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Max Weber never was mainstream,—but who made him a classic of sociology?

Dirk Kaesler
kaesler@uni-marburg.de

Abstract

If by “mainstream sociology” one understands a specific paradigm that dominates the discourses of sociology because its adherents form the majority of the discipline, then Max Weber’s program of a sociology that is built upon “interpretative understanding of social action and thereby with a causal explanation of its course and consequences,” was never mainstream, not during his lifetime (1864–1920) or since. So why has he become this overpowering “classic” of sociology? Who is responsible for this development?

This paper does not aim to reconstruct the entire impact of Max Weber upon the history and presence of sociology since his death. Starting with the observation that he has been made into an indisputable “classic” of international sociology this paper, first aims to depict the sharp contrast between the weak and very selective impact and reception of Weber’s work during his lifetime and its significance for international sociology today. Second, it attempts to explain Weber’s career as a classic of sociology, having become—along with Marx and Durkheim—one of the pillars of a “Holy Trinity” in international sociology. Thirdly, after offering a general overview of forty-three individuals who may be regarded as mainly responsible for the fashioning of the sociological classic Max Weber, it concentrates upon the three most important figures by sketching their quite distinct ideas, interests and roles in this endeavor.

Keywords

Max Weber; mainstream sociology;
“classic” of international sociology this paper, first aims to depict the sharp contrast between the weak and very selective impact and reception of Weber's work during his lifetime and its significance for international sociology today. Second, it attempts to explain Weber's career as a classic of sociology, having become—along with Marx and Durkheim—one of the pillars of a “Holy Trinity” in international sociology. Thirdly, after offering a general overview of forty-three individuals who may be regarded as mainly responsible for the fashioning of the sociological classic Max Weber, it concentrates upon the three most important figures by sketching their quite distinct ideas, interests and roles in this endeavor.

A man and his work (almost) condemned to oblivion: Reception and influence of Max Weber during his lifetime

When the body of the 56 year old Full Professor of Gesellschaftswissenschaft, Wirtschaftsgeschichte und Nationalökonomie of the Staatswirtschaftliche Fakultät of Munich’s Ludwig-Maximilians-Universität, Max Weber, was cremated at Munich’s Ostfriedhof on June 17 1920, only a small number of family members, friends, colleagues and students attended.

Very few of them would have imagined then that Max Weber would become the German sociologist who would rank as the most important founding figure of a continuous tradition in current international sociology. On that day in the Spring of 1920, only his widow, Marianne Weber, might have been convinced, it became at least the aim to which she herself would devote considerable effort; to make the work of her late husband known and important.

Today it goes without saying that Max Weber has been made into an indisputable “classic” of international sociology. No dictionary, no history of sociology and no relevant sociological textbook would fail to make prominent mention of his name and to stress his crucial significance for the development of the discipline. Since this rescue from oblivion, the triumphal march of this early German sociologist continues. For some decades the work of this Wilhelminian scholar has been deemed essential to international sociology. Since the end of “real socialism” and the farewell to its masterminds, Marx, Engels, and Lenin, interest in Max Weber—who has so often been categorized as the “bourgeois Marx”—seems to have grown even further. Weber's work, which had been interpreted by Marxist scholars and by anti-Marxist interpreters alike as an opposing challenge to Marx’s work (Bader et al. 1976; Böckler and Weiβ eds. 1987), has been presented as the historical “winner” of what has been far more than merely an academic debate.

From a sociologist’s point of view it would be more than naive to assume that this development, from a marginalized scholar shortly before the beginning of Weimar Germany to the internationally overpowering classic of international sociology of today, has been the result of the intrinsic value of Weber’s writings alone. From a good Weberian perspective in particular we have to ask ourselves (a) who was responsible for this gradual fashioning of the sociological classic Max Weber, and (b) what were their “interests”—both “idealistic” and “material”—in so doing? Who were the people without whom we would not currently have access to the published work of this scholar, as well to the preconceived knowledge that we are dealing with the ideas of one of the most important sociologists, if not thinkers, of the 20th century?

The present relevance of Max Weber’s work contrasts sharply with his national and international reception and impact during his lifetime. If one looks at the early reception of Weber’s writings as a whole it shows extreme selectivity. It concentrated almost exclusively on the Protestant ethic writings
(1904/5; 1920) and the printed versions of the lectures on *Wissenschaft als Beruf* (1919) and *Politik als Beruf* (1919). After Weber's death in 1920, even Marianne Weber's successful attempts to bring most of the scattered and mostly unfinished texts to the attention of a wider readership with her construction of four collected volumes—*Gesammelte Politische Schriften* (1921), *Gesammelte Aufsätze zur Wissenschaftslehre* (1922), *Gesammelte Aufsätze zur Sozial- und Wirtschaftsgeschichte* (1924) and *Gesammelte Aufsätze zur Soziologie und Sozialpolitik* (1924)—did not alter much of the basically weak reception and influence of Weber's writings during the period leading up to World War II.

Even Max Weber's so-called “magnum opus”, *Wirtschaft und Gesellschaft*, when it was first published in 1922 encountered an echo, which was rather too weak to be worth mentioning. With very few exceptions, it immediately fell prey to a strong influence, which was segmented according to disciplines. The influence of Weber's formulation of a program and a methodology of scientific, interpretative and empirical sociology appears not to have reached out beyond its influence on Werner Sombart, Georg Simmel and, only later, Alfred Schütz. These three scholars, however, were outsiders to the academic and professional institutionalization of the young university discipline of sociology in Germany in their time (Käsler 1984). Indeed, the single fact that during the period from 1922 to 1947 fewer than 2,000 copies of “Economy and Society” were sold, illustrates my argument. The same findings present themselves if one looks at the quite unimpressive impact Max Weber made as an academic teacher. The very small group of people who wrote their dissertations under his guidance did not achieve any relevant scholarly importance and none of his very few “pupils” wrote their Habilitation under his supervision. Max Weber had no successors in any strict sense; a “Weber school” founded by Weber himself did not exist.

**Weber's career as a “Classic” of sociology in Post-war (West) Germany**

Immediately after the end of World War II and after the reopening of (West) German universities it was not so much the German sociologists of the Weimar period, such as Ferdinand Tönnies, Max Weber, Werner Sombart or Georg Simmel, who were read in (West) German sociology but rather the “modern” American sociological writers. It was regarded as the task of the time to connect with mainstream Western sociology, and this formed an important part of the “re-education” policy to be achieved by the (re) establishment of sociology in (West) Germany.

Very few scholars took Max Weber seriously in those years of the German *Wirtschaftswunder*. With the rare exceptions of Friedrich H. Tenbruck, then University Assistant at Frankfurt University, and Johannes F. Winckelmann, retired vice-president of *Hessische Landeszentralbank* who lived near Munich as a private scholar and as late as 1963 was made *Honorarprofessor* of Munich University, dominant German academic sociology was more preoccupied with research on other topics. Let me mention as prominent examples research on the (supposedly) vanishing German class structure (“*nivellierte Mittelstandsgesellschaft*”) by the powerful Helmut Schelsky (Hamburg, Münster, Dortmund), research on the dynamics of the German family and the empirical reality of German industrial factories by the influential René König (Köln), or research on the “Dialectics of Enlightenment”, undertaken by the Frankfurt sociologists, Max Horkheimer und Theodor W. Adorno, after their return to Germany.

It took the 1964 convention of the *Deutsche Gesellschaft für Soziologie* (German Sociological Association) to confront German sociologists with the very different state of Weber's international
reputation. The “Makers” of international sociology gathered in commemoration of their German progenitor: Talcott Parsons (Harvard); Pietro Rossi (Turin); Raymond Aron (Paris); Herbert Marcuse (Boston); and Reinhard Bendix (Berkeley). They all celebrated—not without some critical remarks—the very man who, without any significant contributions by German sociologists, since the more than forty years since his death had gradually become universally acknowledged as a major figure of international sociology. Only through the concerted efforts of these foreign or exiled scholars had the German scholar Max Weber become—together with Marx and Durkheim—one of the pillars of the “Holy Trinity” of international sociology.

The Harvard sociologist Talcott Parsons was mainly responsible for this development, which transformed this German sociologist who had died shortly after the end of World War I and had almost become forgotten at the beginning of the 1950s into an internationally reputed Master of Sociological Thought. It was Parsons’ structural functionalism that had become the internationally dominant theoretical paradigm of sociology from around 1950 until 1965. Parsons, as one of the central figures of this development by his own writings, in particular “The Structure of Social Action” (Parsons 1937), and by his own translations of the “Protestant Ethic” (Weber 1930) and of the first part of “Economy and Society” (Weber 1947) drew this universal attention to Weber and through this created such international involvement with his work.

Regardless of one’s position vis-à-vis Parsons’ interpretation of Weber, it must be stressed that it was Parsons’ work that first aroused broad international interest in Max Weber. Although Parsons’ translations offered sufficient scope for improvement—so that his interpretation of Weber later necessitated a “de-Parsonization” of Weber (Cohen, Hazelrigg, and Pope 1975a; Parsons 1975; Cohen, Hazelrigg, and Pope 1975b; Parsons, T. 1976)—this in no way detracts from his historical importance in promoting Weber as a sociological classic. Even in Germany, the broader “re-discovery” of Weber after World War II was only set in motion by the reception of US-American structural functionalism.

The effect of this belated (re)discovery of Weber’s work became immeasurably more influential than earlier attempts by those sociologists who had in their own works, in some cases with great emphasis, tried to utilize Weber’s categories and approach and who were convinced that Weber should rank among the more important sociologists. In connection with the period between 1920 and 1945, I would like to mention Karl Mannheim, Siegfried Landshut, Hans Freyer and Alfred Schütz as being—at least in hindsight—of particular importance. They all mentioned Max Weber in their own writings most favorably and tried to enlarge on Weber’s perspectives and methodological approach. However, the impact of these authors upon the propagation of Max Weber as a classic of sociology was quite limited, to say the least. Mannheim, Landshut and Schütz were driven out of the German speaking academic system and do not seem to have propagated Weber in their new environments, and Freyer did not pursue his admiration of Weber after 1933.

It was only in the course of the (re)discovery of Weber after World War II that interpretations of Weber’s life and work became important. These derived, directly or indirectly, from persons and groups that were—more or less—directly influenced by Max Weber during his lifetime.

These groups involved people connected particularly to the Heidelberg period of Weber’s life, such as Marianne Weber, Karl Jaspers, Sieg mund Hellmann, Melchior Palyi, Karl Loewenstein, Eduard Baumgarten, Carl Brinkmann, Paul Honigsheim, Alexander von Schelting, Georg (von) Lukács, Helmut Plessner, Ernst Troeltsch, Theodor Heuss, Robert Michels, Hans Gerth, Max Rheinstein,
Ephraim Fischoff, and, with crucial peculiarities, Johannes Winckelmann. We shall return to two of them in the third part of this paper.

Through these thinkers, who were directly or indirectly influenced by Weber himself, and who therefore still stood under the spell of Max Weber “the man”, or rather “the myth of Heidelberg”, a glorification and stylization of Max Weber as an “intellectual aristocrat”, a “titan”, a “demon”, and a “genius” arose which made a distanced and critical view difficult and which hindered an unbiased approach to Weber’s work rather than facilitated it. Moreover, the majority of these editors, translators and interpreters were not sociologists. The interest in Weber’s universal historical framework was frequently a distraction from the sociological content of the work.

Adjacent to this rather emotionally tinted reception another discussion arose which became very influential for the post-war reception of Weber in Germany. It was during the Heidelberg sociological convention that the then relatively young Cologne historian Wolfgang J. Mommsen presented the main theses of his dissertation (Mommsen 1959), in which he hinted at a link of ideas between Max Weber’s concept of “plebiscitarian leadership democracy” (plebiszitäre Führerdemokratie) and the ideological development of the Nazi state. And it was in Heidelberg that the then Associate Professor of Philosophy, Jürgen Habermas, who was also quite young at the time, joined Mommsen’s side: “Wir hier in Deutschland, immer noch auf der Suche nach Alibis” cannot forget, he exclaimed, that Weber’s vision of a “caesaristic leadership” has had disastrous consequences in Weimar Germany: “Wirkungsgeschichtlich betrachtet, hat das dezisionistische Element in Webers Soziologie den Bann der Ideologie nicht gebrochen, sondern verstärkt.” (Habermas 1965: 81)

It took the 73-year-old Karl Loewenstein, Max Weber’s student during his Munich period, to rise during the Gedächtnisfeier of Munich University one month after the Heidelberg convention and defend his revered teacher against such “audacious historical smearing” by “certain young people” equipped with a “substantial degree of intellectual dishonesty” (Loewenstein 1966: 142). Without any doubt, this so-called Ahnherrschaftsdebatte (debate over ideological ancestry) has impregnated the history of Weber’s reception within German post-war sociology quite measurably.

This brings us to a further characteristic of today’s picture of Weber the “classic.” While noting the contrast between the relative lack of impact and “failure” of Weber during his lifetime and his eminent international prominence and “classicism” since 1964, it is nevertheless remarkable that his reception today is still characterized by a surprisingly high degree of selectivity. It is still mostly only that part of his work which was published after 1904, i.e. the famous “Protestant Ethic” and the article on “Objectivity” (1904), which are generally recognized in sociology. The division of Weber’s life into distinct periods as lawyer, agrarian historian, political economist, religious expert, cultural historian, sociologist, philosopher, politician, social researcher, academic theoretician etc. denies its demonstrable continuities and consequently makes a comprehensive understanding difficult, even for many so-called Weber-experts.

This problematic pattern of reception is particularly effective in two different ways: (1) Weber’s plan for an “interpretative sociology” has been separated from his substantive work, and has been dealt with in isolation and misunderstood as “the” Weberian sociology; and (2) the whole wealth of

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1 “From an historical point of view, the decisive element in Weber’s sociology did not break the spell of ideology, but strengthened it.”
research material has been detached from Weber's writings on the methodology of the social sciences such that the two are not understood as being linked to one another.

Another outcome of this high degree of selectivity is the fragmentation of the entire oeuvre into so-called “instructive pieces.” It is doubtless this that leads us to today's quantitatively most important impression of Weber the sociological “classic”. No internationally recognized work in the sociology of bureaucracy, domination, music, religion, the city, or political parties etc. will fail to mention the name of Max Weber as one of the decisive historical precursors of social science. The overwhelming majority of such ritualized obeisance before Weber the “classic”, however, has no function other than that of legitimizing its own undertaking. Weber the sociological classic serves to establish the identity in both content and methodology of a discipline, of a research intention and of the writer.

The present state of the German reception of Weber

Almost 100 years after Weber's death we can state clearly that Weber's work has passed the “test of time”. Since 1945 we can see a preoccupation with Weber, which has been gradually growing internationally. This continuing and increasing reference to Weber's works, and the equally strengthened concern with him, is not actually a renaissance. Weber's reception and influence during his own lifetime was much less strong and “canonical” than in the period after 1945. Weber did not stand at the center of his contemporary sociological discourse, which stands very much in contrast to his present status. Questions can even be asked as to whether Weber himself would have liked the development that made him and his work a classic of an academic discipline called “sociology”!

The present reception of Weber in Germany has been shaped mainly by two developments: the ongoing production of the collected edition of the Max-Weber-Gesamtausgabe (MWG); and the ongoing debate between the disciplines about to which Max Weber “really” belongs.

After some preparatory talks in the autumn of 1974, a group of main editors of a new and complete edition of Max Weber's writings, letters and lectures was founded in June 1975. The original board consisted of the sociologists Horst Baier (Konstanz) and M. Rainer Lepsius (Heidelberg), the philosopher Hermann Lübke (Zürich), the historian Wolfgang J. Mommsen (Düsseldorf), the sociologist Wolfgang Schluchter (Heidelberg) and the private scholar Johannes Winckelmann (Rottach-Egern). In May 1981 a first outline of the edition, explaining the general design and the state of preparations of the MWG, was published; a revised version of this prospectus was published in February 1984. It indicates three sections—Writings and Speeches, Letters and Lectures—with 22 volumes for the section “Writings and Speeches” alone! After Lübke's withdrawal and Winckelmann's death in November 1985, the remaining editors were Baier, Lepsius, Mommsen and Schluchter who bore the responsibility for this enormous undertaking, which has received considerable support from Deutsche Forschungsgemeinschaft, Werner-Reimers-Stiftung, Bayerische Akademie der Wissenschaften and the Tübingen publishers, Mohr-Siebeck. With this substantial support, and an impressive amount of intellectual and material resources at the Bayerische Akademie der Wissenschaften in Munich and the Arbeitsstellen of the remaining editors—of whom Lepsius and Mommsen have passed away during the last years—42 volumes have been published to date.

Not least with regard to various reactions to this editorial enterprise, another aspect of the present reception of Weber can be illustrated; Max Weber still remains an “embattled classic”. His work is
hotly contested within sociology as well as between disciplines, in particular between sociology and its neighboring disciplines such as history, philosophy and political science.

Of particular note was the discussion within German history during the “International History Congress” in Stuttgart in 1985, when the importance and potential use of Weberian concepts and findings for present and future historical research were debated and the papers given were later published in a collection (Kocka ed. 1986). Long before this event, though, Weber had been used as a legitimizing figure within newer German social history in its fight against traditional historical approaches as well as against some more quantitative, systems theory oriented approaches. Even today, Weber quite often serves to defend social history against predominantly narrative methods of presentation adorned by the label of “neo-history.” In this context Weber quite often helps as a model for synthesizing great masses of single case studies into a historical panorama of Gesellschaftsgeschichte from the perspective of universal history.

Next to this discussion between disciplines about the “right” use of Weber's work and methods, another discourse can be identified, namely that within and between different national, sociological interpretative communities. As a good representation of this considerable discussion, attention may be drawn to a collection of essays that have become quite influential within the German debate. It comprises the papers presented at a conference of the “Theory Section” of the German Sociological Society in Kassel in June 1986 (Weiß 1989). In this volume a provisional summary has been attempted of the present state of work on Weber in several areas.

Ever since there has been a—sometimes quite passionate—fight over the somewhat cryptic question as to who might best administer Weber's heritage. This revolves around the issue of the “correct” disciplinary location of Weber's work as a whole, as well as around some more specific questions such as whether Weber was a tragic, pessimistic Nietzschean (Hennis 1987: 167–191) or a “Liberal” with high regard for the British model of liberal personal development (Mommsen 1989). As in Heidelberg in 1964, the 1986 Kassel conference turned into a battle for the figurehead claimed by several disciplines within the realm of the social sciences. This battle has been fought among sociologists, historians, philosophers and political scientists over who actually has a legitimate claim to Max Weber; this internationally acclaimed saint of wisdom.

Besides the significance as a totem offering identity to several disciplines and groups of scholars, Weber has gained some political symbolic value for another historical debate in Germany. After the self-dissolution of the “German Democratic Republic”—and with it the project of a Marxist-Leninist social science in Germany—the image of Max Weber as the prototype of a “bourgeois sociologist”, and by that almost a professional anti-Marxist, collapsed. Starting as far back as Georg Lukács’ 1954 Die Zerstörung der Vernunft (Lukács 1954; Kaesler 1997), this image that had been reproduced for decades was that of the most important social scientist to have been produced by the German bourgeoisie, but one stigmatized as “anti-Marx” or, at best, as a “negative genius” (Jürgen Kuczynski).

Under the banner of the necessary “appropriation of our whole heritage of learning” (Helmut Steiner), shortly before the self-dissolving of the GDR in 1989 even Marxist-Leninist sociology in East Germany had just begun to cautiously approach the person and work of Max Weber from a position other than hostility. At a conference in Erfurt on the occasion of Weber's 125th birthday, a collection of papers on Weber in the then influential Deutsche Zeitschrift für Philosophie (Küttler et al. 1989) and the first publication of the most important texts of Max Weber (Weber 1989) were proof of a change of thinking. Suddenly the work of Max Weber included “some important suggestions and
ways of thinking that may be worthwhile for a Marxist to accept critically.” (Küttler and Hauer 1989: 6). With the de facto disappearance of Marxist-Leninist sociology in Germany, this freshly developed line of an alternative reception of Weber in Germany also vanished.

**Who fashioned the sociological “Classic” Max Weber, what were their driving interests, and which roles did they play?**

In this chapter I intend, first to present a general overview of those individuals who—in my opinion—have to be mentioned as those responsible for the fashioning of the sociological classic Max Weber, and second, to concentrate upon three individuals among them who—in my personal view—have to be seen as absolutely crucial in this endeavor. With reference only to these three central “makers”, I shall make a few remarks about their quite distinct ideas, interests and roles in this undertaking.

From any established sociology of science perspective, not in the least from a Weberian angle, we ask ourselves: Which persons are accountable for the gradual fashioning of the “classic” of sociology, Max Weber? What were their “interests”—“idealistic” and “material”—in so doing? Who were the people without whom we would not have access to the published work of this German scholar today, but also the preconceived knowledge that we are dealing with the work and ideas of one of the most important thinkers of the 20th century, at least for sociology? What kind of roles did these people play in this concerted effort, not without bitter fights among them?

In order to give a first, preliminary—and incomplete—answer, I would like to offer an attempt of compiling some sort of “pedigree” of the “makers” of Weber, the sociological mastermind.

In this “pedigree” one finds forty-three individuals, one woman and forty-two men, without whom, I think, we would not be dealing with the sociological classic, Max Weber, today. As one can see, they come from different national backgrounds, although naturally with a strong German bias, they belong to a broad spectrum of academic generations, ranging from 1870 to 1950 as well as to a broad spectrum of disciplines.

Of course, it would be quite tempting—and not totally without some delicate debates—to comment upon the individual contribution of each of the forty-three. However, and not merely for reasons of limited space, I shall not venture into this controversial task.

I prefer to concentrate instead upon the three people whose most crucial role in the gradual fashioning of the sociological “classic” Max Weber is beyond question: Marianne Weber; Johannes Winckelmann; and Talcott Parsons. Without these three individuals, two of whom were not even professional academic scholars, you would not be reading a paper discussing Max Weber the sociological mastermind.

It is, of course, with **Marianne Weber** that the story of the gradual fashioning of the classic Max Weber must begin. Her role could be characterized as that of **trustee**.

She was not only the caretaker of a heritage that had been left behind, however. Indeed, it was she who created this heritage in the first place. Without her producing the four collections mentioned, together with the (re) construction of the three volumes of his writings on world religions, and the construction of *Wirtschaft und Gesellschaft*, there are legitimate questions as to whether the very idea of a Weberian oeuvre would have developed at all. We should never forget that when Max Weber
died in June 1920, only two books bearing his name stood on the bookshelves of his contemporaries: his dissertation of 1889 (Weber 1889); and his Habilitation-thesis of 1891 (Weber 1891).

This is not the right place to go into too much detail about who this woman was and what her interests were in devoting her whole life after Weber's death to the creation and adoration of her late husband's work and life. She was convinced of the ultimate success of her endeavors without the slightest doubts: “Sein Ruhm ist meines Erachtens erst im Beginn seines Aufstiegs. Die Menschen werden erstaunen, wenn sie seine Werke (10–12 Bände) mit Händen greifen. Ich lebe für seine irdische Verewigung.”2 (Baumgarten 1964: 605). The salvation of the greater part of the unfinished bits and pieces and putting them together so as to present an (almost) finished “work” was the one major contribution of Weber’s widow. Next to this (re)creation of a voluminous scholarly work stands the portrayal of the life of the author of this great work, the portrayal of an outer and inner development of a scholarly mastermind by writing and publishing the Lebensbild in 1926 (Weber Marianne 1926).

Not only are the achievements of Marianne Weber well known, we also know about the problems involved in these two roles, editor of the collected works and portrayer of the Lebensbild. The Max-Weber-Gesamtausgabe has successfully begun to take care of many problems relating to her role as editor. As to Marianne Weber’s portrayal of Weber’s life, several attempts to supplement hers have been under way; the “touch up” of Weber’s life in her great book required correction and this has been effected (Kaesler 1989; Kaesler 2000; Kaesler 2014).

What Marianne Weber had started immediately after Weber’s death, Johannes Ferdinand Winckelmann continued after 1950. His role could also be characterized as that of a trustee, but in his case it was much more the role of promoter of Max Weber’s work and its importance during the 1950s, and right into the late 1970s.

As Johannes Winckelmann may not be as familiar as Marianne Weber and Talcott Parsons, allow me to elaborate him more than the other two. As someone who attended the seminars of Winckelmann during the greater part of his own studies at Munich University (1965–1972), I think I am in a position to judge upon the ideas and interests that motivated this self-declared caretaker and promoter of Max Weber’s work. It had been his firm conviction that Weber’s work would offer a better understanding of the universal historical development of modernity than any other sociological concept, in particular any Marxist approach. His sole interest in Max Weber concentrated upon the writings, his interest in the life of Max Weber, in particular in Weber’s private life (“Tantengeschichten”) was close to nil, and his criticism of any such attempts tended to become fierce.

How did this former judge and life-long civil servant—first in various courts of the Freie und Hansestadt Hamburg (1927–1938), then in the Reichswirtschaftsministerium (1938–1945) and then at the Hessische Landeszentralbank (1946–1951)—get so much on the Weber-track that even as early as November 1945 he was able to write of himself: “Ich betrachte mich als Schüler des weit über Deutschland hinaus bekannten demokratischen Hochschullehrers Professor Dr. Max Weber, dessen wissenschaftliche und politische Lehren ich mir weitgehend zu Eigen machte, da sie meinen

2 “His fame is, in my opinion, only at the beginning of his ascent. People will be astonished when they hold his works (10-12 volumes) in their hands. I live for his earthly perpetuation.”
According to his own account it was as a first-year student at Marburg University in the summer of 1919 that he had read the two small brochures of *Wissenschaft als Beruf* and *Politik als Beruf* and through them had found his “way to Weber”. His desire to study under Weber in Munich could not be fulfilled, because when he arrived there in Wintersemester 1920/21 Weber had already died. This did not stop his eager interest in Weber’s work such that by May 1925 he had already initiated a correspondence with Marianne Weber about his own editorial suggestions (Borchardt 2000: 16), but things had to wait until the end of World War II. As early as 1949 we find his first Weber publication (Winckelmann 1949), then as editor of the second edition of *Gesammelte Aufsätze zur Wissenschaftslehre* in April 1951 he eventually entered the arena of Weber-scholars. By 1985, the year of his death, we were able to reconstruct the enormous achievements of this restless, influential and tricky propagator of Weber’s work, who created a whole network of institutions—such as the Max-Weber-Gesellschaft, the Max-Weber-Archiv (1960–1966), the Max-Weber-Institut (1966–1976), and the Max-Weber-Arbeitsstelle at the Bayerische Akademie der Wissenschaften (since 1978). And the story could—and should—be told of his astonishing success at spinning personal networks that went up as high as the former Bundespräsident Theodor Heuss. I shall abstain from this story for today, and once again not only because of restricted space.

It may be said as some sort of interim balance, however, that Winckelmann’s success in terms of the academic level in promoting and propagating the importance of Max Weber as a classic of sociology in post-war (West) Germany altogether was quite limited. Although Winckelmann’s editions were on the market with the two volumes of the “Protestant Ethics”-texts becoming best-sellers, as well as his most influential collection of Weber-texts with Kröner-Verlag (First edition 1956, 6th edition 1992), with almost 50 000 copies sold, it was not until 1964 that Weber attained a certain degree of importance in academic and even public circles. But Winckelmann fought with growing success with his editions, his institutional letterheads and by creating a complex network of people and institutions that supported the gradual institutionalization of research on Max Weber. The beginning of the work on the *Max-Weber-Gesamtausgabe*, with its Geschäftsstelle at the Bayerische Akademie der Wissenschaften in Munich, and the creation of the network of the additional four Max-Weber-Arbeitsstellen, in many ways marked the climax of his endeavors over the last twenty years of this life, devoted to Max Weber. (Kaesler 2006)

To sum up Winckelmann’s role and motives in contributing to the fashioning of the sociological classic Weber, one might best quote from a letter by Karl Loewenstein dated June 14, 1963 in which he supported the plan to promote the then private scholar Dr. Winckelmann to Honorarprofessor of Munich University. Loewenstein wrote: “Herr Dr. Winckelmann hat [...] dazu beigetragen, einen der Großen des deutschen Geisteslebens der Nachwelt schlackenfrei zu übermitteln. Nur diejenigen, die sich selbst mit dem monumentalen Werk Webers beschäftigt haben, können ermessen, welche

3 “I regard myself as a student of Professor Max Weber, a democratic scholar well known beyond Germany, and whose scientific and political doctrines I have largely embraced because they corresponded to my own intentions and experiences and were directed by a global point of view.”
It is not necessary to say as much about Talcott Parsons, the third major “maker” of the sociological classic Max Weber, as about Winckelmann. The role of Parsons and his structural-functionalism for international sociology has been researched in detail. Without this man and his work it is impossible to understand Western sociology during the period from 1940 to 1970. And, as mentioned before, during those thirty years the name of Max Weber and Parsons’ interpretation of Weber’s work, together with his own translations of central Weber-texts into English, became (almost) as important as Parsons himself and his work. In order to describe Parsons’ role in the fashioning of the sociological classic Max Weber we propose to see in him the—most influential—interpreter of the sociologist Weber.

What were Parsons’ interests in this? The story of him utilizing Weber’s analysis of the origins and effects of capitalism during the first phase of his foundation of his own dealing with capitalism is well known (Jensen 1980: 12–14), as are his (re)constructive attempts to synthesize the works of Emile Durkheim, Max Weber, Vilfredo Pareto and Alfred Marshall, to which he later added a whole set of additional European thinkers such as Freud, Malinowski, Radcliffe-Brown, and Piaget. Instead of reproducing the familiar account of the theoretical and strategic use Parsons made of Weber, as one of those European thinkers for the foundation of his own action-theory based approach I prefer to turn to his re-import of Weber to Europe, and Germany in particular, and to a story reconstructed by our Heidelberg colleague Uta Gerhardt:

Parsons, together with Reinhard Bendix and Benjamin Nelson, had decided in 1962—two years prior to the Soziologentag—that they wished to contradict the critical view on Weber as it prevailed in the early 1960s in Germany. Parsons together with Bendix and Nelson arranged a meeting with Otto Stammer, then President of the German Sociological Association and convenor elect of the Heidelberg Soziologentag, two years in advance of the Soziologentag. On the occasion of the Sixth Congress of the International Sociological Association, which took place in Washington in 1962, Bendix, Nelson, and Parsons met Stammer (whom they had invited to Washington by the President of ISA, for the purpose), to guarantee that their contributions were to be placed in prominent positions in the Soziologentag programme. They corresponded with each other since 1961, to plan their action that was to counteract the criticism against Weber to be expected at Heidelberg from speakers related to the Frankfurt School. (Gerhardt 2001: 338)

Very much in contrast to the Gedächtnisfeier of Munich University of June 1964, which—mainly due to the persistent endeavors of Johannes Winckelmann—became virtually a university initiative, the Heidelberg Soziologentag appears to have been an event mainly engineered by Parsons, together with Reinhard Bendix and Benjamin Nelson, and only then supported by Heidelberg University and the Deutsche Gesellschaft für Soziologie. So much for the state of Max Weber as a mastermind of German sociology in the 1960s!

I shall end this presentation with these short remarks about the three most important makers of the sociological classic Max Weber: Marianne Weber the trustee; Johannes Winckelmann the promoter; and Talcott Parsons the interpreter and international propagandist. These three individuals in their

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4 “Dr. Winckelmann has [...] contributed to the transfer of the work of one of the greater ones of German intellectual life to posterity. Only those who have occupied themselves with the monumental work of Weber can appreciate the devotion and self-emptying of his whole life in the service of Weber’s image.”
distinct roles can be held chiefly responsible for the development that has led to our seeing in Max Weber—whose work stood in the serious danger of descending into oblivion at the time of his premature death—one of the most important thinkers of the 20th century, perhaps quoted more often than Karl Marx these days.

It might be worth noting, though, that if we look at the whole gallery of those people who began this propagating Weber’s work before 1964, many of them were academic “outsiders” for whom upholding Weber’s legacy also became a matter of personal legitimization, mostly with very limited success. The almost tragic figures like Eduard Baumgarten, Hans Gerth, Benjamin Nelson and, to a lesser degree, Johannes Winckelmann might prove this suspicion. Could it be, one might ask, that these outsiders themselves felt some sort of Wahlverwandtschaft, an “elected affinity”, to a man who, for the greater part of his own life, had been an outsider and Querdenker himself, and whose scholarly career during his own lifetime also was more of a failure than a grand success?

Another concluding thought. Taking a sociology of science approach, it would be rewarding to go through the whole list of those forty-three individuals identified as of prominent importance for the fashioning of the sociological classic Max Weber and regroup them in an ideal typical way along at least two factions. There are those who saw the preservation and cultivation of Weber’s work alone as their main aim of their dealing with his work. This, of course, started with Marianne Weber and carried on, with Winckelmann as someone of particular importance after World War II. One might also distinguish from this group of Weber-scholars those whose main aim was the further development and continuation of Weber’s theoretical achievements by integrating it into their own theoretical designs. This strand of Weber–scholarship had, of course, its most prominent and influential representative after World War II in Talcott Parsons, and in more recent times names like those of Jürgen Habermas, Pierre Bourdieu and Anthony Giddens might well be worth mentioning.

Looking at these two groups, one might ask how well communication—and reciprocal appreciation—between them has been developed, and whether one might not state an imminent danger of them drifting apart, and away from fruitful cooperation?

In any case both of these “invisible colleges” of international Weber-scholars—those who care more for the cultivation and historical contextualization of Weber's work, as well as those who concentrate more upon a theoretical continuation of Weber’s work—prove that Max Weber may quite rightly be called “a living classic”!
References


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The fabrication of the sociological classic Max Weber

1920–1945

MARIANNE WEBER (1870–1954)
Melchior Palyi / Karl Loewenstein / Jörg von Kap-herr / Siegmund Hellmann
Karl Jaspers (1883–1969)
Alexander von Schelting (1894–1963)
Karl Mannheim (1893–1947)
Siegfried Landshut (1897–1968)
Hans Freyer (1887–1969)
Alfred Schütz (1899–1959)

Eduard Baumgarten (1898–1982)

JOHANNES F. WINKELMANN (1900–1985)

TALCOTT PARSONS (1902–1979)

1945–1964

Reinhard Bendix (1916–1991)
Benjamin Nelson (1911–1977)
Hans H. Gerth (1908–1978)
Paul Honigsheim (1885–1963)

"Max Weber und die Soziologie heute"

Georg [von] Lukács (1885–1971)

Max Graf zu Solms (1893–1968)

Max Ernst Graf zu Solms (1910–1993)
Friedrich Tenbruck (1919–1994)

TALCOTT PARSONS
Jürgen Habermas (b. 1929)
Raymond Aron (1905–1983)
Wolfgang J. Mommsen (1930–2004)
Herbert Marcuse (1898–1979)
Die Gedächtnisfeier der Ludwig-Maximilians-Universität München, June 1964

Bernhard Pfister (1900–1987)
Johannes Winckelmann
Karl Bosl, Alois Dempf, Karl Engisch, Emerich Francis, Herbert Franke, Karl Loewenstein, Friedrich Lütge, Hans Maier, Jacob Taubes, [Eric Voegelin, Alfred Müller-Armack]

The German Weber-reception
after 1964

Johannes Winckelmann
Friedrich H. Tenbruck
Wolfgang J. Mommsen

M. Rainer Lepsius (1928–2014)
Wolfgang Schluchter (b. 1938)

The editors of the MWG since 1974

Horst Baier (b. 1933)
Gangolf Hübinger (b. 1950)
M. Rainer Lepsius († 2014)
Wolfgang J. Mommsen († 2004)
Wolfgang Schluchter (b. 1938)
Johannes Winckelmann († 1985)
ARTICLE

Towards the Expert Governance: Social Scientific Expertise and the Socialist State in Czechoslovakia, 1950s–1980s

Vítězslav Sommer
vitasommer@yahoo.com

Abstract
This article is concerned with the process of introduction and the further expansion of social scientific expertise in state socialist Czechoslovakia from the mid-1950s until 1989. It presents these developments in the context of the changing policy strategies of the Communist Party elites and describes how closely social scientific expertise, and social scientific knowledge production in general, was interconnected with the broader development of the state socialist governance from the post-Stalinism of the second half of the 1950s to the 1980s perestroika period. This text is structured around three crucial realms of the state socialist governance: state, economy and labor, and socialist society. The first part of the article is concerned with the expertise in the field of state, law and political sciences, which played a significant role during the late 1950s, when the socialist state-building project was finished. The following section focuses on the rise of social scientific expert culture during the reform communist period of the 1960s. The third part of this study analyzes how the reform communist expert culture was transformed by the post-1968 regime in a large expert apparatus in order to build strictly centralized technocratic governance. Finally, this article describes how social scientific expertise responded to the crisis and disorganization of state socialist governance during the perestroika period.

Keywords
Cold War; Czechoslovakia; experts; state socialism;

1 This article was researched and written with the support of the Czech Science Foundation (GAČR) as a part of the project GJ15-19437Y entitled “The Road to Technocratic Socialism: Concepts of Governance in Czechoslovakia (1953–1975)”.
The historiography of the Cold War is increasingly concerned with the interaction between policy-making and knowledge production. The so-called “Cold War social science” became the subject of numerous studies and its introduction in the historiographical agenda marked the transition towards more recent emphasizing of the cultural and intellectual dimension of the post-war period (Solovey and Cravens 2012; Isaac and Bell 2012; Gavin and Lawrence 2014; Engerman 2010). Seeing the “cultural turn” in research on the Cold War from a transnational perspective, a substantial part of the field focuses on the United States and Western Europe. As a result, there are numerous books and articles dealing with the emergence of specific Cold War social knowledge in the Western part of the divided post-war world. Some publications contained fundamental insights in the history of political science, psychology and, more generally, military-funded social scientific research (Engerman 2009; Erickson et al. 2013; Gilman 2003; Robin 2001; Ghamari-Tabrizi 2005). Moreover, scholarly literature concerned with domestic issues like economics or welfare shows how these were significantly influenced by the broader Cold War context and were on the research agenda of military-funded institutions like RAND, SORO, or the Office of Naval Research. Thus how important the Cold War agenda was for knowledge production after 1945 is well documented (Amadae 2003; Mirowski 2002).

The intellectual patterns of social thought in Cold War USA and, to a lesser extent, Western Europe are recognized and widely discussed by historians as well as scholars from related fields. The status of research on similar phenomena in the former Eastern Bloc is quite different, however. Scholarship on the history of expertise and social sciences in State socialism is undoubtedly growing, but it is still more appropriate to speak of a field ‘in the making’ than of a fully developed, well established area of scholarship (Brunbauer et al. 2011; Péteri 1998; Shlapentokh 1987; Sutela 1991). Similarly, transnational and comparative research on East-West contacts, knowledge transfers and research collaborations is promising, but nonetheless has been taken up only recently (Bockman 2011; Andersson and Rindzeviciute 2015).

The attempt to elaborate contextualized histories of the social knowledge production after 1945 would greatly benefit from a discussion of the Eastern Bloc perspective and its incorporation into the recent historiography of Cold War social science. This article presents a national case study of the relationship between the post-war State and social scientific expertise in Czechoslovakia. In what follows, the central issue is State socialist governance, characterized as a complex assemblage of governmental concepts, techniques and practices mobilized by the authorities in order to organize and govern the socialist State and society. Immediately after 1948, Communist Party authorities attempted to realize a Stalinist-style “great leap” towards a socialist society. In the mid-1950s, far-reaching and rapid nationalization of the economy, collectivization of agriculture, mass and violent political repression and a cultural revolution, which arose from the introduction of a Soviet aesthetic and intellectual standard in the arts and sciences, resulted in a serious crisis. This crisis manifested in economic decay and social unrest as well as in a decrease in the public legitimacy of the regime.

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2 The epistemological questions relating to the most recent research on the Cold War social science, primarily the issue of the research field formation, are discussed in Dayé (2014).

3 In the recent research on expertise, this specific activity is characterized as an “interstitial field” located among academic research, politics, media and other domains. As a hybrid and changing entity, the expertise is primarily characterized by its interventions in public affairs. In State socialist Czechoslovakia, social scientific expertise was institutionally based in various research institutions. For example, the most important institutional basis of the social scientific expertise in the 1970s and 1980s was the Institute for Philosophy and Sociology, which was a part of the Academy of Sciences. The expert knowledge was thus primarily produced by the academia and various research institutions funded by the State and affiliated, for example, to ministries or state-owned enterprises. For the sociology of expertise, see Eyal and Buchholz (2010).
Czechoslovakia thus followed a similar pattern to Poland and Hungary, the two neighboring State socialist countries that, at the time, were heading towards the great upheaval of 1956. Although Czechoslovakia did not experience the same level of turmoil as its two Central European allies, it was more than evident that a reconstruction of the state socialist governance was inevitable (McDermott and Sommer 2013; Blaive 2001).

As a reaction to this crisis, the Czechoslovak authorities simultaneously adopted and adjusted the “New Course” policy introduced by Nikita Khrushchev in the Soviet Union (Engerman 2004). Among the substantive features of this concept of socialist governance was the preference of consumption over the Stalinist emphasis on extensive growth and the heroic building of socialism. A general “scientization” of State socialism was also of the utmost importance. It was thus necessary to introduce more sophisticated methods of planning, management and organization at all levels of government. Science and technology were highlighted as active and, to some extent, decisive factors in building and governing the socialist State and society. In the realms of everyday life and cultural production these ideas resulted in the establishment of a distinctive socialist modernity, while in the sphere of policy-making the building of social scientific expertise and expert governance were the orders of the day (Reid and Crowley 2000; Pence and Betts 2008).

This article is concerned with the process of the introduction and further expansion of social scientific expertise in State socialist Czechoslovakia from the mid-1950s until 1989. My aim is to present these developments in the context of the changing policy strategies of the Communist Party and to describe how closely social scientific expertise, and social scientific knowledge production in general, was interconnected with the broader development of State socialist governance. In what follows I will not analyze the development of the individual social science disciplines themselves, but rather trace the translation of their findings from theoretical concepts to expert knowledge. The first part of the article is concerned with expertise in the field of state, law and political sciences, which played a significant role during the late 1950s. The next section focuses on the rise of social scientific expert culture during the reform communist period of the 1960s. The third part of this study analyzes how the reform communist expertise was transformed by the post-1968 regime into a large expert apparatus with the aim of building a strictly centralized technocratic governance. Lastly, I describe how social scientific expertise responded to the crisis and disorganization of State socialist governance during the perestroika period.

This text does not aim to provide an in-depth analysis of particular research projects, scientific controversies or research policies. Rather it strives to present a more general narrative of the relationship between social scientific expertise and socialist governance in post-war Czechoslovakia, which should emphasize continuities and longer trends rather than divide this history into strictly separated and incomparable periods as is more usual in previous historiography. This article argues that the effort to incorporate social scientific expertise in governmental strategies, which was significant for modern governance in general, was increasingly emphasized by the Czechoslovak State socialist regime in all its incarnations from the mid-1950s onwards, albeit with different purposes and results. In order to sketch this narrative, I will give preference to a more general account of long-term development, written primarily from the perspective of intellectual history,

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4 For the concept of scientization, see Brückweh et al. (2012); Wagner (2008).
5 The necessity to de-centralize the Czechoslovak economy by the introduction of more sophisticated and scientific planning methods was highlighted at the Communist Party’s most important public event in 1956, the so-called Nationwide Conference of the Communist Party of Czechoslovakia, held in June 1956. See Celostátní konference (1956).
which analyzes how the rationality of expert governance emerged from the mid-1950s and how it was transformed under reform communism and in the late socialist era after 1968.

**Building the Socialist State in Post-Stalinism**

In the case of Czechoslovakia, the historical period of High Stalinism was relatively short, lasting less than a decade, (from 1948 to 1953). It was characterized by the introduction of extraordinarily radical policy measures in all fields of government. The relationship between the State, the Communist Party and the social sciences was affected substantially by Stalinist policy that aimed to subordinate scholarship to the needs of the Communist Party policy. This resulted in the decline of entire disciplines and scholars were reduced to mere propaganda writers, not to mention the massive purge of universities and other research institutions (Connelly 2000; Voříšek 2012). This “Sovietization” of the social sciences helped to establish a fundamentally different academic culture and social scientific research became closely connected with the Communist Party apparatus.

In the mid-1950s, following the most radical phase of the socialist dictatorship, the political function of the social sciences was reformulated during the first critical discussions about the legacy of Stalinism. According to early post-Stalinist discourse, the social sciences were not seen solely as a propagandist exercise, but as scholarly fields producing knowledge relevant to political decision making. In the late 1950s, legal science was already created as a specific field of expertise that focused on the theory of law, the socialist legal system and the socialist State. This expertise was concerned with theoretical discussions that were politically highly relevant, as well as with direct interventions in the construction of the State socialist legal system.

The phenomenon of the modern State is a prominent object of social scientific examination. Since the 1980s, research projects rooted in various theoretical and methodological traditions from Marxism to governmentality studies have opened up new perspectives on the State and its political and social functions (Burchell et al. 1991; Evans et al. 1985; Jessop 2001; Steinmetz 1999). It is hard to imagine that the research on governance could ignore the modern State as a central object of inquiry. The historiography of State socialism, however, has generally been focused on the Communist Party as a hub of governance in the socialist dictatorship, more concretely on the hybrid governmental setting based on the merging of the party and the State apparatus. Although the centralized, strictly hierarchically organized Communist Party played the role of a sovereign political body, possessing indisputable authority to rule over the institutions of the State, the growing importance of the socialist State can be observed from the late 1950s onwards. In order to resolve the complex issue of the relationship between revolutionary socialism and the modern State, post-Stalinist legal expertise attempted to define a new arrangement of this uneasy partnership. According to the then current theory of State, socialism was developed from the revolutionary movement to the distinct State form. It was thus of utmost importance to theoretically elaborate this new governmental arrangement and to develop a functioning State socialist legislature different from the outdated legal framework of the bourgeois State.

It was significant for the relationship between social sciences and governance in post-Stalinism that the mobilization of legal experts and scholars was initiated by the highly theoretical and interdisciplinary discussion about the socialist revolution and the people’s democracy. The long-

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7 The Czechoslovak conceptualization of historiographical research in the Stalinist era was analyzed in Sommer (2011).
8 These early “revisionist” debates in Czechoslovakia, Poland and Hungary were analyzed in Kopeček (2009).
lasting controversy interwove issues ranging from scholastic contemplations about the Marxist-Leninist theory of revolution to the politically sensitive and highly controversial issue of the relationship between official Marxist-Leninist social theory, based on the particular Soviet experience, and its modifications, which were developed on the basis of distinctive national roads to socialism. This discussion was initiated by an article on the stages of revolution written by legal scholars Karel Kára and Jiří Houška (Houška and Kára 1954). The authors tried to sketch out the structure of the historical “revolutionary process” from the bourgeois revolution to the socialist one. Their concept was based on the definition of distinct “types” and “semi-types” of revolution and the specific theory of revolution reflecting the latest development in East-Central Europe.

This controversy lasted until the late 1950s and was still an influential point of reference at the beginning of the following decade. It raised several important questions related to acute problems of maturing State socialism. At the very heart of the debate was the uncertainty about the nature of governance in a people’s democracy. If this governmental arrangement was the result of a revolution, as was postulated by the official Marxist-Leninist political theory as well as by the founding myth of the regime, what kind of revolution was the one that took place in Czechoslovakia, and to what extent was this Czechoslovak revolution comparable with its Soviet counterpart? A more general and crucial question also came up concerning the very nature of the political system that was established, and then further developed by, the Communist Party after 1945. The source of disagreement was the attempt by certain scholars, among them by Miloš Kaláb, who later became a prominent initiator of the renewal of sociology in the 1960s, to elaborate a theoretical concept of revolution with respect to the specific historical experience of the Czechoslovak socialism. This attempt implied that besides the Soviet model of socialism there was a distinct Central European people’s democracy. This argument clearly collides with Marxist-Leninist dogma. The serious controversy between the proponents of a more flexible approach to political theorizing and the supporters of a theory firmly rooted in the canonical texts of Marxism-Leninism, and in line with the most recent Soviet policy documents, was thus opened.

Although the debate was highly theoretical and scholars aimed to construct complicated historical narratives of “revolutionary processes,” this exchange of opinions had serious consequences for further existence of the social scientific expertise in the country. First of all, this polemic was the first significant occasion since 1948 for social scientists to enter the public stage as policy-relevant experts. Secondly, the issue of socialist State and socialist governance was highlighted by all participants in the debate. It led them to formulate more coherent theoretical accounts of the issue. As was revealed in the critiques of the “Yugoslav revisionism”, Czechoslovak scholars characterized the socialist State as an institutional embodiment of socialism and the most important outcome of the socialist revolution—in contrast to the Yugoslav decentralized self-government, which was portrayed as an anarchist deviation from Marxism (e.g. Kučera 1960).

The post-Stalinist inquiry into the theory of the socialist State was mirrored not only in the publication of book-length texts that aimed to characterize the institutional and legislative

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9 The research on the socialist State and socialist revolution was widely covered and summarized at the major conference dedicated to the building of socialism and communism which was organized in 1961 by the Czechoslovak Academy of Sciences. For the conference papers, see Houška (1962).

10 Among the dozens of texts, extraordinarily important was the 1955 conference discussion between Kaláb and his critics published in Otázky národní a demokratické revoluce v ČSR. Sborník přispěvků přednesených na konferenci Historického ústavu ČSAV 28.–30. IV. 1955 v Liblicích (Prague: ČSAV, 1955).
arrangement of the people’s democracy, but also in practical and immediate expert interventions (Bystřina 1957; Houška and Kára 1955; Lakatoš 1957). In order to also complete the socialist construction in Czechoslovakia symbolically, the Communist Party authorities decided to issue a new constitution. The most important legislative document, which was adopted in 1960, introduced a new constitutional framework—Czechoslovakia officially became a “socialist republic”. The “socialist constitution” was discussed in depth by a specialized commission of legal scholars from universities and the Czechoslovak Academy of Sciences, who drafted individual parts of the document and completed its final version. In the case of the “socialist constitution”, the authorities mobilized scholars in order to provide policy-relevant expertise. In the late 1950s, discussion about the socialist revolution and socialist State, as well as the involvement of scholars in the completion of the 1960 constitution, helped to set the stage for the following decade of the reform communist experiment. It was during the reform communist period that social science experts occupied a prominent position in the realm of State socialist governance.

Socialism Reformed by Experts: New Model of Socialism and Social Sciences

While the post-Stalinist State-building had already required significant input from social science experts, the reform communist project of the 1960s was built entirely upon expert knowledge. The aim of the reform-oriented Communist Party authorities was to reconstruct State socialism in order to establish an economically more efficient and politically less repressive regime. Although the formulation of the reform program was a source of a bitter conflict from the very beginning until 1968, there were no doubts about the need to mobilize social science expertise, alongside the most advanced technologies, for reformist policy-making. Social scientists became important actors in the so-called “Czechoslovak reform”, as public intellectuals—who were supporters of the reform in the emerging public debates—and as experts involved in the numerous politically significant research projects and policy-advice activities (Kusin 1971; Skilling 1976).

In the 1960s, social scientific expertise was supported by the Communist Party and was developed across the various social science disciplines. In fact, the rise of the reform communist expert culture was among the most significant phenomena of Czechoslovak reform communism. In comparison with the legal expertise of the late 1950s, which was firmly rooted in the Marxist-Leninist discourse and was concerned exclusively with domestic issues, reform communist expertise became increasingly integrated in an ongoing transnational debate on modern governance, which had its participants on both sides of the “Iron Curtain.” Czechoslovak social scientists were eager to discuss and conceptualize original topics such as the future of “industrial societies,” the transformation of labor in the age of automation, the relationship between planning and markets, or the changes of human subjectivity in the face of rapid technological change. This helped establish a common platform for intellectual exchange across the Cold War divide and exposed the Czechoslovak social scientific community to various intellectual influences. This effort to cultivate an exchange of knowledge between the East and West was not only motivated by the necessity to adopt the up-to-date theories, concepts and research methods elaborated by social scientists in Western Europe and the USA. It was also a part of an attempt to produce original theories and, more generally, social

12 For the analysis of Czechoslovak jurisprudence expertise in the 1950s based on the archival evidence from the archives of the former Czechoslovak Academy of Science and covering its institutional as well as intellectual development, see Sommer (2016).
knowledge that contributed to contemporary debates from the specific perspective of socialist experience of modernity. This effort was very visible in such diverse disciplines as management studies and sociology. The most intellectually interconnected and relevant project to arise from these efforts was the “scientific and technological revolution” (STR) project, interdisciplinary research headed by philosopher Radovan Richta. I will describe this in more detail in what follows.

The establishment of the reform communist expert culture was enabled by changes in the disciplinary and institutional structure of the social sciences. Of utmost importance was the rebirth of sociology, a discipline that was ideologically denounced and institutionally dissolved during the Stalinist period. Interest in the functioning of socialist society was accompanied by the both enthusiastic and critical reception of contemporary Western sociological concepts, most prominently of the various theories of “industrial societies”. The crucial problem of human existence in modern society became a dominant topic of social scientific inquiry during the 1960s. From the philosophical concepts of Marxist humanism to empirical sociology, the relationship between human subjects and the modern organization of the society was considered the most important research question of the reform communist expert culture. This general “humanist” perspective drove the social science expertise involved in the project of political reform. In short, the interaction between individuals, society and socialist governance became extraordinarily important for a reform-oriented social scientist. For example, the crucial part of the economic reform agenda was the idea that a centrally planned economy was required to be more decentralized in order to achieve economic efficiency comparable with the economic performance of capitalist economies. This perspective was also reflected by the establishment of management studies in the mid-1960s, the aim of which was to develop managerial competencies, values and everyday habits that were seen as crucial for the successful management of enterprises operating in an economy based on market socialism.13

This “humanist” orientation was closely interwoven with another significant source of reform communist social sciences; cybernetics.14 The idea that social, political and economic lives are closed systems that are perfectly organizable and governable when proper organizational and decision-making techniques are applied became extraordinarily influential since the 1960s.15 The image of the world as a system was reflected by researchers dealing with the industrial organization and management as well as, at a more theoretical level, by STR research. In this particular case, reform communist humanism was interconnected with the system-centered perspective, which resulted in the idea of socialist post-industrialism: the communist society in which the automation of production and introduction of perfect planning and organizational techniques will be followed by the development of every individual towards a new humanity freed from manual labor, alienation and other social and psychological burdens of the industrial age.16

13 Apart from numerous books and booklets about management and industrial organization published since the mid-1960s, the most important source for the history of Czechoslovak management studies in the 1960s is the journal Moderní řízení (Modern Management), which has been published monthly since 1966 by the Management Institute based in Prague.
14 For an important work presenting various perspectives on the application of cybernetics in social sciences, see Král (1967). In 1965 the Institute of Information Theory and Automation of the Czechoslovak Academy of Science started to publish the theoretical journal Kybernetika (Cybernetics). For its on-line archive, see: http://www.kybernetika.cz/content.html.
15 For the history of the post-war system thinking in the USA, see Heyck (2015). The Soviet case study is analyzed in Rindzeviciute (2016).
16 For the history of the STR project in the context of Czechoslovak futures studies and forecasting expertise see Sommer (2015). Richta’s Theory of science is recently analyzed in Sommer (2016).
The effort to produce internationally relevant knowledge, or at least to reflect contemporary debates, and interconnect that knowledge with the particular case of Czechoslovak socialism was observable across disciplines and institutions. The STR project was the most ambitious attempt to gain international recognition within this general effort. The interdisciplinary collective of scholars gathered by Richta reflected a wide variety of contemporary social knowledge, ranging from Marxism, Western theories of “industrial societies” and the concepts of automation and post-industrialism to fields like social psychology, urban planning and organization studies. The concept of the STR, which was elaborated by Richta himself, was based on the reception of young Marx, most importantly on his elaboration of the relationship between science, production and labor in the Grundrisse (Marx 1993).17 Richta then developed the thesis of the “science as a direct productive force,” initially elaborated by philosopher of science James D. Bernal in the late 1930s, and used it in conjunction with the Western theories of “industrial societies”, the works of Jürgen Habermas, Raymond Aron, Herbert Marcuse, Walt W. Rostow and Daniel Bell, for example. Richta developed the STR within the original Marxist theory of socialist post-industrialism. It interconnected the changes in labor and production caused by scientific and technological progress with the call for political reform based on broader social participation in decision-making and more individual freedom in socialism. Richta aimed to reconcile the necessity of planning and sophisticated organization with the emancipatory appeals of Marxist humanism (Richta 1969).18

The STR research aimed to elaborate the socialist theory of post-industrialism and simultaneously prepare reformist policy-proposals. The most important publication by the STR research collective gathered by Richta with the approval of the Communist Party leadership in 1965 was the widely translated Civilization at the Crossroads. It contained a theoretical outline, an empirical analysis of the political economy of socialist industrialism, prospective chapters dealing with the forecasting of the future post-industrial society, and detailed policy advice for Czechoslovak reformers. Richta became a prominent public intellectual of the Prague Spring and his contribution to the Action Program of the Communist Party, the official reform document of the 1968 Prague Spring written by experts closely connected to the reformist Communist Party leaders, was also significant.19

The STR project was a prime example of the experts’ aspiration to interconnect theorizing with the elaboration of concrete policy proposals and, in general, to produce expert knowledge that could be used by politicians and applied to the introduction of far-reaching political and economic reforms. Richta’s scholarship was intellectually the most ambitious of the reform communist social scientific projects, mainly because of its conscious effort to merge social theory with expertise relevant to policy and to reflect the vision of a “more humane socialism” rooted in the dominant discourse of academic Marxism as well as the popular political imagination. Other reform communist expert activities also gained political and intellectual significance. A great deal of attention was paid to the economic reform proposals elaborated by the economics experts gathered in the research collective headed by Ota Šík.20 Their proposals built upon the concept of market socialism, which had been widely discussed by economists for decades and which they recognized as an influential model of modern economic organization (Bockman 2011). Šík’s aim, along with his colleagues, was to reform economic governance in order to increase the efficiency of the socialist economy; it was crucial to

18 For the earlier STR texts, see Richta (1963a; 1963b).
19 For the Action Program, see Remington (1969).
20 For the history of economic reforms in Czechoslovakia, see Myant (1989).
solve the problem of the interaction between the plan and the market. Moreover, the idea of a socialist market was accompanied by the conflicting and politically sensitive issue of workers’ self-management and its relationship to the hierarchically structured administration of companies by managers and other experts on labor and its organization (Šik 1968a; 1968b; 1968c; Kouba 1968). According to Ota Šik, the interplay between markets and planning, organized as a dialectical coordination of these two central principles of the economic organization, had the capacity to foster efficiency of the socialist economy as well as to substantially strengthen predictability within economic governance. The carefully managed balance between the plan and the market was characterized by Šik as an important outcome of the scientization of socialist governance characterized by the introduction of expert knowledge into economic decision-making (Šik 1968c).

Although the reform communist expert culture had been emerging since the late 1950s, rising to prominence in the middle of the next decade, its existence was interrupted by the Warsaw Pact invasion of Czechoslovakia in August 1968 and subsequent serious political changes that took the country towards a more authoritarian regime. As a consequence, social scientific expertise, which was organized as an influential actor in reformist policy-making, became the rather short-lived project of the Prague Spring. In 1969 and 1970, the collapse of reform communism was followed by massive purges, most importantly in the Communist Party apparatus and rank and file. These “consolidation” and “normalization” policy measures were extraordinarily harsh in the milieu of social sciences. As well as from removing some key actors, the purges imposed in significant discipline upon those individual scholars who were allowed to keep their positions in academia, and also in the introduction of a different knowledge production regime (Oates-Indruchová 2008). When all of the important ties with the reform communist democratization agenda had been cut off, social scientific expertise usable for the purposes of the post-1968 dictatorship remained in place. The late socialist governmental arrangement refused to realize any meaningful reform of political system or economic organization. However, the aims of the “normalization” governance were structured around notions of economic efficiency, socialist consumption and a depoliticized public, and were thus could hardly be achieved without employing social scientific expertise.

Thus the introduction of significantly more authoritarian rules of knowledge production after 1968 did not result in the entire dissolution of existing expertise. Still, the former reform communist expert culture was substantially reorganized in the course of the personal purges, institutional transformations and more-or-less forced adaptation to the new political discourse. This was based primarily upon the strict rejection of the reform communist political project. Quite a smooth transition to the new kind of expertise was facilitated by those authoritarian and technocratic elements, which had already been present in the reform communist social sciences, however. In the reform communist expert knowledge, humanist claims for more democracy and liberation of human subjects in socialism were in permanent conflict with the demand to scientifically organize the complex social reality of State socialism. Reform communist scholars and politicians sought to establish governance based on scientific rationality, a high level of predictability and policy application of expert knowledge. When strategies of political democratization were removed by

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21 For the reform communist expertise in the field of organization studies, see Král et al. (1967).
22 The words “consolidation” and “normalization” were used by the Communist Party authorities as the official terminology related to the post-1968 purges. After 1989 this terminology was adopted by historians and is still used in scholarly texts dealing with late socialism in Czechoslovakia. For the purges in Czechoslovak academia, see Míšková et al. (1998) and Tůma (2003).
force, the mechanisms and techniques of planning, management, organization and social control remained highly relevant and useful for the political elites of late socialism.

Centralized Technocracy and Decline of State Socialism

After 1968, scholars and experts had to respect new rules of institutional life and social scientific conduct. Among the most important principles was depoliticization, alongside careful following of the ideologically rigid and intellectually narrow-minded official Marxist-Leninist theoretical framework. If depoliticization meant the rejection of any activity reminiscent of independent and critical intervention by experts in the public and policy domains, conformity with obligatory ideological rules required not only vocal denunciation of the reform communist “revisionism” but also active engagement of individual scholars in the system of mutual control and evaluation. As described in the brilliant case study analyzing the “consolidation” of the Charles University Faculty of Arts after 1968, this mechanism was established in the aftermath of “consolidation” purges and enabled effective self-discipline from individual scholars to entire institutions. It resulted in a high level of political and intellectual conformity (Jareš et al. 2012). Since the early 1970s, the expression of controversial attitudes towards the late socialist social reality was restricted almost exclusively to private personal networks or semi-official institutions at the margins of the disciplinary and institutional structure of social sciences.

From the Communist Party authorities’ perspective, the new arrangement of social sciences was in perfect accordance with the attempt to establish more centralized, authoritarian governance. As mentioned above, the first substantial move towards a new kind of expertise was made in the course of the personal purges and a different knowledge production regime was subsequently introduced. Where the reform communist expert culture had been intentionally involved in reformist policy-making and was in a relationship to the Communist Party elites—albeit complicated and conflicting, though not entirely subordinate—late socialist expertise was organized primarily as a supplier of expert knowledge for the ruling apparatchiks and technocrats. The authorities did not deny the importance of expert knowledge for governance. On the contrary, the governmental rationality of the late socialism was built upon the idea of scientific organization. In political discourse, the Prague Spring was described as a time of chaos and general disintegration that had to be eradicated in order for socialism to be saved and preserved in Czechoslovakia; saved and preserved by the policy based on centralized planning and sophisticated top-down organization of economy and society.

The repressive policy of the late socialism, which was based on the public order imperative, produced the culture of conformity by means of social exclusion and enforced discipline of politically and culturally non-conformist collectives and individuals. However, the application of social scientific knowledge in the field of governance was promoted as a way to elaborate sophisticated techniques for planning, measurement, evaluation and centralized control and organization of social phenomena such as welfare, labor, production, leisure, and consumption. If the central task of public order policy was the repression and exclusion of otherness and dissidence, late socialist governance was structured primarily around the search for predictability of social and economic processes.

23 A classical account of “consolidation” was elaborated by dissident scholar Milan Šimečka, who characterized the political and social transformations in Czechoslovakia after 1968 as “restoration of order”. Šimečka argued that the “consolidation” was a far-going process which was realized not only through direct repressive measures but also on the level of the governance of everyday life. See Šimečka (1984).
Social scientific expertise was thus mobilized in order to reestablish and maintain control of the State and society after the allegedly disorganized and decadent era of the reform communist experiment.

The centralized control of institutions, the authoritative Marxist-Leninist theoretical framework, and expert governance of economy and society seemed to serve as safeguards against social disintegration, political unrest and economic decline. Market socialism reform was replaced by the effort to elaborate more advanced techniques of economic planning enabling the more effective organization of a centrally planned economy in order to produce significant economic growth. Similarly, the reform communist emphasizing of futures studies and prediction, which was reflected predominantly in the STR research, was transformed in the project of *prognostika*—centrally organized forecasting expertise concerned primarily with economic issues (Rocca 1981; Sommer 2015). While leisure and free time were discussed by reform communist scholars as a part of a much broader debate about the transformation of human subjectivity in socialist society, under the new regime of labor under advanced industrialism and emerging post-industrialism, the late socialist research on the “socialist life style” was focused on planning and organization of consumption, particularly on the top-down management of the distinctive socialist way of life.24 Emancipation of individuals by self-fulfillment and self-cultivation, conceptualized in the 1960s as a philosophical problem of human subjectivity as well as in the context of the attempt to enable much broader social participation in the socialist governance by creation of genuine civic life, was reversed after 1968 in an effort to create and technocratically organize the “socialist lifestyle”. This research sought to invent techniques enabling everyday life to be governed by mass consumption and authoritatively prescribed social values and cultural preferences (Filipcová and Filipeč 1976; Filipcová and Filipeč 1980).

It was thus quite logical that the last two decades of Communist Party rule were characterized by further expansion of expertise in fields such as economic forecasting, social planning, management, and organization research (Bauerová et al. 1972; Kutta 1973, 1976, 1980; Pavelka 1979). The late socialist technocratic governmental rationality emphasized careful planning of complex social processes and centralized organization of labor, from scientific management of workplace to administration of large sites of production. An inseparable part of this expertise was the effort to employ the most advanced technologies and planning methods. It mirrored the idea that the existing organization of society and the economy was governable by a centralized apparatus of control as well as the application of expert knowledge. The technocratic character of the late socialist expertise was thus reflected, for example, in social planning expertise, which was initially developed in the mid-1960s in the USSR.25 It was an attempt to interconnect economic planning and management with organization welfare in order to “program” various social processes at the individual enterprise level and in surrounding towns and regions.26 An important field of expertise was *prognostika*, forecasting research. Where reform communist STR scholarship had followed a path similar to the post-economic thought of its Western counterparts, after 1968 this trend was disrupted and replaced by the primacy of economic performance and efficiency.27 Forecasting scholarship became subordinate to the economists’ perspective and increasingly dominated by the discourse of economic productivity. This, in turn, led to a significant narrowing of the forecasting discourse. In the course

24 For the reform communist research on leisure, see primarily Selucký (1966).
25 For the comparison of social planning in the USSR and Czechoslovakia, see Stibulová (1976).
26 The Czechoslovak experiments with the application of social planning at the level of cities and regions are described in Novotný and Štráčal (1980).
27 For a wider context of the post-economic thought, see Brick (2006).
of the 1970s, the reform communist theory of socialist post-industrialism was replaced by mere technical processing of empirical data in order that forecasting reports could be elaborated for the purposes of centralized economic planning. Instead of being home to critical engagement with the future, *prognostika* was concerned with the production of allegedly objective knowledge about the prospects of the national economy. It aimed to foster central planning by providing predictions required for the successful management of a socialist economy. The late socialist fusion of technocratic governance and forecasting expertise aimed to produce the image of harmonious development towards prosperous and economically more efficient socialism.28

Unsurprisingly for a time when expert discourse was so dominated by economic rationality, economics rose to prominence. It was, however, not before the 1980s that economists became the most visible social scientific experts and luminaries of the emerging public debate about Czechoslovak *perestroika*.29 In the early 1980s, deterioration of the Czechoslovak economy became a serious concern and some kind of reform, or at least adjustment, of the economic arrangements seemed to be inevitable. It resulted in the relaxation of the strictly ideologically controlled discourse on the economy. In order to cope with the shortcomings of central planning, the Communist Party authorities did not seriously obstruct the economists’ effort to formulate reform strategies and to discuss the reform agenda publicly. Moreover certain decisions made by the authorities encouraged the economists, most importantly the establishment of a specialized forecasting institution in the Czechoslovak Academy of Sciences. The Forecasting Institute, established in 1984 as the Forecasting Center under the Czechoslovak Academy of Sciences and transformed into an institute in 1986, became a hub of *perestroika* economics.30 This institution was in charge of elaborating a long-term prognosis of the Czechoslovak economy until 2010. It gathered together a faculty composed of economists and other social scientists of various theoretical preferences and with different approaches towards the reformist agenda. However, it soon became clear that in this central and politically important forecasting project it was the economists who secured a prominent position in the *perestroika* reform oriented expert culture. Where other social scientific disciplines struggled with the still extremely limited space for independent and critical scholarship without much success, the *prognostika* economists were allowed to formulate their reform proposals comparatively officially. They did not hesitate to utilize the highly technical discourse of economics in order to link the critique of the already existing centrally planned economy with the critique of State socialist governance.31 Bringing together promoters of market socialism with monetarists and neoliberal free-market enthusiasts, they were vocal participants in the *perestroika* debates and managed to establish themselves as irreplaceable, influential experts attaining high visibility as representatives of critical and non-conformist social scientific thought.

Although the debate about State socialist governance was first opened since 1968, the significant dominance of economists in this discussion seriously narrowed the *perestroika* expert discourse and related social and political thinking. Compared to the reform communist expert culture of the 1960s, with its rich disciplinary background and conceptual diversity, 1980s social scientific expertise was

28 For the introduction to prognostika, see Šulc (1987). For the application of prognostika in economics and economic planning, see Komárek (1976; 1977).

29 For the Czechoslovak *perestroika* and economic reform debates, see Pullmann (2011); Myant (2014). The Czech translation of perestroika was přestavba (reconstruction). It is worth to mention that the Czechoslovak přestavba had a different, slower, dynamics than its Soviet counterpart due to the overall ideological dogmatism of the majority in the Czechoslovak Communist Party leadership.

30 For the short history of the Forecasting Institute, see Sommer (2015): 154–156.

31 For the role of economists in Czechoslovak policy debates, see Havel et al. (1998), Eyal (2003).
over influenced by one particular epistemic community. This development mirrored the economic determinism that prevailed in the late socialist expert discourse and was also in accordance with the global phenomenon of the expansion of economics during the 1970s and 1980s (Fourcade 2009). Similarly to the USA and Western Europe, economics gained extraordinary presence in intellectual and policy debates. The economic style of reasoning, supported by economists’ claims about the allegedly objective nature of the knowledge they produced, became an almost universally accepted rationality penetrating various domains of social thought, policy-making and everyday life (Rodgers 2011; Doering-Manteuffel and Raphael 2010). In Czechoslovakia, this development occurred under the strictly ideologically controlled and technocratically oriented regime of knowledge production imposed on scholars and experts after 1968. Social knowledge underwent its economization under the specific conditions of the late socialist dictatorship when pro-growth development of the centrally planned economy was a political priority and authorities labeled critical thinking about society as non-acceptable ideological subversion.

The rise of economists was thus extraordinarily important in a country where social scientific discourse was guarded by ideological watchmen until 1989, where every opportunity for public critical discussion was controlled by censors. Any relaxation of these rules that could potentially lead to a more open exchange of opinions significantly affected the further development of whole disciplines or the wider reception of these debates by the Czechoslovak public. In the case of economics, such relaxation of ideological control went quite far during the perestroika period. In the course of the officially sanctioned debate about the “rebuilding of economic mechanism”, understood by the Czechoslovak authorities to be crucial for the future of socialism, the economists were allowed to discuss politically important questions and set the agenda of the perestroika expert discourse. Although alternative reform proposals were elaborated, for example, by scholars dealing with research on environmental problems and the relationship between the environmental crisis and governance, such expertise was less important for the authorities and also politically more controversial than economics because of its ‘subversive’ questioning of late socialist power relations. With its specific language and purely technical style of reasoning, economics was seen as a value-free and objective technology of governance. Such specific, and to some extent one-sided, construction of the perestroika expert discourse thus resulted in the leading role of economists in the late 1980s reform debates. Simultaneously the impact of other branches of expertise on the non-expert public was limited and the policy relevance of these alternatives remained marginal.

Conclusion: With Experts from Socialism to Capitalism

The collapse of State socialism in Czechoslovakia in November and December 1989 was extraordinarily quick and smooth. Gathered under the banner of “non-violence” and “humanity”, the Czech and Slovak revolutionary public gave birth to the civic movement aiming, among other things, to overcome the alienation between the State and its citizens caused by the technocratic and over-bureaucratized late socialist regime. For a while it seemed that a new form of governance based more on the civic values of the revolution than on the economic calculation and, more generally, authority of expert interventions was possible. However, the revolutionary enthusiasm gradually

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32 The most important policy-oriented forecasting texts written in the late 1980s were Komárek (1990), and Zieleniec, (1990). Prognóza a program (Komárek 1990) contained the text of the so called “General Prognosis”, which was the main outcome of the Forecasting Institute’s research activities.

33 For the most comprehensive reform proposal based on the environmental perspective, see Vavroušek (1990).

34 The culture, values and future imaginaries of the 1989 revolution in Czechoslovakia was analyzed in Krapfl (2013).
receded and the everyday governing of a former socialist state in post-socialist conditions became the most important task of a new administration composed of dissidents and perestroika economists (Eyal 2000). When the second round of the economic reform debate was launched after 1989, the continuity between the post-socialist and late socialist expert cultures became more than evident. In the transition from the socialist, centrally planned economy to the new economic organization, the economists’ authority was significantly strengthened and their expertise gained even more attention among the non-expert public. The economists were eager to reinforce the impression that their prominent presence in policy-making, as well as in public debates, was a necessary condition for the success of the transition from the socialist dictatorship to liberal democracy. Immediately after the Velvet Revolution, several economists influential in the perestroika period entered the halls of power and occupied prominent positions in policy-making bodies, most importantly in the Czechoslovak government (Myant 2003; Burian 1998). Although different concepts of economic reform were proposed, the most influential one was its free-market and, more precisely, neoliberal variant authored by the former Forecasting Institute researchers Václav Klaus, Tomáš Ježek, Dušan Tříska, among others. It seems that the strong presence of economists in perestroika debates, during the Velvet Revolution and on into the 1990s, enabled the breaking point of 1989 to be bridged and brought about significant continuity between late socialism and post-socialism.

The almost unquestioned personal continuity between the late socialist economic expertise and the post-socialist political elite was accompanied by the continuity of governmental rationality, in a specific form. Similarly to the late socialist regime, post-socialist governance was backed by a highly economistic expert discourse and was structured around the primacy of economic calculation, efficiency and productivity. After 1989, this one-sided understanding of the social reality merged with the discourses of “democratic transformation”, the “return to Europe”, and the building of a prosperous economy based on free-market competition and large-scale privatization. The reform strategies elaborated by the community of free-market economists during the 1980s and after 1989 were presented to the public in a highly authoritative way as the only realistic, strictly rational, objective and truly scientific alternative and as the proven, solid knowledge background of the policies leading to a prosperous future (Zieleniec 1990). Under the guidance of those economic experts who became policy makers after 1989, the transformation of governance from technocratic socialism to neoliberal capitalism was carried out with a high degree of continuity and on the common ground of robust economic determinism in social thought and related expert knowledge.

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ARTICLE

Democracy, Totalitarianism, and Dead Ends in Sociology

Reinhold Sackmann
reinhold.sackmann@soziologie.uni-halle.de

Abstract

The 20th century did not only see the rise of academic sociology, but highlighted also strife within societies that resulted in specific forms of totalitarianism. Literature on the relationship of sociology and these totalitarianisms is controversial. Some authors state that sociology is not a natural antipode to dictatorships: Some sociologists like Sombart and Michels were active proponents of totalitarian ideology, and most totalitarian regimes quite liked the technical expertise of social research. Other authors propose that a flourishing sociology needs a certain form of society characterized by openness and tolerance to find a constructive climate for theoretical reflection, empirical research and practical relevance. From this position one could state that there is a “Wahlverwandtschaft”, a structural affinity, between sociology and democracy.

The article uses empirical material from a case study on the development of sociology at the University of Halle between 1900 and 1990. The University of Halle is an interesting case in so far as in this East German university the societal context varies between monarchy, democracy, fascism and communism. The empirical material focuses on published and unpublished work of Friedrich Hertz and Rudhard Stollberg, as well as archive material to their activities and conflicts. Specific dead ends of forms of historical sociology and empirical sociology are analysed with regard to the guiding question, whether there is a structural tension between forms of totalitarianism and sociology.

Keywords

German sociology; Nazism; Ernst Grünfeld; Friedrich Hertz; Rudhard Stollberg;

Dead ends in the development of a scientific discipline can be the result of either an external force bringing a fruitful development to a sudden end, or the effect of an inner logic driving one to the conclusion that a certain approach fails to achieve the results that one hoped to gain. In social sciences the causal attribution of a dead end is sometimes a topic of heated debate, because a forced standstill may attract later generations to pursue the same path whereas a self-declared failure signals “change path” to potential successors.
Discussions in post-fascist West German sociology were highly controversial after a published remark by Helmut Schelsky that German sociology in the Weimar Republic had reached a dead end due to a failure of its scientific programme: “the melodies were played through” (Schelsky 1959: 37). Sociologists like René König (1984, 1958), who were in exile during the Nazi period, were shocked by this remark, and repeatedly argued that large parts of German sociology were driven into exile by force. Strong sociological programs were beginning to spread, especially at the end of the Weimar period. In hindsight, this discussion in German sociology at the end of the 1950s intermingled personal and professional interests and scientific analysis. There was the delicate issue that a minority of sociologists remained in Germany under the Nazi system, including Schelsky, which both Schelsky and König kept in the background.

In this paper I want to generalize this controversy with the following research question: Why does sociology run into dead ends in totalitarian societies? It seems that some disciplines are more interwoven with social structure than others. It is likely that social sciences would be more dependent on a certain structure of society than natural sciences. For example, it would be astonishing to see a discussion on the dead end of chemistry at the close of the Weimar Republic. Some of the best chemists of that time were driven into exile, which stopped some development in this field, but no one thinks that chemistry as a discipline was at a dead end at this time. As a guiding, and quite risky, hypothesis to be tested and refined in this paper, one could even emphasize that the scientific discipline of sociology is not only more dependent on social structures than other scientific disciplines, but that it is somehow related to a certain social structure—namely democracy—in order to flourish. This might be described as an elective affinity between sociology and democracy.

The argument develops in five sections in this paper. In the first, tendencies are described that point towards this affinity. The second examines theories explaining the tension between sociology and totalitarian regimes and an affinity between sociology and democracy. The third sketches the empirical methods used in this analysis. The fourth presents results from a case study of sociology in an East German university during the 20th Century. The fifth and final section discusses the results of the case study and draws attention to the limits of this study.

1. Totalitarian and democratic societies and sociology

Do we have material to form an initial suspicion that there might be an elective affinity between sociology and democracy and some tension between sociology and non-democratic societies? To answer, one has to be more precise on the meaning of sociology. We define sociology as a science that is institutionalized as an academic discipline and which refers to formation processes of societies that call themselves societies. Therefore, we do not include in this definition of sociology any social thought, e.g. of Aristotle, that was not put forward with reference to explicitly mentioned societies. So, by this definition, one prerequisite of sociology is a society that develops a concept of society. In our narrow sense of the meaning of sociology, we also do not include sociological thinking that is not institutionalized in academic disciplines. Social philosophers like Comte, Marx, and Spencer were important for the history of sociology, but they were not successful in institutionalizing sociology. Thus, we propose that institutionalization of the academic discipline sociology is a second prerequisite for the existence of sociology.

The second concept used here is democracy. This is defined as a formal state government structure in which at intervals a broad electorate chooses its governing parties in free and fair elections. Decisive for this concept is that there exists a legal state separate from the personal possession of the
ruler, and structured pluralistic political processes that transfer power for a limited period of time. A seemingly vague notion in this definition is the term “broad electorate,” which reflects degrees of democratization: i.e. the percentage of certified voters in relation to the adult population of a state territory.

There is scientific consensus on the fact that the concept of society is a semantic used primarily by modern societies following the big shift of semantics at the beginning of the 19th century (Koselleck 1979: 24). Some stress Paine’s rather broad idea of “society” in pamphlets before the American War of Independence as the starting point (Paine 1776) as the innovative set of semantics. Others point to the reference to the more elaborate argumentation of Joseph Sieyès in generalizing the Tiers État at the onset of the French revolution (Sieyès 1789; Institut für Sozialforschung 1956: 23), and others the theoretical conception of a civil society by Hegel (1820) (Riedel 1975). Most agree, however, that the concept of society is not a semantic of pre-modern societies, most of which were not democratic. It was the legitimations of two decisive political revolutions that began civil societies’ use of the concept of society, as both Paine’s and Sieyès’ pamphlets show. The case of Hegel, however, illustrates that once the concept is established one does not have to be a personal supporter of democracies to use these semantics.

Auguste Comte introduced the notion of sociology. Most commentators agree, however, that the inception of a social science called sociology did not occur before the turn of the 20th century. Durkheim, Simmel, and Weber are seen as major figures in the development of the concept of sociology as a scientific discipline (Abel 1970). In hindsight, some sociologists would add several names of founding fathers and mothers, some may remove one or two of the aforementioned trio, but the majority would mention at least one of them.

During the period in which sociology was established, in one programmatic paper defining sociology as a discipline published in 1894/95 in German, French, and American scientific journals, Simmel tried to establish a formal sociology. This focused on processes of interdependence, and tried to detach itself from concepts of a holistic society. In 1908, he rephrases the “problem of sociology” and states that “… the claims, that the science of sociology tends to raise, [are] the theoretical continuation and reflection of practical power, which in the nineteenth century the masses have acquired in opposition to the interests of the individual. But the feeling of importance and the attention, which the lower classes forced from the higher classes, is supported especially by the concept of “society” (Simmel 1992: 13). In other words, Simmel parallels a social movement—a movement towards more power sharing—as a “practical power” that is transferred to the scientific discipline sociology. Already Simmel’s contrasting of “masses” and “individual” signals that he does not see the academic discipline of sociology as a supporter of this democratic movement; in his time “masses” was a derogatory term for large electorates.

Whereas Simmel is successful conceptually in separating the concept of a value-free academic sociology and a democratic movement, he does not achieve this differentiation; he is an object of evaluation by a conservative state himself. Despite his broad audience at Berlin University, and his seminal publications, he is not made a full professor until 1914. The Prussian ministry rejects his faculty’s application for Simmel’s professorship in 1898, arguing that he is a sociologist; i.e. a suspicious radical (Rammstedt 1992: 888)! Simmel’s case illustrates how the establishment of the academic discipline of sociology might be characterized by three prerequisites: A process in a society establishing a society semantics; theoretical and empirical scientific work that forms the basis of a new discipline; and a government structure that allows this new discipline to institutionalize itself at university level. The historical pattern of the establishment of sociology in its origin countries shows
that the last point, which is sometimes neglected in the literature, is quite important (Sutherland 1978). Democratic government structures seemed to be more open to the institutionalization of the new social science, sociology. Sociology is institutionalized in universities in the two largest democracies—USA and France—at the turn of the 20th century. The case of the United States is interesting. By 1909, there were 55 full-time and 372 part-time professors in sociology (Steinmetz 2010: 9), despite the fact that the coherence of the theoretical and empirical scientific work of American sociology was still rather vague. French sociology was institutionalized in universities in 1896. Indeed, the classic work of Durkheim shows its intellectual vigor at that time. In contrast, three large countries with mixed monarchy-democracy structures, Germany, Austria, and Great Britain, did not institutionalize sociology at university level before 1907: Britain in 1907 (Halsey 2004: 3); Germany in 1919 (Sutherland 1978); and Austria last of the three in 1919 (Langer 1988). The German case is instructive. Despite the strong scientific program of early 20th century sociology, now regarded as classic, there was not a single sociology professorship before the democratic revolution in 1918. In terms of an elective affinity between sociology and democracy, one could state that historically sociology had a higher likelihood of being chosen for institutionalization in universities earlier in large democratic societies. At the same time less democratic governments rejected attempts to institutionalize sociology because it was seen as too democratic, even when sociologists tried to distance themselves from democratic movements. Ringer’s (1969) classic study elaborates the difficulties and ambivalences of the modernist German mandarins, who are at the center of the establishment of German sociology. An irony of the negative logic of an elective affinity between sociology and democracy is that democratic revolutions in Germany and Austria successfully institutionalized sociology in universities, but in a continuation of its democratic lag, some of the first sociologists picked by the conservative university establishments—such as like Freyer in Leipzig or Spann in Vienna—were anti-democrats.

If there is an elective affinity between democracy and sociology, one might suppose that there is also an inherent tension between totalitarianism (as an antidote to democracy) and sociology. We define a totalitarian regime as a state government structure in which one party governs without set time limitations, without legal opposition, and cherishing some kind of Weltanschauung. In contrast to pre-modern government structures in totalitarian regimes there is a state but, unlike democracies, there is no pluralistic competition between parties and no formal procedures of a change of government initiated by the broad electorate. Rather vague in this definition of totalitarianism is the notion of a specific Weltanschauung as most 20th century dictatorships legitimize themselves using some kind of more-or-less systematic ideology. According to this definition, National Socialist Germany, the Communist Soviet Union and Mobutu’s Zaire would all be totalitarian regimes, despite the fact that the number of books explaining the dominant ideology varies in these States.

With regard to the general description of the relation between sociology and totalitarian regimes, contrary to some exaggerations by early post-war sociologists one can state that there were a number of sociologists working in fascist Germany (Klingemann 1996) and Italy, however much institutionalized sociology was hampered. In Germany, all efforts by right wing sociologists like Freyer or Sombart to gain broad support from the new Nazi dictatorship were unsuccessful because the Nazi establishment viewed sociology as a product of the hated Weimar Republic (Stölting 1984).

The relationship between communist totalitarian regimes and sociology differed according to the scientific traditions in the various countries. In the Soviet Union, sociology was banned between the mid 1920s and the 1960s. In East Germany, sociology was not allowed between 1950 and the mid 1960s. In Communist China, sociology as a scientific discipline was kept away from universities until
With regard to the thesis of tension between sociology and totalitarian regimes, there is no clear indication that professional sociologists, or their organizations, refused to work in totalitarian regimes, as show in the examples of Freyer and his colleagues, or Polish sociology in the post-war period. However, in both fascist and in communist regimes, we find explicit policies against the discipline of sociology, more than against any natural science, and also somewhat harsher than any measures against most other social science disciplines.

A third type of state that was dominant in most parts of the world during the first half of the 20th century is still rather neglected by research on the history of sociology. Given that this case study’s empirical material does not refer to this type of society, I just want to mention general tendencies. Societies governed by a colonial regime are non-democratic by nature; they crumble as soon as general elections are allowed. If one looks at sociology in colonial societies, in very few cases sociology can be detected before de-colonialization. Colonial rulers appear to institutionalize anthropology/ethnology instead of sociology to signal a distance between rulers and the ruled (Keim 2008). This one might conclude that there is also a tension between sociology and colonial regimes, as with other non-democratic societies.

To summarize this first chapter, it can be stated that, in general, there is a higher likelihood of sociology being institutionalized as an academic discipline in democracies than in non-democratic societies. Periods of tension, and sometimes even explicit suppression of sociology, is not uncommon in fascist and communist regimes. In other non-democratic societies, like colonies without self-rule or mixed monarchy-democracy structures, sociology tends to either not be institutionalized or be institutionalized later than in democracies. However, the elective affinity between democracy and sociology and its counterpart—a tension between non-democratic societies and sociology—seems more probabilistic than determinist, like most social science correlations (Goldthorpe 2002). Considering the nature of actions behind this elective affinity, one gets the impression that a rejection of sociology by non-democratic regimes is more common than an active exclusive propagation of democracy by sociologists who vary in their views. Why is there tension between totalitarian regimes and sociology?

2. Theories on the relation of totalitarian regimes and sociology

Explanatory theories that state a causal link as to why totalitarian regimes quite often form a tense position towards sociology are scarce. Merton (1968) gives a rather general theory. He sees a structural co-evolution of democracy and science with science getting the opportunity to develop independently in democracies. According to Merton, the guiding imperatives of science in general are universalism, communism, disinterestedness, and organized scepticism. Tensions between science and totalitarian regimes result from anti-rationalism in the Weltanschauung of the latter and from the “rather … diffuse, frequently vague apprehension that scepticism threatens the current distribution of power” (ibid: 615).

Merton’s is a theory of tension between science in general and totalitarian regimes, but offers no specific hypothesis as to why sociology should be special within all science disciplines. Different suggestions are put forward in the literature. Some authors take it as an effect of the demography of the scientists in sociology: e.g. because the proportion of Jews among sociologists was higher than...
in other disciplines, the proportion of its émigrés was high during the fascist regime in Germany (Käsler 1984). If this hypothesis is conclusive, the tension between totalitarian regimes and sociology could be simply coincidental.

Another group of authors stresses more central ideas of sociology that tend to clash with totalitarian regimes. For example, Eisermann (1959) sees sociology as a born opposition science and Neuloh (1986) views its critique of ideology as a nuisance for totalitarian rulers. More complex, though similar, for Geiger (1956) sociology is born at the end of the 18th century when a “free society emerged against the organized state” (ibid: 11) by criticizing power per se. As an empirical, analytical, and critical science, sociology continues this idea, which runs counter to the central principles of totalitarian states, which only have use for self-glorification and social engineering. If Geiger’s hypothesis is correct, the critical inner logic of ‘sociology proper’ will clash with totalitarianism, which would only tolerate a ‘sociology light’ in the form of social engineering and Geiger would not call this sociology.

There are interesting variants of Geiger’s theory in the literature. Habermas (1992) states that German sociology arises as a sociology of culture and knowledge critically related to German humanities that cannot be continued in forced exile. Whereas Habermas focusses on the inner logic of ideas finding a dead end in external events, Stölting (1984) stresses the motives of the totalitarian regime. Nazi elites believed in the public image of sociology, which was seen as liberal, socialist, and connected with the Weimar Republic. Therefore all attempts of right wing sociologists like Freyer were rebuffed and only a ‘sociology light’ of single working sociologists without institutionalization of the discipline sociology was tolerated. According to Stölting’s theory, the decisive moment of the tension between sociology and a totalitarian regime is that the latter believes in the critical, democratic image of sociology, independent of the fact that individual sociologists try to set themselves apart from this image.

The normative implications of Geiger’s theory are stressed in a strand of the literature, which accentuates that only a critical ‘sociology proper,’ fighting actively against totalitarianism, would be worthwhile. Wittebur (1991) claims that nearly all émigrés failed, as they did not write the seminal analysis that shattered totalitarian rule. In a similar vein, Hannah Arendt criticized sociology as a discipline of social engineering, taking Mannheim’s ideology and utopia as an example (!), whereas she wanted an existentialist critique of the diabolic nature of totalitarianism (Baehr 2002).

In conclusion, theories on the tension between totalitarian regimes and sociology (as well as an elective affinity between sociology and democracy) fluctuate between normative statements and empirical observations; between system level typologies and explanations of patterns of action. It seems important, therefore, to be precise about the mechanisms by which the tension between sociology and totalitarian regimes is produced, and through which the elective affinity between democracy and sociology created. This is important, as dead ends in sociology through totalitarian regimes might be produced in different ways. One thesis is that dead ends of sociology in totalitarian regimes occur more or less coincidentally, more as a reflection of the composition of its actors than of any attribute of its science. An alternative hypothesis in line with Geiger is that there are parts of the scientific core of sociology proper that are suppressed by totalitarian regimes because they are opposed to their power structure. A sub-hypothesis of the Geiger thesis is that only sociology light in the form of propaganda or social engineering can survive in totalitarian regimes. A fourth hypothesis is that totalitarian perceptions of sociology as a critical science bring about dead ends in sociology. A fifth hypothesis states that the forced shift of context via emigration produces dead ends in sociology.
3. Methods

The empirical method used in this paper is a case study on sociology at an East German university during the 20th century. As a qualitative case study no generalizing conclusions on the frequency of occurrence of observed practices can be drawn. The aim of the analysis is to understand whether there are mechanisms that tend to produce tension between a totalitarian regime and sociology and, if yes, to determine their nature.

East Germany during the 20th century seemed to be a good place for such a study as there were a broad variety of regimes. From 1900 to 1918 there was a mixed monarchy-democracy regime, from 1918 to 1933 the state was democratic, in 1933 this gave way to a fascist regime, which collapsed in 1945 and was followed by a communist dictatorship until 1990, returning to democracy after from 1990 to 2000. Halle University, the subject of this study, was founded in 1694 and throughout 20th century was a “normal” middle-sized university. During the period of the observation it was part of the federal state of Prussia until 1933 and since 1990 has belonged to the state of Saxony-Anhalt. This study is interested in dead ends and for this reason we end it in 1992. Thus re-establishment of sociology after democratization will be ignored.

Being interested in institutionalized sociology as a scientific discipline, we use a very narrow definition of core sociology; professors who have the word sociology in their title. We will concentrate on two main figures, Friedrich Hertz (1930–1933) and Rudhard Stollberg (1965–1990), who fulfill this criteria. A third figure, Ernst Grünfeld (1929–1933), will also be briefly reviewed. Grünfeld seems to be an interesting case with which to study the relationship between sociology and its dead ends. For these three professors rich material was both accessible and used. Besides publications of these authors, Hertz kept the manuscripts of his Halle lectures, which we copied at the archive for the history of sociology in the Karl-Franzens-Universität Graz, Austria. Halle University archives were consulted on all three subjects of study. In the case of Rudhard Stollberg, documents from the archives of the secret state police (Stasi) were used to reconstruct the conflicts around the dissertation of Bahro. An archive of the department of sociology in communist times was also found in the attic of its building which included, among other objects, titles of seminar papers (cf. Pasternack and Sackmann 2013).

4. Case study: Dead ends of sociology in Halle

The following chapter analyses dead ends in the development of sociology at the University of Halle during the 20th century. Although we will focus on the end of the Weimar Republic and the time of the communist German Democratic Republic (GDR), it is useful to briefly review the situation before 1918, which we call a monarchic-democratic regime.

4.1 Grünfeld and sociology before sociology

From 1694 until 1918 there was neither a sociology professorship at the Halle University nor. one single university course that includes the term sociology in its title. Why doesn’t sociology exist as an academic discipline in Halle before 1918? It seems futile to give an answer to a question on the non-existence of an object, as there is no parallel in Halle to the efforts of Simmel and his Berlin University to establish a professorship of sociology. We think it is useful to take a closer look at the Ernst Grünfeld’s dissertation, defended in 1908 at the university of Halle, as it gives an idea as to why both the potential for the existence of sociology as a discipline and its rejection are traceable in Halle. Sciences of the state (Staatswissenschaft) was the term used before 1945, which self-characterized
the social sciences in Germany at that time. It encompassed disciplines that today one would call macroeconomics, business studies, political sciences and sociology. Dominant within this spectrum was macroeconomics. In Halle, Gustav Schmoller, Johannes Conrad, and Heinrich Waentig were all situated in the main stream of German economics; the Historical School of Economics, which was organized around the Verein fuer Socialpolitik.

Waentig, the supervisor of Grünfeld’s dissertation on Lorenz von Stein, can be seen to be interested in sociology as his 1894 dissertation was on the predecessors of Auguste Comte. Waentig’s political orientation, however, is quite unusual in the German Kaiserreich. He was to become a deputy for the left wing Social Democratic Party to the Prussian Parliament during the Weimar Republic. Grünfeld’s topic is also quite unusual as it is the first comprehensive monograph on Lorenz von Stein in Germany. Lorenz von Stein is praised in this book as the first social scientist in Germany to introduce “Gesellschaftslehre” (discipline of society) in a systematic way in 1842 and thus became—despite heavy criticism by Treitschke and Roscher—the founding father of the Historical School of Economics. After a thorough discussion of the strengths and weaknesses of the work of von Stein, in the last chapter Grünfeld (1910: 206–257) evaluates the effects of von Stein’s social science approach. In this context, there is an interesting discussion of the relation between sociology and social sciences. Grünfeld argues that demands for chairs in sociology are unfounded as it is still “fruitless” (ibid: 213), whereas the integration of social science approaches in economics—as done by Bücher and von Schmoller—was more productive. More generally, Grünfeld states that the introduction of sociology in the US and France, which he observes is due to the “universality, its other side being a certain superficiality” (ibid: 245), characteristic of these countries. This “seems to be not adequate for the German essence” (ibid.), which aims at specialization. Therefore, the incorporation of social science topics into economics, which happened in Germany, is seen as preferable to the institutionalization of sociology.

Analyzing these passages, one can state that even in progressive groups of young German social scientists, sociology was seen as something “not German”; something that was only suitable for “superficial” democratic countries. The somewhat bizarre argument of national characteristics of sciences proves the pattern of a non-introduction of sociology in monarchic Germany. Beside the vivid language of this treatise written two years before the foundation of the German sociological association, it also shows that even scientific authors like Grünfeld, writing on topics which are rather sociological, seem to have the impression that it is prudent to distance themselves from “foreign” sociology to be successful in academia.

4.2 HERTZ AND THE DEAD END OF SOCIOLOGY 1933

After the democratic revolution of 1918, the position of sociology in the academic field changed. The first professorship of sociology was awarded in Germany in 1919 and following this we see a broad institutionalization of sociology in German universities (Lepsius 1981). In Halle, institutionalization took place in 1929–30, when the Austrian Friedrich Hertz was appointed as an ordinary professor of economic sciences of the state and sociology. In part, his appointment was an effect of a democratization process, as the Prussian ministry insisted, against a reluctant department of the sciences of the state, on making a designation that included sociology (Böhme et al. 2013: 56–57). Hertz was a compromise candidate acceptable to the liberal minded economic department which, at the time, consisted of Aubin and Jahn. Ernst Grünfeld and Friedrich Hertz are added to the professorial body in 1929–30.
Even among the non-uniform sociologists of his day, Hertz was unusual. He was a scientist of the state by training and had attended the universities Vienna, Munich, and the London School of Economics. He was also a founding and leading member of the “Sozialwissenschaftlicher Bildungsverein in Wien”, the first German speaking social science organization (Müller 2004), a journalist, a manager of the industrial association of Austria, and a high level civil servant of the Austrian State after World War I. Scientifically he was mainly known for his critical works on race theories (Hertz 1915) and on nation building (Hertz 1927), as well as works on the structure of the Austrian economy WWI.

Why did institutionalized sociology end at Halle University in 1933? In the case of Hertz’s sociology, the fascists despised the content of its orientation, theories, publications, and lectures. Hertz not only attacked the pseudo-science of race studies in books since 1904, he also criticized the first professor of race studies, Hans Günther—who was appointed by the Thuringian provincial Nazi government (Hertz 1930)—as unscientific. Soundly attacking a core Weltanschauung of the totalitarian regime made Hertz one of the most hated professors at the university among Nazi students. A Nazi press campaign tried to denounce Hertz as a terrorist who helped to organize a violent attack on Günther, and the preparation of a Nazi student boycott led to Hertz’s resignation as professor. To save his life and that of his family he fled to Vienna and later was exiled in Britain.

What kind of sociology ended at Halle University with Hertz’ departure in 1933? We are fortunate that, in addition to his publications, we have the manuscript of a lecture on “Current sociological theories,” which he gave in the winter semester of the 1932/33 academic session and with which he planned to conclude the summer semester of 1933. In general, his publications (Hertz 1927; 1929; 1931a; 1931b) show that his empirical work is in the tradition of historical sociology, which tried to analyze societies by comparing conditions and results in macrosociological constellations and by sketching lines of development both realized and sometimes only seen possibilities. Steinmetz (2010) claimed that this kind of historical sociology was typical of German Weimar sociology and, due to its rejection in countries of exile, found a dead end. Its approach was taken up again in the 1960s.

It seems appropriate to look more closely into what was meant by historical sociology at that time, particularly by Hertz. Two main projects were pursued within historical sociology. One tried to achieve a history of ideas in their societal embedding: for example, the development of the modern idea of “race” is sketched and analyzed as an ideology to transfer feudal prestige notions to larger units like nations (Hertz 1931a). In this line of thought, developmental notions are as important as analytical components. Another set of ideas that were even more prominent in Weimar sociology, aim to develop a typology of pure types of forms and their logic from historical and spatial comparisons. In an article on forms of nation Hertz (1927) proceeded by describing the history of the idea of nation, defining its core components as being related to states—a form of community feeling with different contents and a will of belonging—and distinguishes and analyses types of nation state, language nation, descent nation, race nation, belief in common descent nation. Similarly in 1929 Hertz’s historical and contemporary material on migration is a starting point for typological work that forms general theories on relations of patterns of action and societal formations.

Hertz’s empirical approach in his 1932 & 33 lecture on sociological theory is very much in line with Abel (1929), whose book forms the core of his lecture. In his fascinating book, Abel argues that looking into the work of current German sociologists can solve the problem of a foundation of the specialized discipline sociology, both in theory and method. He states that Simmel was right in his approach to single out interaction as a basic object of a specializing science of sociology, but also
criticizes him for his lacking systematization, methods and for the impracticality of differentiating form and content. Abel is even more critical towards the phenomenological approach of Vierkandt. He supports von Wiese in his effort to reconcile quantitative methodology and theory, but is critical of his lack of empirical work. Abel presents Weber’s work as seminal as he solves problems of both theory and methodology, and also conducts empirical studies. In his lecture, Hertz wholeheartedly supports Abel’s argument, which would become important step towards the later synthesis of classical sociological theory by Parsons in 1937. Using Hertz’s own terms, a sociology that is centered on patterns of action that relate to historical circumstances should be value-free and empirical. This is completely in line with Abel’s theorizing. The only difference is that Hertz also supports practical sociology, which is problem-centered.

If there is such a strong reciprocal link between German Weimar sociology and international sociological theory building, as seen in the work of Abel and Hertz, are we still able to proclaim a dead end to the Weimar sociology? I think there is still a positive answer to this question. The local answer is that after Hertz’s forced exile, his kind of sociology was no longer followed in Halle (and many other places in Germany). Further, Parsons follows Abel’s lead, but he cuts back historical dimensions in his work from the 1930s to the 1950s. And Hertz, continuing his work in exile, shifts his empirical focus from historic-typological theory building towards historical narrative, increasingly neglecting general theory (Hertz 1957; 1962; 1975). Therefore, the specific Weimar historical sociology that combined typological work with vast historical material came to an end due to a forced change of context.

### 4.3 Stollberg and Sociology in a Communist State, 1965–1990

Between 1950 and 1964 no academic sociology was allowed in the communist German Democratic Republic (GDR). After this sociology was reintroduced at university level, though at a quantitatively very low level (Meyer 1996). In Halle, Rudhard Stollberg, held the only professorship for sociology from 1965 to 1990. These 25 years provide a much better opportunity to look more into the details of the relation between sociology and totalitarianism than the shorter Nazi period. Was there a tension between the two, and if so, what form did it take?

Stollberg studied political economy at Halle University during the 1950s. Under the GDR, it became common to appoint in-house professors, as is the case with Stollberg. Like most GDR sociologists, he specialized in the sociology of work. He was proud of his international conferences on the topic, he even hosted a meeting of a research council of the International Sociological Association (ISA) in spring 1989. As a loyal member of the communist party, he belonged to the core group of sociologists who organized teaching and research programs for the GDR.

According to Geiger (1956) a totalitarian regime like the GDR would not tolerate ‘sociology proper’, but only ‘sociology light,’ which served the demands of the state for ‘glorification’ and social engineering.

To corroborate this thesis, we analyze an important study of Stollberg and his team on shift work (Jugel, Spangenberg and Stollberg 1978). We do find some glorification of the regime insofar as socialism is treated as constituting a lifestyle in contrast to capitalism without structural contradictions (ibid: 5, 24). Shift work, for example, is seen as “progressive” (ibid: 8) in socialism where in capitalism it is exploitative. However, ‘glorification’ is not the book’s central purpose. Its focus is more on social engineering. It provides a quantitative empirical study on the effects of shift work on family life, leisure, and health, describing quantitatively how shift work is disruptive to
family life and health, but less so to leisure. Is it adequate to qualify this empirical work somewhat derogatively as social engineering and not, as Stollberg would have termed it, as practical sociology? For practical purposes, both seek means to achieve specified goals, not at finding and evaluating ends of action. In this context, Stollberg’s book tries to find ways to alleviate the negative effects of shift work by giving advice to managers of companies (ibid: 18, 125–128). For example: the same shift rhythms for spouses are less disruptive for families; cooperation with child caring institutions lessens stress on children; a switch of shift start times to 7 am, 3 pm and 11 pm would be more healthy than the existing patterns; participation of children in the decision making of families (ibid: 49); and participation of workers at the introduction of shift work (ibid: 128) are encouraged. Despite the somewhat paternalistic orientation of most parts of the book, there seems to be no clear dividing line between social engineering and practical sociology. There is, however, a difference between ‘sociology light’ and ‘sociology proper,’ per Geiger, insofar as Stollberg observes that a general discussion of the pros and cons of an extension of shift work is beyond the limits of his book. The extension of shift work is an established fact, set by the political decisions of the Communist Party and the Communist State, which are cited in the book, and which are beyond criticism. However, even trying to keep to the limits of ‘sociology light,’ Western scientific observers (and communist functionaries in the GDR) saw Stollberg’s implicit critique of shift work as “sociologist of work Rudhard Stollberg keeps to be the only person who […] dares to express a fundamental critique of shift work” (Voigt 1986: 160). One could conclude that both sociological authors and totalitarian observers police the thin line between ‘sociology proper’ and ‘sociology light.’

A more fundamental problem of following the approach of ‘sociology light’ in a totalitarian regime is brought to light in the conflict around the dissident Bahro, which seems to have influenced Stollberg’s scientific work more than the controversies around his sociology of work (Pasternack 2013a: 130–142). Rudolf Bahro wrote a PhD thesis in 1975 on low and high level managers in industrial companies in the GDR, which included an appendix of notes on 48 interviews. Stollberg did not supervise this work, but wrote an expert opinion which was slightly critical (evaluated “cum laude”). Stollberg did not know that the State Secret Police had already read the interviews and were appalled by the critical content of the managers’ comments on their work. They intervened by declaring the dissertation “secret” and appointing two additional expert opinions for the dissertation, which stood in opposition to the three existing positive opinions to produce an overall negative vote and the dissertation was thus failed. Bahro was imprisoned, but was successful in smuggling a more radical book out of the country, which was published in West Germany in 1977 as one of the most important dissident works in East Germany. Later that year Bahro was expelled from the country and went to West Germany. Thus in 1977, Stollberg had to criticize himself for his insufficient vigilance in his evaluation of the dissertation in various committees: the party organization at the university; the management of the university; and in a party assembly, where he had to defend himself against eviction from the communist party. There were no further sanctions against him, except for more intensive observation by the State Secret Police.

Reflecting on the relationship between sociology and totalitarian regimes, two aspects of the Bahro affair stand out: Bahro’s empirical material work, which annoyed to a key organization of the totalitarian regime, the State Secret Police; and that Stollberg was criticized for his evaluation of this work without recommending that the empirical material should be censored. Stollberg was a sociologist loyal to the regime and so not recommending this was impossible. Therefore the Secret Police secretly noted that Stollberg’s self-critique at the public humiliations was not convincing: “His whole behavior was artificial” (Pasternack 2013a: 136). The arbitrary repression of a totalitarian
regime is nearly impossible to adapt to, thus, even 'sociology light' is a strategy in which reprimand cannot be avoided.

For me the reaction to this kind of repression seems to be important. There is, as Merton (1949) lists, a whole range of behavior with which to react to a situation in which one’s demands diverge from the opportunities a society offers. Rebellion would be a way to openly oppose the power structure. In her analysis of the work of GDR sociologists during communism, Sparschuh (2005: 40) criticizes the lack of open opposition in GDR sociology. There was no important dissident or central reformer within GDR sociology like Zaslavskaya (1984) in Russia. In a totalitarian regime, the arbitrary and harsh sanctions of the regime make rebellion an unlikely pattern of behavior: “Fear produces inaction, or more accurately, routinized action” (Merton 1949: 204). A plausible reaction to totalitarianism, therefore, is ritualism; in other words one no longer believes in the accepted goals of the society, but nevertheless one conforms to the means of achieving it. In Stollberg’s book on “Why and what for sociology” (Stollberg 1985) one sees a tendency to transform ‘sociology light’ into a ritualistic science. We will look closely at this book in what follows.

A general statement, which hinders sociology like other social sciences in communism, is that the totalitarian regime determines its Weltanschauung as a theory that is already complete. Ettrich (1997) sees this preset theory as a major difficulty that crippled the development of sociology in the GDR. In a personal footnote in his book, Stollberg states that the following questions are open: “Enthält der historische Materialismus sämtliche oder nur die Grundaussagen einer allgemeinen Soziologie?” (Stollberg 1985: 69). Both statements imply that the totalitarian Weltanschauung historical materialism must be kept as the dominant sociological theory. The aim of sociology is “to help by its research to uncover laws, formulate them scientifically, which determine the functioning and the development of society. These laws are called social or sociological laws” (ibid: 9). Despite this natural science terminology, Stollberg is not able to formulate “laws” beyond the Weltanschauung. He states that there are three important questions for sociology: “What historical stage is reached?” (ibid: 49); “What relations show up?” (ibid: 53); and “In what direction moves the development?” (ibid: 54). These questions (and their answers) indicate that the Weltanschauung of historical materialism should guide the research. As western sociology is interwoven with its method of production, it is deemed a “manager of social ills” (ibid: 158). One can conclude that, from this perspective, neither cooperation with international sociology nor autonomous work on sociological theory appears advisable, both are risky because one either competes with historical materialism or risks becoming ‘infected’ by foreign dangers. The way out of this dilemma is to concentrate only on sociological methods (ibid: 97–136), special sociologies (ibid: 75), and saying that sociology “fulfills an instrumental function” (ibid: 87). The concept of a ‘sociology light’ that Stollberg presents in 1985 is very similar to that in his book on shift work. He tries to keep to empirical, practical sociology, accepting the limits that the Weltanschauung places on his work. The low-key, subdued enthusiasm still perceptable in his work of the 1970s is turning into ritualism. The necessities of ‘sociology light’ are described as facts of a small world to which one has to adapt. Therefore, the risk of ‘sociology light’ within totalitarian regimes lies not only in the danger of being arbitrarily attacked—even if respecting its limits—but also an internal risk insofar as a growing experience of these limits turns scientific enthusiasm into ritualistic action.

Was the kind of GDR sociology that Stollberg represented at a dead end in 1990? In contrast with discussions on the dead end of parts of Weimar sociology in 1933, we find a number of actors who try to answer this question in the early 1990s, but hardly any contemporary discussion on this topic.
Stollberg himself indirectly answers the question; he became ill and did not participate in the debate. In terms of Merton's typology, one could say that Stollberg reacted to the new constellation of the sudden unforeseen revolution in the GDR by retreat, which seems to support the interpretation that the inner nexus between the totalitarian regime and 'sociology light’ was already loosened in the 1980s.

Younger East German scientists tried to evaluate the strengths and weaknesses of the sociology of work in Halle (Schippling 1992). It has been said that its empirical knowledge of a decline of work satisfaction between 1967 and 1987 was valuable, but it lacked a stronger link to the critical theoretical thinking of West German authors.

West German observers, like the director of the institute for research on work and professions, judge this work even more critically. Stollberg’s sociology of work was not sociology in a scientific sense because as it only aimed at adaptation of work and did not question its societal purpose (Buttler, 1992). West German Lothar Peter, a frequent guest at Halle in GDR times, was more differentiated: Despite the fatal connection of sociology with historical materialism and its concept of law, the sociology of work by Stollberg produced sound empirical research that he interpreted normatively instead of pursuing more the phenomena of a change of values (Peter 1990).

It is worth noting that because of the specific circumstances of the East German transformation—a combination of an East German revolution and unification with the far bigger West Germany—it was West German scientists who evaluated the continuity or discontinuity of East German sociology. Lepsius, as the West German organizer of the re-establishment of a department of sociology in Halle University, reported on the scientific qualifications of the GDR department staff. He evaluated them as scientifically sound and not as informants for the State Secret Police and recommended the prolongation of their contracts (Lepsius 2002). The ministry of the new Bundesland, however, refused to continue the employment of any of the sociologists without giving any specific reason. Thus the dead end of GDR sociology in Halle after reunification was institutionally quite radical.

In conclusion, one can state that there was a certain tension between sociology and the communist totalitarian regime of the GDR. This did not hold only for the period of its repression before the mid 1960s, but which lasted until 1990. The tension between this science discipline and the totalitarian regime is stronger than for many other disciplines, because the totalitarian Weltanschauung is seen as a superior sociological theory thus crippling open sociological reflection. Geiger’s thesis of a selective acceptance of a ‘sociology light’ in the form of social engineering has been found to reflect both the self-declared aims of its scientists, who would prefer the term practical science, and the constraints of regulated State research policy. Surprisingly however, and not in accordance with Geiger’s views, even the practice of ‘sociology light’ produced a number of conflicts, principally due to the arbitrary nature of the totalitarian regime, which enforced rigid programs of ‘sociology light’ which had a tendency to act out an inner logic and regress towards ritualistic science.

5. Elective affinity of sociology and democracy? Tension between totalitarianism and sociology?

Can we draw any general conclusions from this study? First, one has to draw attention to its quantitative limits; a single case study never guarantees statistical robustness. It would be worthwhile to test hypotheses of this study with a higher number of countries and years. However, that was not the aim of this research, which just focuses on the mechanisms causing either a tension
between sociology and totalitarianism or an elective affinity between sociology and democracy. From the existing literature, five hypotheses were put forward—coincidental; against core of sociology proper; only sociology light; totalitarian perception; shift of context—which can now be refined in light of the empirical material of 20th century development at one “normal” East German university.

The first thesis claims that the higher percentage of sociological dead ends as the Weimar Republic turned to a fascist regime was coincidental; that it was mainly the result of a higher percentage of Jews in this discipline. As I have shown, all sociology professors in Halle University were forced into exile in 1933. Even within the broader perspective of all professors of science of the state, of which sociology was a part, still 100% were forced to resign. Current estimates for all faculties state that the Nazis evicted 9% of all Halle professors (Bruch 2013: xxxiii–xxxiv). Therefore, in Halle social science evictions were 10 times higher than in other disciplines. Current estimates for German sociology professors evicted or exiled depend on methods used, but range from 42% (Wittebur 1991: 134) to 78% (Neuloh 1986: 24), a current reanalysis with data from Kürschners Gelehrten Kalender estimates 66% (Holzhauser 2015: 143). This would be about four times higher than the German average of 16% (Bruch 2013: xxxiiii). The high specificity of sociology and social sciences being forcefully evicted makes a coincidental relationship highly unlikely. Therefore, we have to refute the first thesis at least as most important process. A closer look at the Halle cases in the sciences of the state shows that Aubin was transferred for political reasons. Hertz, as we have seen, was also forced to resign for political reasons. (The fact that his grandparents practiced Judaism is, in this case, coincidental not causal.) Jahn was forced to resign in 1937 because the Nazis classified his wife as Jewish. Grünfeld, despite being Christian, was dismissed in 1933 because his grandparents were of the Jewish faith. For individuals the label “Jewish” was arbitrary as the Nazi criterion did not refer to individual actions or decisions but set irrational descent criteria.

A second thesis says that totalitarianism clashes with ‘sociology proper’. In the case studies above, we showed that a clash between totalitarian Weltanschauung and ‘sociology proper’ can sometimes cause tension. Hertz’s scientific books analyzing race studies as an unscientific enterprise, brought about the political pressures for him to resign. Hertz is rather singular in his scientific focus but, as Schleiff (2009) has shown, the organizers of the founding session of the German Sociological Association in 1910—especially Weber—arranged things systematically in such a way that the race paradigm was shown off as prototypical social science, which always comes to wrong conclusions because uses natural science theories to explain social phenomena. Therefore, being against race theories was part of the core mainstream sociological epistemology until 1933. As Hertz was present at some of these sociological meetings he could legitimately, with his critique of race theories, be seen as representative of mainstream ‘sociology proper.’

Despite being very different in content, historical materialism as a Weltanschauung of 20th century totalitarian communist regimes also clashed with sociology, as we showed in the case of Stollberg. Regarded as a superior sociological theory, historical materialism hindered sociological theory building in communist countries. Other sociological theories risked being interpreted as competing with the Weltanschauung. To avoid this risk many sociologists, like Stollberg, tried to refrain from theory building to reduce the likelihood of sanctions.

Thus, one has to conclude that an important mechanism that brings ‘sociology proper’ into tension with totalitarian regimes lies in the social science content of their Weltanschauung. Despite the fact that both Nazi and Communist Weltanschauungen tried to claim a natural science status for their ideologies, both had social science arguments that clashed more with sociology as a science than they did with natural sciences.
There is no systematic evidence that the tension between ‘sociology proper’ and totalitarianism results from a critical stance towards power in general, as Geiger suggests. A sufficient number of sociologists are willing to cooperate with totalitarian regimes for different reasons, a number that appears to be neither higher nor lower than in other disciplines.

A third hypothesis states that ‘sociology light,’ in the form of propaganda or social engineering, is more likely to be tolerated by totalitarian regimes than other forms of sociology. Indeed the case of Stollberg shows that social engineering was both the kind of sociology the state wanted and what its sociologists tried to deliver. In comparing Hertz and Stollberg we see that both propagate practical sociology, which some will classify as a form of social engineering because its aim is to find solutions to fixed problems. We see a clear increase of means at hand between 1930 and 1980 for such an orientation because empirical methods flourished at this time. However, the case of Stollberg shows that even ‘sociology light’ has the potential for tension with totalitarian regimes and that there seem to be different forms of conflict between ‘sociology light’ and totalitarianism.

Some are the results of a somewhat naïve interpretation of applied sociology, which are shared by advocates of practical sociology in totalitarian regimes and critics of social engineering. Both imply that a benevolent/malevolent manager/ruler will, with the help of social engineering, be able to rationalize his rule (and thereby stabilize the totalitarian regime). However, conflicts with ‘sociology light’ arise because the manager/ruler who is central to this totalitarian concept sometimes resists change (Pasternack 2013a: 128). Even the implicit critique of ‘sociology light’ of the empirically measured effects of shift work is seen as challenging to the existing order (Pasternack 2013c: 181–187). Conflicts of this kind are not uncommon in many fields of science, as all science—even sociology light—can innovate and challenge established views and routines. As shown in the analysis of the Stollberg’s book on shift work, one observes both an attempt to keep to the limits of ‘sociology light,’ but also to hold on to the critical information of empirical work. One could even say that conflicts of this kind, which take place both within the role set of social scientist within a totalitarian regime and beyond in relation to different interaction partners, are driving forces that may also motivate.

‘Sociology light’ is also affected by different kinds of conflicts about the general nature of the arbitrariness of totalitarian regimes. The circumstances around the Bahro affair show that standard procedures of ‘sociology light,’ such as including empirical appendices or evaluating PhD theses according to scientific criteria, can sometimes result in dangerous, publicly humiliating. We saw that these arbitrary interventions in social science work, which are far more common in totalitarian regimes than in democracies, seem to affect the habitus of ‘sociology light’ because a possible form of reaction to these interventions is ritualism. It would appear that the difference between ‘sociology light’ and ritualism is small as neither problematizes certain core values/institutions/aims. Seen from the perspective of the individual however, ‘sociology light’ is something practiced in a subdued consent, which allows the authorities to test limits from time to time and sometimes be proud of progress. Ritualism is a stricter form of hypocrisy because external forces are seen as so strong and incalculable that only a strict limitation of one’s actions can protect against sanctions. As we have seen, Stollberg seems to have shifted his ‘sociology light’ concept in the direction of ritualism, which reduces inner involvement with science. Other observers of ‘sociology light’ in totalitarian regimes have also come to the conclusion that it appears ritualistic in some core passages (cf. Ettrich 1997). In conclusion, the third thesis has to be refined on the basis of the empirical study. Sociology light in the form of social engineering is a concept that is more tolerated by totalitarian regimes and aimed at by sociologists, but it is no safeguard against tensions with totalitarian regimes and possible degenerations of its inner logic towards ritualism.
A fourth hypothesis states that the totalitarian perceptions of sociology as critical science bring about dead ends in sociology. We showed how external images were important in the development of sociology in Halle. In the case of Hertz and the dead end of sociology in 1933, we showed that he was among the few sociologists who was really a critical scientist and therefore, in his case, his status cannot be said to have brought about the dead end. In the case of Grünfeld and early sociology up until 1918, however, we showed that the image of sociology as a foreign (democratic) discipline was important in causing even young critical scientists with an affinity to sociological themes to distance themselves from the new discipline. In the case of Bahro, one can see that even raw empirical material can be seen as “critical” because totalitarian regimes tend to suppress any descriptive information about their state. Thus, in conclusion, the fourth thesis also has to be refined. Non-democratic regimes’ critical perceptions of sociology are decisive for the initial blocking or repression of sociology. However, at later stages the everyday procedures of sociology can cause tensions with totalitarian regimes that are largely independent of its general image.

The fifth thesis sees causal effects of a forced shift of context via emigration as resulting in dead ends in sociology. Hertz’s case supports the thesis that Weimar historical sociology could not be undertaken in exile. Like most other sociologists above the age of 55, he was not able to continue his work as a professor in exile (Fleck 2007: 236). Unlike many émigrés he had good language skills, he had contacts within both academia and public life in Britain, and he had sufficient resources to continue scientific work. Despite these exceptionally positive circumstances, the project of typological historical sociology shifted its focus in a way that strengthens Steinmetz’s (2010) hypothesis that historical sociology in the Weimar sociological tradition came to a dead end because it did not flourish after a forced change of context. Similarly, we saw that the GDR tradition of sociology of work ended with the end of the GDR. One possible cause is that it was already on the brink to petrifying into ritualism already before the demise of the GDR. More important, however, were the harsh West German political and scientific evaluations, which brought it to an end with the change of society. With reference to thesis five, we can confirm that forced shifts of context in a number of countries are an important cause of dead ends in sociology in the 20th century.

In conclusion one can state that sociology, out of reasons of its own logic—both as ‘sociology proper’ and ‘sociology light’—tends to be victimized more often in totalitarian regimes than many other scientific disciplines and therefore, dead ends of scientific lines of thought caused by shifts of political regimes are rather more likely than in other disciplines. We have shown a number of mechanisms that produce systematic tensions between sociology and totalitarianism. There are also a number of indicators that show how sociology prospers better in democratic than in totalitarian regimes. However, this elective affinity between democracy and sociology seems to result from democracy giving sociology more opportunity to follow its scientific aims than any specific preference or sociologists being more democratic in their views than other scientists. A systematic comparison of disciplines in their relation to political regimes was, however, beyond the limits of this study.

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FORUM

Passions, doux commerce, interest properly understood: From Adam Smith to Alexis de Tocqueville and beyond

Andreas Hess
a.hess@ucd.ie

Back to the future: Smith’s Theory of Moral Sentiments

Sometimes the changing perceptions of classic books and their authors tell a larger, often more complex, story. So it is with Adam Smith’s *Theory of Moral Sentiments* (TMS). TMS was Smith’s first and last book. Having gone through five editions—the last one being prepared almost from the deathbed—, its various editions sandwiched his other classic book, *The Wealth of Nations* (WN) for which Smith became a household name. Today Smith is first and foremost identified as one of the founding fathers of political economy, a fact that is perhaps no better expressed than by the Bank of England’s twenty pound note, showing the man and paying reference to WN. Such was the perception of Smith—until the onslaught of the current economic crisis.

As the current interest and wave of recent reception and scholarship makes clear, Adam Smith is making an unexpected comeback. What is new is that over the last ten years, and pretty much overlapping with the current economic crisis, the talk is now less about Smith the political economist and author of WN, or about political economy or the benefits of open markets and commercial activities, than about Adam Smith the moral philosopher and his ground breaking TMS. It seems that scholars have begun to discover and excavate the ‘real’ Adam Smith, the moral philosopher who wrote about sociability, virtuous behaviour and sympathy and who reasoned that these features must not necessarily be seen in contradiction to, but rather helped to embed market relations, commercial activities and pursuit of self-interest for the purpose of the common wealth.

Perhaps there never really was an Adam Smith Rätsel—an Adam Smith conundrum, that is, the assumed contradiction between the author of TMS and the author of WN—but rather a case of prolepsis, a re-projection onto Smith in the sense that he was assumed to have said things he could
not possibly have said. The story of such a conundrum deserves its own intellectual history since it is such an obvious misrepresentation of the man and his work, revealing perhaps more about the changing times and his critics than about Smith himself.

What I would like to do in this short paper is to outline briefly how the story of Smith’s (mis)representation is very much the story of its time expressed in thought, or to paraphrase Hegel, “ihre Zeit in Gedanken gefasst”. I will do so by focusing primarily on Smith and how he saw the passions and the interests and *doux commerce* coming together. In this context I will also briefly refer to a more political reading of this relationship in Tocqueville, mainly because it adds another transcontinental dimension to the debate. I will close with some speculation as to why Smith could have been misrepresented, mainly by looking briefly at those who seem to have been left behind in the development and debate and who, in anticipation of being forced to talk about their own backwardness, evaded the argument by trying to blame both the message and the messenger—a kind of hermeneutical incorporation in which the harbinger of the news gets obliterated (an unsuccessful variation of R. K. Merton’s obliteration through incorporation so to speak).

**Sattelzeit**

The German historian Reinhart Koselleck has coined the term *Sattelzeit* to signify a transitional period comprising roughly one hundred years between 1750 and 1850, a time comprising not only the late Enlightenment and the French Revolution and its wider political and social and cultural repercussions but also the take-off of the industrial revolution including the rise of commercial and market activities and networks. This was a crucial ‘axial’ time in which political concepts and notions were redefined or underwent radical revisions, or where new concepts emerged in tandem with, or as a reaction to, new realities and experiences. Particularly noticeable was that the new omnipresence of commerce, trade and industry posed a problem for traditional political language as it had been transmitted from classic times (and despite having been already re-conceptualized and enriched in the early modern period, especially in the Renaissance and the Reformation). The questions that this new axial age posed were: How could one conceive of radically altered circumstances? Was it possible to maintain the validity of classic virtues when everything else was changing? How did the new activities of commerce and trade relate to the passions and how could one make sure that the new virtues prevailed against the vices? And, last but not least, how did latecomers to the development and jealousy (of trade, of commerce, of industry) fit into the debate?

Thus the *Sattelzeit* became the great semantic tombola in which every concept received either a new meaning, was enriched by extension or was, as happened in some cases, overtaken and replaced by another concept. The tension between older notions and the attempt to conceptualize the new manifested itself in the writings in a number of prominent scholars. What is even more remarkable
was that these attempts at reconceptualization or semantical change were not limited to one country or nation. Rather, what remains fascinating about this axial time period is the international character of the intellectual network of this newly constituted republic of letters in which the most productive thinkers not only looked at their local or national circumstances but also for new evidence or new experiences outside their own country: no Hume and Smith without Montesquieu or Rousseau, no Kant and Hegel without Hume and Smith, and, to extend the argument beyond Europe, no American Federalists and no Tocqueville without Montesquieu, Hume or Smith.

The Passions and the Interests
Hume had been the first philosopher to alert his readers to the extent to which commerce, trade and industry had become modern features and needed to be addressed in their own right. As he rightly observed, there were no reflections about commerce or industry in the political tracts of Machiavelli or other republican thinkers; and apart from thoughts on property, there was also not much to be found in later contract theorists such as Hobbes and Locke. Hume’s essays rightly deserve the praise of having put commerce and trade and the way they relate to politics on the intellectual agenda. Hume was also right in dismissing earlier critics like Mandeville, who had famously argued in his *Fable of the Bees* that pursuing one’s interests had become the new virtue and that therefore political economy was really all about hypocrisy. Against such notions Hume tried to provide a more balanced view, partly drawing on Montesquieu’s ideal-type distinction in *The Spirit of the Laws*, i.e. monarchies, republics and despotism and their respective features honour (monarchies), virtue (republics) and fear (despotism).

Hume did see some merit in Montesquieu’s praise of England, despite some of the Frenchman’s exaggerations. Both were of the view that a constitutional monarchy like England did not fit any ideal type and that its governance actually combined republican and democratic features with that of a monarchy. ‘Mixed government’ was a hybrid form that was based on the impersonal rule of law, that guaranteed personal liberty and free opinion, tolerated a diversity of interests and that seemed in terms of trade and commercial activities more supportive when compared with the praxis of other nations. However, in the last instance it was not just institutions and mixed government that held all together but a collective ‘psychological’ predisposition which Montesquieu called the ‘spirit of the laws’. It was this spirit that explained the functioning of the English political and social system. It kept the passions in check and allowed for a myriad of different interests to be heard. Hume managed to build on Montesquieu’s argument and to take it a step further. Hindsight and having been in a position to study the developments in trade and commercial activities since Montesquieu helped. Hume also benefitted from other debates such as those in pre-revolutionary France. And last but not
least he could study the developments of Scotland first hand, particularly the changes that had occurred after the Act of Union with England.

Smith took over the baton from Hume—and by that I do not just refer to ‘economic’ matters. Benefitting greatly from teachers like Hume and Hutchinson and from the networks and institutions of higher learning in the ‘capital of the mind’ that Edinburgh (and earlier Glasgow) had become Smith produced his own great first work. *TMS* was a work of synthesis, perhaps the first attempt systematically to study sociability and to think about what held modern society together and what made it function successfully. As Albert O. Hirschman rightly pointed out, as a moral philosopher Smith was first and foremost concerned with what would replace traditional rule, inherited status and the values and norms associated with it. Could classic virtues still influence or even be able to control the passions? And what would happen to the passions in a new commercial system marked by different classes and interests? Any theory that still assumed that the world had not changed and that feudalism, agriculture and traditional values were still the most important features was clearly insufficient.

Smith pursued, almost with Durkheimian passion, the question of what kept a modernizing society together and what prevented it from falling or drifting apart. *TMS* is the attempt to spell out what we can realistically expect from each other and how we can build trusting and reciprocal relationships—and accumulated common wealth. Smith was looking for the possibilities of a new form of sociability, one that was able not only to bind the passions but also to steer them into the right direction, so that both the individuals and society as a whole would benefit. Smith’s *TMS* is a study of a moral psychology that does not treat individuals as if they were islands or monads who do not communicate with each other. *TMS* is a phenomenological description of how our moral actions are grounded in sociability and guided by sentiments like empathy and sympathy (compassion would be another word for these capabilities).

Smith argues that most of what constitutes moral action is due to listening to an inner voice, a kind of internalized normative reasoning that we have acquired while growing up—he calls it the ‘impartial spectator’—a capacity to reflect through an ‘imagined other’ about what is just or unjust, fair and unfair, appropriate action or not. The point is that its ultimate reason is not just pursuing one’s own selfish interests but to act in a way that is good for both, ego and alter. As one attentive reader has pointed out, for Smith ‘moral’ becomes almost synonymous with ‘social’; in other words, for him there is no social life and no social action that does not have a base in some form of sympathy or involve some form of moral judgement.

Of course Smith also knew that humans can sometimes behave selfishly or in egocentric and totally self-interested fashion. But in the long run he trusted that any person would naturally strive for what
he called ‘emulation’, the kind of soft competitiveness that is the outcome of a learning process in which private interest is increasingly replaced by public interest and a concern for the common good. For that to happen, however, human and anthropological features and sentiments like that of sympathy, resulting in turn in mutual expectations and concern for others, must, in order to grow, be safeguarded and protected by social institutions such as the family, the school or university, the law or the state. To put it differently, civil society had to draw on resources that economic reasoning and action alone were not able to deliver.

For some interpreters Smith’s WN seems to have argued the exact opposite. Sympathy, empathy, indeed any form of compassion should be substituted by the pursuit of self-interest. In other words, individual profit seeking should become the driver if not the all-determining prime activity. Against such one-sided reading any attentive reader of both TMS and WN will detect that Smith argues actually none of that—at least not on purpose. What WN intended was, first, to take a closer look at features like commerce, trade, industry which had come to play a more prominent role in modern society. Second, WN was written with the intention to think how these new features in our lives and the social action related to them could be used more beneficially so that the entire society—in Smith’s times this meant in the first instance ‘the nation’—could benefit from the new system called political economy.

Albert O. Hirschman famously referred to such thinking as ‘political arguments in favour of capitalism before its triumph’. If we look at the beginnings this way capitalism was not invented as a nasty trick to fool everybody, but markets and commerce existed for the purpose of helping the individual to free him- or herself from dependency or recurrent cycles of poverty and crises and from the shackles of traditional, feudal, agricultural society. It was meant to be an improvement, and at that a much more stable undertaking than what had been known until then. In contrast to Mandeville or those who defended unreconstructed republican values and virtues as if nothing had changed Smith thought of how the new features and activities of commercial society could actually contribute to the greater public good. He did not favour nor did he foresee the enrichment of the few in a the-winner-takes-it-all society or the enslavement of the many in a new industrial capitalist system. Smith always strove for balance, not for disturbance, or even worse, disintegration.

Indeed one searches in vain in Smith’s WN to find the word ‘interest’ or ‘private interest’ pejoratively used and beyond the mere technical sense of the word. The same is true for TMS. What one finds though is the idea that being involved in commerce, trade and industry would be beneficial to both the private and the public interest. More wealth was created than was the case in traditional societies, which depended overwhelmingly on agricultural production and a static system of property and ownership of the few—an unjust system by any standards. Smith’s idea was that passions could and should be channeled through interest as long as they were properly understood so that in the end
both ego and alter, and by extension society as a whole, would benefit from the new system. That for Smith was the deeper meaning of being engaged in *doux commerce* as the French called it, of importance here being the adjective ‘gentle’. To think of the commercial agent solely in terms of a profit-seeking human yet with an animal-like instinct and lacking the basic quality of empathy, sympathy or compassion, was not a notion one finds defended or encouraged by Smith. To read into Smith a *laissez faire* argument or a defense of unregulated markets or a hands-off state position is a-historical and false. In contrast, the political economical system described in *WN* was to be embedded in moral action (as described in *TMS*); i.e. being involved in commerce and trade did not mean to stand outside the moral realm or to act without morals. One has to read Smith in the intellectual history of his time to understand the relationship between the *TMS* and *WN*, not re-project and think of Smith a free marketer or even worse, a neo-liberal before his time.

**Echoes of Smith in Tocqueville**

Smith took an interest in American affairs and more than once declared that it might be a good idea for Britain to let the colonies establish their own commerce, markets and taxation system—from which he hoped Britain would profit more than it did with North America under colonial rule. As we know, some prominent thinkers among the American Federalists had taken Scottish Enlightenment ideas to heart and intended to put them into practice. Yet, the name of Adam Smith and that of David Hume or the title of any of their treatises or books never show up in any of the drafts nor were they mentioned in the minutes of the deliberations in Philadelphia. In the *Federalist Papers* Hume is only mentioned once and then only on the penultimate page. Such omission was probably related to other issues; after all, Hume had made no case in favour of any form of political theology while in Smith’s *TMS* religion is mentioned but is, unlike Locke, not essential to the central argument about sociability and sympathy. Both Hume and Smith would have had no truck with the Deism of some of the more prominent American Founding Fathers and the same applied vice versa.

On the other side Hume’s *History of England* and his *Essays* were much read texts. Adams refers to them repeatedly. But then again, Smith’s name comes up only very late in the correspondence between Adams and Jefferson and then only once in the context of Adam’s reading of Senator Tracy’s popular introduction to political economy. However, not a few observers have detected an elective affinity between the arguments made by Hume and Smith and Madison, particularly in Federalist No 10. The passages about the impossibility of a pure democracy resemble very much Hume’s arguments in his essays while the reflections about controlling passions and their effects in larger republics in which interests are pitched against interests sounded very much like a combination of Smith’s *TMS* and *WN*. 
More than four decades separate Tocqueville, the author of *Democracy in America*, from the Philadelphia Convention and *The Federalist Papers*. While Tocqueville cannot claim to have coined the term ‘interest properly understood’ is was Tocqueville who had filled it with life and who had given a detailed description on how sociability worked in America. Again what applies to Smith also applies to Tocqueville: while he was interested in political economy he was interested in what held the new society together and what made it work, something that cannot be reduced to political economy. Like Smith, Tocqueville saw that commerce and industry were activities that were of prime importance not just to Europeans but particularly to those who came to settle in America. For him, and that had been the whole purpose of his journey, their function appeared here to be more visible and, due to the lack of feudal shackles and an *ancien régime*, in purer form. The question was then how such interests in commercial activities related to habits and mores and how the new commercial activities could be reconciled with other aspects of the new democratic system.

Tocqueville had been an avid reader of the *Federalist Papers*; however, through reading Jean Baptiste Say’s political economy, and later Nassau Senior’s popular account of the same subject, he began to reflect on how the Americans managed to combine self-interest with the common good to form mores or what he called the ‘habits of the heart’. What is important to bear in mind here is that Tocqueville favoured a holistic view; he was interested in the political, social and cultural practices and dimensions rather than just economic activity or interest per se. For him, whoever discovered how ‘interest properly understood’ worked held the key to an understanding of modern man and society.

So how did it work? Tocqueville points out that for Americans there was no way that individual pursuit and individual motivation were ever to be abolished; they could only be controlled and be kept in check, in the first instance by being bound up with the pursuit of the common good. The trick lay in convincing the other person that it was in each other’s interest to be good. Such a conviction, once in motion, then becomes something like a social *perpetuum mobile*, a self-prolonging praxis which helps to bring forward a common sense of purpose, something that resembles very much what Smith had in mind when he talked about sociability (and was very different from Kant’s ‘unsocial sociability’). According to Tocqueville Americans did this successfully by taking small steps and by combining daily engagement with the notion of usefulness instead of favouring grand gestures, sacrifices or grand theories. Size mattered as well. For Tocqueville the small townships of New England in particular became local laboratories for democracy and contributed to maintaining the larger democratic entity that is the U.S. They did so by relying and having trust in a mixed system of governance which in turn consisted of direct democracy elements and involvement on the local level, and a system of staggered or layered representation organized along the subsidiarity principle (local community, county, state, federal government).
A sense of political equality and seeing personally the results of one’s engagement seemed to work, despite being conceived by Tocqueville as being circular and functioning rather like a self-fulfilling prophecy. To be sure such a system also had its negative sides, something of which Tocqueville was equally aware: because ‘enlightened egoism’ was almost never criticised in the American system it helped to create an unreflected consensus in which any opposition was immediately regarded as threatening to the majority consensus and was therefore in danger of becoming quashed or being labelled as dissent. As Tocqueville warned, democracy’s habits of the heart and ‘interest properly understood’ can have their negative counterpart in the tyranny of the majority. But that is perhaps another story altogether and would take us far beyond Hume’s and Smith’s understandings of the passions or the original notions of *doux commerce*.

Tocqueville wrote about commerce, trade and industry towards the end of Koselleck’s *Sattelzeit*. Taking 1850 as the historical cut-off point neither at the time of the founding of the American republic nor at the time of Tocqueville’s visit almost five decades later does America qualify as a capitalist country (if we mean by capitalist not just a system which is based in the division of labour, trade and commerce, but a system that is mainly based on industry and in which profit is generated from surplus production for the sake of making a profit). That development only took place from the middle of the 19th century onwards in the decade leading up to the Civil War, with the Civil War being the great catalyst that would turn the U.S. into a capitalist country. Any suggestion that Montesquieu, Hume, Smith, the Federalists or Tocqueville were simply early proselytizers for, or legitimists of a capitalist system that would manifest itself only decades later, or that the mentioned were somehow able to predict the future and act accordingly, is a serious case of prolepsis and mistaken.

**Back to Europe: The Smith conundrum in perspective**

The last point brings us to the final step of my argument. We have almost gone full circle but with one unanswered question remaining: how indeed was such a misreading of Smith and others possible, or, in other words how could Smith turn into a capitalist apologist? The answer lies, I suspect, mainly in the history of Continental Europe and particularly one country—Germany.

Karl Marx was not the only one to spot that for most of the early 19th century and perhaps as late as 1848 Germany was far behind its main competitors France and Britain—politically, socially and economically. In one of his earlier writings, the *Introduction to Hegel’s Philosophy of Right*, he complained bitterly that while the French had had their political revolution and the British their industrial revolution Germany remained a backward country that only had one aspect that worked in its favour: the capacity for philosophy and critique. From later works, especially from the *Grundrisse* and *Das Kapital*, we can detect that Marx was actually quite fond of Smith. He liked
Smith’s rigor and empiricism even though his critique of political economy contained a critical dialectical twist against Smith: Marx used largely the language of political economy while criticizing, socializing and even transcending it at the same time (the latter in the sense of Hegel’s aufheben or Aufhebung).

It was less the left-wing Hegelians like Marx than the more conservative-leaning hawkish Hegelians in the Prussian bureaucracy who read Smith as an advocate, or even worse, as an apologist for British aspirations. While an earlier and more progressive thinking Prussian circle of reform-oriented minds (Kant, Hegel, the two Humboldt brothers, von Stein and Hardenberg would qualify, Marx was never part of it although he stemmed from a rheinisch Prussian environment) still read Smith mainly as an enlightenment philosopher who had a keen interest in explaining sociability as emerging in tandem with the fast developing commercial society, later leading Prussian statesmen, secretaries, civil servants including not a few state sponsored academics like List (and later the so-called Katheder socialists like Schmoller), became alarmed about how uncontrollable the new political economy and the sociability it favoured would become. Particularly in the period after the War of Liberation against the French, roughly until the 1848 revolution and the fatal and for all purposes too-soon abandoned parliamentary convention in Frankfurt, a more conservative Prussian state and bureaucracy favoured a more state-centred approach that made sure that when it came to liberty and order, order would come first and economic, social and cultural liberty second. Forgotten was Smith’s TMS and in came a narrow reading of WN. Smith became ‘the other’, not only in terms of sociability and morals but also in terms of an economic reading of WN.

The later 19th century and the twentieth century changed all perceptions of Smith beyond recognition. TMS became soon forgotten, not just in Germany (having said that it made a short and surprise return in the height of the Weimar Republic only to disappear again soon after), relegated as it were somewhere to post-Humean but still pre-Kantian philosophic status. In turn WN was on the up, now somehow freed from the moral constraints of TMS. It enjoyed all the success I have referred to in the beginning until the algebraization of economics took over from Marshall onwards. Such is the history of modern capitalism, it cannot even get its theoretical origins right.

**Timeline**


1742: Bernard Mandeville: *Fable of the Bees*.


1754–62: Multiple volumes of David Hume’s *History of Great Britain* appear in instalments (all volumes are finally published in 1762 as *History of England*).

1776: Declaration of American Independence; death of David Hume; publication of Adam Smith’s *The Wealth of Nations*; first German translation of *WN*.

1777: First complete (posthumous) edition of David Hume’s *Essays: Moral, Political, and Literary*. Various earlier editions of the *Essays* were published between 1741 and 1772.

1784: Immanuel Kant: “Idea for a Universal History” (Kant declares A. Smith to be one of his favourite authors; Kant discusses ‘unsocial sociability’ in his essay).

1787: Philadelphia Convention.


1789: Beginning of the French Revolution.

1790: Death of Adam Smith; publication of revised 6th edition of *The Theory of Moral Sentiments*.

1791/95: First German translation of Smith’s *TMS*.


1803: Jean-Baptiste Say: *A Treatise on Political Economy*.

1803–06: G. F. W. Hegel: Jena lectures (with textual evidence that Hegel had read Smith’s Wealth of Nations).

1806–13/14: Napoleonic control over Prussia.

1813/14: Prussian War of Liberation.

1814/15: Vienna Congress; recovery of Prussia’s lost territories.

1831/32: Trip of Tocqueville and Beaumont to America.

1835/40: Publication of Tocqueville’s first and second part of *Democracy in America*.


1843: Karl Marx: “Critique of Hegel’s Philosophy of Right” (in *Deutsch–Französische Jahrbücher*).

1848/49: Revolutions in Europe; March Revolution in Germany; National Convention in Frankfurt (kleindeutsche option pursued by Prussia after Austria declares itself Empire).
Backhouse & Fontaine (eds.): A Historiography of the Modern Social Sciences

Matthias Duller
matthias.duller@uni-graz.at

Backhouse, Roger E., and Philippe Fontaine, eds., A Historiography of the Modern Social Sciences
Cambridge, New York: Cambridge University Press, 2014
pp.248.
ISBN: 978-1107037724
Price: $ 78.00

Four years after The History of the Social Sciences since 1945 (Backhouse and Fontaine 2010), British historian of economics Roger Backhouse and his French colleague Philippe Fontaine follow up with another edited volume titled A Historiography of the Modern Social Sciences. While the first volume provided a substantial overview of the various interrelations between the different social science disciplines, the volume under review pursues a methodological agenda towards a comparative interdisciplinary history of the social sciences. The editors’ central claim is that the histories of the social sciences have been written primarily by disciplinary experts for their disciplinary peers. Therefore these histories were oriented toward their respective disciplinary scholarly standards rather than historiographical ones. In order to allow for a literature that captures the social sciences after World War II as a single historical phenomenon, the editors argue, one needs to arrive at a better understanding of the various historiographical traditions within the disciplines.

The seven essays comprise an introduction by the editors followed by disciplinary chapters on the historiography of history (Kevin Passmore), anthropology (Henrika Kuklick), sociology (Charles Camic), psychology (James H. Capshew), economics (Roger Backhouse and Philippe Fontaine), and political science (Robert Adcock). All of the chapters are written by eminent historians of their fields and prove useful for the general reader insofar as they provide orientation with more (e.g. Adcock) or less (Passmore) concise expositions of the most important disciplinary histories since 1945 (and some of them before). What unites the chapters is their concern with basic historiographical debates of the history of science, like internalism vs. externalism or presentism vs. historicism, as well as various analytical approaches from biography to immanent theory discussion to institutional analyses. More innovative aspects concern other commonalities such as the apparently similar chronological dynamics of the emerging specialty of disciplinary histories that took place roughly around 1970, at least partially as a consequence of the massive institutional growth during the 1950s and 60s. In addition, the chapters show a variety of functions that historians of disciplines exerted
within those disciplines, ranging from legitimizing poorly recognized fields to theoretical reflection, open criticism and, finally, self-contained historical scholarship. Among the disciplines covered, it is the historians of economics and historians of psychology who have most clearly departed from their core disciplines and established relatively independent subfields with more or less distinct intellectual traditions. Historians of economics have been very close to intellectual history and were often related to “heterodox” economic thought. Historians of psychology have, among other things, prominently endorsed critical histories investigating power relations as well as the impact of psychological discourse on public life more generally. In contrast to these, historians of sociology have remained significantly closer to the discipline’s core theoretical debates, as Camic’s article aptly demonstrates.

Insights like these will make the book interesting to anyone who shares the authors concerns about a common history of the social sciences. By pointing to the surfeit of internally oriented histories of the social science disciplines the editors have, in my view, identified an important obstacle to such integrative literature. However, although some of the chapters (above all Kuklick, Camic, and Adcock) are individually interesting, the book as a whole leaves the reader a bit unsatisfied for several reasons. For one, it is almost exclusively concerned with the Anglo-Saxon world (mainly the USA). Germany and France receive occasional and cursory mention; with the rest of the world entirely ignored. More importantly, the authors seem to struggle with the somewhat confusing task of writing the history of how the history of their disciplines has been written. Passmore’s overloaded recital of alleged turns in historical scholarship is the most telling example. Only the analyses by Camic and Adcock succeed in analytically distinguishing the history of disciplines from the disciplines own historiography. But most importantly, the whole project of historiographical meta-reflection as it finds itself reflected in the volume is, in my view, not very useful in light of the objective to work “toward a comparative interdisciplinary historiography” (p.16), mainly because it implies virtually no comparisons. Instead the book delivers insights into the (micro-) histories of historians of disciplines. Judging from the chapters, the different historiographies appear to be characterized by similar pressures; that a reflexive subfield, to which the historians are exposed, is revealed within any discipline. Since neither these historiographical issues, nor any substantial issues are explicitly compared or theorized anywhere, it remains unclear in what ways this exercise enhances comparative historiography of the social sciences after 1945.

A simpler and better answer to the problem of comparative historiography of the social sciences would have been to actually compare the disciplines or, at least, to reflect upon the possibilities of doing so. One can only speculate why this most obvious concern receives little attention in this volume. Reading the introduction, which includes some discussion of previous comparative history of the social sciences, one gets the impression that this might be a result of the editors’ methodological preference for a history of ideas approach together with an apparent bias against social science history. In several footnotes (sic!), sociologically-minded works are characterized as having a “narrow conception of human agency”, employing “accounts […] geared toward defending the progress of scientific reason” (p. 9), or viewing “history as the mere instrument of a reflexive science.” (p. 9) Not only do these allusions seem inappropriate in light of the interdisciplinary sermons upon which this book is based, they are also programmatically ill-conceived. The field of comparative history and historical sociology is an area where boundaries between history and social science are at their most permeable and, as a consequence, it offers the best methodological tools for the issues Backhouse and Fontaine are addressing. In disregarding this tradition and insinuating the existence of a general opposition between history and the social sciences, it is unfortunate that the editors miss this important point.
BOOK REVIEW

Erickson et al.: How Reason Almost Lost Its Mind

Wolfgang Goederle
wolfgang.goederle@uni-graz.at

Pp. vii+259
Price: $35.00 (cloth)

How Reason Almost Lost Its Mind is an unusual book, for several reasons. It is one of the very few examples of collective authorship of more than two or three writers in the Humanities/Social Sciences. Six renowned authors, some of whom were very well known, gathered in Berlin for six weeks in the summer of 2010 in order to produce the manuscript.

The study that resulted from this process tries to investigate the gap between reason and rationality that opened up after World War II in the thinking of intellectuals who advised politicians and decision-makers in the United States during the Cold War era. This gap constitutes what the authors seek to analyze, they coin the term “Cold War rationality”. Unlike the historical actors of the study, the authors do not believe in the universality of the idea of rationality that emerged, matured and finally diffused into other fields of research, and even daily life, between the 1950s and the 1980s.

The first chapter begins with mathematician Merrill Flood trying to gain insights into human decision-making by observing and questioning his three teenage children. Neither Flood nor most of his colleagues at the think tank RAND in Santa Monica had any doubts about the fact that rationality basically concerned rules, though conceptions of rationality had changed since they became central to “western” thinking during the Enlightenment. Erickson et al.¹ give a rough outline of the historical direction of rationality between the 18th and mid-20th centuries, before raising the question of how rationality and rule-bound calculation processes could become so close after World War II.

In chapter two, the procedures that allowed the Berlin Airlift to operate are described. Operation Vittles had to be planned and calculated according to a rule-set that was supposed to allow the population of Berlin to be supplied at minimum cost and with minimal effort, but that maximized output. Limited computing capacities rendered the complex calculations necessary for these procedures difficult, although the US Air Force had already begun to streamline operational

¹ The order, in which the authors appear on the cover was determined by computerized randomization.
efficiency via scientific computing. Instead of sticking to an optimal solution found by the algorithm, which could not be achieved as decision-makers had no access to complete sets of information, the introduction of “satisficing” results helped to shape a way out of the dilemma.

While chapters one and two shed some light on the formalization of decision-making under the constraints of a trend towards rationalization, the subsequent chapters scrutinize rule-bound rationality in action.

Chapter three investigates the Cuban Missile Crisis, presenting the different viewpoints of researchers and advisors. While Herman Kahn—one of the key figures of the book and author of *On Thermonuclear War* (published in 1960)—believed in rational choice theory, its critics had severe doubts about its validity under conditions such as the Cold War. Among these critics were Charles E. Osgood and Irving Janis, both of whom were psychologists. Their concerns concerned the limits of rationality under extreme conditions. Osgood suggested a de-escalation of the arms race, and so developed a program that he called GRIT (Graduated and Reciprocated Initiatives in Tension Reduction). Janis’ concept of groupthink sought to explain the interference of emotions with rationally correct choices.

Consequently, chapter four deals with small group interactions, studied by social psychologist Robert Freed Bales of Harvard, who also worked with RAND. Bales recorded group interactions and tried to extract fundamental dynamics in order to develop a model that allowed for the description of the basic processes in group formation. His method aimed to find reliable parameters for the building of stable and functional groups in terms of split-second decisions. In other words, the kind of decisions to be made by small groups of officers in the context of the Cold War.

Chapter five focuses on Game Theory and both its mathematical and psychological dimension. Its development is scrutinized and illustrated by the famous Prisoner’s Dilemma, as described by Anatol Rapoport. According to the authors, Rapoport over concentrates on interactions and neglects the personalities of the people involved. Another laboratory researcher on the prisoner’s dilemma, Morton Deutsch, places too much emphasis on personal qualities. Both fragmented decision making in certain ways in order to make the underlying procedures objects amenable to formalization.

Chapter six describes the end of Cold War rationality. After two decades of formalizing decision-making in order to safeguard the (Western) world against the consequences of a possibly irrational decision, in the late 1970s it became apparent that there was a gap between the formal standards established and actual human performance. Game theorists reacted with disillusionment to this insight. At this time, Cold War rationality lost much of its energy, though it did not disappear completely and still lives on in certain fields of research and expertise.

Beside the fact that “How Reason Almost Lost Its Mind” is a lucid, coherent argued and very well written masterpiece of the History of Science, it has further qualities that make it an extremely important contribution to a broader collection of academic fields. It delivers an outstanding social history of a particular, small, though very important and influential group of white Americans during the Cold War era. The people, scrutinized in this work shaped a certain perception of the “West” for most of the Post War era for most of the “western” world. “How Reason Almost Lost Its Mind” provides extensive and lucid insights into a particular cultural history, and raises questions that deserve further research. The dynamics of Soviet Cold War rationality, for example, upon which they touched briefly in the introduction.
However, exceeding its original value as an extremely rich and original investigation into the idea of rationality in the second half of the 20th century, the study lives up to the promise of collective and truly trans-disciplinarian scholarship in the Humanities and the Social Sciences.
Anthropology has historically had a reputation as a “welcoming science” that, from its earliest organizing efforts, attracted a higher proportion of women and people of color than other disciplines. The primary method of knowledge production in the discipline, ethnographic observation, has a “profound” or “mundane” availability that, at its most basic, does not necessitate special equipment or complex measurement techniques, and the discipline’s overall epistemological project of giving a complete account of human history is inherently democratic and inclusive. However, the professionalization of anthropology in the United States in the early twentieth century was characterized by the constitution of increasingly restrictive boundaries between “amateur” and “professional” work that limited entry through requirements of stringent training and qualifications, placed emphasis on specialization and insularity, and restricted the rhetorical flexibility of and access to the ethnographic monograph. In this professionalization, women and racial minorities were increasingly marginalized: even when they were able to obtain restricted professional certifications, publication contracts, and research grants, they rarely obtained permanent professional and faculty positions. Compounded with this, anthropology has also had profound ties to colonialism. In the U.S. this is especially evident in the ways anthropology claimed intellectual jurisdiction and authority over the interpretation of Native American and African American communities and in the discipline’s contributions to governmental mapping and collecting projects, which mutually solidified government surveillance and control and anthropology’s claims to professional scientific status.

The central argument of Risa Applegarth’s intriguing study is that despite this marginalization and inequality in the early professional history of anthropology, which she traces in the introduction, women and people of color developed novel, alternative genres of publication that allowed them to claim the epistemic and rhetorical resources of anthropology while also developing critical, alternative anthropological knowledge that challenged foundational assumptions of the discipline’s exclusions and claims to objectivity. In addition to drawing upon work in science studies, Applegarth’s study engages with the “new genre studies” in scholarship on rhetoric and composition, in which “genre” is conceptualized as a flexible constellation of regulated but improvisational
strategies that writers adapt to their needs in order to achieve rhetorical social actions, rather than as fixed and formal rules of composition. Genres are constraining but also productive sites where community boundaries and norms may be defined and where a variety of collective actions may be coordinated; and thus, genres connect rhetorical activity with relations of power by “disciplining” or “normalizing” actors in ways that reflect and shape social relationships. Individual texts never perfectly enact social norms, and writers can combine or adapt rhetorical strategies across genres, which makes an examination of genre a particularly productive focus to reveal discursive and epistemic tensions and innovations. As a side note, Applegarth also points out that scholars in rhetoric and composition studies have recently taken a particular interest in employing ethnography in their own research, because of its promise as an interpretive and generative process of research into rhetoric.

In chapter one, Applegarth examines the “ethnographic monograph” as the “privileged genre” for the production and presentation of anthropological knowledge. In particular, she argues that the variations in the genre (length, extent of interpretation, qualifications of authorship, target audiences, and especially the ways fieldwork is textually described) were restricted as part of a collective process of demarcating scientific from non-scientific practices. In the 1920s and 1930s, in particular, writers developed rhetorical and epistemological representations of the “scientific rationale” of fieldwork that excluded or deemphasized gender and racial inequalities, personal and embodied relationships, and historical location in ongoing cultural dynamics that characterize the ethnographic encounter.

In response to this increasingly restrictive genre, women and people of color in the United States developed short-lived alternatives in the 1920s and 1930s, which Applegarth examines in chapters two, three, and four of the book. The first alternative Applegarth identifies is the “ethnographic” or “field autobiography,” exemplified by Ann Axtell Morris’s 1931 Digging in Yucatan and 1933 Digging in the Southwest and Gladys Reichard’s 1934 Spider Woman. Unlike traditional autobiographies, which tend to emphasize family and personal history, these narratives relate the author’s personal experiences of undertaking field research, and thus focus almost exclusively on periods in the field. This form of narrative draws rhetorical resources from technical monographs by its presentation of specialized anthropological knowledge and its focus on the rigors of field research, allowing the authors to represent themselves as competent anthropological researchers, but it also renders the practice of that research emphatically personal and gendered by foregrounding the identity and embodied presence of the researcher. Claims are grounded in “embodied authority” instead of by relating impersonal “objective” findings that minimize the representation of the researcher’s presence in the text. The narratives of these ethnographic autobiographies follow the actual embodied movements of researching as a process of discovery, registering sensory and emotional responses, and showing how the researcher learns by negotiating practical social situations and training her body over time. These authors also narrate the various disruptions and often-restrictive material circumstances that impose themselves upon the researcher, but which are typically elided in technical monographs. Morris’s and Reichard’s studies work to educate audiences about the methods and practices of study, actively “recruiting” the readers rather than withholding insider information, and they represent fieldwork as a domain offering adventure, intellectual stimulation, and freedom of activity (“gendered pleasures” otherwise limited for women). In these and other ways, the ethnographic autobiography presents an alternative to the systematic exclusiveness of professional ethnographic monographs and its typical presentation of knowledge as authoritative, finalized, and impersonal.
The second alternative genre is the “folklore collection” assembled by “native ethnographers,” exemplified by Ella Cara Deloria’s 1932 Dakota Texts and Zora Neale Hurston’s 1935 Mules and Men. The status of both of these women as members of the communities they studied presented tensions for the ways they participated in and observed those communities and blurred distinctions between their status as subjects and objects of anthropological knowledge. As insiders, their detailed knowledge of cultural practices was valuable in anthropology, but they were also critical of the abstraction, decontextualization, and static depiction of cultural practices that characterized collections of folklore compiled by other anthropologists. Instead of grounding their knowledge in this objectifying epistemology, Deloria and Hurston represented their specific relationships and experiences in the communities they studied. Their studies demonstrated the ways community members challenged their claims and actively participated in the production of knowledge, and thus disrupted the epistemological authority that anthropology claimed over the interpretation of community practices. These authors also emphasized the communal contexts in which folklore was produced, and they showed the vibrant uses folklore had in the ongoing lives of dynamic communities. This strongly contrasted with the typical “elegiac” anthropological narrative that sought to preserve “authentic” native stories before they supposedly-inevitably disappeared. Deloria and Hurston did not abstract folklore texts by characterizing them as generalized “types” for collation and comparison, but attributed them to particular authors, communities, and contexts. And like the field autobiographies discussed above, they introduced themselves into the narrative and highlighted the overtly racialized and gendered positions they occupied in their communities, and by implication in the scientific community.

The third alternative, discussed more directly by reference to Gladys Reichard’s 1939 Dezba, Woman of the Desert and nearly a dozen other exemplars, was the “ethnographic novel.” These works sought to depict accurate ethnographic knowledge of communities through the device of a fictional narrative. Reichard and others drew from the features of the genre of literary novels, which allowed them to humanize the psychology of native people by displaying the motivations and emotional attachments of their characters, to address the complex relationship between individual choices and social forces by narrating characters with different but intersecting trajectories over time, and to comment on and criticize government policies and challenge the finality of anthropological authority by highlighting multiple, competing voices within the text. A particularly popular topic of these ethnographic novels was the experiences of Native Americans with government-sponsored boarding schools intended to assimilate native youths to the dominant white American culture. The narratives of these ethnographic novels often highlighted the multi-faceted and ambivalent relationship people had with these schools, and exposed their often-harsh practices and fundamentally problematic and dubious results for native communities.

Rather than rejecting scientific discourse, the writers of these various alternative genres sought to draw upon the epistemic and social prestige of science for more diverse and inclusive ends. These innovative, deviant genres were short-lived and the authors were marginalized from the centers of the discipline’s institutional power, so it is perhaps not surprising that they were subjected to “historical erasure” from the record of anthropology. However, Applegarth notes that they are self-reflexive in a number of surprising ways, and that their recovery may hold insights for contemporary attempts to develop critical ethnographic knowledge in anthropology and other disciplines. Applegarth’s study makes major contributions both to the recovery of these fascinating texts and to the study of rhetorical and epistemological strategies of depicting knowledge in these and other texts.
Applegarth concludes the book by laying out a case for a kind of research that she calls “rhetorical archaeology.” Genres, in this view, are “material traces” in which researchers may decode the “epistemic negotiations” of academic disciplines, which are otherwise so difficult to reconstruct in historical retrospect. Writers exercise rhetorical agency in producing texts through the ways they repeat, subvert, establish, or destabilize the rhetorical possibilities of genres, and they thus also shape what other writers in turn view as possible or effective ways to act. Tensions and alternatives that are otherwise submerged in a discipline become visible in these traces, and a discipline’s knowledge can be examined as contested and negotiated, incomplete and developing over time by the ways rhetorical strategies are enacted in texts. Such an approach can interpret the relative “position” and “sequence” of rhetorical actors to help understand their relative success in shaping the discipline, just as archaeology uses these notions to interpret material artifacts. For Applegarth, this archaeological metaphor contrasts strongly with evolutionary metaphors for professional and scientific progress by conceptualizing the agency of historical actors as well as the interpretive work to recover the traces of those actions.
BOOK REVIEW

Bourdieu & Chartier: Sociologist and Historian

Daniel R. Huebner
drhuebne@uncg.edu

Pierre Bourdieu and Roger Chartier: The Sociologist and the Historian, translated by David Fernbach
Pp. xxii+82
ISBN: 978-0745679587
Price: $49.95

This slim volume consists of the edited transcripts of five interviews of sociologist Pierre Bourdieu conducted by historian Roger Chartier in December 1987 (broadcast in January 1988 on the French radio program À voix nue), along with an introduction added recently by Chartier. The interviews were previously published in French in 2010 as Le Sociologue et l'historien (Éditions Agone, Marseille, & Raisons d’Agit, Paris), and have been translated very effectively into English by David Fernbach. In his introduction, Chartier seeks to contextualize the interviews by locating them in the chronological development of Bourdieu’s work and its public reception, and in the development of the discipline of history in France and its engagement with the social sciences. Because of the title and the co-authorship implied in the attribution of this text, the reader might expect a dialogue of ideas between scholars and disciplines on a more equitable ground, but instead Chartier is constrained by his position as the interviewer, and the encounters dwell on Bourdieu’s thoughts and concepts. Indeed, Chartier brings unique reflections to the interviews, and serves as an excellent interviewer, but it is Bourdieu’s conceptual framework that is debated throughout, and not Chartier’s own contributions to historiography and the history of written culture.

Bourdieu and Chartier reflect on the nature, constitution, and uses of “fields” and “habitus” especially, among many of Bourdieu’s other concepts and claims. There are, for example, productive discussions of the constitution and transformation of scientific and artistic fields, of the sociology of intellectuals, and of the possibilities of scientific progress, which draw heavily from Bourdieu’s (previously or soon-to-have-been) published studies. The reader also encounters interesting reflections on Bourdieu’s relationship to the work of other scholars, including Michel Foucault and Norbert Elias, that are perhaps unique. The text is enlivened by anecdotes and examples from Bourdieu’s own life, and Bourdieu offers interpretations of his published work as well as sociological reflections on its polarized reception. In his introduction, Chartier argues that the real virtue of the interview format is in reviving Bourdieu’s way of thinking in real time, and in recording him in a context freed from the various academic and political roles he was compelled to play.
In particular, Chartier’s introduction highlights the “anxiety” that Bourdieu evidently felt in his efforts to understand the incomprehension of, as well as the violent resistance to, his work. There are glimpses in the interviews of Bourdieu seeking to convey how his self-reflexive sociology puts him in the position of seeking simultaneously to carry on discourses at several levels. In one interview, he draws from Gaston Bachelard’s characterization of the use of “quotation marks” in science to help make his point (p. 16): sociologists engage in recording value judgments as social facts, for example, and so put those judgments in quotation marks, simultaneously claiming them and disclaiming or seeking to comment upon them. Sociology, for Bourdieu, attempts both to “preserve and destroy” distinctions in its analysis, and always maintains a running “metadiscourse” that implicitly warns, “Be careful what you read” (p. 18). He acknowledges the “impossible” situations that confront researchers who attempt to study the social worlds they, themselves, inhabit, and he underscores the skepticisms that plague the social sciences.

Still, there is much more to this volume than a revived sense of Bourdieu’s thoughts and anxieties, and Chartier sells himself (and Bourdieu) short if we miss the work these two scholars undertake to conceptualize the relationship between sociology and history, and in particular to use history as a crucial proving-ground for Bourdieu’s concepts. These aspects of the interviews will appeal to historians and sociologists of the social sciences, even if the discussions serve to raise many more questions than they answer. At several points in the interviews Bourdieu and Chartier challenge the taken-for-grantedness of the classificatory schemes of investigators, whether they are historians anachronistically applying contemporary notions to historical problems or sociologists ethnocentrically applying their class-based notions to other groups in contemporary society; both disciplines alike must continually take up the task of “questioning one’s own system of questioning” (p. 13) as the very object of their analysis.

Chartier argues that Bourdieu’s notion of “field,” in particular, provides a tool for conceptualizing historical discontinuities lying beneath apparent continuities, and for preventing the “reductionist naiveties” of historians who directly relate social positions to cultural practices without examining the “mediations” or reformulations between position and practice imposed by the state of a field (pp. 62, 74). In these endeavors, Chartier argues, historians and sociologists have a common object of analysis: the conditions of emergence of social fields. These two interlocutors also consider the challenges of conceptualizing long-term processes in terms of fields, and one particularly novel, but inadequately resolved, direction of their discussion is whether scholars can use the notion of shifting and differentiating fields to understand discourses that are, in some sense, “about” the same thing but employ different languages and practices available in the fields of different contexts. Chartier uses Molière’s theatre and Bourdieu uses Flaubert’s novel as examples of “discourse about the social world” before the differentiation of a “sociological” field (pp. 74-8). This leads to further, although relatively brief, reflections on the work that “form” or “mode of writing” (including the various forms of narration and evocation) does in objectivating reality in different ways.

Even though Bourdieu and Chartier challenge simplistic distinctions between the disciplines of history and sociology, across the interviews they both seem to propose that the stance taken toward the objects of analysis in the two fields, and the reactions that those stances provoke publically, are very different. History takes its object to be “always at a remove” and the interests of the subjects of its analysis as of “a different order” from those of the investigators (p. 45). This stance “neutralizes,” or provides “protection” from the radical challenge of social analysis, even allowing it to function as a discourse that “comforts and reassures” – “supplying roots, references, identities to those who perhaps feel a lack of these” – or as Bourdieu quips, writing history books becomes the “production
of Christmas presents” (pp. 5, 45-7). Sociology lacks the claim to neutralizing distance, and instead takes as its object the “present” in the sense of whatever is “still sufficiently alive to be the object of struggles,” even when that includes past events such as the French Revolution (p. 16). It is constituted as a discipline, then, in a provocative stance toward ongoing struggles, leading the discipline to be subjected to a “permanent test” of justifying its existence, and obliging it (at least potentially) to “a permanent lucidity about its own existence” (p. 48). This distinction between social phenomena objectivated either as “past” or as “present” is certainly intriguing, but its problematics and implications, perhaps especially for endeavors in “historical sociology” or “contemporary history,” deserve to be further worked out.
In this contribution to a series of histories on national sociologies, Kristoffer Kropp holds that disciplines are not to be seen as ‘natural’ or logical divisions of a unified intellectual framework but as sociological constructions. Adopting ideas from Bourdieu, he depicts established scientific disciplines as bounded social spaces that exist alongside and interwoven with other social spaces, and most particularly fields of action. Kropp understands a discipline to be a social formation that organises frameworks of cognition and communication geared to the production of knowledge. A disciplinary space comprises norms that constitute it as a field of power and struggle: struggles with other disciplines and external bodies to establish its autonomy and informal struggles between advocates of different conceptions of its specific forms of knowledge. Across the world, disciplines have been introduced in state structures and private-controlled organisations, as universities and research institutes, in response to changing economic and political demands. In consequence, the general process of institutionalising scientific knowledge in a system of disciplinary practices has taken various different forms that reflect variations in trajectories of economic and political development.

In Denmark, Kropp argues, the political project of building a social democratic welfare state has conditioned the ways in which sociological knowledge has been pursued and institutionalised. What came to be called ‘sociology’ originated in distinct strands of philosophical and statistical explorations of social issues. During the 1870s and into the 1920s, a sociological approach based on British empiricism and the evolutionary thought of Herbert Spencer was developed in work by Hoffding, Wilkens, and Starcke. This became the basis of teaching programmes that, however, did not outlive their founders, who did not secure the financial requirements for a lasting base for the subject. The work of statisticians in building on survey methods proved no more successful.

Not until 1936 was a full professorship established, at Aarhus, when German émigré Theodor Geiger was appointed to a post in the Business School. Despite his classic work on social stratification, the chair ended with his sudden death in 1952. At Copenhagen, economists had successfully established a chair of sociology in 1950 and appointed the Norwegian quantitative researcher Kaare Svalastoga, who built a Department of Sociology to which a number of teaching assistants and assistant professors were appointed during the 1950s and 1960s. A rival Department of Organisation and Industrial Sociology was formed within the Copenhagen Business School (not then part of the
university) and the government established the Institute for Social research, both of these undertaking commissioned research independently of the University Department.

This tripartite structure was not to be a secure base for the discipline because of intellectual differences between the units. Svalastoga’s commitments were to American-style functionalism and survey research, while the DOIS adopted a more critical approach, inspired by C. Wright Mills’s visit to Copenhagen in 1957. This diversity was further stretched when the Faculty of Humanities in Copenhagen University established a new professorship to train school teachers in sociology and formalised this as a Department of Cultural Sociology in 1967. Thus, there were two rival Departments of Sociology within the same university. Kropp, unfortunately, does not explore the reasons why Svalastoga was, apparently, so opposed to broadening the remit of his Department and what university politics lay behind the formation of two rival Departments in separate Faculties.

The growth of student discontent after 1968 proved fateful, with the Department of Cultural Sociology supporting student participation and the Department of Sociology opposing it. Discontent was furthered by the growth of a Marxist commitment among some younger members of Svalastoga’s Department. He return of a right-wing government in 1982 led to cutbacks in sociology programs at the new universities of Roskilde and Ålborg and both Departments in Copenhagen were closed down in 1986. Not until 1992 was a new, single Department of Sociology re-established on a pluralistic and more policy-oriented basis.

Sociology in Denmark is now institutionalised as a discipline and is well-embedded in interdisciplinary programmes and structures. It is organised in relation to the Danish welfare state as professional strategies have taken advantage of funding sources available. Danish sociologists have, however, been seeking to recast their activities in line with more detached academic concerns. The subject’s institutionalisation is, therefore, weak: Kropp finds it to be as weak as it was 100 years ago as it lacks academic autonomy.

This is an interesting and thoughtful analysis of the development and current state of sociology in Denmark and it will be a useful resource for many. What is missing from Kropp’s analysis is any conception of what sociology ‘ought’ to look like as a discipline. A socially constructed ‘discipline’ becomes the carrier for intellectual idea, but is there an intellectual space that needs to be occupied by a particular kind of sociology? What is the intellectual space of sociology in scientific culture and what ought to be the relationship between this and the departmental structures that define the social spaces of the discipline? Should there be a one-to-one relationship between these in all cases, in some leading cases, or in none? The fundamental question is whether sociology can survive as an intellectual project without such disciplinary organisation.
Per Wisselgren has written a remarkable account of an extraordinary series of events in the period of pre-academic reformist social science. The story is about a bequest by a sickly student enthralled by the German historical school of economics, and of the prospects of sociology as a means of social transformation. The bequest was to create a foundation. Shortly after it was drafted and signed, its author put it into effect by killing himself. Wisselgren tells his story, and the story of the student’s intellectual and familial milieu, in a sensitive and compelling way that takes one back to an era in which “the social” and the poor were being discovered and conceptualized.

The donor, Viktor Loren, had a childhood marked by the death of his parents. He grew up with a literary, intellectual family—the Leffler’s—where he was close to the daughter, Anne Charlotte. She became a prominent writer and force in the Loren Foundation. Loren’s father was an alcoholic Goteborg brewer who, after his wife’s death, first passed the young Viktor to a business family, who became his guardians, and then on to the Lefflers. Viktor was wealthy enough to pursue whatever life he wanted. But, after a brief and unhappy stint as a student in Uppsala, he went to Germany and, as a student at Leipzig, got caught up in the discussion of “The Social Question.” He took courses given by Wilhelm Roscher and Wilhelm Wundt, as well as statistics and history and he read Adolf Wagner. According to him, he became “half a Katheder Socialist” (65).

The date was 1877. The social question had barely emerged in Sweden as a respectable topic per se, much less an academic one. Wisselgren describes the experiences of Swedish academics who became interested in the poor and in the social question in this and earlier periods as not on a path to success. The political options were not promising. Viktor denied that he was a Social Democrat. He agreed with their demands, and rejected attacks on them, but thought that the forces arrayed against them were too strong. Like the Fabians and the Verein fuer Sozialpolitik, which Wisselgren discusses, Viktor thought reform required another path; social science. But, with the exception of economics, there was no social science in Swedish universities and resistance to this was also strong.
Viktor caught a bad cold during his first year in Germany, and spent much of the next few years reading, thinking and travelling in warmer climates. He absorbed the social science of the time, in which socialism and Darwinism were prominent themes. He wrote a “dissertation” on his ideas about social science, but it was never of sufficient quality to present at a German university. His health continued to decline, presumably a result of tuberculosis, and he devoted his energies to his last will and testament. This established a foundation for the support of social science and the scientific study of the social question. In December of 1885 he shot himself.

Here the story shifts to the execution of the will, and how Loren’s fortune was spent. Loren selected five board members who made the decisions. Wisselgren reconstructs their thinking as a kind of “thought-collective.” They worked from scratch; there was no established social science to which to give money. So the money went into development supporting training of native talent, mostly in Germany. From the beginning, those supported aimed at getting into university positions both new and established, which required effort on multiple fronts. There was a “chicken and egg” element to this: positions were needed, but also people to fill them. There was also the question of why existing academic resources were not sufficient, economics was already somewhat established, but still fragile. Economics, however, was to be the bridge to academic positions.

Lawyers have a saying: where there is trust, there is a lawsuit! Loren’s Gothenburg relatives contested his will. The suit failed, but it delayed the implementation of the foundation. The responsibility fell on five trustees, including his friend Anne Leffler and her brother Johann, a physician named Key, an academic economist from Uppsala named Davidson, and Sonja Kovalevsky, a mathematics professor and literary figure close to the Lefflers. The list is unusual, but not out of step with other reformist movements of the era that tended to comprise doctors, scientists, economists, and literary types in avant-garde circles. What is fascinating about Wisselgren’s treatment is that he has miniaturized the complex and confusing picture of social movements of the period in Sweden to fit the small and tightly-knit bourgeoisie of the Swedish capitol, a city that contained all of the elements of the larger society.

Because of the small scale, the number of individuals involved is both small and intimately associated and so Wisselgren is able to give, in spite of limited archival material, a very striking portrait of the participants, the milieu, and the motivations of the people involved. This makes this a unique contribution to a literature that is now expanding on the lives of the reformers. It is comparative with *Envisioning Sociology: Victor Branford, Patrick Geddes, and the Quest for Social Reconstruction*, by John Scott and Ray Bromley (2013). The Swedish situation differs in some interesting respects, however: the importance for social discussion of playwrights, notably Strindberg, and literature generally, especially in relation to “the Social Question”; and the relative accessibility of both parliamentary and academic institutions. Wisselgren makes much of the role of literature and literary depiction in relation to, and also partly as a rival to, the nascent idea of a social science.

There is one other fascinating difference. Many of the nineteenth century social reform movements were religiously related and/or motivated. In the case of, Comte’s religion of humanity it was an actual church, or, in the case of Branford and Geddes, greatly concerned with the spiritual aspects of social life. In contrast, Loren and his friends were not only Darwinians, like most of the pre-academic self-described sociologists, but also materialists in the sense of Turgenev’s Bazarov. This attitude appears in Loren’s own writings though, as Wisselgren notes, there is some ambiguity as to whether this is representative of Loren’s own standpoint. In any case, the reformism of Loren himself is remarkably free of anything to do with religion.
So what did the foundation do? Its initial work was very similar to the work of the labor statistics movement, which was developing elsewhere. It collected facts about income, household budgets, and work hours, as well as interviewing managers and working families. Wisselgren notes that this was also LePlay’s method which, incidentally, was also taken up by Branford and Geddes. Objectivity was the goal. Being “scientific” was understood in terms of objectivity and objectivity was understood as presenting statistics. But presenting was only part of the problem; facts had to be acquired and there were no established methods. Methods had to be made up on the spot. These problems were common to other, contemporary statistical operations involving data on labor: sampling—getting responses, finding ways to make the responses understandable by consulting managers and owners as well as employees, and delimiting research domains, all of which was done by surveying occupations. The Loren foundation started with bakers, moving on to other types of work, such as seamstresses. Wisselgren tells us a good deal about who the surveyors were and how they worked. They were more like inspectors, travelling and observing people and settings. But they did work on a large scale collecting data and employing assistants. Urban von Felitzen collected data on 24,760 individuals. One assistant, Anna Soderberg, collected “information about just over 2,600 female workers by visiting them in their homes every day with a 22-item questionnaire” (171) over a period of two years. Wisselgren notes that this was work undertaken by the upper-middle class about the lower classes. Inevitably it reflected class-based social attitudes.

Careers, however, presented a problem. Johann Leffler was eager to get a stable academic position, but there was the problem of qualifications, in addition to that of having a position into which to go. The foundation jump-started the process of creating a Chair by sponsoring lectures at the University College of Stockholm. The foundation also funded—over a long period of time and at great cost—the European education of three figures: Knut Wicksell; Gustav Steffen; and Gustav Cassel. The story of with whom they studied and visited is fascinating as an insight into the topography of late nineteenth century social science and reformism.

Eventually the money ran out, but it was spent more or less as Loren intended with happy results, in this sense at least. The work reached the public and stimulated concern for reform. The State did step in and engage with the problems as well as start collecting labor statistics. By the turn of the century, universities were opening up to social science, and the favored beneficiaries of the Loren foundation were given professorial chairs. This was, as Wisselgren puts it, “the academic breakthrough.” And it was a significant one—Wisselgren recounts in detail the twisted paths that had led to these positions in each of the three cases, and the long periods of precarious and dependent living they had endured. Moreover, as he makes clear, their support by the foundation was dependent on a board which did not agree, in its later incarnations, on their merits, and fell out with some of the people it had heavily supported in the past. This was the underside of the coziness of intellectual life in Stockholm; everyone knew everyone else and had opinions to match.

There is an odd coda to this. The three people whose international education the foundation supported had backgrounds in science and mathematics. Academic opportunities for sociology came combined with economics. The focus of the Loren foundation had been the labor part of reform, rather than the myriad of other issues that were present elsewhere and, indeed, in Sweden itself. This led to the odd result that Swedish sociology became bound up with economics, and in particular in this generation, with the new marginalism and what became neo-classical economics. Wicksell in particular was a brilliant mathematicizer. They each had access to the press and were journalistically inclined. Again, this was not entirely uncharacteristic of international sociology: Giddings and Park were both working journalists for much of their working lives. But in the smaller echo chamber of
Sweden—and particularly the Uppsala-Stockholm region—what they said and did made a difference, and provoked controversies. What this failed to lead to was the creation of a distinct discipline of sociology. But it did bring about a social policy orientation that arguably led directly to the Swedish welfare state.

Wisselgren does an outstanding job of explaining who the participants were, the role of literature, the details of the decision-making process and finances of the foundation. He reveals the complexity of a series of events that might pass unnoticed, and recaptures the motives and actions of people who were not important social scientists, but who were the enthusiasts for social science that made possible social science as an institutional fact. Viktor Loren is a curious and anomalous case. Giving ones life in this way for the hope represented by the social sciences is unique. But he was not alone in his belief in the prospects of social science based reform, a belief that somehow took hold in this period in spite of the meager evidence for it. This belief—really a form of faith—was an essential condition for the academic fields that eventually emerged. We are in his debt for this deeply thought through study.