ARTICLE

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

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Abstract (English)

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dedicated much of his professional career to the study of Nazi concentration camps, his work remained largely unpublished.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

Abstract (Deutsch)

In der gegenwärtigen Debatte über das Verhältnis von Soziologie und Nationalsozialismus überwiegen Publikationen, die sich mit der Geschichte der Soziologie *im* Nationalsozialismus beschäftigen. SoziologInnen, die selbst Aspekte *des* Nationalsozialismus erforscht hatten, wurde wenig Aufmerksamkeit zuteil. Basierend auf Archivmaterial werden in diesem Paper die großteils unveröffentlicht gebliebenen Forschungen des Soziologen Elmer Luchterhand (1911–1996) vorgestellt, der als US-Army-Offizier an der Befreiung des Konzentrationslagers Hersbruck teilgenommen hatte. Es wird argumentiert, dass die Gründe dafür weitgehend methodologischer Art waren: Auf der Suche nach dem „richtigen" Weg, Genozide zu erforschen und im Bewusstsein dessen, dass wenig davon zur Verfügung stand, was er „methodologische Klassenzimmer-Rezepte" nannte, konnte Luchterhand seine zweifelnden KollegInnen nicht von der Sinnhaftigkeit seiner Vorhaben überzeugen.

Keywords

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

INTRODUCTION

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

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

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

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

THE CONTEXT: SURVIVORS' REPORTS AND PSYCHOLOGICAL WARFARE

It is easy to identify the first studies on concentration camps by trained or future social scientists. Some of these were even published before 1945: Paul Martin Neurath (2004 [1943]); Bruno Bettelheim (1943); Eugen Kogon (1946); Viktor Frankl (2008 [1946]); Benedikt Kautsky (1948 [1946]); Ernst Federn (2012 [1946]); Hans G. Adler (1960); and the aforementioned Anna Pawełczyńska (1979), to name but a few of the most cited authors. They were concentration camp survivors, and social scientists. Their accounts were meant to be more than "survivor's reports." Instead, they had research objectives, even though this is precisely what was disputed by their

academic colleagues, the general public and even their fellow survivors. Bruno Bettelheim, for example, was asked if he had obtained permission to publish his observations from SS guards and fellow prisoners (Fleck and Müller 2006: 189), and Paul Neurath not only had difficulties in getting his book acknowledged as a PhD thesis at Columbia University, but was also told by a friend that: "… just because one repeatedly uses the word 'society' and speaks of a rule of game one is not a sociologist."[1]

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

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

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[1] In German: „Weil man society 10 mal wiederholt und [von] einem rule of game spricht, ist man noch kein Soziologe. Du verzeihst die Bosheit…" (Letter by Felix Reichmann to Paul Martin Neurath, 12 April 1943. Paul Martin Neurath Papers, Paul F. Lazarsfeld-Archives, Department of Sociology, University of Vienna, see Fleck et al. 2004 in general). In the same letter Reichmann wrote: "Even if 100 'Dachauers' sent you critical material, it would never be reliable sociological data. A patient is not able to describe his condition in the same way a doctor can. Not only because he does not see the connections, this would not be true in your case, but simply because the doctor feels no pain." [In German: „Auch wenn hunderte alte Dachauer Dir kritisches Material brächten, es werden nie reliable sociologische data draus. Der Patient kann seinen Zustand nie so beschreiben wie der Arzt. Nicht bloss weil er die Zusammenhänge nicht sieht, das würde in Deinem Fall nicht zutreffen, sondern einfach weil der Arzt keine Schmerzen hat."]

from 14,000 interviews by the Jewish Agency in Budapest (Goldstein et al. 1991). Like Rosenberg, there were some who had been present at the liberation of the camps as members of the US Army: Herbert A. Bloch, who interviewed survivors in Buchenwald and in the Mauthausen subcamps at Ebensee and Lenzing where he studied the resocialization of 547 women prisoners (Bloch 1947); and Elmer Luchterhand, whose work I will present in this paper in some detail.[2]

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

ELMER LUCHTERHAND: WORKS AND FINDINGS

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

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

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

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[2] For a general overview regarding the sociological analysis of concentration camps see Fleck et al. 2004, Suderland 2014, and Kranebitter 2014: 19–32 and 79–119.

[3] Army of the United States, Separation Qualification Record, undated [1946], provided by Erika Luchterhand.

[4] Department of Commerce – Bureau of the Census, Sixteenth Census of the United States: 1940, Population Schedule, Wisconsin, Milwaukee, April 20, 1940 (ancestry.com).

[5] The Wisconsin State Journal, Vol. 140, No. 68, Madison, June 8, 1932, page 1. According to the Jail Register Dane County (Wisconsin Historical Society Archives, Dane County Series 4, Sheriff, Jail Register, 1872-1931, January – 1933, November, Vol. No. 9), Elmer Luchterhand and Edward Pollock were arrested on June 8, 1932, for the reason "Disorderly and viol. city traf. ord.," and sentenced to a bail of 200 dollars, which they paid. According to the Register, which lacks more detailed information, Luchterhand was then sent "To Mendota for observation", which meant the "Wisconsin State Hospital for the Insane".

[6] "Admissions for the month of June 1932" (Wisconsin Historical Society Archives, Mendota Mental Health Institute, Admissions and Discharge Registers, 1881-1960, Series 2166, Volume 10, p. 156).

1932 by an "Order of Court." His "condition when discharged" is given as "Improv[ement]."[7] The newspapers from that period point to the influence of a broad political protest movement that eventually succeeded in getting him released.[8]

In March 1943, Luchterhand was drafted into the US Army. In his position as Sergeant and Public Relations Writer of the 261st Infantry, 65th Infantry Division, he was among the troops who liberated the Hersbruck concentration camp—a subcamp of Flossenbürg concentration camp—on April 20, 1945.[9] His exact duties are unknown, but it seems reasonable to assume that he was given a kind of special army assignment not dissimilar to Rosenberg's regarding re-education. In a "Summary of Military Occupations", his Separation Qualification Record states:

> Public Relations Writer: Worked with a battalion intelligence section, interrogating German Prisoners of War. Handled civil affairs in a number of towns before arrival of military government. Assisted regimental information and education officer. Edited regimental newspaper and supervised the work of 20 company public relations representatives. Served as chief of public relations section, XX Corps.[10]

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

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

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

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

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

[10] Army of the United States, Separation Qualification Record, undated [1946].

the last possible moment in 1939—started his career in the US with a widely-read paper on 'The Nazi Party: Its Leadership and Composition' (Gerth 1940, see also Gerth 2002: 87–94). Becker, who had worked for the Office of Strategic Services (OSS) in Britain, France, Luxembourg, Germany and Austria from 1944 to 1948,[11] wrote extensively about the relationship between Nazism and German youth movements (see Becker 1946).

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

Social Characteristics of 52 Veterans of Nazi Concentration Camps		
Category	**No. of Cases**	**Percentage of Cases**
Age at induction		
16-20	12	23 %
21-30	21	40 %
31-40	12	23 %
41-50	6	12 %
58	1	2 %
Sex		
Male	35	67 %
Female	17	33 %
Marital status at induction		
Single	35	67 %
Married	17	33 %
Married with children	8	
Occupation at induction		
Business manager	6	
Artist	2	
Dress designer	1	
Farm laborer	2	
Farm operator	1	

[11] In a "Faculty information sheet" dated April 27, 1949, Becker lists the following tasks during and after WW II: "Leader, Morale Operations Unit attached to Secret Intelligence for Germany, Office of Strategic Services. Operations in field, not at desk, as 'black propagandist.' During 1947-48, Chief, Higher Education, Military Government for Hesse, Dept. of the U.S. Army. In charge of Universities of Marburg, Frankfurt, Giessen College of Agriculture, Darmstadt Institute of Technology. Designated professor of sociology, 1949. University of Cologne, to succeed Leopold von Wiese. Requested to return as Chief of Higher Education for entire American Zone in Germany, 1948." (University of Wisconsin-Madison, Division of Archives, Biographical File Howard P. Becker, Faculty information sheet", April 27, 1949)

Housewife	5	
Journalist and writer	1	
Musician	1	
Office worker	5	
Political party functionary	1	
Salesman	5	
Social worker	2	
Student	5	
Teacher (university, college)	3	
Beautician	1	
Butcher workman	1	
Electrician	2	
Factory supply clerk	1	
Mechanic	1	
Plumber	1	
Sewing machine operator	2	
Shoe worker (skilled)	2	
Tool and die maker	1	
Approximate class position at induction		
Upper class	1	2 %
Middle classes		75 %
Upper middle	6	
Middle	18	
Lower middle	15	
Working class (skilled)	12	23 %
Country of longest residence before induction		
Germany	31	60 %
Others		40 %
Poland	9	
Austria	7	
Czechoslovakia	3	
Netherlands	1	
Hungary	1	
Places of longest residence before induction		
Cities over 100,000		67 %
Berlin	11	
Vienna	6	
Breslau	3	
Frankfurt on the Main	3	
Düsseldorf	2	
Krakow	2	

Prague	2	
Amsterdam	1	
Cologne	1	
Dortmund	1	
Essen	1	
Hamburg	1	
Nürnberg	1	
Other cities (2500 - 100,000)	11	21 %
Towns (1000 - 2500)	4	8 %
Villages (under 1000)	2	4 %
Declared religion before induction		
Jewish	41	
Protestant	2	
Catholic	2	
Agnostic	1	
Atheist	6	

Table 1: Social Characteristics of 52 Veterans of Nazi Concentration Camps (from Luchterhand 1952: 9–11). For a discussion of "representativeness," compared to the "prisoner's society" of the Mauthausen concentration camp, see Kranebitter 2014: 80–83.

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

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

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

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

[14] Minutes of the Departmental Appointments Committee, March 1, 1967.

Canada. Before going to New Haven he taught at Sarah Lawrence. This Fall he will be teaching courses in bureaucracy and organization and the individual and society.[15]

It is noteworthy that many members of departmental staff, soon including Luchterhand, took an active part in the social protests of the time, which was indicative of the college's general intellectual climate. Between them, they funded the Martin Luther King Jr. Lectures given by prominent leftists like Howard Zinn.[16] They discussed and supported protests—such as a strike by the United Federation of Teachers in 1968—and encouraged research on the faculty's surrounding neighbourhoods in Brooklyn.[17] They also actively demanded that the "summary expulsion of students involved in the May 18, 1968 sit-in be deplored on the grounds that such action was in violation of the spirit of due process"[18] and that criminal charges against them be dropped.

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

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

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

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

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

[19] Elmer G. Luchterhand: The Start of Field Work and Method, 8 February 1982, Elmer G. Luchterhand Papers, Sub-Group III, Series 9, Box 15.

The aim of his early studies, on which I want to focus in this paper, was to test the hypotheses formulated by Bruno Bettelheim. Bettelheim had a hard time finding someone willing to publish his article (Fleck and Müller 2006). Yet it was widely read immediately after it appeared in print in 1943. Bettelheim's core assertion was that the longer prisoners stayed in concentration camps, the more they would identify with the Gestapo or SS, taking on their values. "Old prisoners" would therefore regress to a childlike state, identifying with the aggressor. Survival for other prisoners would then be a matter of staying sane by choosing the right coping strategies of one's pre-camp personality, by splitting one's personality into one which was present and one that observed what was going on ("de-personalization"). In other words, isolating oneself from the prisoners' society, and especially from "old prisoners" by individual means. "It seems easier to resist the pressure of the Gestapo and the Nazis if one functions as an individual," Bettelheim concluded (1943: 452). Bettelheim's individualistic thesis had a great impact on concentration camp research. Many of the researchers already mentioned, especially Bloch (1947) and Arendt (1950), referred to it, and well-known books like *The Order of Terror* by Wolfgang Sofsky (1993) repeated the same argument.[20] According to this view, the SS had been successful in its aim of atomizing, dissolving, and disaggregating the prisoners' society, making survival arbitrary and only possible on an individual basis. According to Sofsky, ultimately "the state of aggregation of society approximated dissociation" (Sofsky 1998: 1155, my translation).

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

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

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

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[20] Terrence Des Pres (1976, the German version was only published in 2008) would be another example of an individualistic approach close to Bettelheim's, even though Des Pres stressed major differences to Bettelheim, and was also seen as his opponent (Pollak 1988: 166–172). Whereas Bettelheim (1943) in his psychoanalytic approach stressed the importance of individual (pre-war) autonomy against disintegration, De Pres (1976) stressed the prisoners' needs to adapt morally to a new situation by developing a kind of survivor's morale. Even though there is an important and serious contradiction here, it could well be argued that both positions only mark the opposites of individualistic approaches, therefore more strongly resembling rather than contradicting each other – ultimately, whatever the individualistic "reason" for survival in its concrete theorization may be, both highly overestimate the individual freedoms available for social actions in the circumstances of a concentration camp (see Kranebitter 2014: 221).

By reading statements about incidents of theft, for example, to his respondents, nine interviewees admitted to having committed theft in the camps, while five more interviewees admitted to having thought about thieving (Luchterhand 1952: 140).[21] Since most of these incidents occurred when the prisoners had been on transports or had recently been transferred to another camp, Luchterhand concluded that far from increasing in likelihood the longer one was imprisoned, thieving was likely to be abandoned in favour of behaviour like the infamous "organizing." This denoted the acquisition of food or consumables illegal in terms of SS "norms," but did not include stealing from other prisoners, which carried harsh sanctions within the prisoner society. Therefore, Luchterhand cautiously proposed the thesis that "the longer one lived among prisoners the more one became acculturated, taking on the norms of the main mass of prisoners" (ibid.: 152), i.e. a prisoner code. Certain behaviours and the change in behavioural patterns in the camps pointed towards the emergence of a social system among prisoners influencing their behaviour to a large extent. He also concluded this from the observation that patterns of interpersonal relations generally changed positively—i.e. resulting in more intense social ties—with longer imprisonment (ibid.: 124–127). This was also based on his respondents' accounts that suicides mostly happened during early stages of imprisonment (ibid.: 243), on the finding that members of the underground organizations were mostly so-called "old prisoners," prisoners who had been incarcerated for a long period (ibid.: 212–217); and from the observation that prisoners' earlier traumatization "tended to adapt in harmony with the prisoner code" (ibid.: 258) during longer imprisonment.

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

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

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

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[21] Later, Luchterhand also considered these numbers to be very low, given situations conducive to theft. In the author's opinion, this result is unlikely to have been achieved in later interview projects with concentration camp survivors, which is one example that highlights the value of Luchterhand's research in the light of current research on concentration camps.

Mean Imprisonment Time of 47 Respondents Classified by Pattern of Association			
Pattern of Association	**No. of Cases**	**Per cent of Total**	**Mean imprisonment time (months)**
Lone wolf	4	8,5 %	21,8
Stable pairs	10	21,3 %	31,2
Small groups	20	42,6 %	39,0
Large groups	13	27,6 %	43,0
Totals	47	100,0 %	

Table 2: Number and mean imprisonment time of former concentration camp prisoners per "pattern of association" (from Luchterhand 1967: 252). Since involvement in small and large groups also included involvement in "stable pairs," Luchterhand stressed the importance of the latter.

Stressing the existence and importance of a prisoner social system for survival was a genuine sociological perspective. Luchterhand continued to take this perspective in his later work, in his longitudinal study, when he interviewed most of the older sample again in 1970 and 1971, for example. His main finding at this time was that since no correlation could be found (except for gender) between stress (measured by a "mental health" or stress index used by Gerald Gurin) and the length or place of imprisonment, post-war stress symptoms were likely to be post-war-products and not necessarily connected to the camp era. On the contrary, and paradoxically, imprisonment, especially longer stays in the camps, appears to have had stress-releasing and de-traumatizing effects as well as going through similar situations more often. Luchterhand differentiated strongly between different types of situations, such as induction, roll calls, transports, and death marches. It seemed that "the stress manifested by our sample members is chiefly a consequence of stressor situations in post-camp life" (Luchterhand 1972: 5).[22] Again, with a seemingly straightforward thesis he questioned central psychological theories such as "survivor guilt," which manifested differently in the different post-war societies.

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

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

[23] The paper, dated 17 November 1973 and addressed to the National Endowment for the Humanities, Washington D.C., is titled 'Purpose' (Elmer G. Luchterhand Papers, Sub-Group V/VI, Series 14/15/16/17, Box 19, Request for Grant Money: Different Applications, Research Proposals).

It is worth noting that Bruno Bettelheim seems not to have recognized Luchterhand's criticism, or rather refused to answer it. There was only one psychoanalyst who responded to his research, the Austrian Ernst Federn, who corresponded with Luchterhand while he worked on his dissertation and who was a close ally of Bettelheim during his time in the camps. In a letter dated April 1969, Federn contradicted Bettelheim's overemphasis of the "identification with the aggressor," while simultaneously rejecting Luchterhand's sociological approach in general:

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

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

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

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

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

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[24] Letter by Ernst Federn to Elmer G. Luchterhand, 9 April 1969, Elmer G. Luchterhand Papers, Sub-Group I, Series 1, Box 3.

[25] The Ad Hoc Group on "The Life History Approach" was organized by Daniel Bertaux – for the congress program, see the *isa bulletin* No. 16 (spring 1978). Internet: http://www.isa-sociology.org/publ/isa-bulletin/isa-bulletin16.pdf (http://www.isa-sociology.org/publ/isa-bulletin/isa-bulletin16.pdf) (accessed: 25.09.2016).

[26] Letter by Elmer G. Luchterhand to George L. Mosse, 28 August 1978, Elmer G. Luchterhand Papers, Sub-Group IV, Series 11/12/13, Box 18.

[27] Letter by Elmer G. Luchterhand to Vice-President and Provost of Brooklyn College Donald R. Reich, 31 August 1978, Elmer G. Luchterhand Papers, Sub-Group IV, Series 11/12/13, Box 18.

the same colleague who had called for the minute's silence at the conference in Uppsala for his opinion on his research, the colleague advised him not to submit his proposal to two institutions due to methodological concerns:

> "They are both quite likely to turn them down, on the grounds that your design is not sufficiently systematic. I do not sympathize with such a view, but I don't know any way of getting around it."[28]

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

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

> Thank you very much for your letter of August 28. I have read your paper with interest. It seems to be a very fruitful line of approach, though it is difficult to see where it would lead you or what will hold it together eventually. I take it that it will be a study of this local concentration camp and of this minister who worked in it. I do not know whether you are going to stress message in the final manuscript or not. Interesting though your project is, I do not think it is suitable for the *Sage* monographs which Walter Laqueur and I are editing. We are looking for greater synthesis than such a local study would provide, though a local study could be fraught with more general implications. Moreover, the series does not include books on method. From what I could gather from your outline, therefore, your work would not be suitable for us. We also have too many manuscripts on Germany at the moment, and our priorities are historical manuscripts dealing with other nations. I therefore do not think that our *Sage* series is suitable for the publication of this work. However, I wish you and your collaborator much luck. Local studies are very badly needed, and I hope you will persevere with your project.[29]

Unfortunately, it is not clear which parts Luchterhand sent to Mosse, either from Luchterhand's papers or from the George Mosse papers at the Leo Baeck Institute in New York.[30] However, it seems reasonable to assume that there is an obvious misunderstanding in Mosse's reaction. Luchterhand had not stated that he intended to write a "local study," but rather a study of the nucleus of National Socialism in a small German town from a "multiperspective." Research on the system of subcamps was practically non-existent before the 1980s (see, e.g., Orth 2007), and there were definitely no projects interviewing those who "ran the camps." Comments such as those contained in Mosse's letter may have deliberately misunderstood the author's intentions. Everett C. Hughes seemed to have experienced similar misunderstandings (see Fleck 2015a), so it is probably fair to assume that publishers, of whatever profession, were not particularly eager to learn about sociological studies conceptualized in an unorthodox way.

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

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

[30] Research in the George Mosse papers was undertaken at the Leo Baeck Institute in New York in April 2017 with the permission of the donors of the Mosse papers. I want to thank the staff at the Leo Baeck Institute for their help.

It seems, however, that Luchterhand expected methodological criticism like Mosse's. Various manuscripts titled "On Methods" point to the fact that he had been thinking about methodology for a while and had grappled with finding the "right" way to both research and describe genocide. Starting with lengthy standardized interview guides in the 1950s, he ended with interviews based in a phenomenological approach (Luchterhand 1979: 17). While always using highly developed techniques, he made flexible adjustments to them according to the interview situation—not least in his interviews with SS men from the early 1970s—and may thus not have been regarded as being "consistent" in his research design. After teaching research on genocide for several years, he concluded: "These classroom efforts seem to satisfy my students, but I personally regarded them as unsatisfactory and not useful for the development of a theory of genocide" and wrote about the "dilemma in the presentation of material itself – an editorial dilemma but one that is made more difficult by the inherently repulsive nature of all discussions of genocide."[31] While in general he was convinced that the "pandemic questions" which the Nazi genocide threw up could only be answered by "highly experienced journalists, as well as social scientists who are willing to venture beyond text book research methodology,"[32] he was always sceptical regarding application of the "usual methods" and not only anticipated, but also provoked, criticism.

> Methodologically this may seem questionable to those whose research stays strictly within the conventions of method as I suppose some of us have taught it in our academic lives. But our classroom methodological prescriptions do not fit easily the problems of studying the SS and their doings.[33]

To summarise, one could formulate the following argument. Luchterhand permanently grappled with forms of researching genocide and presenting research findings, since to him, "[i]n terms of method, then, there is no royal road"[34]. This methodological grappling, however, never satisfied either him or his supervisors and colleagues in different fields. Beginning his research on holocaust survivors using standardized (yet not very rigorously applied) questionnaires, he analysed them using quantitative methods (Luchterhand 1952, 1967 and 1980). For his *Doggerwerk* study, however, his interviews were far less standardized and designed for hermeneutical analyses (Luchterhand 1979; Luchterhand and Wieland 1978, 1981). It is interesting to note a certain anachronism in these methodological decisions. Using quantitative methods made sense given their promotion by the US Army and their growing domination as part of a general trend towards positivism (Steinmetz 2005a: 16–17; Steinmetz 2005b: 280–281). But it was an odd decision given that his supervisors—Howard P. Becker and Hans Gerth—were among the few sociologists to openly resist the positivist turn (Steinmetz 2005b: 308).[35] Apart from this, the limits of quantification in Luchterhand's research were obvious. There was no way of achieving a representative sample of concentration camp survivors in his research design (Kranebitter 2014: 83–84), and quantification frequently hid contradictory evidence, a point stressed quite harshly by Becker in an earlier review of the thesis:

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

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[31] Elmer G. Luchterhand: Toward a Theory of Genocide. A Loosely Joined Framework of Concepts and Approaches, unpublished and undated manuscript, Elmer G. Luchterhand Papers, Sub-Group I, Series 2, Box 4.

[32] Untitled manuscript, 28 February 1984, Elmer G. Luchterhand Papers, Sub-Group I, Series 2, Box 4.

[33] Untitled manuscript, undated, Elmer G. Luchterhand Papers, Sub-Group I, Series 2, Box 4.

[34] Manuscript titled "Method", undated, Elmer G. Luchterhand Papers, Sub-Group I, Series 2, Box 4.

[35] Here, Steinmetz lists only seven "sociological critics of methodological positivism" (Steinmetz 2005b, 308) for the American 1950s and 1960s.

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not always come in such neat packages. [...] Summarizing, the reader regards the dissertation as falling distinctly short of standards of scientific detachment and accuracy. This judgement, be it noted, is not based on conviction that scientific accuracy and elaborate quantification are synonymous. In addition, the dissertation is poorly organized; it does not give an impression of coherence and unity, and the reader is of the impression that some parts coming late in the sequence were written earlier. He may be wrong in this inference, but the impression still remains. It is therefore recommended that the writer of the dissertation seriously consider the elimination of some of the more glaring defects before turning in a final draft.[36]

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

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

[37] In a *Curriculum Vitae* from the early 1970s, Luchterhand wrote: "IN PREPARATION: A book on the longitudinal study of 52 survivors of the Nazi camps." (CV, untitled, Erika Luchterhand collection).

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

[39] In his autobiography, Mosse is critical of the quantification trend brought to history by social historians: "New kinds of history were pioneered, such as the use of statistics in historical research, and an attempt was made to build bridges between history and other social sciences. The old order was definitely dead. Yet I was once again the insider as outsider: I took part in departmental business and made close friends with many of the new arrivals, but once Bill Aydelotte had pioneered the statistical approach to history, I stood aside from the scholarly discussions. I had no training in mathematics and was not enthusiastic about the nature of this research, especially as its practitioners seemed to find it difficult to arrive at coherent, publishable, analyses." (Mosse 2000: 135).

certainly connected to the pre- and post-1968 methodological debates in sociology as well as historiography, both of which tended to view his research and material as arbitrary and preliminary; in short 'unscientific', leaving Luchterhand 'caught between two stools'.

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

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

CONCLUSION

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

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

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[40] Elmer G. Luchterhand: Planning the Study, 18 December 1988, Elmer G. Luchterhand Papers, Sub-Group I, Series 2, Box 4.

[41] For a general discussion of anti-sociological stances in theorizing about concentration camps, see Suderland 2014.

may well have played a role in the reception of Luchterhand's work. Because of his history as a labour organizer in the 1930s, the beginning of the Cold War and McCarthyism forced Luchterhand and his family to move to Canada from 1953 to 1958.[42]

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

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

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

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

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[42] Interview with Erika Luchterhand, New York, April 18, 2017.

[43] Interestingly, the methodological question of how to produce knowledge of any genocidal event within the social sciences, i.e. beyond survivors' memoirs and legal reconditioning and their implicit limitations for a broader elicitation, seems to be one that has not been elaborated on in the social sciences. Certainly, the "National Socialism and Sociology" debate, which for the most part adheres unconsciously to the thesis of the singularity of National Socialism and the Holocaust, does not focus at all on this side of the problem. Since genocide on a macro level and camps as social systems have not disappeared since, and since the question of how to deal methodologically with sites of mass violence is not a banal question at all, much could be learned from those grappling with these problems in the first instance.

[44] From the many examples that could be cited here I only want to point to the Spanish Buchenwald survivor and novelist Jorge Semprún, whose book "Literature or Life" is a general plea for a literary description of events in contrast to a scientific (especially sociological) one – another recurring theme in the history of sociology (see Lepenies 2006). Franz Kafka's descriptions of bureaucracy, for example, to Semprún seemed timeless, because they were written "in his own register, one of literature, not of sociological analysis" (Semprún 1994: 154; my translation – see also Kranebitter 2016).

Contradicting this view, some sociologists have proposed methodological positions that explicitly refuse the "temptation of methodological exceptionalism" (Dobry 2009), calling instead for the application of 'normal' methodological and conceptual instruments (ibid: 74).

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

References

Adler, H. G. (1960) *Theresienstadt 1941–1945. Das Antlitz einer Zwangsgemeinschaft*, Tübingen: J. C. B. Mohr.

Allen, William S. (1965) *The Nazi seizure of power: the experience of a single German town 1922–1945,* Chicago: Quadrangle Books.

Arendt, Hannah (1948) The Concentration Camps, *Partisan Review* XV (7): 743–763.

Arendt, Hannah (1950) Social Science Techniques and the Study of Concentration Camps, *Jewish Social Studies,* 12 (1): 49–64.

Arendt, Hannah (1989): Die vollendete Sinnlosigkeit, in: Hannah Arendt: *Nach Auschwitz. Essays & Kommentare 1*, Berlin: Edition Tiamat, 7–30.

Baehr, Peter (2010) *Hannah Arendt, Totalitarianism, and the Social Sciences*, Stanford, California: Stanford University Press.

Bannister, Robert C. (1992) Principle, Politics, Profession: American Sociologists and Fascism, 1930–1950, in: Stephen P. Turner, and Dirk Käsler (Eds.) *Sociology Responds to Fascism*, London; New York: Routledge, 172–213.

Bauman, Zygmunt (1988) Sociology after the Holocaust, *The British Journal of Sociology* 39 (4): 469–497.

Becker, Howard Paul (1946) *German youth: bond or free,* London: Kegan Paul.

Berger, Ronald J. (1995) Agency, Structure, and Jewish Survival of the Holocaust: A Life History Study, *The Sociological Quarterly* 36 (1): 15–36.

Berger, Ronald J. (2012) *The Holocaust, Religion, and the Politics of Collective Memory. Beyond Sociology*, New Brunswick: Transaction Publishers.

Bettelheim, Bruno (1943) Individual and mass behavior in extreme situations, *Journal of Abnormal and Social Psychology* 38 (4): 417–452.

Bloch, Herbert A. (1947) The Personality of Inmates of Concentration Camps, *American Journal of Sociology* 52 (4): 335–341.

Christ, Michaela, and Maja Suderland (Eds.) (2014) *Soziologie und Nationalsozialismus. Positionen, Debatten, Perspektiven*, Berlin: Suhrkamp.

Christ, Michaela (2011) *Die Dynamik des Tötens: Die Ermordung der Juden von Berditschew. Ukraine 1941–1944,* Frankfurt am Main: Fischer.

Collins, Randall (2015) Koalitionen im Geiste, in: Christian Dayé, and Stephan Moebius (Eds.) *Soziologiegeschichte. Wege und Ziele,* Berlin: Suhrkamp, 235–283.

Des Pres, Terrance (1976) *The Survivor: Anatomy of Life in the Death Camps*, New York: Oxford University Press.

Des Pres, Terrance (2008) *Der Überlebende – Anatomie der Todeslager*, Stuttgart: Klett-Cotta.

Dobry, Michel (2009) Critical Processes and Political Fluidity: A Theoretical Appraisal, *International Political Anthropology* 2 (1): 74–90.

Federn, Ernst (2012) Versuch einer Psychologie des Terrors, in: Bernhard Kuschey (Ed.) *Ernst Federn. Sozialismus, KZ. Psychoanalyse und Sozialarbeit*, Vienna: Löcker, 33– 76.

Fleck, Christian (2015a) Ein Amerikaner in Frankfurt. Everett C. Hughes' nicht veröffentlichtes Buch über die Deutschen nach dem Ende des Nazi-Regimes, *Österreichische Zeitschrift für Soziologie* 40 (2): 167–188.

Fleck, Christian (2015b) *Etablierung in der Fremde. Vertriebene Wissenschaftler in den USA nach 1933*, Frankfurt am Main; New York: Campus.

Fleck, Christian, and Albert Müller (2006) Bruno Bettelheim (1903–1990) und die Konzentrationslager, in: Amalia Barboza, and Christoph Henning (Eds.) *Deutsch-jüdische Wissenschaftsschicksale. Studien über Identitätskonstruktionen in den Sozialwissenschaften*, Bielefeld: transcript, 180–231.

Fleck, Christian, Albert Müller, and Nico Stehr (2004): Nachwort, in: Paul Neurath (2004) *Die Gesellschaft des Terrors. Innenansichten der Konzentrationslager Dachau und Buchenwald*, Frankfurt am Main: Suhrkamp, 409–454.

Foreman, Paul B. (1959) Buchenwald and Modern Prisoner-of-War Detention Policy, *Social Forces* 37 (4): 289–298.

Frankl, Viktor (2008) *... trotzdem Ja zum Leben sagen. Ein Psychologe erlebt das Konzentrationslager,* München: dtv.

Gerson, Judith M., and Diane L. Wolf (Eds.) (2007) *Sociology Confronts the Holocaust. Memories and Identities in Jewish Diasporas*, Durham; London: Duke University Press.

Gerth, Hans (1940) The Nazi Party: Its Leadership and Composition, *American Journal of Sociology* 45 (4): 517–541

Gerth, Nobuko (2002) *'Between two Worlds'. Hans Gerth. Eine Biografie 1908–1978*, Opladen: Leske + Budrich.

Goldstein, Jacob, Irving F. Lukoff, and Herbert A. Strauss (1991) *Individuelles und kollektives Verhalten in Nazi-Konzentrationslagern. Soziologische und psychologische Studien zu Berichten ungarisch-jüdischer Überlebender*, Frankfurt am Main; New York: Campus.

Hackett, David A. (Ed.) (1995) *The Buchenwald Report,* Boulder; Oxford: Westview Press.

Halpert, Burton P. (2007) Early American Sociology and the Holocaust: The Failure of a Discipline, *Humanity & Society* 31 (1): 6–23.

Jagoda, Zenon, Stanisław Kłodziński, and Jan Masłowski (1994) Das Überleben im Lager aus der Sicht ehemaliger Häftlinge von Auschwitz-Birkenau, in: Hamburger Institut für Sozialforschung (Eds.) *Die Auschwitz-Hefte. Texte der polnischen Zeitschrift ‚Przegląd Lekarski' über historische, psychische und medizinische Aspekte des Lebens und Sterbens in Auschwitz*, Hamburg: Rogner & Bernhard, 13–51.

Kautsky, Benedikt (1948) *Teufel und Verdammte. Erfahrungen und Erkenntnisse aus sieben Jahren in deutschen Konzentrationslagern*, Vienna: Verlag der Wiener Volksbuchhandlung.

Klingemann, Carsten (2009) *Soziologie und Politik. Sozialwissenschaftliches Expertenwissen im Dritten Reich und in der frühen westdeutschen Nachkriegszeit*, Wiesbaden: Verlag für Sozialwissenschaften.

Kranebitter, Andreas (2014) *Zahlen als Zeugen, Soziologische Analysen zur Häftlingsgesellschaft des KZ Mauthausen* (=Mauthausen-Studien, vol. 9), Vienna: new academic press.

Kranebitter, Andreas (2016) Die Vermessung der Konzentrationslager. Soziologiegeschichtliche Betrachtungen zum sogenannten Buchenwaldreport, in: Regina Fritz, Éva Kovács, and Béla Rásky (Eds.): *Als der Holocaust noch keinen Namen hatte. Zur frühen Aufarbeitung des NS-Massenmordes an den Juden | Before the Holocaust Had Its Name. Early Confrontations of the Nazi Mass Murder of the Jews.* Vienna: new academic press, 63–86.

Kranebitter, Andreas, and Kenneth Horvath (2015) National Socialism and the Crisis of Sociology. Editorial, *Österreichische Zeitschrift für Soziologie* 40 (2), 121–128.

Kühl, Stefan (2014) *Ganz normale Organisationen: Zur Soziologie des Holocaust*, Berlin: Suhrkamp.

Lenz, Hans-Friedrich (1982) *„Sagen Sie, Herr Pfarrer, wie kommen Sie zur SS?" Bericht eines Pfarrers der bekennenden Kirche über seine Erlebnisse im Kirchenkampf und als SS-Oberscharführer im Konzentrationslager Hersbruck*, Giessen; Basel: Brunnen Verlag.

Lerner, Daniel (1971) *Psychological Warfare Against Nazi Germany. The Sykewar Campaign, D-Day to VE-Day,* Cambridge; London: MIT Press.

Luchterhand, Elmer (1949) *The Nazi Concentration Camps: A Study in Survival and Rebellion.* Unpublished MA thesis, University of Wisconsin.

Luchterhand, Elmer (1952) *Prisoner Behavior and Social System in Nazi Concentration Camps.* Unpublished PhD thesis, University of Wisconsin.

Luchterhand, E. G. (1964) Survival in the Concentration Camp: An Individual or a Group Phenomenon?, in: Bernard Rosenberg, Israel Gerver, and W.F. Howton (Eds.) *Mass Society in Crisis: Social Problems and Social Pathology*, New York: The Macmillan Company, 223–224.

Luchterhand, Elmer (1967) Prisoner Behavior and Social System in the Nazi Concentration Camps, *International Journal of Social Psychiatry* 13 (4), 245–264.

Luchterhand, E. G. (1972) *The Resocialization of Nazi Camp Survivors*. Unpublished manuscript, Brooklyn College of the City University of New York.

Luchterhand, Elmer (1979) *Knowing and Not Knowing: Studying Involvement in Nazi Genocide.* Unpublished paper prepared for the International European Oral History Conference, University of Essex, Colchester, England, March 23-25, 1979.

Luchterhand, Elmer (1980): Social Behavior of Concentration Camp Prisoners: Continuities and Discontinuities with Pre- and Postcamp Life, in: Joel E. Dimsdale (Ed.) *Survivors, Victims, and Perpetrators. Essays in the Nazi Holocaust,* Washington: Hemisphere Publishing Corporation, 259–283.

Luchterhand, Elmer G., and Norbert Wieland (1978) *The Focused Life History in Studying Involvement in a Genocidal Situation in Nazi Germany.* Unpublished paper prepared for the Ad Hoc Group No. 20 on "The Life History Approach", 9th World Congress of Sociology, Uppsala, Sweden, August 14–19, 1978.

Luchterhand, Elmer G., and Norbert Wieland (1981) The Focused Life History in Studying Involvement in a Genocidal Situation in Nazi Germany, in: Daniel Bertaux (Ed.): *Biography and Society: The Life History Approach in the Social Sciences*, London: SAGE Publications, 267–285.

Mosse, George L. (2000) *Confronting History. A Memoir*, Madison: The University of Wisconsin Press.

Neurath, Paul Martin (2004 [1943]) *Die Gesellschaft des Terrors. Innenansichten der Konzentrationslager Dachau und Buchenwald*, Frankfurt am Main: Suhrkamp.

Orth, Karin (2007) Die Historiografie der Konzentrationslager und die neuere KZ-Forschung, *Archiv für Sozialgeschichte* 47: 579–598.

Pawełczyńska, Anna (1979) *Values and Violence in Auschwitz. A Sociological Analysis*, Berkeley; Los Angeles; London: University of California Press.

Pollak, Michael (1988) *Die Grenzen des Sagbaren. Lebensgeschichten von KZ-Überlebenden als Augenzeugenberichte und als Identitätsarbeit*, Frankfurt am Main; New York: Campus.

Rammstedt, Otthein (1986) *Deutsche Soziologie 1933–1945. Die Normalität einer Anpassung*, Frankfurt am Main: Suhrkamp.

Reinprecht, Christoph (2014) Die österreichische Soziologie und der Nationalsozialismus. Aufbruch, Verdrängung und verletzte Identität, in: Michaela Christ, and Maja Suderland (Eds.) *Soziologie und Nationalsozialismus. Positionen, Debatten, Perspektiven*, Berlin: Suhrkamp, 237–262.

Semprún, Jorge (1994) *Schreiben oder Leben*, Frankfurt am Main: Suhrkamp.

Sewell, William H. (2005) The Political Unconscious of Social and Cultural History, or, Confessions of a Former Quantitative Historian, in: George Steinmetz (Ed.) *The Politics of Method in the Human Sciences. Positivism and its Epistemological Others,* Durham; London: Duke University Press, 173–206.

Shils, Edward A., and Morris Janowitz (1948) Cohesion and Disintegration in the Wehrmacht in World War II, *The Public Opinion Quarterly* 12 (2): 280–315.

Sofsky, Wolfgang (1993) *Die Ordnung des Terrors. Das Konzentrationslager*, Frankfurt am Main: Fischer.

Sofsky, Wolfgang (1998) An der Grenze des Sozialen. Perspektiven der KZ-Forschung, in: Ulrich Herbert, Karin Orth, and Christoph Dieckmann (Eds.) *Die nationalsozialistischen Konzentrationslager. Entwicklung und Struktur*, Vol 2, Göttingen: Wallstein, 1141–1169.

Steinmetz, George (2005a) Positivism and Its Others in the Social Sciences. Introduction, in: George Steinmetz (Ed.) *The Politics of Method in the Human Sciences. Positivism and its Epistemological Others,* Durham; London: Duke University Press, 1–56.

Steinmetz, George (2005b) Scientific Authority and the Transition to Post-Fordism: The Plausibility of Positivism in U.S. Sociology since 1945, in: George Steinmetz (Ed.) *The Politics of Method in the Human Sciences. Positivism and its Epistemological Others,* Durham; London: Duke University Press, 275–323.

Suderland, Maja (2009) *Ein Extremfall des Sozialen. Die Häftlingsgesellschaft in den nationalsozialistischen Konzentrationslagern*, Frankfurt am Main: Campus.

Suderland, Maja (2014) "Das Konzentrationslager als giftigste Beule des Terrors". Soziologische Perspektiven auf die nationalsozialistischen Zwangslager, in: Michaela Christ, and Maja Suderland (Eds.) *Soziologie und Nationalsozialismus. Positionen, Debatten, Perspektiven*, Berlin: Suhrkamp, 365–405.

Turner, Stephen P. (1992) Sociology and Fascism in the Interwar Period: The Myth and its Frame, in: Stephen P. Turner, and Dirk Käsler (Eds.): *Sociology Responds to Fascism,* London; New York: Routledge, 1–13.

Turner, Stephen P., and Dirk Käsler (Eds.) (1992) *Sociology Responds to Fascism*, London; New York: Routledge.