scholars, but in both Poland and Czechoslovakia, continuity was stronger than wartime losses and emigration. In the early postwar years, sociology faced a spectacular revival supported by the initially modest interest of communist party officials: it was assigned a significant part in the Marxist transformation of societies. The so-called sovietization or Stalinization and higher education reforms in the late 1940s meant institutional abolition, similar to what Russian sociologists had experienced over a decade earlier. Enrollment was cancelled, departments renamed or closed, publications stopped, associations dissolved, and many scholars forced to early retirement. In Poland, because of strong interwar traditions and networks, the intelligentsia ethos, and a relatively short period of political pressure, the Stalinization period was not as harmful as in Czechoslovakia, not to mention Russia. Most of the sociologists simply changed research topics and stayed at universities, ready to return to a sociological focus in more conducive circumstances.

IV. After the Thaw: The political easing after Stalin's death had profound consequences for all three contexts. However, time seems to be a crucial factor in the ability for sociology to recover. In the Polish case, that meant people were educated in the late 1940s by interwar scholars who returned to sociology only after a few years break between 1949 and 1955. Czech sociologists were rehabilitated only in the late 1950s, and Russian sociologists in 1962 after almost 20 years. Continuity was the strongest in Poland and still present in the Czech case, but in Russia, the thaw was simply a new beginning. All the authors propose viewing this period as a time of regeneration; however, sociology reverted to an already established institutional frame divided into: 1) universities focused on teaching, 2) research academies of science, and 3) professional research institutes sponsored by government agendas.

In sociological theory, Marxism-Leninism still dominated (with some revisionist interpretations in Poland), and interest in empirical studies or methodology was a safer political choice. Sociologists started to travel, participated in ISA conventions, and gained better access to the international circulation of knowledge. Foreign organizations provided scholarships, funds, and possibilities to travel. At that time, the first wave of serious internal diversification also began, as many sub-disciplines emerged.

During the 1960s, sociology was essential to providing empirical data about society to reform it; the state introduced new founding schemes, public opinion surveys, and ordered reports to design new policies. The cultural dissemination policies needed animators, and the rapidly developing sociology of work meant hiring thousands of sociologists at large companies. This increasing need for expertise turned scholars into state socialist managers. All the authors underline the role of pragmatism, ritualistic references to Lenin, and the strong presence of mediators between the political establishment and the academic community. How-

ever, Skovajsa and Baron also point out that the stable funding sources and large investments in research provided by the state were crucial for sociology's revival—in all three countries.

V. 1970s and 1980s: After the thaw period, Poland entered stable decades of development marked by ritualized political concessions. Even the antisemitic campaign in 1968 had limited influence on sociology, in contrast to the Czech case, where the consequences of the Prague Spring marginalized most sociologists. In Russia, after an intensive revival called the "golden years" of sociology (1965–1972), political pressure rose. In both of the latter cases, what followed was further professionalization within strict ideological limits, or the "continuation of sociology without sociologists" as Skovajsa and Baron put it (p. 74). Continuity with the former milieu was broken, but institutional structures remained.

Sociology was still seen as useful. It remained closely related to industrialization, an increased focus on expert knowledge, and the "serving society" agenda, which actually allowed scholars to avoid political pressure under the umbrella of objective research and data-based scientific conclusions. The price was nepotism and corruption in enrollment as well as the violation of the intellectual rights of banned scholars and a facade of the peer-review process. At the same time, unofficial seminars, "oral" sociology,[3] and "suitcase" sociology[4] allowed the discipline to develop. Polish scholars, remaining in the best political situation, were active in the Solidarity movement, supported students' protests, and managed to keep international connections.

VI. Transitions: The perestroika in the USSR and the rapid transitional period are indisputable tipping points for sociologies in the region. In all three cases, those processes meant an unlimited opening for international cooperation, a publishing market boom, and the influx of foreign funds—almost a "Marshall Plan" for sociology (Titarenko and Zdravomyslova, p. 69). At the same time, financial difficulties limited the positions available for both new faculty and dissident sociologists, and state support and spending on higher education shrunk rapidly. Both the Czech and Russian authors note that cohesion in academia broke once again, this time because of economic reasons, resulting in a missing generation of scholars. In Poland, a sense of continuity remained.

The "opening" to the West was difficult and limited. A good example might be the case of the Central European University, whose departments were initially opened both in Warsaw and Prague but later moved to Budapest. The restocking of long-awaited literature easily fueled many careers and a publishing boom. The Czech authors remain especially critical towards this period, underscoring with disappointment that almost no profound research was conducted in the new political circumstances. Overall, despite the high hopes of many sociologists, the transition period was not so much a return to an imagined "normal social sciences." Nor did sociology become a central discipline during the transition; this position was quickly taken by economists.

[3] Based on oral accounts without publications or outcomes that can be traced by censors.

[4] A practice of smuggling back home copies of foreign literature from fellowships, usually in one's private luggage.

The 2000s were marked by an educational boom fueled by the private sector and growing fragmentation, but were also characterized by further internationalization. In the Czech and Polish cases, access to the European Union had a profound impact on the system of funding and higher educational reforms. The general tendency was to enforce internationalization and the productivity of scholars by introducing a scoring system for publications, grants, and cooperation. Bucholc as well as Skovajsa and Baron point out that the reforms were wrongly presented as politically neutral and technocratic. Commenting on the most recent developments seems to be the most challenging and difficult in all three cases.

It is worth underlining that three of the five authors are women; however, the narratives in the books refer mainly to male scholars and their achievements, especially in the Czech case. Female sociologists seem to be present and influential rather in Poland and Russia; whether this is either a national specificity or the authors' sensitivity is difficult to discern. Gender studies is mentioned as a growing field of research from the 1990s. Titarenko and Zdravomyslova even devote a whole chapter to this topic—unfortunately, without any meta-comment on gender relations in sociology as a profession. I would appreciate more information about the discipline's social structure, intelligentsia reproduction, and mechanisms of selection considering gender and class, as well as a wider perspective on the distribution of resources and power relations inside the discipline. Such an approach demands more research, which is provided mainly by Skovajsa and Baron. What is probably a main difference between the sociology of sociology in comparison to the history of sociology is that the latter lacks such a meta-analysis.

To sum up, all three books offer important insights into (post-)Soviet sociologies. On the one hand, they help to break the vicious cycle of Arjun Appadurai's "local informant," as mentioned by Bucholc—the indigenous scholars limited to sharing their knowledge of the local context with Western recipients. On the other hand, they reproduce it. As we learn from Titarenko and Zdravomyslowa, Russian sociology has faced a rise in methodological nationalism in recent years. Some Russian scholars claim that the adaptation of external approaches is useless because their social milieu needs separate localized theories to understand it properly (143–46). At the same time, Polish sociologist P. Sztompka represents the opposite strategy. He advocates for a global sociology insensitive to regional differences (and institutional power plays).[5] The tensions of globalization or internationalization are demonstrated not only in the books discussed, but also in the readership and its consequences.

[5] Piotr Sztompka and Michael Burawoy. (July 2011). "Another Sociological Utopia & Last Positivist," *Contemporary Sociology: A Journal of Reviews,* 40(4), 388–96, https://doi.org/10.1177/0094306111412512.

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BOOK REVIEW

Rindzevičiūtė: Power of Systems

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Rindzevičiūtė, Eglė. 2016. *The Power of Systems: How Policy Sciences Opened Up the Cold War World*. Ithaca, London: Cornell University Press, 2016.
306 pp.
ISBN: 978-1-5017-0318-8
Price: $ 49.95

To those working on the history of the social sciences in the Cold War era, *The Power of Systems* is a long awaited book. The first reason for this high anticipation is the organization on which the book focuses: the International Institute for Applied Systems Analysis (IIASA), an international think tank located in a beautiful castle south of Vienna, Austria. Thanks to some personal recollections of IIASA (Brooks and McDonald, 2000; Levien, 2000) and a few historical studies on the foundation (e.g., Riska-Campbell, 2011), a basic narrative about IIASA has been established, which presents it as the culmination of the political thaw between the United States and the Soviet Union. The joint creation of a scientific organization that—placed on neutral ground—addressed problems of a global scope appeared as a means to further the mutual rapprochement. Systems analysis, an amorphous complex of policy analysis techniques that was developed at the infamous RAND Corporation and had spread into various fields of policy making in the U.S. (Jardini, 2000; Light, 2003), came in handy: an umbrella term without precise meaning, it neither enforced restrictions on the scientific work to be done at IIASA nor suggested an ideological leaning (Duller, 2016). At the same time, it used many of the contemporary buzzwords of "high modern social science" (cf. Heyck, 2015) such as system, structure, function, interdisciplinarity, etc. Beyond this standard narrative, however, not much was known.

The second reason for the eager anticipation of the book is that an author would be writing in English about parts of the Cold War arenas that were inaccessible to most contributors to the English literature in the field: the actions, plans, and hopes of actors in the Soviet Union and their allies. Only a few studies exist in the English literature on the Cold War social sciences that overcame this language barrier, among them the classic study by Slava Gerovitch (2002) on Soviet cybernetics, David C. Engerman's (2009) book on American Russia experts, and a recent volume on the history of science studies on both sides of the iron curtain (Aronova and Turchetti, 2016). Most likely due to the fact that the field only came into

blossoming in the last decade, there is still a lack of English studies on Cold War social science in places outside of the English-speaking world (see Boldyrev and Kirtchik, 2016, for a more differentiated overview).

Thus, the expectations were certainly high, and yet, *The Power of Systems* meets them. The manuscript attempts to make three large contributions on theoretically distinct levels. The first contribution is on the level of historical materials and data. Rindzevičiūtė supplements intensive archival research with personal interviews and succeeds in interweaving these threads into a rich discussion of political and scientific developments that led to the establishment of IIASA (chapters 1 and 2). She further investigates how this context informed IIASA's organizational culture and activities (chapters 3 and 4), and how it permeated the knowledge produced there (chapters 5, 6, and 7). While some publications exist that focus on the Western part of the story, Rindzevičiūtė, to my knowledge, is the first to explore and publish an account of how the Russian side acted and thought about this project of East-West dialogue. What is more, she does so with precision and a love of detail that make her book a valuable source for further research efforts in this area—an outcome that could, however, have been improved by providing an index with all the names mentioned in the book. Because of the richness of the historical data provided, one easily loses track of the acting persons apart from the main proponents.

The second contribution takes the form of a thesis in the history of science, and it features prominently in the subtitle of the book, *How Policy Sciences Opened Up the Cold War World*. Arguing against authors like Paul Edwards (1996), Rindzevičiūtė claims that the philosophy dominating the policy sciences helped to overcome the cleavage between the Cold War opponents. Policy scientists had grown increasingly critical of an exaggerated belief in the capacities of science. Many of the alleged certainties produced by earlier scientific studies had turned out to be uncorroborated, and positivist epistemology had seen severe attacks from a broad range of camps. To cope with uncertainty and risk had become the major challenge of policy science, and it contributed to opening the Cold War world by disseminating this challenge as a new virtue of decision-making. While the problems were global in nature, no side possessed the true solution. Openness towards all sides had become the recipe of choice in the policy sciences, and from there it informed and slowly transformed the viewpoints of the political actors.

This argument by Rindzevičiūtė is strong, innovative, and I think very plausible. Alas, it is not explored fully. Rindzevičiūtė does not investigate the close relation between the discourses at and around IIASA on which her book focuses and the contemporary debates in the philosophy of science and science and technology studies (STS). This is unfortunate, because quite a few IIASA affiliates provided crucial contributions to these latter debates, among them Brian Wynne, Michael Thompson, and Jerome Ravetz (who is mentioned in passing on p. 200, though misnamed as Jeremy—after a brief correspondence with Ravetz, I can confirm that he participated in the meetings to which Rindzevičiūtė refers). Wynne, who had studied with David Bloor and Barry Barnes in Edinburgh, published classic studies

of the function of rituals in privileging expert over lay knowledge. Based on his IIASA experiences, Thompson wrote an anthropological essay "Among the Energy Tribes," where he first proposed the ideas that he later developed into a cultural theory of risk (Thompson, 1984). And while Ravetz never was an official member of IIASA, his concept of a post-normal science (PNS) starts from the same concerns as those held by Rindzevičiūtė's actors: that in the globalized era that characterizes coeval policy making, we must make decisions in the face of huge uncertainties; time pressure can make it irrational to wait for scientific certainty. I would argue that to explore the proximities between her policy scientists and the scholars working in STS or related fields would have helped Rindzevičiūtė to see them as parts of a larger debate over the nature of science in the outgoing 20th century. Such a study would have also allowed for an innovative perspective on a distinct chapter in the history of the philosophy of science.

The third level on which the book aims to contribute is related to the concept of governmentality. While referring back to Michel Foucault's lectures at the Collège de France, recent authors reject one of the aspects deemed most fundamental by Foucault, namely governments' attempts to create, influence, and control the minds of citizens. Without this aspect, governmentality refers to nothing more than to systems of thought that influence policy-making. The difference appears obvious. While neo-liberalism can be described as a technique of governmentality in the Foucauldian sense, the systems approach in policy making cannot—or at least, Rindzevičiūtė does not do so to a degree that convinced me. By and large, it is unclear how the concept of governmentality helps the argument that she is making. Or, to be more precise, the argument would be as convincing if she had replaced governmentality with another term—philosophy or epistemology being possible and, in light of the previous remarks, perhaps more adequate candidates.

"This book is about science and power," states a bold claim in the book's introduction (p. 2). This claim is a bit exaggerated. Science and power are the props used to stage the drama. But their function is to provide a frame for the narration, not to enact it. True, the book is about an organization at the intersection of science and political power, and it is about how this intersection played out in the lives of a set of actors related to this organization. However, it is neither very specific about the scientific ideas, nor is it, in a setting informed by the sociology of knowledge and ideas, about explaining the specific shape of scientific ideas by reference to their place of origin. My main criticism of this otherwise outstanding book is that its objectives were too ambitious, and that while Rindzevičiūtė's knowledge about the fields and discourses involved is impeccable, restricting the text to making one or perhaps two of these contributions might have resulted in an even more convincing text.

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BOOK REVIEW

Hess: Tocqueville and Beaumont

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Hess, Andreas. 2018. *Tocqueville and Beaumont: Aristocratic Liberalism in Democratic Times*. Cham, Switzerland: Palgrave Macmillan.
First Hardback ed., v + 153 pp.
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Price: € 51,99

In this concise and very readable intellectual joint biography of the French writers and politicians Alexis de Tocqueville (1805–1859), best known for *Democracy in America* (1835/1840), and Gustave Beaumont (1802–1866), with *Ireland* (1839) as his most famous work, Andreas Hess innovatively presents us with a picture of a research partnership of pathbreaking significance for the history of social and political science. In contrast to earlier individual biographies of both men, this is the first to present their considerable output as the outcome of a life-long collaboration in both intellectual and professional practice. Tocqueville and Beaumont were thinkers who in debates with each other defined and responded to key events and conditions of their time. In Hess's approach, their many and varied topics of analysis gain greater contemporary relevance than is usually the case. This book will appeal to a broad audience of specialist historians of ideas, social science, and politics as well as to students in a wide range of fields. It also serves as a model for revisiting many other partnerships in the history of social science, where mutual collaboration has been put in the shade by falsely aggrandizing searches for individual "greatness."

What Hess describes as a two-man research "machine" was a highly productive partnership of mutual influence that began at law school and continued unabated till Tocqueville's death. Early on they came to understand the methodological importance of comparative research in the quest for a better understanding of democratic modernization processes, and their first joint project centered on what policy lessons could be learned from penal reform in the U.S. Using their long correspondence as a source, Hess shows how their partnership was characterized by collaboration on many fronts, particularly in critical, but supportive, discourse. They shared evidence and sharpened their analysis in letters and conversations in political chambers, on journeys, and over dinner. In particular Tocqueville's better-known *Democracy in America* needs to be understood in the light of his partner Beaumont's more skeptical but equally successful novel, *Marie, or Slavery in the United States* (1835), as an example of their complementarity. Whereas Tocqueville undertook interviews with policy makers and civic dignitaries in the United States, Britain, and elsewhere, thus presenting a

broader vista of change, Beaumont described in empathetically observed detail the social conditions of those excluded from a democratic voice.

The concept of aristocratic liberalism, a concept central to the title of the book and of particular ideological significance during post-revolutionary attempts to restore the monarchy in France, is perhaps a bit harder to get to grips with here than their collaborative thought processes. It broadly signifies the search for a stable constitutional system framed by legal checks and balances and a strong and well-educated civil society to safeguard liberty against the excesses of both radical egalitarianism and conservative religious authoritarianism. As Hess notes, it was a perspective attractive to surviving young aristocrats, like Tocqueville and Beaumont, who welcomed the new democratic trends set in train by the revolution, but also wished to preserve some of the culture and intellectual freedoms of the old regime. At its heart it expressed anxiety about the tensions between the Enlightenment desire for liberty and the growing radical demands for equality from those without voting rights, anxieties aired in some depth in the writings of both these writers. Here, the book well illustrates the need for such a typifying ideological concept to be contextually and fully unpacked to be understood in all its interest group complexity. It is this complexity that continues to have many resonances today, rather than its aristocratic origins.

Finally, it should be noted that there are of course three research partners here: Tocqueville, Beaumont, and Hess himself. As a longstanding writer and commentator on continuing Western debates over the fate of democratic liberalism through seemingly never-ending times of modernization, violent sectional politics, and ideological oppression, Hess is clearly in awe of this pair of thoughtful public intellectuals, neither of them sanctimonious "Weltverbesserer," both of them valiant but unsuccessful in their attempts to make compromise-enhancing policy reasoning a political platform in its own right. His admiration might explain the lack of attention paid to the contradictory legacy of empire-building Napoleon, who took the reign in the bloody aftermath of the anti-aristocratic revolution and whose imposed codification of French law into a more liberal, egalitarian, and meritocratic system has remained surprisingly durable throughout 19th and 20th century European history. Like history itself, history writing is never short of unreasonable moral dilemmas.

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BOOK REVIEW

Durkheim and Hubert in Brazil

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Benthien, Rafael, Miguel Palmeira and Rodrigo Turin (Eds.). 2016. *Hubert, Henri, Estudo Sumário da Representação do Tempo na Religião e na Magia*. São Paulo: Editora da Universidade de São Paulo (=Biblioteca Durkheimiana vol. 2).
160 pp. Bilingual: Brazil Portugese and French.
ISBN: 978-8-531-41596-8
Price: Por R$ 28,80

Consolim, Márcia, Márcio de Oliveira e Raquel Weiss (Eds.). 2017. *Émile Durkheim, O Individualismo e os Intelectuais*. São Paulo: Editora da Universidade de São Paulo (=Biblioteca Durkheimiana vol. 1)
192 pp. Bilingual: Brazil Portugese and French.
ISBN: 978-8-531-41595-1
Price: Por R$ 25,60

The so-called "classics" occupy a central position in many sociological communities around the world. The works of Karl Marx, Max Weber and Émile Durkheim have been taught to generations of students, and mastering these works is a crucial capital in the intellectual disputes that structure the academic field in different countries. As a peripheral country long exposed to hegemonic European culture, Brazil would not escape this fate. Weber is one of the most quoted authors in Ph.D. theses in Brazilian social sciences (Cunha Melo, 1997) and both he and Marx have been key intellectual sources for leading Brazilian scholars who have sought to explain the strange assemblage of modernity and traditionalism that shaped Brazilian capitalism (Schwarz, 1998; Werneck Vianna, 1999). Even high school textbooks on sociology have chapters about "the classics."

Despite this continuous interest in the classical texts, Brazilians for a long time relied on second-hand editions and dubious translations, which contributed to a canonical approach that lacked historiographical reflections about the social history of the texts. Many classical works did not have a Portuguese version until the last decades of the twentieth century. *Economy and Society*, for instance, was read by Brazilian students in the 1950s and 1960s in the legendary translation published by Fondo de Cultura Económica (Zabludovsky, 2002), which prompted a Brazilian sociologist writing in the mid-1970s to call this famous Mexican translation a "key book" in Brazilian sociological thinking (Dias, 1974).

However, in the last years, there has been a growing body of scholarship devoted to the history of the discipline, which has given the sociological community in the country new translations and critical editions that have greatly improved teaching and researching. In 2004, the Brazilian sociologist Antônio Flávio Pierucci coordinated a new critical edition of "The Protestant Ethic," translated from German by José Mariani de Macedo (Weber, 2004). This edition depicted the complex history of the text and highlighted all the changes and modifications made by Weber after the first version, which was edited in 1904–1905. Besides, Pierucci carefully detailed Weberian concepts and avoided Parsons' terminology. The publishing house Cosac & Naify (sadly bankrupted in 2015) released several classical works in anthropology in luxury editions accompanied by excellent commentaries and introductions. As a result, the Brazilian audience can now read the four volumes of Levi-Strauss' *Mythologiques* in great critical editions. It is within this context that a group of Brazilian researchers set out on an ambitious project: to publish critical and bilingual editions of key texts from the so-called "French School of Sociology." *L'individualisme et les intellectuels*, by Émile Durkheim, and *Étude sommaire de la representation du temps dans la religion et la magie*, by Henri Hubert, are the first results of their work.

These books are the first outputs of the Brazilian Centre for Durkheimian Studies, which has links to similar centers around the world dedicated to the study of Durkheim's oeuvre. The Centre is directed by Professor Rachel Weiss from the Federal University of Rio Grande do Sul, who wrote a Ph.D. thesis on the Philosophy of Morality in Durkheim. Weiss edits the volume on individualism and intellectuals with Márcia Consolim from Federal University of Sao Paulo and Márcio de Oliveira from the Federal University of Paraná. The volume on Hubert is edited by Rafael Benthiem from the Federal University of Paraná, Miguel Soares Palmeira from the University of São Paulo, and Rodrigo Turin from the Federal University of Rio de Janeiro. This group of sociologists and historians is highly qualified for the mission, as many have experienced academic seasons in France and thus have developed deep connections with the French intellectual field. Weiss and Oliveira had already co-authored a book on Durkheim, while Consolim has been researching the history of French sociology in the early twentieth century since 2010. Benthiem is a historian who has investigated the connections between archaeologists and sociologists in the famous *L'Année Sociologique*, while Palmeira and Turin have done archival research and written on the history of historiography.

The volumes are edited by the publishing house of the University of Sao Paulo, which is a relevant fact. The so-called "USP" was established in 1933, and the first professors of Philosophy, Languages, and Literature and Human Sciences were French intellectuals. Lévi-Strauss, Fernand Braudel, and Roger Bastide were some of the many young scholars drafted by the Paulista elites to become professors in São Paulo, and as a result French intellectual culture established deep roots in that city.

The two books follow the same format: After a brief presentation by the editors and a note on which version is being translated, the text is rendered in Portuguese in a way that the reader can check the French original text on the opposite page. There is also a commentary

section, which presents a collection of articles authored by Brazilian and non-Brazilian scholars about Hubert and Durkheim, and an appendix with historical materials, such as letters, pictures, or reviews published during the lifetime of both authors.

The volume dedicated to Hubert brings together contemporary analysis by Christine Lorre, Jean-François Bert, and Rafael Benthien, and articles authored by coeval colleagues Marcel Mauss, Jean Lafitte, and Salomon Reinach. The reviews were carefully chosen so that the readers can have a glimpse of the original reception of the work and the kind of intellectual exchanges amid French intellectual circles at that time. The contemporary analyses focus on different features of Hubert's work and life. Lorre writes about Hubert's archeological research and his job at the Musée des Antiquités Nationales (MAN); Bert briefly analyses the relations between Hubert and the group behind *L'Ánnée Sociologique*, exploring Hubert's contributions to major works authored by Mauss; Benthiem, during his turn, delivers a theoretical essay on the relations between Hubert's concept of time and the philosophy of Aristotle. The volume closes with a moving autobiographical note written by Hubert to his son before leaving to take part in the First World War.

The volume on Durkheim's text on individualism presents to the Brazilian audience a central piece for understanding the relationship between sociology and public life. It starts with a good presentation by Consolim, Oliveira and Weiss, which outlines the history of the text itself, the political battles in the Third Republic, and the divisions within French cultural system brought on by the emergence of the Dreyfus affair. The original text is presented with its original notes, and it is followed by the commentary section. Louis Pinto writes about the "universalism" in Durkheim's discourse; Consolim analyses the social history of the *Revue des deux mondes* and *Revue Bleue*, two central magazines in nineteenth-century French culture; Susan Jones explains the meaning of individualism in Durkheim's theory; Weiss outlines a general presentation of the text and its place in Durkheim's oeuvre; William Miller expands on the main ideas of the text and claims its contemporary relevance. The section with historical materials includes the original article by Ferdinand Brunetiére, which prompted a vigorous response from various *intellectuels,* and a very short text by Durkheim on the role of intellectual elites in democratic societies. One must notice that Brunetiére's piece is presented with the footnotes published in its second version, which came to light after the author was severely criticized for his defense of the French Army and its attack on the intellectuals.

The result of this pioneering work is definitely positive. There are very few critical editions of classical texts in Brazil that present such a meticulous historical work as the one done by Weiss, Oliveira, Consolim, Benthien, Palmeira, and Turin. The collective nature of the editorial work is remarkable in the two volumes, and the texts written by the editors themselves show a coherent sociological approach, highly influenced by Pierre Bourdieu's theories. In Brazilian sociological community, Bourdieu's prestige is enormous, and his work is regarded as almost a synonym for the "sociology of the intellectuals." These two volumes are a nice example of the strengths of this intellectual tradition.

However, one must acknowledge a few weak points. First of all, the volume on Durkheim is a bit repetitive, as many analysts present the Dreyfus affair in great detail, which makes the reading quite tiring. Some texts address the same questions in a similar fashion, which makes them redundant, a problem that does not happen in Hubert's volume. One also misses analyses on the circulation of Hubert and Durkheim's work in Brazil, which could provide great insights for the readers about the historical dissemination of these works in a comparative perspective. Regardless, these are very small problems, and the final result is nevertheless positive. I hope that the group involved in this initiative continues to work together, as we certainly need more solid historical work on the so-called classical works of sociology.

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BOOK REVIEW

Heufelder, Argentinischer Krösus

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Heufelder, Jeanette Erazo. 2017. *Der argentinische Krösus. Kleine Wirtschaftsgeschichte der Frankfurter Schule* (The Argentine Croesus: Short Economic History of the Frankfurt School). Berlin: Berenberg Verlag.
205 pp.
ISBN 978-3-946334-16-3,
Price: € 24,00

The so-called Frankfurt School is one of the most studied groups of intellectuals-cum-scholars of the 20th century. After Martin Jay's Ph.D. thesis (1973) and Rolf Wiggershaus' voluminous monograph (1986/1994), one would have thought that there was nothing left to be uncovered. But the publications on the School did not come to a stop, and some latecomers did have to say something new about the particular aspects of the group under the leadership of Max Horkheimer (Dahms 1994; Faber & Ziege 2007; Faber & Ziege 2008; Steinert 2007; Boll & Gross 2009; Ziege 2009; Wheatland 2009; Lichtblau & Herrschaft 2010). In all studies on the history of the "Institute," one name regularly showed up without being portrayed in depth: Felix Weil. The German ethnologist, documentary filmmaker, and author Jeanette Erazo Heufelder, unknown in the history of the social sciences until now, offers a fascinating look behind the walls of the Institute and into the behavior of its members in this concise biographical study on Weil.

Felix Weil was born in 1898, eight years after his father Hermann arrived in Buenos Aires as an employee of a Dutch merchant starting the trade of grain from South America to Europe. Shortly before Felix was born, his father quit his job and founded together with two of his brothers a new firm "Gebrüder Weill und Partner" (Weill Brothers & Co.), which was listed as "Hermanos Weil y Cía" in Argentinian documents. Within a very short period of time Weil's father became a kind of tycoon in the international trade of grain. At this time, before World War One, Argentina was on the rise in economic terms, as one of the richest countries in the world, and Buenos Aires was a real metropolis not only of the Southern Half of the Globe, but also worldwide. When Felix turned ten his father stopped working, sold his company, and returned to his native Germany, where he lived the life of a coupon cutter. Felix attended a *gymnasium* there and graduated on the eve of the Great War; as an Argentinian citizen he was not drafted into the Emperor's *Reichswehr,* but he volunteered as a helping hand organizing subsidies for the trenches. Hermann's expertise in foreign affairs brought

him even a dinner with the Emperor and his generals. Hermann's name was afterwards (mis-)used as the author of pamphlets calling for victory-peace as the aim of the German Reich.

Meanwhile, Felix started to study at the newly erected university in Frankfurt economics and joined one of the liberal Germanic fraternities, a so-called *Burschenschaft*; later he transferred to Tübingen, one of the characteristic small cities hosting a university. There Felix encountered for the first time a lecture on Marxism by the renowned economist Robert Wilbrandt. Felix was taken by the revolutionary mood of the campaign to nationalize the German industry in which Wilbrandt participated with other professors, like Emil Lederer and Joseph A. Schumpeter, as one of the experts under the presidency of Karl Kautsky, who was then still the leading theoretician of the German Social Democrats. Wilbrandt's right hand there was Karl Korsch, who became one of the exponents of what later was called Western Marxism. Felix entered politics and was banned from Württemberg shortly afterward. He nevertheless managed to finish his studies and graduated from the University of Frankfurt. (Without further detail, on p. 32 Heufelder mentions Alfred Weber, the younger brother of Max, as Weil's supervisor there, which sounds wrong because Alfred Weber taught in Heidelberg since 1907).

Surprisingly enough, Felix just married, returned to Buenos Aires in 1920 to show his wife the land of his early youth, and did some business there to keep a promise he made to his father. Besides these activities, Weil wrote an article about the "Labour Movement in Argentina," which was published in 1923 and reprinted two years later in the famous journal *Archiv für die Geschichte des Sozialismus und der Arbeiterbewegung*.

Before that, Felix entered another "business" when he became the delegate and informal collaborator for the Communist International responsible for South America. Felix reported to Sinovjev under the nom-de-guerre Lucio about Latin American parties willing to enter the Comintern.

After a year Felix returned to Germany, telling his father that he was not fit to do business, but remained friendly with him and persuaded the old man to give money philanthropically not only for medicine but also for the establishment of an institute dedicated to Marxist scholarship. Shortly before he died, Hermann Weil became a Doctor h.c. of the institute. Felix was looking for qualified people to run the institute but kept himself a low profile. To find people for the second rank was much easier than identifying and persuading a German professor to run such an institute. The first candidate, Kurt Albert Gerlach, died unexpectedly before the contract between the donor, the university, and the Prussian Ministry for Education had been agreed upon. In these negotiations Weil was forced to use Äsopian language, hiding some of his intentions. It is well-known that this Äsopian approach later became the *façon de parler* of the Institute, such as talking about Critical Theory when Marxism was meant, etc.

In the early days the Institute functioned as a knot in a network connecting the German Social Democratic Party with the newly established Marx-Engels Institute in Moscow. Its

first director, Carl Grünberg, was an economic historian and teacher of the first generation of the Austro-Marxists, and the founding editor of the before mentioned *Archiv*. The Soviets wanted to edit the works of the founders of Marxism but needed the consent of the Germans, who possessed the papers of Marx and were legally owners of his legacy. Grünberg and Rjasanov, the founding director in Moscow, were close collaborators, and Weil agreed to finance photographing the Marx Papers and handing over one copy to Rjasanov, while holding another one for Frankfurt. In addition to this scholarly collaboration, the Institute was populated with people who later became famous as spies, like Richard Sorge, or members of the illegal branch of the KPD, Germany's communist party.

From 1923 onwards Weil put a lot of his inheritance into left-wing cultural political endeavors, with the Institute as just one of the recipients. Others included the then famous Malik publishing house.

Weil's friendship with Max Horkheimer and Fredrik Pollock dated from before the founding of the Institute, but neither Horkheimer nor the others around him were willing to accept "Lix," as he was nicknamed, as a scholar despite his heavy efforts to become accepted as such one. One reason for this, which Heufelder does not consider, might have been the multitasking personality of Weil. He never concentrated on one activity alone but always juggled quite a number of balls. His dedication to support and develop Marxist social science never weakened, and as result he lived his last couple of years meagerly, compared with the luxury he enjoyed in his younger years. If one needs to name an individual as the embodiment of a Salon Bolshevik's social character – a verdict most probably developed vis-à-vis the members of the Institute around Horkheimer – Felix Weil would be a very good candidate, and ironically he used this label himself late in his life (p. 112).

Before Hitler became Chancellor of Germany, Weil left again for South America because his family's old company lacked leadership and lost its leading position on the international markets. Weil decided to stop the trade of grain across the Atlantic and to diversify his own and other family members' wealth. Ingeniously he established a net of companies and trust companies, and recruited trustworthy men to run all these businesses on a daily base. His genius in handling money was greater than he himself was willing to recognize. During the entire Nazi period, the Dutch branches of the Weil imperium were not uncovered by the Nazis, who were focused on taking money from their Jewish victims.

In Buenos Aires, Felix Weil continued his multifaceted life, remained in contact with the Comintern, financed the Argentinian Communist Party, and acted as a consultant for the new government's Minister of Finance, Federico Pinedo, together with later luminaries such as Raul Prebisch. In 1933 Weil published in Spanish a book on income taxation and instructed tax collectors. In addition to all these activities, he commissioned the erection of a new Art-Déco skyscraper and reserved the highest floor for himself. Troubles with relatives hindered Weil from using the new apartment because he moved to New York, fearing the Argentinian state attorney and intrigues of relatives from whom he had generously borrowed money. There he met Horkheimer and the other members of the Institute.

Meanwhile, the two friends Horkheimer and Pollock had secured complete control of the Institute, and they did not want to offer Weil a say in the running of it. The generous founder and still a major financier of the Institute was once again sidestepped. As a form of compensation, Horkheimer arranged for Weil to meet the daughter of a banker family from Horkheimer's native Stuttgart. Shortly afterwards she became Weil's third wife.

The initial will to establish the Institute as a network of foundations, companies, and associations that was transparent and accessible to outsiders turned into an arcane building that no one had a chance to influence individually. Besides the Kurt Gerlach Memorial Foundation (named for the first director who died before taking over its leadership), a Social Science Association, the Herman Weil Memorial Foundation, the Société Internationale des Recherches Sociales SIRES Realty Corporation (and some of its sub-firms that were active in suburban housing development), and a SOCRE Corporation contributed one way or another to the well-being of the Institute, in particular the dictatorial director Horkheimer, who owned an apartment in Manhattan and later a newly designed and built bungalow in California. Heufelder makes it completely clear that Horkheimer and Pollock pulled the lines; the socially awkward Felix Weil was the puppet out of whose pockets dollars fell into the hands of the puppet players. However, a new wife on Lix's side made it more difficult to continue channeling money from Weil's accounts into Horkheimer's purse.

In the 1940s Weil devoted his energy to writing a book he had promised 20 years earlier, which finally appeared under the title *Argentine Riddle* (1944) in New York. Two years later, when Juan Perón became president of Argentina, Weil opted for US citizenship and never returned to his home country.

The bad economic situation of the Institute forced Horkheimer to accept an ordinary white-collar job at the American Jewish Committee, in whose name he directed the Studies in Prejudice from 1943 until his last days in the USA in 1950. Horkheimer, Pollock and Adorno became reinstated as professors at the University of Frankfurt, but the initiator and financier of the Institut für Sozialforschung, Felix Weil, remained in California.

When Weil published a short piece in a newspaper there calling himself a member of the Institute, Professor Horkheimer lectured him immediately that he had only received this title honorably because of his loyalty and contributions, and that he should consult its director before publishing anything that appeared to be written in the name of the Institute.

In the late 1960s, Weil took over a new job as a US Major. Out of necessity, he started teaching American soldiers in Ramstein, West Germany, on taxation and municipal budgeting. In his last years before dying in September 1975, Weil was working on his memoirs where he wanted to correct the yet-to-be-established narrative of the Horkheimer Circle. The unfinished manuscript ended up with Weil's only son from his first marriage, who let several people use it since then.

Hochfelder did a fine job of illustrating the biography of a very unusual man, both in writing and in selecting the visuals. A portrait of Weil, painted by George Grosz in 1926, is on the cover of the book (late in life Weil was forced to sell it at an auction for 9000 dollars). One

can find three more pictures in the book: one showing a group of young radicals from the early 1920s and two from Buenos Aires in the 1930s.

One cannot blame someone with a different intellectual background for not placing Weil and his life more in the personal and intellectual environments in which he lived and acted. Hochfelder consulted Weil's unpublished manuscripts and some of the published secondary literature; given the fact that Weil's activities were dispersed over at least three different countries, one cannot expect more. The small book is nevertheless highly recommended for all persons interested in Marxism, communism, and the Frankfurtists, as Bert Brecht called his Californian neighbors in his diary.

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BOOK REVIEW

Normal Science?

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Christian Dayé and Stephan Moebius (Eds). 2015. *Soziologiegeschichte. Wege und Ziele* (History of Sociology: Ways and Aims). Berlin: Suhrkamp Verlag.
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Price: € 22,70

Soziologiegeschichte picks up on some of the themes that were first explored in Wolf Lepenies's magisterial four-volume edition *Geschichte der Soziologie* (1981). Since the latter appeared more than a third of a century ago a state-of-the-art critical companion that deals with the history of sociology has long been overdue. *Soziologiegeschichte* discusses an array of methods, different historiographic approaches, and the purpose and meaning(s) of the history of sociology. However, while this companion is a worthwhile attempt to spell out what the state of the art is, it falls somewhat short at being comprehensive, not least because it fails to look beyond the one discipline that is its main subject.

In Part I Christian Fleck sets the agenda with a list of 'dos' and 'don'ts'. He zeroes in on actors and performers, ideas, instruments, concepts and methods and, finally, institutions and environments, including some of the more problematic aspects of the history of the discipline. In relation to actors and performers Fleck makes a case for employing more social science methods when writing sociological history, for example by shifting our attention from individual contributions, charismatic figures and great names to cohorts and/or entire communities of social scientists. He also rejects Whig-like history writing; for too long sociologists have regarded earlier attempts just as predecessors, prototypes or as somehow incomplete or immature attempts, always surpassed by the latest attempt.

Despite his leanings towards a strong sociological program, when writing the history of sociology Fleck remains open to other suggestions, especially when discussing instruments, concepts and methods. More specifically and in relation to institutions he stresses the importance of institutional change, the range of conducive or not-so conducive environments and the challenging conditions that often give the sub-discipline a critical political edge (regime change, oppression, exile, etc.).

Some of the questions and problems that Fleck identifies are picked up and deepened in the second part of the book. Thus Jennifer Platt discusses the role of biography and, just like

Fleck, makes the case for focussing not just on the grand names and charismatic figures but also widening the horizon to include a broader, more inclusive spectrum of practitioners and giving more attention to those who, for whatever reason, did not achieve fame.

In what seems to be an immediate rejoinder to Platt, Dirk Kaesler points to the role and function that the classics have played and continue to play in the history and likely future development of the discipline. In Kaesler's view the classics serve as important reference points and disciplinary markers; they provide theoretical and conceptual devices that prevent sociologists from having to start anew every time or to shop around without any guidance and orientation, a point that is also reiterated by Donald Levine's contribution, just with the difference that Levine's focuses more on paradigms than personae.

To the reviewer Randall Collins', Andrew Abbott's and Charles Camic's contributions in Part II represent the core of this volume. Each contribution stands for a different paradigm or approach to the question of how to conceptualise and deal with the history of sociology. Thus, Randall Collins defends an extreme model of sociologization. For him biographies of sociologists are mere results of interaction chains and ritualised networks that primarily serve the purpose of accumulation of cultural and academic capital. The result of the emerging discourse is predictable: a kind of sociological duplication or reflection of the social world and the networks in which sociologists move and encounter each other. In such a conceptualization creativity is not something that is the property of individuals but the predictable outcome of interactions in more or less distinguished academic networks. In other words, tell me to which citation cartel you belong and I will tell you what your chances of 'discovery' and academic capital are.

Like Collins, Andrew Abbott is also interested in duplication but adds that it is also necessary to explain variations within the discipline. For him the answer to the question of why there is so little observable progress and so much variety is that sociology has never and probably never will be a science in the narrow sense of the word. It is more likely to remain an academic discipline which deals with observed facts but that includes their interpretation, too. Inevitably a myriad of epistemological interests and observable facts lead to numerous different meanings and explanations. Some may have a syntactical dimension, i.e. they are relational; some are more likely to remain semantical and refer mainly to meaning-making; while others will be more pragmatic in orientation, i.e. they will remain limited to and bound by action. If that description is true then we have to conceptualise the discipline in a rather different way and think of it perhaps more in terms of a configuration that resembles a number of fractals, i.e. smaller structures that are contained or reproduced in larger structures (Abbott invokes the image of the fern to explain the nature of fractals). There is simply no cumulative process to speak of.

Charles Camic's response to Collins and Abbott is that both approaches don't make for good history writing. Change is an essential historical fact that can't be omitted; however, neither Abbott's fractals nor Collin's ritualised networks and academic communities are able to account sufficiently for the fact that once in a while something new comes into being. Camic is

good in criticising what is left out in Collins's and Abbott's approaches but doubts must remain as to his (and Neil Gross's) alternative proposal, i.e. the new sociology of ideas that is more perceptive of the role of characters and personalities. On first impression the thick description that the new sociology of ideas promotes sounds exciting; yet, as we have seen for example in the case of Gross's intellectual biography of Rorty, thick description can consist of recounting endless banal facts and minor events in order to make a point that could have equally been made in much more concise fashion.

George Steinmetz's and Johan Heilbron's contributions to the discussion add more colour to what would otherwise remain a pretty dry sociological-historical program. Steinmetz reminds the reader of what is often omitted from the discipline's history – colonialism –, while Heilbron provides evidence that sociology even in its early days as a discipline did not develop in concentric circles – let's say from an inner core to the periphery and then projected onto the global stage – but instead relied on international networks.

The remaining contributions from Part III of the book suggest some fine-tuning that any proper history of the discipline might want to consider. Picking up from Fleck's agenda-setting essay in Part I Martin Endreß launches a fundamental critique of what he calls 'presentism' by which he means a kind of analysis that relies on the construction of types and prototypes and in which everything is seen from the perspective of hindsight and in which the latest achievement always looks best simply by virtue of having come last. This makes for odd history writing. As Gerald Mozetič shows in his case study of Gumplowicz, one should avoid such intellectual traps and self-fulfilling prophecies. Many individuals (amongst them not a few that are now considered classics) did not live up to ideal-type disciplinary boundary maintenance of what would later come to constitute 'the discipline'.

While the book is highly readable and contains some excellent contributions, some omissions are evident. The volume itself reminds the reader that such disciplinary undertakings have the function of border maintenance. Who is 'in' and who is 'out'? There is quite a bit that is left out from this text: from Peter Baehr's discussion of the sociological canon, founding figures and classics, and his useful distinction between founders and discourses, to the Cambridge School (the contributions of Quentin Skinner and John G. A. Pocock in particular come to mind), the historical semantics approach of Reinhart Koselleck, the rhetoric devices as discussed in Hayden White or the performance-related sociological approaches from Erving Goffman to Jeffrey Alexander, to name just a few. From the perspective of enhancing our sociological imaginations *Soziologiegeschichte* delivers a basic but rather frugal package; for more exciting, imaginative and creative ways of writing a discipline's history one probably has to peep across the border and shop around in other departments and disciplines.