Editorial

InterDisciplines – Journal of History and Sociology

Another journal for History and Sociology? Are there not plenty of established periodicals to provide a forum for discussions between historians and social scientists? Is the dialogue between the two disciplines really so vibrant that the scientific community needs another journal?

Looking at the current state of history and the social sciences, the proposition of interdisciplinarity has lost the revolutionary appeal of its heydays in the 1960s and 1970s. Today, historians take notice and make use of theories from the social sciences, and even the strongest advocates of quantitative social research basically accept that social facts are embedded in historical contexts. But while a reciprocal acknowledgement via footnotes is firmly established, actual collaboration in research projects, conferences and seminars is still rare. The respective attempts reveal that the interdisciplinary dialogue is fraught with difficulties and still challenging. Both disciplines have their own sets of questions, theories and methods, routines and loyalties, and this in turn makes a real engagement costly and potentially frustrating.

InterDisciplines recognizes the risks of interdisciplinarity but is convinced that a mutually irritating dialogue will lead to advanced results. *InterDisciplines* understands that institutions are required to make this happen, and that is why it supports research at the interface of history and the social sciences, featuring joint issues, articles and discussions.

As an online publication, it offers the speed that primarily project-based interdisciplinary work requires. Moreover, it provides opportunities to explore new forms of presenting findings as well as documents and data. Publishing films and audio-sources, images and tables that would have to be left out of a printed journal, *InterDisciplines* remains sensitive to the important interdisciplinary issue of how history and the social sciences

generate and organize knowledge. The online publication and analysis of images, statistics, audio- and video sources as well as intermediate data might also provide the opportunity to reflect upon the working process of the publication and the performativity of our sources and work material.

The international exchange of doctoral students and scientists is common practise today. An online journal offers the best opportunity to publish research results of multinational cooperations. In general, online publications have a wide outreach. This, together with its open-access policy, makes *InterDisciplines* available for researchers interested in and willing to contribute to interdisciplinary exchange.

Many features of the Bielefeld Graduate School in History and Sociology have been incorporated into the concept of the journal, like interdisciplinary cooperation, international exchange and outlook, a critical, vivid and creative research atmosphere, soft disciplinary boundaries and flattened hierarchies. In this sense, *InterDisciplines* is a showcase for the graduate school. At the same time and like the school itself, *InterDisciplines* wants to attract new talents and give these researchers the opportunity to test their ideas and findings in the environment of a peer-reviewed journal.

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In short

InterDisciplines is dedicated to work at the interface between history and the social sciences and to research discussing the relation of these disciplines. The journal's aim is to identify differences as well as the relation and interconnections between the disciplines, with a focus on areas where they can complement each other with respect to specific research problems. The journal is also open for related neighboring disciplines. First and foremost, the journal addresses historians and sociologists but also aims to appeal to a wider audience of interested scientists and students who are committed to interdisciplinary debate and exchange.

InterDisciplines publishes historical case studies that discuss theoretical and methodological issues from the humanities, the social and cultural sciences or articles that reflect on questions concerning the relation between the disciplines. It invites survey articles from the whole range of historical and sociological research as well as programmatic texts on basic questions of the cooperation between history and sociology. It presents empirical studies at the interface of history and sociology and research projects of current relevance.

InterDisciplines offers guest editors the possibility to publish themed issues covering a specific topic in more depth. All those who are interested are invited to submit proposals. The editing of the articles will be done in Bielefeld.

Generally, *InterDisciplines* will appear twice a year, with at least one themed issue per year.

InterDisciplines addresses an international audience and will be published in English.

InterDisciplines will be available on the internet free of charge. The e-journal offers readers easy access and is an ideal place for up-to-date discussions.

InterDisciplines is edited by the directors of the Bielefeld Graduate School in History and Sociology, Jörg Bergmann, Alfons Bora and Thomas Welskopp, as well as Peter Jelavich, Professor in the Department of History, Johns Hopkins University (Baltimore) and Kathleen Thelen, Professor of Political Science at the Massachusetts Institute of Technology (Cambridge, Mass.) All submissions will be blind refereed by two peers.

Introduction

The frontispiece shows the University of Bielefeld nearly forty years ago – a construction site. Since then, »Bielefeld« has become a trademark in sociology and history. Especially the so-called Bielefeld School is well-known for theory-driven, interdisciplinary historical research. In 2009, Bielefeld University celebrated its fourtieth anniversary with several festive events like an exhibition, conferences and ceremonial acts. At that moment of taking stock, a book was published under the title »Was war Bielefeld ?« (What was Bielefeld?), followed by a recently published volume with meanwhile classical texts in social history resp. historical sociology (Hitzer & Welskopp 2010).

The anniversary as well as the foundation of the Bielefeld Graduate School in History and Sociology (BGHS), supported by the Excellence Initiative of the German government, had been one of the reasons why the organizers of the first »Annual Seminar« of the graduate school chose to look at the state of the dialogue between the disciplines, asking if there was an »End of Messages« between history and sociology.

On the one hand, the »Annual Seminar« provided the opportunity to discuss current research in fields of research like »Semantics«, »Communication/Media«, »Knowledge«, »Social Equality«, »World Society/ Transnational History« and »Ethnicity« in a truly interdisciplinary fashion. On the other hand, the »Annual Seminar« proposed a temporal perspective, inviting to review past developments of interdisciplinary research and to assess its future prospects. The review firstly revealed how entangled research into history and society had been in the work of canonical authors like Georg Simmel, Max Weber, Émile Durkheim and Norbert Elias. Speakers also reflected on their own work at the interface between sociology and history and offered their estimations on the current state of the interdisciplinary field, suggesting that it resembled far more a busy construction site as depicted on our journal's cover than a

cable from a telephone receiver apparently leading nowhere, which had been chosen as the poster for the »Annual Seminar«. The future prospect brings us back to the frontispiece. In the last weeks the first delves of spade had been done and the first construction works for the new building for the Department for History and Soziology, unified under one umbrella, begann.

The first issue of *InterDisciplines* has taken up the questions and discussions of the »Annual Seminar«. It also takes a historical perspective on the relationship between history and sociology (or the social sciences). The first two contributions start with a short look back before evaluating the current situation and pointing to future prospects. Thomas Welskopp states that recently the two disciplines have shown a renewed interest in each other. The article argues that sociology may profit from history's self-reflective turn during its clash with postmodernism. History's profession to an enlightened constructivism and its insistence both on the temporality of all social practices and on the historicity of all concepts could spark a new consciousness within sociology in regard to its theoretical and methodological foundations. History, in turn, can learn from sociology in terms of the multiple patterns of change, which are discussed there. Although the range of «sociologies« which will be able and willing to cooperate with history may be narrow, there are some approaches that do share important assumptions with history about the inner workings of organizations, the nature of human actors, and development over time. Their interests seem to converge in the general question of how to appropriately conceptualize and explain social dynamics and change.

Jürgen Kocka has the impression that, in contrast to preceding decades, the distance between history and the social sciences has widened in the last quarter of the 20th century. In recent times, however, new opportunities and approaches at cooperation between economic history and economics, between history and the social sciences have emerged. The author discusses these changes and shows in what ways history and the social sciences can benefit from one another.

Klaus Nathaus and Hendrik Vollmer ask for future prospects of a collaboration between historians and sociologists, a collaboration which they perform in practice in their joint article. Employing Pierre Bourdieu's concept of field, the paper distinguishes ›oppositional‹ and ›autonomous‹ interdisciplinarity as two modes of research between historiography and sociology. Whereas oppositional interdisciplinarity is described as a movement of challengers introducing new theories, methods and issues to their respective disciplines and thus perhaps ultimately transforming it, autonomous interdisciplinarity means a sustained positioning at the border between disciplines, requiring intensive committment with a neigboring discipline. The article sketches the two scenarios with reference to past examples and assesses their conditions and future prospects. This exercise in mapping the field is meant as an invitation to a debate on what kind of interdisciplinarity historians and sociologists want to establish.

Hartmann Tyrell's and Stephen Mennell's paper base mainly on their lecture and the commentary given at the «Annual Seminar«. As above mentioned: they look back to the past: Hartmann Tyrell's contribution deals with the history of the sciences of history and sociology. It shows how Ranke formulated one of his essential world-historical insights as definitely closing off from contemporary discourses of the social and the »social movement«. Almost one hundred years later Max Weber integrated this insight of Ranke in his early days into his sociology. Moreover, Tyrell highlights the importance of Dilthey's »studies of society and history« for a joining of history and sociology. Essential for this was his understanding of »social differentiation«. Before and after 1900, that line of history rejected every programme and idea of social or cultural history. Also for this reason, since the 1890s sociology has developed by a clear distance to history. This is illustrated by the example of the sociologists Simmel and Durkheim. The final remark glances briefly at Max Weber's sociology, which rightly so may be called historic sociology.

In the style of Richard Sennett (2002) or Geoff Eley (2006), Stephen Mennel reflects on his own intellectual development as a historical sociologist and sociological theorist and stresses both the need for theory

in the writing of history and the necessity of avoiding the wrong – unhelpful – kind of theory. As a former student of Talcott Parsons, Mennell recognized the danger posed by »the fallacy of misplaced abstractness« and the trap presented by the prestige of over-abstract concepts. According to Mennell, many sociological theorists after Parsons have fallen into this trap, as have, under their influence, more empirical sociological and historical researchers. Mennell recommends the processual thinking of Norbert Elias, another major figure in the history of »Bielefeld«, as more fruitful alternative.

The last part of *InterDisciplines* contains miscellanea. This first issue presents a report on a conference about discourse analysis in the history of science in Zurich in May 2010. A review on the above mentioned anthology »Was war Bielefeld« goes back in history once more, while the short introduction of the Bielefeld Graduate School in History and Sociology could be read as »What is Bielefeld today«.

References

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Irritating flirtations

Reflections on the relationship between history and sociology since the 1970s

Thomas Welskopp

History and sociology today – still an odd couple?

There are signs that history and sociology have become interested in each other again and that both disciplines display a new openness to study one another or even to cooperate. The *Bielefeld Graduate School in History and Sociology* may serve as a prime example. It has been designed as an institution where interdisciplinarity is truly practiced, and where mutual inspiration is as welcome as a sharpened sense of what the disciplinarity of either discipline is all about. The new journal *InterDisciplines* is supposed to provide an easily accessible forum for such an endeavor. One of its driving questions is on what common ground history and sociology can meet for a deepened mutual understanding and a prospective intensified cooperation. This essay will both try to explore such a potential common ground in a very preliminary way and to draw some conclusions from earlier flirtations between the disciplines. I will argue that the first close encounter during the 1970s had not been a one-night-stand but rather a short-lived Platonic relationship. Thus a renewed mutual interest cannot easily build upon established traditions but has to start over identifying what might be attractive in sociology for historians and what might be appealing in history for sociologists (Welskopp 2005a).

Despite the legacy of pioneers like Max Weber and a thriving Anglo-Saxon Historical Sociology – to which there is virtually no counterpart in Germany – the interest of sociologists in ›history‹ as a discipline is far from being self-evident. In the Durkheim and Comte tradition, important and sometimes dominant schools of sociology have professionalized

themselves by explicitly sharpening their own disciplinary profiles in a conscious departure from history (Nelson & Winter 2002). Large scale macro-sociology, with its focus on quantitative variable analysis, has devalued ›history‹ to the opposite of ›systematic‹, to the mere residue of phenomena you cannot explain theoretically but have to describe in a pre-analytical narrative. Only in this sense, John Goldthorpe maintained in 1997 »for any kind of macrosociology, […] ›history‹ will always remain as a necessary residual category« (Goldthorpe 1997: 22, note 18).

›History‹ in such a view is a last resort in case of insufficient modeling or the pastime of some literary romantic too stupid to quantify. A renewed interest in history among quantifiers and especially the more hardcore proponents of rational action theory – if it is voiced at all – then boils down to include more backward data into a retrospective longitudinal analysis. Yet this does not go deeper than to reducing history to a collection of ›things past‹, a vast reservoir of data to be fed into variable-testing models which are not famous for their historical sensitivity. It goes without saying that with such a notion of ›history‹ the social sciences can continue quite well into the future without historians.

During the 1980s and much of the 1990s, many historians, in turn, among them a considerable number of social historians, had forsaken sociology in favor of seemingly more fashionable partners: discourse analysis and literary criticism. This contributed to increased methodological consciousness but privileged language and semantics to a degree where the grip on ›the social‹ threatened to get lost. The questions of generalization and synthesis were pushed into the background without resolve. It is a little irritating to see that some younger discourse historians now look to rational choice models in order to re-identify historical actors and draw the boundaries around discourses. As the essay will argue, it is the methodologically more open social historians of a younger generation and those historians who have gained a ›praxeological‹ approach from the works of Anthony Giddens, Pierre Bourdieu and the likes who display a renewed interest in sociology and whose practice may in turn be of interest to sociologists.

This implies that some strands of sociology lend themselves to a more productive inspection than others. They comprise, for example, the historically oriented ›mechanisms‹ approach of historical sociologists like Doug McAdam, Sidney Tarrow, and the late Charles Tilly. A whole host of qualitative studies may be inspiring for historians even if they cannot apply their elaborate research techniques themselves, due to the fact that most of their study objects are dead. Industrial sociology, especially in the ›classic‹ sense of the 1940s and 1950s, has started to become valuable source material for historians. New Instititionalism (Scott & Meyer 1994; Mahoney & Thelen 2009; Streeck & Thelen 2005; Hall & Thelen 2009) finally resembles much of what historians do if they practice an »actor-oriented institutional analysis« (Welskopp 2002).

Those social scientists increasingly do look at ›history‹ in order to integrate the temporality of social phenomena and their ›historicity‹ into their conceptual thinking. Here, as I will argue, may the common ground be found for an intensified dialogue and potential cooperation. In the best of all worlds a growing interest in ›history‹ as a theoretical perspective on temporality and ›historicity‹ might inspire a deepend curiosity about ›history‹ as a discipline which has become much more theoretical and methodologically conscious since the time it was equated with the naïve narration of past events. The question then is what social scientists and historians can learn from each other, from their respective ways of dealing with theory and methods in a world conceived as profoundly historical (Sewell 2005).

In my essay I want to explore some venues of such an enterprise from the viewpoint of an historian whom his interest in theory has brought into the field of history in the first place. I will start out with a discussion of an influential strand in German historiography whichin the late 1960s and early 1970s programmatically defined history as a ›historical social science‹ and set out to reshape national history writing into a comprehensive, theoretically guided and comparative ›history of society‹ (*Gesellschaftsgeschichte*). In the second part of this essay I want to venture beyond this position, which in my view did not much more than re-establish the conceptual dualism of ›theory‹ that by definition must not con-

taminate the actual historical narrative and of a ›history‹ as a narrative whose theoretical status remained obscure. To be fair, however, acknowledging ›theory‹ as an important topic in the historical disciplinary discourse has been the great and lasting achievement of this specific German variant of social history. It is precisely this sustained explicit focus on ›theory‹ and methodological debate which makes the German *Gesellschaftsgeschichte* a valuable test case for an interrupted convergence between history and sociology.[1]

I will argue in the remainder of this essay that ›history‹ can only exist as a thoroughly theoretical endeavor but that its ›theorizing‹ is profoundly shaped by its self-conception of what temporality and ›historicity‹ mean for conceptualizing the past. In closing I will reflect on how this may influence the theoretical approaches the social sciences are debating when they talk about the necessity to include ›history‹ in their explanations.

History as *Gesellschaftsgeschichte* and ›historical social science‹

It was only with the jump start of a new generation of social historians in Germany in the late 1960s that a minority strand of the discipline explicitly left the camp of *Geisteswissenschaften* and proclaimed history another ›social science‹. In its initial phase protagonists like Hans-Ulrich Wehler and Jürgen Kocka professed to an open minded interdisciplinary dialogue with sociology, economics, political science, and, to a lesser extent, psychology. Yet it is crucial to disentangle what that meant. First, the social scientific turn entailed a re-reading of Karl Marx and Max Weber, mediated by the influences of émigrés like Hans Rosenberg or re-

1 German social history, unlike its British or U.S. counterparts, always lacked a positivist tradition which might have played into the hands of an a-theoretical specialization. It also saw a limited trend from Marxism to the post-structuralist Foucauldian discourse analysis, being not really Marxist in the first place and remaining sceptical vis-à-vis the full scale turn towards a new substantialism of language, for a British-American example of this trend from Marxism toward poststructuralism see Eley 2006.

claimed authorities like Eckart Kehr and Otto Hintze. This reception was decisively shaped by the fact that it occurred as a re-import via the U.S. Thus Weber in particular was adopted in a structural functionalist fashion attributed to Talcott Parsons. This is not to say that ›historical social science‹ was structural functionalist in any theoretical sense. Yet its influence stressed the structuralist factor in Weber's work and let him appear first of all as the theoretician of ›rationalization‹, a variant of modernization theory. Another transatlantic import was, second, the reception of modernization theory proper (Wehler 1975; cf. critically Mergel 1997). This was, after all, the first flirtation of German history with the positivist sociological tradition. Yet very much like sociology in Germany, which adopted empirical social research with breathtaking speed despite its peculiar history, social historians never problematized the positivistic implications of the Anglo-American school of modernization. This was due to the fact that, third, a second strand of Weberian thinking, paired with a specific understanding of Marxism, entered the field via the social philosophy of the Frankfurt school, most notably attributed to Jürgen Habermas. This was the ›epistemological Weber‹ as embodied in his concept of ›ideal types‹. ›Historical social scientists‹ were structural realists but never became positivists. Instead, the theoretical coupling of ›knowledge and interest‹ as set forth by Habermas lay the groundwork of their turn to the epistemology of ›ideological criticism‹.

The proclamation of a ›history beyond *Historismus*‹ actually propagated a ›history beyond hermeneutics‹ (Iggers 1984; 1985). German social historians performed a sharp structuralist turn first of all because of the radically anti-hermeneutic inclination to distinguish themselves from *Historismus* and *Strukturgeschichte* alike (Mommsen 1971). On two epistemological levels ideological criticism replaced hermeneutics and lent legitimacy to a particularly strong emphasis on structures: First, social historians systematically called in question that the past could be uncovered by exploring the intentions of the historical subjects. Ideological criticism contended that these lacked full insight into the all-powerful structural constraints they were acting under. History, Habermas had written, was more than »people intended to do reciprocally«. Social historians in Ger-

many charged this formula with the radicalized meaning that history was a matter far different from what the contemporary subjects had been able to experience and comprehend. ›Experience‹, therefore, was distorted reality, distorted by ideology and manipulation. In consequence, the historian had to move beyond the level of past statements and the language of the sources. The structural constraints of action themselves had to serve as the proper focus of inquiry (Kocka 1986: 76 f.; idem 1975: 24 ff.). Second, social historians challenged all historiographical approaches – most prominently *Historismus* – that used hermeneutic methodology in order to ›understand‹ the past through the intentions of the ›personalities‹ involved. They discounted these approaches not only as being methodologically naïve but as being ideologically affirmative and politically apologetic. Only structural analysis critical of the historical ›agents‹ and their hermeneutic historians alike would be able to unveil the ›real‹ past from ideological distortions.

Given the salience the notion of ›structure‹ acquired in ›historical social science‹ it is surprising how little attention it drew in the theoretical discussions among German historians during the 1970s and 1980s. Although social historians like Jürgen Kocka in *Strukturgeschichte* criticized the usage of the term as being unspecific, they did adopt its *strukturgeschichtliche* formulation as set out by Reinhart Koselleck for a completely different purpose. In his »theory of historical time« this notion defined a specifically modern type of ›experience‹: Historical time, he suggested, had accelerated under the conditions of modernity to a degree that even ›structural factors‹ could now be experienced as recurring events (Koselleck 1989: 144-157). Whereas for Conze and Schieder ›structure‹ was a descriptive concept that addressed the basic coherence of social totality and Koselleck made it a junction term between impersonal developments and individual experience, Kocka gave it the above-mentioned epistemological twist. He borrowed Koselleck's argument but drew from this the opposite conclusion to treat ›structure‹ as a force by definition beyond the grasp of human experience. Consequently, in his eyes a truly comprehensive explanation of history required a structural analysis of the conditions, restraints and unintended effects of ›agency‹ taken as far

as possible. ›Agency‹ – beyond interest and conformity to ›structure‹ – thus shrank to a mere residue at the margins of the historical account (Kocka 1977: 167f.; idem 1986: 76f.).

Thus the notion of ›structure‹ in German social history did reflect the dualism of ›secondary systems and mechanisms‹, and historicist hermeneutics were reduced to a marginal phenomenon rather than any understanding as fashioned by the Western sociology of that time. The heritage of *Strukturgeschichte* was unwillingly carried on including its historicist elements. As a descriptive category it bore the burden of indiscriminately relating to institutions and patterns of collective behavior. The term could also simply address statistical proportions. It became charged, however, with a vague materialism that re-established a clear causal chain between the economic, social, and political dimensions of historical analysis. This meant that a concept of ›structure‹ derived from an unacknowledged sociological source (›German sociology‹) was paired with a remotely Marxist model of ›base‹ and ›superstructure‹ that attributed to the economic, social, and political levels of society different measures of ›structuredness‹ and ›agency‹ (cf. the critique Welskopp 1999).

The gist of my argument is that ›historical social science‹ in Germany has never been as receptive to the developments in Western social sciences as it had claimed to be. Its advances towards sociology, political science, and economics remained short-lived and highly selective. Furthermore, its appropriations of approaches from these disciplines were mediated by both a specific structuralism not accounted for and unacknowledged historicist remnants. Social science history did endorse the explicit ›application‹ of theories borrowed for strictly ›instrumental‹ purposes from the neighboring fields. Yet it is doubtful whether the soaring discussions on theory in the 1970s (before they faded out during the 1980s) ever acquired a genuine theoretical quality. In retrospect, they rather featured a mere rhetoric of technicality designed to mark the distance to narrative *Historismus* and to claim superiority for a social history aspiring to the unequivocal terminology of the natural sciences (Etzemüller 2001: 346, note 115). In fact, the example of modernization theory serves particularly well to show that critical reception confined itself to short inau-

gural reflections (Lorenz 2000). These represented claims jumping rather than thorough theoretical scrutiny. Once appropriated, the concepts assumed a rather unproblematic status only subjected to empirical qualification.[2] ›History proper‹ thus came to occupy the space between the model applied and the sequence of diversions from its prescribed standards that were uncovered from context in the course of analysis. When charged with theoretical predicaments, ›social science history‹ professed to cheerful eclecticism. Yet this remained firmly embedded in overarching modernization rhetoric.

Theoretical eclecticism and long-term dedication to modernization theory are symptoms that social history still upheld the unitary notion of history as inherited from *Historismus*. It is striking that, on a closer look, German social historians actually privileged ›process‹ over ›structure‹ – given that for them ›process‹ was structure in motion and ›structure‹ a synchronic constellation of elements. Consequently, they appropriated only those concepts from sociology or economics that represented models of singular linear processes. Hansjörg Siegenthaler has aptly defined such theories as »anticipations of historically singular sequences of events in abstract terms«. Theories of such a linear architecture, turning a somewhat idealized historical path into a normatively charged processual model, were in vogue in contemporary economics (e.g. the ›long waves‹ approach to business cycles) as well as the sociology of that time (modernization, Marxism) (Siegenthaler 1999: 280). The parallel structure of these theories and the underlying idea of an integral historical process facilitated the negligence to discuss the status theory could acquire in history or what history actually was, after all. This was *Historismus* snuck in through the back door.

The structuralism of a genuine, non-sociological nature reveals its salience when we take into account that Jürgen Habermas' social philosophy served as an unquestioned epistemological authority and much consulted political ally but not as a source of theoretical inspiration. In his *Gesellschaftsgeschichte* Wehler does make a reference to Habermas' distinction

2 A very good example is Kocka 1985.

between ›work‹, ›domination‹, and ›language‹, but only to reify it in the very next sentence by translating these terms into ›economy‹, ›rule of the state‹, and ›culture‹, segmented layers of society interlinked by patterns of social inequality (Wehler 1996[3]: 7). This means that Wehler does not follow Habermas in spelling out his twin concept of ›system‹ and ›life-world‹. However skeptical one may be about the feasibility of this concept – it is all too evident that Wehler hypostatizes the perspective of ›system‹. Whereas for Habermas the ›life-world‹ is the sphere of ›agency‹, of the reproduction of ›systems‹, of the confrontations between ›systems‹ and ›life-world‹ and, therefore, the site of history, Wehler treats ›systems‹ as segmented entities that are themselves capable of acting like collective subjects as the driving forces of historical conflict and change (Johnson 1993). When the term ›life-world‹ awkwardly resurfaced in the context of *Alltagsgeschichte* (history of everyday life) in the mid-1980s, Wehler denounced it as ›neo-historicist‹. What Habermas would term systemic evolution is a unitary process of historical development for Wehler, qualified only by the interference of important ›personalities‹. Communicative action is no part of the ›agency‹ he describes when entering the sphere of ›the political‹.

The uneasy coexistence of materialist structuralism and unacknowledged historicism also shaped the relations of German social history and American historical sociology. In their quest to find allies and inspiring examples to whose authority they could refer, German social historians transcended boundaries and called to attention historical sociology of the kind as practiced by Theda Skocpol, Barrington Moore, or Charles Tilly – among others. When it came to justify the call for historical comparison, social historians readily referred to the pioneering work done in that field, yet without encouraging imitation. Historical sociology remained a friendly but alien ally – alien in the sense that this discipline shared with social history the macro-causal perspective and the inclination to comparative work but differed markedly in other fundamental respects: Social history would just not follow suit with historical sociology's universalist models and theories and its preference for multi-case comparisons (Tilly 1984). Comparison in social science history devel-

oped as a much more contextualized project pairing two or at the most three cases. It was not universalist in outlook but aimed at individualizing the case of foremost interest (Haupt & Kocka 1996). The nation state remained the standard unit of comparison, whereas historical sociology tried to move beyond national boundaries (McAdam & Tarrow & Tilly 2001). It is true that recent comparative work in Germany has ultimately exceeded these limitations (Welskopp 1995). Yet it is evident that even comparison had for a long time been part and parcel of a national history that translated its underlying unitary notion of history into historical singularity. The call for comparison arose when the influential hypothesis of a ›German divergence from the West‹ (*deutscher Sonderweg*) was launched, the attempt to explain the crimes of *National Socialism* by reference to the peculiarities of German national history.

German social science history did re-introduce the task of theoretical reflection to the historian's responsibility. Yet it shied away from the question whether theory building was also a task historians had to confront in the future. It also hedged a limited spectrum of theories and failed to put forth criteria to assess competing theoretical proposals on other grounds than their applicability to a concrete historical subject matter. Thus it could actually not have come as a surprise when more recent revisionist approaches engaged in theoretical discussions of their own, taking them away from the hegemony of social history. Their orientation shifted from sociology to cultural anthropology, linguistics, literary criticism, and discourse analysis. This move certainly produced a new one-sidedness and failed to recognize that the theoretical instruments of sociology were undergoing profound change as well. The theoretical discussion, focusing heavily on poststructuralism, completely neglected, for example, the development of ›practice theory‹ in sociology (Reckwitz 2000; 2002). Yet it helped with establishing a theoretical discourse in history that gradually became more than a mere struggle about which theories to borrow from the lead discipline then in vogue.

Nothing goes without saying – the language of turns

It should be clear from the brief treatment of social history as a ›historical social science‹ that this strand of historiography had aspired to reach a new theoretical quality by becoming as similar to the natural sciences as possible – or to the social sciences of that time which likewise tried to emulate the scientific jargon of their colleagues in the laboratories (Welskopp 2006). This tendency was never exercised to its full promises, however, and finally gave way to a more or less uneasy coexistence of watered-down modernization rhetoric and pragmatic empirical history writing along more traditional narrative lines. The general decrease of theoretical interest within social history made the bold advances of new strands of historical research the more threatening. It became obvious that not only the utilization of certain theories from neighboring disciplines was questionable but that the whole theoretical foundation of history as a discipline was at stake. Among German social historians only Reinhart Koselleck had so far developed his own vision of the genuine theoretical qualities of ›history‹ itself: as a conception both of the temporality of human experience and of the historicity of the language used by the historical agents in the process of shaping and creating their experiences (Koselleck 1972; 1989: 107-207; 2000).

Koselleck pointed out to the central role of semantics for the change and persistence of social and political constellations long before the ›linguistic turn‹ – belatedly – hit Germany. And it seems that after twenty-five years of deconstruction and discourse analysis the early exuberance of dismissing the social agent and charging all concepts and notions of a past ›reality‹ with essentialism has considerably faded away (e.g. Scott 1988a). Ironically, some young proponents of discourse analysis seek refuge in actor concepts derived from the most antiquated rational choice approaches, whereas some current variants of rational action theory display a growing willingness to accept at least a broadening of their actor concepts in order to include cultural factors like ›bounded rationality‹ or ›framing‹ (Graf 2005; Frings & Marx 2005). It has also become evident that some discourse analysts (not the later Foucault) only had replaced the essentialism of ›structural realism‹, of which they found social history

guilty, with a new essentialism derived from the ontological qualities they tacitly attributed to language. The persistent rhetoric of ›turns‹ in the *Geistes-* and *Kulturwissenschaften* might mask the sobering fact that some aspiring historians have taken a turn too many and reached their point of departure again.

Yet what remains from the quarrels around the ›linguistic turn‹ and what any historian can learn from Koselleck in his (or her) attempts at theoretical explanations is the notion of the profound historicity of social and political language (Koselleck 2007). This means that social scientists and historians alike do not have a terminology at their disposal which is uncontaminated by the historical processes in which historical agents coined the terms that described – and constituted – their social worlds. It also means that any social analysis which ignores the contemporary ways of ›semanticizing‹ social relations and relations of inequality, of inclusion and exclusion, is incomplete because it is in danger of missing the linguistic mechanisms that not only made the world intelligible for those participating in it but provided the verbal tools to shape it.

To acknowledge the historicity of all concepts demonstrates the recognition of the historicity of all social sciences, including history. This must not be equated with relativism – of which the beleaguered social historians found the ›postmodernist‹ historians guilty. It rather draws the methodological conclusions from the insight that our – the historians' – objects of observation are no guinea pigs but human agents very similar to ourselves. Their limited awareness of the conditions and unintended consequences of their actions which we can pinpoint retrospectively must warn us of our own ›bounded rationality‹ and is no reason for intellectual hubris. Their likeness connects the observers and the observed and provides, on the one hand, an epistemological key to all social analysis – a ›fusion of horizons‹, as hermeneutic philosopher Hans-Georg Gadamer once said, indifferent of the actual familiarity or unfamiliarity of the observed historical context. On the other hand, as Anthony Giddens maintains, it is this conceptual connection that may actually provoke changes in the observed contexts (cf. Giddens 1991: 210-217). Historical agents – at least as long as they live – may adopt the con-

ceptual offers of social scientists and historians and react to them, thereby unwittingly altering the social constellation originally analyzed. This reflects that the social sciences, again history included, are nothing more but also nothing less than the self-referential loop in societies reflecting on their own present and past (Welskopp 2005a: 126 ff; Welskopp 2007[3]).

Everyday Constructivists

The ›linguistic turn‹ gave social historians the jitters because of its inherent epistemological contention that there was no ›real history‹ out there but only the talk about it.[3] This virtually vaporized the business principle of ›structural realism‹ most social historians were then operating under (Lorenz 1994). So-called radical constructivism even went a step further in denying that there is something like a ›reality‹ at all out there which preordains social relations and human agency. To the disgust of zealous discourse analysts, the ›radical constructivists‹ went so far as to doubt the ontological pre-existence and, therefore, determining qualities of language, insisting on the constitutive power of situative ›language games‹ to reconstruct and modify registers of language by performative instantiation and discursive usage (Foerster 2008[10]; Glasersfeld 1995; Larochelle 2007).

On a closer look, however, social historians – instead of making ›constructivism‹ a synonym for all evils of the world – could have turned this approach into a powerful argument against the ›essentialism of language‹ of some strands of discourse analysis that have more or less reified the ›discourse‹ into an anonymous system of relations between signs following ingrown rules independent of the awareness or will of the historical interlocutors. In fact, ›radical constructivism‹ does not make the business of the historian an impossible illusion. It rather provides the discipline with a robust epistemological foundation. The key to this lies

3 See the contributions by Keith Jenkins, Joan W. Scott, David Harlan, and Frank Ankersmit in Jenkins et al. 2007. A *locus classicus* Hunt 1989; Scott 1988b.

in the core insight of this approach that the ›constructive‹ nature of all information about ›reality‹ (including ›real‹ history) is not an epistemological problem specific to the social sciences and history but constitutive for all human beings trying to cope meaningfully with the world around them (Hacking 1999; 2002).

Thus every social agent moves about his environment as an ›everyday constructivist‹, trying to get along in his (or her) social relations on the basis of his know how, his theories about the world.[4] Anthony Giddens – following Emile Durkheim – has termed this know how ›practical consciousness‹, and he describes it as the stock of incorporated and mostly tacit knowledge social agents draw upon in their physical interaction with other social beings, artifacts, and their natural surroundings. Although most of this knowledge works without explicit verbal instantiation – through the bodily movements of human agents alone – it nevertheless provides the basis for their ›continuous reflexive monitoring of action‹, their conscious navigation through space and time. In times of crisis or on request human agents, according to Giddens, are able to lift segments of their ›practical consciousness‹ onto a discursive level and make it part of their ›discursive consciousness‹ (Giddens 1984: 41 ff.; Welskopp 2001).

›Practical consciousness‹ is a much more open concept compared to the more hermetic notion of ›habitus‹ as elaborated by Pierre Bourdieu (although both variants of a theory of social practices are compatible), which focuses on the ›economics of symbolic action‹ and targets mainly forms of distinctive behavior. Yet to mention Bourdieu's ›habitus‹ in this context is important, because it stresses ›historicity‹ to a considerably greater degree. ›Habitus‹ is literally burdened with history, distilling the essence from experiences of generations into the fuel that keeps the agents moving. It has been inscribed in the social agent over a long time, forming and reshaping his body in the process (Bourdieu 1990: 52-65). This is a distinct possibility in Giddens' structuration theory as well, but his broad view even comprises short-lived encounters and idiosyncratic

4 The ›classic‹ formulation in Berger & Luckmann 1966.

habits, which nevertheless draw upon a repertoire of tried reactions with a history of their own (Giddens 1976; idem 1984: 34 ff.).

For history as a discipline and its ways of conceptualizing its subject matter this means that historians (and social scientists in general) try to observe and explain the behavior of human agents who are forced to constantly construct their environment by means of mobilizing a practical form of knowledge which is profoundly historical in nature. It is safe to assume, however, that these ›everyday constructivists‹ go about their constructive task with more or less ›realistic‹ intentions, since they aspire and expect to ›get along smoothly‹ with the ways they interact with others. ›Radical constructivism‹ loses its frightfulness because everybody does it and most of the ›construction‹ is directed toward very pragmatic ends. In this sense, historians are nothing more than chroniclers of past encounters of constructivist endeavors with the unacknowledged conditions and unintended consequences of social action as instantiated in observable social practices.

From the viewpoint of a sociology of science, history as a discipline only systematizes what human agents do anyway all the time: It provides orientation in a present enclosed in two virtual temporal dimensions – the past and the future, both out of reach for the agents who nevertheless need orientation to interact meaningfully with their surroundings. From this need for an orientation better suited to the conditions of the environment than other forms of references to past events there concludes that human agents will have ›realistic‹ expectations when they speak of ›history‹ in contrast, for example, to myths, tales, or fictional fables. There can be no doubt that the ›histories‹ written by historians remain ›constructions‹ which make sense of an unattainable, unstructured past. Yet they write as plausibly as possible about the practices of ›realistic‹ agents for an audience with a ›historical realism‹ in mind and the pretension to be able to distinguish between ›facts‹ and ›fiction‹. Yet the audience's ›realism‹ is directed to somewhat like ›truth‹ rather than (the unattainable) past ›reality‹. History in this sense is the self-referential loop in society's dealing with its past which it does not want to leave completely to memory.

Practice and discourse

On the level of the social actors' everyday life, language constitutes the self-referential loop regarding their practices. Whereas, as I will argue, speech acts – and eventually discourses – are as much practices as is the handling of artifacts, they do contain, at least potentially, more than one layer of meaning. This makes language the mode of self-reflection, of, as Anthony Giddens has put it, the ›reflexive self-monitoring of social action‹.

So far, one important ›historical‹ characteristic of the historian's way of conceptualizing social phenomena – probably in contrast to some social scientific approaches – has been identified in its focus on the linkage between language and historicity. Within the theory of social practices, the definition of language hereby is very broad. It includes, on the one hand, all sign systems that convey any kind of ›meaning‹ (symbols, icons, pictures, gestures as well as written and spoken words), and on the other hand – think of the concept of ›practical consciousness‹ – vast areas of human agency that can be ›semanticized‹ but which normally are not verbalized. For the latter, ›tacit knowledge‹ is a prime example, a stock of rules and resources agents draw upon without verbal reference, a form of know how observers can describe verbally because it carries its meaning in the very acts of the agents but remains impossible to be reproduced as a ›working knowledge‹ in writing or otherwise.

Yet even the most recent formulations of ›practice theory‹ still reproduce a certain dualism of ›practices‹ and ›language‹ (or ›discourse‹), as if verbal utterings were no form of practice and practices by definition were free of lingual elements (Reckwitz 2003; idem 2006: 3-41). I suggest that it could be useful not to distinguish between ›practices‹ and ›language‹ but to differentiate ›practices‹ (including purely verbal practices) according to the share of verbal elements they entail. Then you have completely non-lingual practices (someone hammering a nail into a piece of wood), solely lingual practices (someone giving a speech), and many forms in between. Yet language-based practices are not just like any other form of practice. Because of the surplus of meaning inherent in any speech act the language elements in practices bear multiple potential connotations which

can prompt other attached practices, additional speech acts for example, to form redundancies to the original practice which eventually may spin off into a state of verbal autonomy (which can be called ›discourse‹). A soccer match, for example, comprises a lot of non-verbal practices (alongside a lot of verbal practices like orders, shouts, card-carrying referees and uncountable insults) which can be represented to a wider audience by means of the purely verbal practices of the reporter commenting on the game. Match and comment may make the newspaper headlines the next day – a scandal may evolve (the hidden foul-play, the incapable referee, the fired coach) which may develop into dimensions that no longer have a lot to do with the original ball kicking on the pitch. I suggest to distinguish practices of a first, a second, a third and so on order, not to declare some practices (of the first order) more important than others, but to get an idea of the linkage between original practices (which may be purely verbal practices) and the mostly verbal practices commenting on and reflecting this original practice (Welskopp 2005b; Biernacki 2000; Sewell 1999.

What will be the historian's gain from this complicated distinction? He (or she) gains a sharpened sense for the origin of his sources, for the level of observation and for the reflection that goes into the practices of the second (and so on) order. This, in turn, enables him to construct his plausible story of past practices in a much finer grain. It is all too evident that this distinction also makes his task of ›historicizing‹ a flow of events much easier. Furthermore, the power of language to constitute and shape social practices has been stressed throughout this essay. Here we can pinpoint this power where it comes to bear. Distinguishing practices of the first and second (and so on) order according to their share of verbal elements and reflexive content makes it possible to explain the actions of a human agent plausible as being ›realistic‹ in the sense of a specifically ›bounded rationality‹ even if he self-confessedly only executes a divine order. Finally, what the historian gains is a real integration of ›practices‹ and ›discourse‹ within an encompassing theory of social practices. Quite analogously to Giddens' enterprise to turn the traditional dualism of ›structure‹ and ›agency‹ into a duality, we find a duality in the

concept of ›discourse‹ which denotes it as practice and its reflective mode at the same time.

This brand of theorizing might not appeal to many sociologists not occupied with practice theory. Yet it may shed some light on the way historians deal with theory in general. It shows why historians tend to receive, develop, and apply theoretical concepts which keep ›in touch‹ with the phenomena blinking through the remnants of a past irretrievably lost. One of the main tasks of theory in history – apart from epistemological meta-reflection and a social theory one might as well call historical ontology – is to make these remnants speak, to constitute phenomena which are then theoretically interpreted and laid out in a narrative containing the details necessary for an adequate historical understanding. Whether one may find this unfortunate or not – historians are not able to produce their own material by a methodically prescribed procedure.

Institutions, systems, and temporality

Whereas practice theory works for history exceptionally well on the level of ›face-to-face‹-interaction (Barnes 2001; Coulter 2001; Schatzki et al. 2001), the conceptualization of aggregated, ›collective‹ action or institutional ›action‹ requires considerable additional thought, since the theoretical offers at hand – like Weber, Giddens, Bourdieu, Foucault, or, in the current German context, Andreas Reckwitz – do not provide more than unrefined building elements toward an actor-oriented analysis of institutions and systems which I want to advocate here. First of all, it is necessary to note that ›collective‹ action is *not* a simple aggregation of individual actions as suggested by the ›bathtub‹ model of the rational choice people. Following Weber, such rather unlikely instances have to be understood to be very complex processes in which primary group structures, institutions, and systemic relations are involved, concentrated at a specific locale: the focus, and tipped off by a specific constellation of strains and opportunities. Likewise, institutional ›action‹ must not be treated as the action of a ›super-individual‹. I deem it far better to speak of the outcomes of institutional processes as ›institutional effects‹.

The primary distinction, then, is between the human agents that occupy institutions and the institutions themselves which are the situative result of three interlocking sets of social relations. ›Institutions‹ in my sense are conceptualized from the notion of ›organizations‹, although, following Weber again, not all relatively stable sets of social relations which can be termed ›institutions‹ are ›organizations‹ proper (like marriage, therefore the ›relatively‹). The wording, therefore, differs from the usage of ›institutions‹ in the ›New Institutional Economics‹ where the term often denotes ›values‹ or ›contracts‹. ›Institutions‹ in practice theory may be built around ›values‹ or ›contracts‹ but cannot be pictured without a specific social and cultural surplus that holds them together. This view pairs well with the notion of ›institutions‹ as advanced in ›New Institutionalism‹, because it retains the social actors involved and the power relations, micropolitics, and contingency that characterize organizations specifically. In contrast to those variants of organizational sociology which – following a systems theoretical approach – are in search of general features of organizations, ›praxeological‹ historians and ›New Institutionalists‹ alike are interested in the specificity of concrete types of organizations (Thelen 2004; Mahoney & Thelen 2009).

Actor-oriented analysis of institutions thus means an approach that does not make the social agents disappear as soon as they enter an organizational context. Rather, both the agents involved and the institutional structures are characterized by ›material‹ foundations of different qualities. The agent finds his (or her) ›body‹ as the ›material‹ center of his activities, regardless of how non-essentialist we must conceive the body. For our purpose here the ›body‹ simply represents the agents' situating in time and space and in a context that bears an influence on his range of possible actions. An institution or organization ›regionalizes‹ – in Anthony Giddens' terms – the actions of the social agents involved. The institution, in contrast, interlinks the agents internally by relations of communication which are based on ›material technical means‹, on specialized artifacts. It also produces – since it is designed to do so – institutional effects affecting a large number of human agents outside the institution, again via ›technical means‹ that multiply the actions of indi-

vidual members of the institution (Giddens 1984: 319 ff.; Welskopp 1999: 100-119).

As mentioned above, institutions consist of three interlocking sets of social relations: first, the relations between the social agents involved and their social background ›outside‹; second, the internal relations between the members of the institution; and, third, the relations between the institution as an institution and the human agents (or their institutions) affected – those relations I suggest to call ›institutional effects‹. These effects are the raison d'être of institutions in general: to magnify the consequences of individual action beyond the level of ›face-to-face‹-interaction (Welskopp 1999: 100-119; Mahoney & Thelen 2009).

The first rationale behind this conceptualization is that an institution is more of a social cosmos than both the aggregate of its members and the quality of its outcomes can explain. Moreover, it is the feedback loops between the three interlocking sets of social relations that actually produce ›bounded rationalities‹ specific to the respective institution, ›bounded‹ less in the sense of ›limited‹ but rather in the sense of ›geared to a specific internal logic‹. The functional logic of institutions is widely felt by the social agents affected by its effects, but how it works to produce specific ›institutional effects‹ remains hardly intelligible for someone being outside. It is for this reason that organizational complexes are frequently described by organic language, by mechanistic metaphors, as collective individuals, or as manipulative conspiracies.

The second rationale behind this model design is to capture the temporality of institutions. Institutions ›regionalize‹ social action in time and space; therefore it would be more apt to speak of the specific ›time geography‹ institutions command as their organizational resources (Soja 1989). To say that institutions produce effects beyond the level of ›face-to-face‹-interaction means that they both control a qualitatively wider range of social relations over time and space than any individual agent without institutional background, and that they sustain the absence of its members and outside targets. The ability to cover extended spans of time and areas of space is, therefore, another raison d'être of institutions. It can be described as an asset, as a set of resources of the institution

itself. »Binding time and space« – as Giddens puts it – thus becomes a defining characteristic of institutions, and this is a viewpoint that reverses our conventional understanding of time as something chronological and external to social relations which affects all social objects equally and alike. Seen this way, time and space can be accumulated and stored by institutions as ›institutional potentials‹ which may explain institutional development if no other variables interfere, and which must be taken into account as one possible form of change over time that normally escapes social scientific theory building (Giddens 1984: 180 ff.).

Unfortunately, the social world cannot be exhaustively described as a conglomerate of agents and institutions. There are social systems that are relatively stable over time and sometimes extensive in reach which consist of institutions and networks of agents but whose cohesion and functioning cannot be attributed to a single control center, although it seems as if someone must pull the strings. Markets, money systems, and registers of language usually serve as favorite examples. Max Weber termed such societal structures rather helplessly ›as if‹-systems (Weber 1980[5]: 14), and Anthony Giddens takes refuge in projecting these structural sets as organized around ›structural principles‹, shared ›axes of structuration‹, without being able to establish plausibly what these ›axes‹ – decidedly no ›superstructures‹ – actually consist of (they appear as the virtual – and empty – centers spiral nebulae revolve around) (Welskopp 1997).

Here, practice theory might make progress by invoking some basic ideas of Niklas Luhmann's systems theory, even if we take into account that his and his followers' mode of theorizing differs fundamentally from the historians' way of conceptualizing. Combining his notion of system with an actor-oriented approach designed to interpret the phenomenological might even send him spinning in his grave. Yet this granted, especially his concept of *autopoiesis* (›self-generation‹) seems useful in this context (Luhmann 1995: 32-38). We can translate *autopoiesis* into the notion of a specific ›functional logic‹ shared by networks of agents and institutions interlinked by mutual relations of exchange which form strong feedback loops. Individuals as well as specialized institutions (e.g. business firms) may participate in common markets shaped by a very specific functional

rationality without somebody laying down the rules or dictating an all-encompassing contract obliging all participants to each other. By this balance we arrive at the notion of ›bounded rationality‹ again, and again this means a special functional logic rather than limitations, only that this ›bounded rationality‹ informs a whole set of agents and institutions, sometimes covering entire sectors of society, like economy (Luhmann 1995: 187-197).

Time, space, and multiple forms of change

The pervasive influence of temporality and ›historicity‹ on the conceptualizations of historians following some strands of practice theory has been amply demonstrated, and so the question of »what is the specific ›historical‹ in history« may be closer to an answer now. Renate Mayntz put it this way: »First of all, ›historical‹ does not always mean ›past‹, but stresses the fact that it denotes a concrete case (sequence of events etc.) exactly localized in space and time. What distinguishes both disciplines [macro-sociology and history] is more the orientation at explanation than the identification of the objects of analysis in present times or the past« (Mayntz 2002: 9; 2004). History must insist in the relevance of situating agents, institutions, systems, and events precisely in time and space. Their physical uniqueness matters. They cannot be reduced to mere ›cases of x‹ or a collection of variables. The legitimate level of isolating them from their context always remains to be established and justified (as, for example, with comparisons).

This does not mean, however, that history can do without isolating its objects from context at all or do without abstractions altogether. Yet it is a fundamental task not to let the single object get irretrievably lost in some form of summation or aggregation. Basically, all generalizations in history must be able to trace and illuminate the single case and its documentation (since history is even less capable of producing its own evidence than the systematic social sciences). There is no doubt that ›individualization‹ is not the self-serving purpose of history writing, as Leopold von Ranke has been misquoted for a hundred and fifty years by now. Rather, ›individualizing‹ is an ancillary operation in the process of

generalization, in order to demonstrate that and in how far ›cases‹ are exemplary (representative is a statistical, not an epistemological term in history) for a more encompassing relationship. ›Individualizing‹ also serves to explain exceptions, and in doing so helps to establish the limits on the validity of a model employed. Yet even then, the individual case must not go down the drain. Theoretical concepts in history must not lose touch with the phenomenological, since it is to better explain the phenomenon that theory is applied to history in the first place.

A major consequence for conceptualizations in history following from this is that generalizations are never universalist or all-encompassing but frequently assume the form of typologies. Typologies specify their claims to validity in time and space. They usually consist of several layers of generalizations, reaching a level abstract enough for theoretical debate and comparison, but on a more concrete level illuminating singular phenomena by connecting models with concrete names, times, and locales. This mode of generalization strongly calls for a comparative perspective. Contextualized comparisons are the only mode of reaching generalizations without reducing the compared objects to faceless sets of variables. When I stress the usefulness of contextualizing comparisons, I do not only argue against this methodological reductionism but also against the methodically ›boundless‹ contextualization done in the traditional historical single case study (Welskopp 2010: 8 ff.). Therefore, my notion of contextualizing comparisons meets the view of recent neo-institutional approaches which utilize a comparative perspective to get away from the idiosyncracies of case studies and actually aim for a distancing effect that allows for identifying general patterns and systematic variations across a limited number of cases (as an example Thelen 2004). It is of utmost importance that the objects of comparison are only constituted in the process of establishing the specific comparative logic, by selecting a common theoretical base which makes the objects appear as variations of a *tertium comparationis* and by formulating criteria for the measurement of similarities and contrasts (Heintz 2010: 3-6).

This, of course, has bearings on how historians conceptualize processes. First of all it is important to note that historians are not by definition the scholars of change. They are and must be equally interested in the reproduction of social phenomena, in the conditions and limits of stability. Thinking in terms of the capability to bind time and space points into that direction. From a historical viewpoint it appears more apt to stress the contingent aspects of change. However, contingency in this respect means a greater but nevertheless limited number of possible outcomes, variability rather than chance. The contingent dimension of change results from the complex interaction of potential sources of structural dynamics whose interference may produce sequences of events which are perverse and hard to predict.[5]

Simple models of transformation, like in evolutionary theory, are often correct in describing important aspects of change but do not grasp the whole story (Sewell 1992; 2005: 81-123). Specific evolutionary processes might stop overnight. They mingle with cycles of reproduction, with conjunctures, or sudden episodes (like the complete breakdown of a social order). Evolution, moreover, does not only mean unfolding from a shared origin but also encompasses unpredictable jumps caused by mutation. Luhmann explicitly preferred the latter metaphor in his conceptualization of system change, as embodied in his notion of *autopoiesis*, although, strictly speaking, the large-scale, long-term processes of functional system differentiation do evoke the impression of unfolding. The attribution of the binding capacity of time and space to institutions does not mean that institutions necessarily live this capacity out to its full potential. Even the most streamlined company may fall victim to a hostile takeover financed by some age-honored pension fund from a completely different segment of the economy (and different part of the world). Path dependency is, for historians, a very attractive concept and seems a promising way for historical thinking to make inroads in the social sciences. However, the concept of path dependency is not unequivocal. In some formulations, path dependency focuses on sequences of decisions,

5 On the concept of ›sequencing‹ very illuminating Thelen 2004.

and decisions are but one mechanism to bring about change (Liebowitz & Margolis 1995). The long lasting formative power of certain mentalities, for example, is usually not meant by the term. Path dependency sometimes denotes a critical turning-point which determines the course of events ever after (Abbott 2001: 240-260). Sometimes it is applied to a progressively diminishing spectrum of alternatives as the result of choices taken. Even path dependency, therefore, does not suffice to tell the whole story. Finally, we have to differentiate between changes within and between systems. Business cycles do not by nature transcend capitalism (at least up to now), although each major cycle may transform capitalism in a way unaccounted for in economic theory. This is usually the point where the historian claims his retrospective obstinacy.

Theory and narration: Perspectives of an interdisciplinary future for history and sociology

History as a discipline has always been ready to forget about its own past. This past, therefore, abounds with aborted learning curves. Thus it may be that the social sciences can point toward the vast number of naïve narratives that historians are still turning out, graveyards of empirical facts nobody really cares for, enclosed by an insurmountable concertina fence of impregnable footnotes. They can turn this into the argument that history as a discipline, after all, has nothing to offer to its neighboring scholarly communities. Yet in my opinion history, under the legitimatory pressure of ›postmodernism‹, has developed into one of the most self-reflective among all *Sozial-* und *Geisteswissenschaften*. There is a large body of research out there now on the history of historiography which is theoretically competent and has more to say than merely to highlight the areas of the discipline's own forgetfulness (as in the case of German historians under National Socialism, for example).

This does not mean, however, that historians will give up narrativity as their foremost means of representing their findings. The anti-narrational turn German social history proclaimed in the late 1960s and 1970s basically led nowhere. Early critics remarked that this was inconsequential and a mere failure to live up to one's own standards. For a very short

time, anti-narrativity just spoiled readable historical prose, some books of the early 1970s dotted with tables and graphs and sporting entire chapters on technical definitions. Yet this was never true for the more prominent scholarly works of the time. Even if dedicated to a distanced vocabulary, historians like Jürgen Kocka or Hans-Ulrich Wehler clung to a narrative plot, and the degree to which Wehler's chapters on politics appear personalized must be surprising with such a self-declared ›structuralist‹. I, in turn, rather think that the continued stress on narration has a theoretical foundation and echoes the expectations of an audience that may naïvely – and in a futile way – yearn for ›historical truth‹ but that also longs for stories with a beginning and an end that are intelligible for people who organize their memory in much the same way.

The theoretical reason for the continued leading role of narration in historians' writings lies in their insistence on situating actors and events in time and space, in the irreducibility of the human agent with his characteristic life cycle, in the embeddedness of cases in their contexts, and in the multiplicity of change. It is a way of dealing with contingency, and the mode of earlier concepts of ›social mechanisms‹ – since then acknowledged as being too sweeping and mechanistic – to dissolve contingency into causality, is no feasible alternative. Of course this cannot justify the old a-theoretical historiography which tended to dissolve contingency into a streamlined narrational flow. This does not mean that theoretical reflections have to be kept away from the book market. On the contrary, the development and production of theory – complete with the representation of findings of this kind in print – are in my opinion one of the main tasks that research-oriented historians have to fulfill. It may be at this level – as maintained throughout this essay – that historians may enter into fruitful discussions with their colleagues from allegedly more systematic disciplines.

There are some encouraging signs that this may be fruitful if pursued systematically. Some political scientists, for instance, experiment with ›analytic narratives‹ as a legitimate form of presenting their research, although it has become clear that Margaret Levi, among others, wants them to supplement rational choice analyses in order to make them more

intelligible for readers. The statement may also be true that it has not become entirely clear what a sociologist actually believes to be an analytic narrative. For the historian, a narrative grounded in theory and reflecting contingency while enabling generalizations would fulfill the promise associated with the term (Bates et al. 1998; Levi 2002).

Interaction analysis and close readings of cultural settings among face-to-face relations – following Erving Goffman's tradition and others – constitute a second field where the interests of historians and sociologists intersect. Whereas most historians are by definition of their subject matter unable to directly observe the symbolic interactions *in actu*, the ›tools‹ of symbolic interactionism still prove very useful for them. Especially historians of the middle ages and early modernity, who are indeed occupied with societal ties acted out in face-to-face encounters, resort to this theoretical perspective. They use the concepts in order to build an imaginary sphere of past action, against which the available sources and scarce remnants of the past can be interpreted in a more intelligible way.

Another area of overlapping interests may be mapped between the ›actor-oriented institutional analysis‹ some historians are doing in a ›praxeological‹ perspective and those neo-institutionalist sociologists and political scientists who also stress the role of agency, power relations, and contingency and who consciously apply a notion of ›history‹ as temporality to their work.[6] One of the methodologically salient features works like Kathleen Thelen's in the ›New Institutionalism‹ share with ›praxeological‹ historians is their inclination towards contextualized comparisons. Although probably more interested in discovering – and pinpointing – causal relationships, they also generalize in the form of typologies and take advantage of the fact that comparisons enable generalizations without reducing the objects of comparison to a numerical sample.

6 An excellent example is Thelen 2004.

I am aware of the fact that these mostly qualitative directions in sociology only represent a minority in the discipline. On the whole, quantitative studies, macro-sociological enquiries, and much abstract model construction seem on the rise here. Rational action theory is no doubt in advance rather than in retreat, countering blames of ›reductionism‹ with the programmatic statement that ›reductionism‹ of this kind was the only way to retain a notion of ›the social‹ in a social scientific world increasingly dominated by cognitive science, life science, and neurobiology. There is certainly not much to expect in terms of the interdisciplinary dialogue between history and sociology here, since not history but the sciences are clearly the addressees of the quantifiers' arguments.

On the side of history, some disciplinary features seem irreducible as well, making a cooperation difficult for sociologists. The narrative story, most importantly, will remain the prominent ›end product‹ of a historian's professional activity. It is his (or her) commodity. But this narrative can be deeply informed by theory; it can be structured along strictly analytical lines; it can imply models and concepts, can compose full fledged comparisons into a coherent storyline and still thrive for an elegant prose readable by more people than a few peers. Narrative structures organize historian's monographs, whether shaped as case studies – which would be digestible for sociologists – or as a storyline linking the start of a book with its ending. Historians are still driven by the illusion that they write for a public broader than the community of their expert peers. There are indeed examples of such mastery out there (unfortunately more in the Anglo-Saxon world than in Germany).

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History, the social sciences and potentials for cooperation

With particular attention to economic history*

Jürgen Kocka

> In the view of certain experts, the general historicization of the social sciences since the 1960s, reflecting above all experience with developing countries, has here and there reached Western economics, as well […]. [T]he days of pure economics appear to be numbered with the breakthrough of a socio-economic standpoint that encompasses the historical dimension. The constellation is favourable. The lessons offered by transformation processes in the ›Third World‹ point in this direction, as do the revival of scientific neo-Marxism and the outcomes of many a debate in the discipline.

Thus begins the introduction to Hans-Ulrich Wehler's influential reader on *History and Economics* from 1973 (Wehler 1973: 11). Even then, this was much more an expression of hope than a sober assessment of the situation. The hopes pinned on the historicization of economics were consonant with the strong expectations of ever closer bonds between history and the social sciences, connections that had developed and proved their value in various new approaches in international history since the 1930s. Such new developments were promoted at different places, in France in the pages of *Annales*, in Britain by Marxist historians

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close to the journal *Past & Present*, in the United States by representatives of historical sociology as well as, in other ways, in the ›New Economic History‹ of the 1950s and 1960s, and in West Germany in social history and ›historical social science‹ (*Historische Sozialwissenschaft*) that emerged in the 1960s.

What did these various currents have in common? Firstly, they all stressed structures and processes over actions, persons, and events as dimensions of investigation. Secondly, they adopted analytic approaches that went beyond a hermeneutic reconstruction of meanings. In the 1960s and 1970s, this meant seeking the explicit definition of concepts, experimenting with theoretical orientation, sometimes turning to quantifying procedures, and applying comparative approaches. Thirdly, in both programme and practice they sought close cooperation with systematic neighbouring disciplines, especially sociology, political science, and economics. Fourthly, they tended to emphasize the economic dimension as a subject of study and as an explanatory category, albeit in very different forms. And fifth: At least in West Germany in the 1960s and 1970s, all this developed in a politico-intellectual atmosphere fraught with criticism of tradition and imbued with hopes for reform, both with respect to academic practices and to society at large, frequently with more or less leftwing orientation, but nevertheless in very different ways (Iggers 1984; Raphael 2003).[1]

1 In 2008, the British historian Eric Hobsbawm (*1917) noted: »I had the luck of belonging to a worldwide generation of historians who revolutionised historiography between the thirties and the historiographical turn in the seventies of the last century, mainly through new links between history and the social sciences. It was not simply a matter of a single ideological school. It was about the struggle of historical modernity against the old, conventional historiography of Ranke, whether under the banner of economic history, French sociology and geography as in the Annales, of Marxism or of Max Weber« (Botz et al. 2008: 74).

On the whole this turned out to be to the advantage of economic history. This subdiscipline of history (cf. Schulz et al. 2004), with a strong tradition dating back to the late 19th century, attracted increasing interest – often together with social history – from students and the public, arousing great expectations that it could enhance the explanatory power of history as a whole as well as the historical interpretation of the present.

Moving apart

In the decades that followed, some of the high-flying hopes were met, the programme of ›historical social science‹ made an impression, but on the whole it remained a minority phenomenon, and since the late 1970s and 1980s the relationship between history and the social sciences changed, becoming more distant again. The following sketch is extremely abbreviated and informed by a German perspective.[2]

Over the last decades, a wide variety of developments have taken place in the field of history, including a powerful trend away from social history to cultural history. In the 1980s, the proponents of the history of everyday life (*Alltagsgeschichte*) criticized the structural bias of earlier economic and social history. They called for greater attention to be paid to actions, perceptions, and experiences – the subjective dimension of history. Interest was soon to grow in the reconstruction of symbolic forms and the interpretation of cultural practices. Whereas the focus had often been on broad structures and processes, the charm of micro-historical approaches was now discovered. This was sometimes accompanied by sweeping mistrust of big concepts and analytic approaches. ›Why‹ questions were up-staged by ›how‹ questions. New emphasis was placed on narrativity. Language became more and more important, both as a subject of research and as a reflective medium of research and presentation. The history of concepts (*Begriffsgeschichte*) served as a bridge between social and cultural history, increasingly in a constructivist spirit with

2 For accounts of the British, French and US-American experience see Sewell 2005: 22-80; Eley 2005.

much sense for the formative power of ideas, concepts, and categories both in the past itself and in the act of investigating it. All this did not mean that the preceding paradigm was simply displaced. Rather, conflicts were numerous and new combinations were forged. Whereas Karl Marx, Max Weber, Émile Durkheim, Talcott Parsons, and Jürgen Habermas had in the past lent social scientific force to historical studies, they were now often succeeded by Clifford Geertz and Georg Simmel, Michael Foucault, Jacques Derrida, and other anthropologists and post-modern thinkers. But frequently, theoretical orientations were now dispensed with altogether. In all, the dominant reasons for studying history had shifted. The main concern had once been to learn from history. Now, history became interesting as a basis of gaining identity or as a way of dealing with the other (Iggers 2005; Iggers & Wang 2008: 270-316; Conrad & Kessel 1994; Kocka 2003; Kocka 2006).

Certainly, economics as a discipline have not been historicized over the last decades. Something like an ›action and micro-theoretical turn‹ may have taken place within the field. But it focussed attention on the achievements and claims of an a-historical theory of humanity. Economics has continued to be strong in formalized models. It attributes its theoretical productivity to the abstraction from cultural factors and historical contexts and operates with a timeless concept of man. It is thus in stark contradiction to the historical and cultural sciences, which see human ›nature‹ not as an anthropological constant but as the outcome of historical processes. From the perspective of a historian, the economists' a-historical ways of looking on human reality are extremely under-complex and simplistic – in spite of its sophisticated theoretical apparatus which is difficult to understand from the outside.

This holds true despite theoretical discussions and developments in the discipline which have challenged and changed the self-interest maxim of traditional, main-stream economics, redefined the concept of rationality, and moved away from the traditional homo oeconomicus model so frequently criticized.

Economics, to a large part, are a highly professionalized and self-satisfied discipline which mostly cooperates only reluctantly with history and other social sciences. It believes it does not need their help. On the contrary, it understands itself increasingly as a general science of action not restricted to the market sphere. With its own premises and issues, it has embarked on the investigation of areas – such as the family, fashion, or politics – traditionally the domain of other social sciences and history (Hodgson 2001; Tanner 2004; Biervert & Wieland 1990).

What can be said in a few words about political science? Some of its practitioners are interested in broadly based comparative research with an historical depth of field, e. g. Theda Skocpol, Peter Hall, or Kathleen Thelen. The Committee on History and Political Science established by the American Political Science Association (APSA) in 1990 soon had several hundred members. Influential German political scientists, too, exercise their interest in historical approaches, for example Klaus von Beyme and Manfred Schmidt. On the other hand, Peter Hall has recently criticized the growing de-historicization of American political science, describing its increasing preoccupation with the rational choice paradigm, which is more interested in the effects of preferences than in their origins, changes, and volatilities.

> Over the past twenty-five years, social science has changed dramatically. The most striking development, especially in America, has been a bifurcation, separating scholars interested in culture from those concerned with material forces. On one side of the yard, history and anthropology have moved closer to cultural studies. On the other, political science has edged toward economics. Like the kid left to play alone, American sociology has flirted with the others without being able to draw them into a game of its own (Hall 2007: 127).

As far as developments in sociology are concerned, it is even more difficult to generalize. Without a doubt, many sociologists have contributed greatly to developing the program of a historical social science in Germany, among them Max Weber and C. Wright Mills, Ralf Dahrendorf and Charles Tilly, M. Rainer Lepsius and Wolfgang Schluchter, to mention only a few. Historians have continued to benefit greatly from the

work of sociologists who take an interest in history or are at least open to history, from comparative historical sociology in the social sciences (e.g. Shmuel Eisenstadt, Björn Wittrock, Dietrich Rueschemeier) to impressive contributions by historical sociologists (e.g. Michael Mann) and influential theoreticians like Pierre Bourdieu and Anthony Giddens, who are also frequently quoted by social historians. Recently, the sociologist Wolfgang Streeck published a study on capitalism in Germany in which he advocated and practiced a close combination of the social sciences and history (Streeck 2009). On the other hand, in Germany the places where historians and sociologists work together in more than sporadic fashion are few and far between, far more so than thirty to forty years ago. Without a doubt, there has been no general historicization of sociology. Historians and social scientists, not least sociologists, continue to differ greatly in their interests, languages, footnote methods and forms of presentation. The vision of a ›historical social science‹ that merges elements of the participating disciplines has not been realised when it comes to history and sociology, either (Adams et al. 2005; Ellrich 2000; Mahoney & Rueschemeyer 2003; Wehler 2000; Welskopp 2005).

There are exceptions to the trends discussed. There are new alliances that have replaced old ones, for instance between social and cultural history on the one hand, and cultural anthropology and ethnology on the other. There is a great deal of interdisciplinary cooperation in individual problem-oriented fields such as research on violence, ageing, migration, integration, and conflicts. These fields are by themselves extremely diverse. But as far as the relationship between history and economics, political science, and sociology is concerned, the boundaries have not become more permeable over the past three to four decades. On the contrary, the disciplines have remained much more self-contained than the proponents of a ›historical social science‹ had envisaged in the 1970s.

Economic history remains a lively, internally diverse and productive field. But on the whole it has lost ground in recent decades, in Germany and other countries. Its decline is evident in the shrinking number of positions and in shrinking attention for results of economic history research outside the subdiscipline itself. The shifts within history I have

outlined above have reduced faith in the explanatory power of economic factors and have lowered the expectations with respect to economic history among general historians and their audiences. Presently, social historians far more frequently lean towards cultural history than towards economic history as they had in the past (Conrad 2001). For their part, economists continue to take an only moderate interest in economic history.

New opportunities and approaches at cooperation

However – now I come to the more optimistic part of this paper – new opportunities and new approaches at cooperation between economic history and economics, between history and the social sciences have emerged in recent times. Let me first deal with two changes within the field of economics before turning to recent developments in the study of history.

First, a highly interesting discussion has been taking place between economists and science theoreticians on the foundations of economics, which has come to my attention primarily through economic historians such as Hansjörg Siegenthaler and Jakob Tanner (Siegenthaler 1999; Tanner 2004). The advance of game theory has informed a few economists who have abandoned the notion of the individual as a utility-maximising monad to concern themselves with interactional relations and decision-making procedures; and hence, in principle at least, with the changing world in which interaction takes place and decisions are made. This movement goes beyond the methodological individualism that has marked traditional economics. Along similar lines, there is the discussion on ›bounded rationality‹, which by its more radical manifestations is well on the way to denying the construct of the utility-optimizing individual (Gigerenzer & Selten 2001; Siegenthaler 2005). Insight into the often very limited ability of individuals to weigh up alternatives in fully informed fashion and to choose rationally between them and their opportunity costs has directed attention to the important role of ›stop rules‹ and decision shortcuts, which in turn have to do with habits, shared conventions, mental models, and with processes of understanding and learning. These again are in varying measure path depend-

ent and have a history. Neurobiological research appears to confirm this. In principle and in the intra-disciplinary cogitations of certain economists – at least among a small, reflective minority in their field – this would seem to clear a broad path to history, to the cultural sciences, and to a reflective economic history, in principle.

There is a second development in economics that commends cooperation with economic history: the persistence and further development of institutional economics. When Douglass North and others lent it new impetus around 1970, especially in addressing the property rights paradigm, Knut Borchardt explicitly pointed out how much this had been anticipated by scholars such as Gustav Schmoller and Werner Sombart from the German Historical School of Economics in the late 19th and early 20th centuries (Borchardt 1977). New Institutional Economics address the historical setting of economic processes. It asks about the rules and norms of markets. Who draws them up and monitors them? What does it cost to sanction breaches of the rules? When and why do the institutional arrangements of a society change? What are the consequences of, for example, a shift from collective to individual rights of disposal? A broad concept of institution is commonly used, covering all sorts of regulatory systems from law to conventions, standards and customs (North 1990; Richter & Furubotn 1996).

This opens the door wide to cooperation with historians who – like Werner Abelshauser and Volker Berghahn – discuss, together with other social scientists, the performance and limits of the German (»Rhenish«) model of capitalism with its high degree of organized coordination, in comparison to other more market-based varieties of capitalism in Britain and the US. Business history, dealing, for example, with transaction costs or entrepreneurial networks, also addresses issues of institutional economics (Abelshauser 1999 and 2005; Berghahn & Vitols 2006; Berghoff & Sydow 2007; Berghoff & Vogel 2004a; Hall & Soskice 2001; Thelen 2004; Streeck & Thelen 2005).

Economic sociology broadens and extends the issues treated by institutional economics beyond the institutional. Jens Beckert and Richard Swedberg, two major authors in this field, point out that the role of

social, cultural, and political conditions for the operation of economic exchange systems is a classical sociological issue, long relegated to the background after 1945 before reviving since the 1980s. They speculate about the reasons for the renewed interest in economic sociology:

> The changes from Fordist regulation to more flexible types of organizational structures, the transformation of Eastern European economies, and the process of globalization make the economy appear to be in a state of dramatic change with the final outcome, the implications and sometimes even the directions as yet unclear. These economic changes will have profound effects on society at large. They will change the role of the state, will change non-economic variables like social capital into important economic resources, and they will affect the family through radical changes in types of employment. But on the basis of which theory can these changes be understood? (Beckert & Swedberg 2001: 381 f.)[3]

Their answer is to point to the need for a new link between economics and sociology. It should be added that this situation offers an opportunity to connect with historical and economic history research, as long as the economic historians involved do not adopt too narrow a perspective but argue on a broad front, addressing questions such as trust, religion, family structures, networks, and the state (Beckert 2002, 2007; Beckert et al. 2007; Smelser & Swedberg 2005).

So much about some of the changes outside economic history and outside history that open up new paths to cooperation. But changes have also been taking place in history that are worth looking at. Again, I will concentrate on two complex developments.

(1) The culturalistic turn in history has frequently led historians to neglect economic history and economic issues. But the cultural history turn can also open up new types of access to economic history which could be of interest for economists and other social scientists. This has been

3 Karl Polanyi's classical study on the embeddedness of capitalist economies receives new attention, see Polanyi 1944 and 1957/2001.

cogently demonstrated in »Economic History as Cultural History«, edited by Hartmut Berghoff und Jakob Vogel (Berghoff & Vogel 2004b). I cite some examples from this volume and refer to further studies.

Some scholars like Adam Tooze and Robert Salais, convinced of the formative power of language, address the history of concepts and examine the categories used by social scientists and statisticians for mapping societies of the past, such as ›workers‹ and ›employees‹, ›labor‹ and ›unemployment‹. They not only try to find out which social realities were reflected by the emergence and diffusion of such concepts. They also explore how such frequently used concepts helped to structure and shape societies of the past: the semantic mapping of social reality as a contribution to forming social identities, groups, and classes (Conze 1972; Tooze 2001, 2004; Salais 1986).

There are studies on the history of labor (or work) and on how these concepts were differently defined between countries and languages, in theoretical treatises as well as in the language of collective bargaining or social policy. This way they investigate work experiences and labor relations in the interaction between tradition, markets, and government intervention (Biernacki 1995; Zimmermann 2001).

There is the booming history of consumption, concerned, among other things, with the interplay between cultural orientation, gender, and market behavior, extending into the history of commercial and service companies, which, at least in Germany, is traditionally less well researched than the history of manufacturing enterprises (Haupt & Torp 2009).

There is the micro-historical study on a North Italian village in the late 17th century by Giovanni Levi. Using sophisticated methods, this economic and cultural historian shows how very much transactions in this village, decisions on buying and selling, were embedded in a supra-individual network of relationships in which honour, reciprocity, and self-interest were linked over the times and were thus part of a ›culture‹. According to Christoph Conrad,

> the crux of Levi's reconstruction is that he exposes the atomisation of individual transactions and actors as an illusion. Even so banal an act as

> buying an animal or a small garden plot can be explained only by the network of social, familial, and symbolic determinants. The micro-analysis thus reconstructs a covert collective reality concealed by the atomisation of civil law categories and thus of sources. Levi impressively shows how dependent the individual economic actor was on social practice before he became the focus of liberal economic theory (Conrad 2004: 55; Levi 1989).

Finally I mention the research done by a group under Pierre Bourdieu on home ownership among skilled worker and white collar worker families in the Paris banlieue. The study investigates the »social structures of the economy« (Bourdieu 2005). In particular, it looks at »how preferences arise and are disseminated in a society that render such notions as ›sense of property‹, ›leafy suburb‹, and ›being my own boss‹, understandable […]. Government capital formation programs are examined, as are advertising images and leitmotifs, individual sales talks and loan negotiations.« Christoph Conrad concludes: »In this context of a cultural history of the economy, the point is not to trace societal conditioning in preference formation – every economist would admit that – but to understand the economic actor model itself as the outcome of societal and cultural preparation« (Conrad 2004: 59; see also Nolte 1997; Haskell & Teichgraeber 1993).

These few examples should suffice to demonstrate that the culturalistic turn in history over the past two decades offers not only risks but also opportunities for renewing economic history. However, only if two conditions are met: First, discourse and conceptual history must be linked to the history of practices, which is difficult to achieve. Secondly, economic history benefits only if it adopts a broad understanding of its tasks and does not restrict itself to economic matters in the narrower sense, for instance the issue of efficiency. Then it can produce results that should also be interesting for economists and social scientists if, for example, they are interested in the embeddedness, emergence, and preconditions of markets.

(2) Bernard Bailyn recently identified as »one of the deepest tendencies of late-twentieth-century historiography: the impulse to expand the range

of inquiry, to rescale major events and trends into larger settings, and to seek heightened understanding at a more elevated and generalized plane. In every sphere of historical study – intellectual, cultural, political – the scope of inquiry has broadened. Large-scale comparisons and parallels are explored, national stories become regional, and regional studies become global« (Bailyn 2009: 44).

Indeed, transnational, interregional and global approaches are quickly gaining ground and make it presently the single most important trend in the discipline. With a certain necessity, this trend reinvigorates some basic principles of »historical social science«: attention for large-scale structures and comprehensive processes, the sharp definition of concepts and analytical rigor, explicit reflections on the choice of concepts, on decisions about space and time of investigation and on epistemological implications. The relation between comparative history and entangled history is intensively debated. Eurocentrism and Western biases are to be overcome in a productive way. All this also leads to a renewal of theoretical considerations within the practice of history. It may lead historians to a new openness vis-à-vis social science approaches (Haupt & Kocka 2009; Osterhammel 2009; Conrad et al. 2007).

All this holds particularly true with respect to economic and social historical studies in the expanding field of global history. Just a few examples: The debate on the ›Great Divergence‹ between economic developments in (parts of) China and (parts of) Britain in the 17th, 18th and 19th centuries is a good case in point. New studies on global labor history profit from social science concepts and models (capitalism, class formation), if only with the goal of modifying them with respect to the Non-Western world. Anyway, historians' interest in the world historical phenomenon of capitalism seems to grow again, including interest in classical theorists from Adam Smith and Karl Marx to Joseph Schumpeter and Karl Polanyi. The present financial and economic crisis strengthens public and academic concerns for capitalism and for analytical approaches which may guide its historical investigation (Appleby 2010; O'Brien 2006; Kocka 2010; Pomeranz 2000; Van der Linden 2008; Vries 2003).

Outlook: What history has to offer to the social sciences

No doubt, there are many historians who do their work without drawing from the resources of the social sciences. On the other hand, most economists as well as many sociologists and political scientists define their topics and investigate them without any historical orientation. But there is an area of cooperation and overlap in which historians and social scientists meet in ways useful for both sides. This area is growing again.

On the one hand historians can use pertinent concepts, models and theories from the social sciences in order to specify their questions and define their subject of study, develop explanatory hypotheses and structure their ›narratives‹ (or better: argumentations). Sometimes they find methods useful which have been developed in the social sciences, e. g. when handling mass data. Dialogues with social scientists may help them to reflect upon the conditions, particularities and consequences of their procedures. Usually they make very selective use of the reservoir of social science ideas, concepts and methods, and they incorporate them into argumentations of their own. The more historians are ready again to deal with the conditions and consequences of events, experiences, discourses and actions, i. e. with structures and processes, the more urgent it becomes for them to utilize social science resources for historical investigations. This holds particularly true for economic history (Kocka 1977, 1986: 83-89; Meran 1985; Tilly 1982).

On the other hand, in this article several examples were given to show how research in economics and other social sciences might profit from historical approaches and insights. On a more general level, I want to underline two contributions which history can make to social science research and its presentation.[4]

First, historians take contexts serious. They insist on the reconstruction of contexts and are sceptical vis-à-vis the rapid isolation and selective correlation of variables. Historians can offer help as to contextualization. They can demonstrate how economic, social, political, and cultural di-

4 With a similar thrust Sewell 2005.

mensions play together. They are specialists for embeddedness, arguably at least as much as the sociologists who have established embeddedness as a concept. This is where Robert Solow saw the most important service economic historians can offer to economic theorists:

> Few things should be more interesting to a civilized economic theorist than the opportunity to observe the interplay between social institutions and economic behaviour over time and place […]. Therefore an economic historian should be an observer and re-creator of the codes, loyalties and organizations which men create and which are just as real to them as physical conditions'. Add to that a command over two-stage least squares and you have the kind of economic historian from whom theorists have most to learn, if only they are willing to try (Solow 1985/ 2006, 241 f.).

Second, historians are interested in change over time. They tend to argue in terms of ›before‹ and ›after‹ (stressing simultaneity is another aspect of the same temporal logic). They know that new things emerge, but that they are influenced by preceding constellations. They are aware that observable structures of the present are going to change and will be different in the future. It is this temporal pattern of understanding human reality as a process which strongly influences the descriptions, explanations and interpretations of historians as much as they may differ from one another in other respects. It can also enhance the analytical power and the rhetorical effects of social scientists if they adopt such perspectives for parts of their argumentation. This would mean to analyze social systems as social processes. It would mean to perceive social phenomena of the present time as products of preceding constellations, processes and actions (in addition to analyzing them according to the rules of empirical social science). It would also mean not to expect that the future will be a mere prolongation of the present, but something different, although influenced by the present, and although the limits of variability can be ascertained as well. Following such a temporal perspective, historians sometimes concentrate on the emergence of problems, on attempts at problem solution and on non-intended consequences of such attempts. The dimension of time, the relation between past, present and

future, is central and defines the way in which reality is perceived. What historians can offer to social scientists are ways of temporalizing the social realities under investigation.[5]

This is an argument in favour of transfers across disciplinary lines, but not in favour of levelling the differences between disciplines. Historians can offer such impulses to economists and other social scientists only as long as they do not fully yield to the methodological rules and customs of their partners. To quote Robert Solow again:

> As I inspect current work in economic history, I have the sinking feeling that a lot of it looks exactly like the kind of economic analysis I have just finished caricaturing: the same integrals, the same regressions, the same substitution of *t*-ratios for thought. Apart from anything else it is no fun reading the stuff anymore. Far from offering the economic theorist a wide range of perceptions, this sort of economic history gives back to the theorist the same routine gruel that the economic theorist gives to the historian. Why should I believe, when it is applied to thin eighteenth-century data, something that carries no conviction when it is done with more ample twentieth-century data? (Solow 1985/2006: 243)

This may be putting it a bit too strongly, but basically I find it convincing. The point is that economic history is important for economists and social scientists not only – and not primarily – when it adopts their approaches and applies them to past phenomena, but when it is self-assured enough to stick to basic principles of the historical discipline. Interdisciplinary cooperation presupposes disciplinary differentiation.

5 With a similar thrust Streeck 2009. The underlying perception of history as a discipline is sketched more thoroughly in Kocka 2008.

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Moving Inter Disciplines

What kind of cooperation are interdisciplinary historians and sociologists aiming for?

Klaus Nathaus and Hendrik Vollmer

When in 2009 the first Annual Seminar of the Bielefeld Graduate School in History and Sociology looked at the state of the relationship between the two disciplines with the question whether or not there was an ›end of messages‹, the committee of organizers had in mind a particular episode of interdisciplinarity: the establishment of social science history in Germany, promoted by historians at the newly founded Bielefeld University. From this point of view, the 1970s appeared to be a time of lively interdisciplinary exchange, with historians borrowing theories and methods from the social sciences and sociologists becoming interested in the empirical data that historians generated. Asking provocatively whether the disciplines have stopped sending messages to each other, the convenors of the doctoral conference implied that the new graduate school could, should or inevitably has to define itself in reference to a heritage of interdisciplinarity at Bielefeld University.

A year later, our picture has become far more complex. More discussions, further reading, establishing contact with historical sociologists and social-scientifically-minded historians abroad and not least with the experience of interdisciplinary encounters at the graduate school have widened our perspective in many respects. The following article is an attempt to present the resulting ideas about interdisciplinarity in a systematic way. It tries to identify conditions and factors that facilitate an orientation towards neighboring scientific disciplines, differentiates levels of interdisciplinarity and assesses the potential of research that is ›inter disciplines‹. Our aim is to formulate questions that we would like to see

taken up by others who want to contribute to an advanced understanding of interdisciplinarity or locate themselves between disciplines.

The text is the outcome of the collaboration between a sociologist and a historian who found a common language by borrowing concepts from Pierre Boudieu's social theory. Drawing from Bourdieu seems all the more appropriate as his work has been an attempt to overcome the »sterile divisions« and »false quarrels« that he saw as the result of the compartmentalization of academic work in the social sciences (Bourdieu 1988: 779). In keeping with a programmatic concern with reflexivity – which we will introduce during the course of this paper to be one critical condition for perpetuating interdisciplinary communication – this is also an opportunity for discussing the usefulness of theoretical concepts in describing the nascent field. Moreover, we will give special consideration to the notion of fields, a theoretical category of potentially high significance for socio-historical research within our projected long-term assembly.

The text consists of two main parts that sketch two different scenarios of interdisciplinarity. We refer to the first mode as ›oppositional interdisciplinarity‹, which can be understood as an episode in a dialectic process of disciplinary development. In this scenario, the relationship between scientists from different academic realms serves them as a resource to challenge dominant positions within their disciplines and with the potential to ultimately transform them. At the end of this process, the interdisciplinary challenge becomes incorporated into a reconfigured discipline. We will then discuss a second mode of interdisciplinarity, which we have named ›autonomous interdisciplinarity‹. This mode involves a more sustained and substantial dialogue by researchers from different academic fields using a shared set of questions and methods. Autonomous interdisciplinarity outgrows existing disciplines. It achieves a high degree of independence from neighboring scientific fields by establishing its own incentives, field-specific capital and institutions. It implies the emergence of an interdisciplinary field sui generis. Outlining the two modes of interdisciplinarity, we will discuss the requirements, structures

and perspectives for future research at the interface of, or in a realm beyond, history and sociology.

We think that a debate about the level of interdisciplinarity that history and sociology should strive to establish is needed to realize the full potential of research ›inter disciplines‹. Superficial references to another discipline are not enough even to strike up oppositional interdisciplinarity, let alone to assemble an autonomous interdisciplinary field. While interdisciplinarity may once have sounded like heresy to disciples of disciplines, there is now a universal consensus that interdisciplinary research is a »good thing«, an acceptance that is surely encouraged by the fact that interdisciplinarity is funded. We think that interdisciplinarity has to go further than this and reach a state of mutual irritation. Any interdisciplinary dialogue, whether it brings about the reconfiguration of a discipline or the establishment of an autonomous field, requires substantial issues to engage with. To try and differentiate the »false quarrels« from the right ones, to take Bourdieu's lead, seems necessary to establish a sustained interest in interdisciplinary communication. We also suspect that once historians and sociologists start to discuss the level of cooperation they are willing to achieve and maintain they will find out that interdisciplinarity is more controversial than currently realized.

Oppositional interdisciplinarity as an episode in disciplinary change

Before we can talk about interdisciplinarity, we have to begin with a few remarks on the character of scientific disciplines and how we would like to consider them as particular social fields. Fields are distributions of participants and resources across positions (cf. Bourdieu 1993; Martin 2003). These distributions emerge from participants relating to one another through repeated interaction in which they strategically invest various resources. Participants' positions are the outcome of such reiterated interrelating, and position-takings are a shorthand for a broad set of participants' individual efforts and strategies at claiming and defending positions within the field (Bourdieu 1993: 30; Emirbayer & Johnson 2008: 14-17). Various resources and forms of capital are utilized in po-

sition-takings, with the respective value of resources defined by the structure of distributions within a field, and the embedding of the field as a whole in other fields. Which kind of cultural capital, for example, turns out to be valuable within a given field depends on participants' habitus, i.e. their disposition to recognize and evaluate ›proper‹ cultural competence (cf. Bourdieu 1984). The standing and autonomy of any given field with respect to other fields may best be estimated by the relative value of that kind of capital that can only be generated and acquired by participants within the field (cf. Anheier et al. 1995). Such field-specific currencies tend to be referred to by the notion of ›symbolic capital‹ (Bourdieu 1991b: 66-76; Emirbayer & Johnson 2008: 25f.). Scientific capital is symbolic capital mobilized in terms of scientific knowledge, reputation, publications, contributions to or recognitions within scientific discourse (cf. Bourdieu 2004: 33f., 55-62). This resource can only be gained by engaging in position-takings within scientific fields, and it cannot be generated on the spot by deploying other kinds of resources, for example economic capital. Participants can gain scientific capital by investing money, but they have to spend it on actual research, publications, and so on, and wait for these investments to turn out one way or the other, rather than by simply buying knowledge, reputation, or truth (cf. Bourdieu 1975; 1991a). The standing and autonomy or, if you will, the power of the field, is determined by its ability to define and monopolize access to its genuine currency, the symbolic capital generated within it.

The core property of disciplines therefore is their autonomy, an autonomy that has initially to be won and subsequently be defended within larger scientific fields (Cambrosio & Keating 1983: 327f.). Academic fields have their own reward system in which peers – not state regulators, corporate sponsors, philanthropists or rating agencies – define scientific standards and evaluate objects of study. Scientists themselves distinguish between ›good‹ and ›bad‹ science, and they award reputation as the field-specific capital accordingly (cf. Crane 1976). While autonomy is undoubtedly a requirement for science, it is also a structural factor that tends to work against change. In a field in which capital is distributed

unequally, there is a strong tendency for participants to be drawn towards the most reputable peers, as these have the power to consecrate. This in turn strengthens their position and allows them to set the agenda of the respective field. The eminence of the most reputable peers attracts not only followers, but also challengers who, by addressing the same phenomena, problems and questions that the dominant actors define, reinforce the structure of the field (cf. Bourdieu 2004: 11-13).

This means that autonomy does not only save scientists from non-scientific impositions, but that it also enables isomorphism, a convergence towards the centre of gravity of the disciplinary field (DiMaggio & Powell 1983). The practical consequence is that new research questions are primarily generated from disciplinary discussions and get ever more intricate, self-referential and less irritated by phenomena that most people outside the discipline would see as ›important‹. At that stage, scientists invest mental effort by complicating existing positions in order to carve out niches in a crowded space. An overproduction of highly particular studies in turn facilitates mutual ignorance. To outsiders the disciplinary field, the relevance of this research appears questionable and the interest in its findings is limited, as internal differentiation gets harder to communicate as ›groundbreaking‹ to wider audiences.

When a discipline has reached such a phase of stagnation, turning towards other disciplines might open the field for new theories, methods and objects of study. Challengers may draw upon interdisciplinarity to formulate new research programs that can diverge from the disciplinary mainstream to some extent. They can ›discover‹ subject matters that have been neglected by their colleagues in the discipline but are already studied in another field; they can adapt methods that are tried and tested in neighboring disciplines to their own work. Crossing the boundaries between disciplines also raises the awareness for specific ways of thinking and disciplinary reflexes, as »contacts between sciences, like contacts between civilizations, are occasions when implicit dispositions have to be made explicit« (Bourdieu 2004: 42). The dialogue with scientists from other disciplines forces scientists to explain what is specific about their ›domestic‹ field (Kocka 1991).

But these are already some of the possible and positive effects of interdisciplinary work on individual scientists. What is more important here is the question why participants of autonomous fields enter interdisciplinary exchange in the first place, because it requires a considerable investment of time and work while it is far from clear whether there is any return in disciplinary reputation. Three conditions are to be mentioned: Firstly, the importance of intrinsic motivation deriving from individual habitus as a result of biographical trajectories should not be underestimated, as it never entirely surrenders to and can rarely be subsumed under the collective habitus of a discipline (cf. Bourdieu 2004: 42-44). For some scientists, some topics or questions are more engaging than others, and if the knowledge about a certain phenomenon is more advanced in another discipline, one might engage with that literature for quite some time. Secondly, opportunity structures are required. Scientists need money and time to be able to venture into other fields; they need institutions such as publications or conferences to have an exchange with like-minded scientists and communicate their research to peers outside the interdisciplinary circle. Thirdly and most importantly, the symbolic capital of a neighboring field has to be accepted in a domestic discipline. This points to the fact that disciplines, notwithstanding their autonomy, compete with each other for public attention and recognition of potential funding bodies, most importantly the state. The majority of scientists of one discipline are not familiar with current developments in other academic fields, but they do have a general idea about which disciplines are expanding, as they see new university departments being created, research institutes set up and certain scientific expertise circulating in the wider public. The association with this success can help interdisciplinarians to raise awareness among their fellows and convince them to take their interdisciplinary proposition seriously, even though they may have difficulties explaining to their more disciplinary-minded colleagues the merits of concepts and methods which are alien to the discipline. This makes fields that receive a lot of recognition among other scientists as well as the general public the best candidates for a successful interdisciplinary cooperation.

If intrinsic motivation, opportunity structures and convertible capital are given, scientists can engage in interdisciplinary exchange. It then depends on the responsiveness of the respective field whether or not the import of theories, methods and topics has an effect on this discipline. It would appear that this has got much to do with the timing of field saturation and interdisciplinary engagement. An interdisciplinary impulse for disciplinary change is more likely if a field is declining; such a challenge might be futile when a discipline is at a stage where there are still many possibilities within the field. If this is true, there is a dialectical relationship between disciplinary reproduction and interdisciplinary challenge: Stagnant disciplines fall in public recognition and produce discontents who then draw on the symbolic capital from other academic fields to challenge the status quo and ultimately reconfigure the field.

To illustrate these points we will sketch the development of social history from its ascent in the 1960s and 1970s to its stagnation around 1980, when it became the main target of oppositional interdisciplinarity itself. According to historians who played a prominent part in this episode (cf. Kocka 1996; Eley 2005; Sewell 2005), social history owed its success partly to the revisionist climate of the 1960s in which its proponents came of age. With historiography still focussing on the political history of the nation state and ›great men‹, social scientific literature in the broader sense – most notably the writings of Marx – nurtured an intrinsic motivation in understanding history, an interest that tedious course work in history seminars had rather dampened, as Geoff Eley remembers from his undergraduate days at Oxford University in the late 1960s (Eley 2005: 1-12). Works from the social sciences made aware and sometimes took the side of marginal groups – such as workers, criminals, women, slaves – and studied aspects of life – for example, work, deviance, popular protest and the economy – that the discipline had neglected or treated as less significant in the general course of history. On top of that, the claim that economic structures and class conflict shaped societies and drove change provided a theory to explain historical processes and understand not just the finer nuances, but the ›bigger picture‹.

At this stage, social history aspired to become a history of society (Hobsbawm 1971).

While the intellectual climate of the 1960s certainly fed the enthusiasm for social theory and an interest in the experiences of the marginalized, a strong intrinsic motivation to study the respective subject matters would not have been sufficient to make social history a highly influential scientific movement. A second condition was the symbolic capital that economics, political science and sociology had to offer to historians. Sociology in particular was growing in the 1960s and 1970s, in institutional terms as well as in public and scientific recognition (Lepsius 2008: 83ff.). From the perspective of historiography, the social sciences seemed to occupy a higher plane of scientific sophistication, theoretical rigor and methodological exactness. The important issue in this context is not whether these claims were justified, but that the superiority of social scientific approaches was widely believed, within and outside the respective fields. Historians who have found themselves at the margins could employ the esteem of the ascending social sciences to challenge the establishment of their own discipline, exposing the shortcomings of existing research and drawing up a new agenda with strong references to the interdisciplinary partner. At this level, interdisciplinarity offered resources for challengers within historiography to form a powerful opposition, independent of how intense their dialogue with social scientists really was.[1]

The third precondition for the rise of social history besides intrinsic motivation and symbolic capital were opportunity structures that facilitated the risk-taking behavior of aspirational newcomers. The expansion of higher education meant that a number of younger historians, among them the interdisciplinary challengers, came into positions that were relatively secure and well funded. These positions gave the proponents of social or social science history time, money and visibility to publish programmatic texts, launch journals, organize conferences and teach gradu-

1 Thomas Welskopp (in this volume) assesses that the intensity of interdisciplinarity in German social science history was rather limited.

ate students, turning opportunities and resources into lasting institutions. There were efforts to make the relationship between historians and social scientists more binding in joint research projects. The main thrust of social history's interdisciplinarity, however, was directed at the historical discipline itself. This strategy was successful, as the field ultimately incorporated its critics, adopted its research agenda and methods. According to William Sewell (2005: 29), social history »briefly became hegemonic in the field in the United States«, and Jürgen Kocka (2003: 26), who speaks more cautiously of interdisciplinarity as an undertaking of a heterogeneous minority of historians, nevertheless states that »(s)ocial history has successfully penetrated its opponents.« Subjects, methods, questions and not least a sharpened sense of theoretical self-reflection entered into the disciplinary discourse and reconfigured the field of historiography.

The example of social history illustrates what oppositional interdisciplinarity can achieve, but it also shows how it may turn into a conservative force that subsequently becomes the target of an interdisciplinary challenge itself. Critics pointed out a materialist or structural determinism within social history which paid little to no attention to the experience and agency of historic actors and which was unaware of the importance of culture as a social category in its own right. This criticism was brought forward by the proponents of *Alltagsgeschichte* (history of everyday life) and the ›cultural turn‹ who employed theories, topics and methods from anthropology and literary theory (Sewell 2005: 44-46). Again, we see a challenge that began at the periphery of the field – notably feminism (Tilly 2005: 21 f.) – and that was formulated as an interdisciplinary project, only this time as cooperation between disciplines locating themselves in the humanities. And as with social historians, who generally engaged with the social sciences only to a limited extent, the actual intensity of the interdisciplinary exchange between cultural historians and anthropologists seems to have been low, as history borrowed the concept of culture as a system of symbols and meanings from anthropology at a moment when the latter already became skeptical of the coherence of culture that this concept implies (Sewell 1999).

In the light of these past developments, what are the current prospects for oppositional interdisciplinarity between history and sociology (or the social sciences)? This is a question that we would like to see debated. To start off the discussion, we will present a few arguments that are partial and subjective but may serve our current purpose and stimulate response.

Looking at historiography, there are signs that the discipline has reached a point at which the preoccupation with culture that characterizes the central theoretical discourse of the discipline is ripe for challenge. ›Turns‹ are announced in quick succession, with methodological and theoretical issues becoming ever more particular and harder to communicate. At the same time, even eminent proponents of the cultural turn argue that »big structures« and »large processes« have to be brought back onto the agenda, as they are obviously important and their negligence would render historiography irrelevant in current debates outside the discipline (Sewell 2005: 77; Eley 2005: 198). This would facilitate a renewed interdisciplinarity between historians and social scientists, a refocusing on ›the social‹, which should not be mistaken for the social history of the 1970s. Furthermore, historical research that orients itself to sociology, political science and economics has actually been continued, even though somewhat removed from the mainstream of the discipline. This line of study may serve as a ›tradition‹ that current interdisciplinarians can build on. Finally, there are also strands in sociology such as the New Institutionalism (Powell & DiMaggio 1991), the New Economic Sociology (Smelser & Swedberg 2005) and Bourdieu's relational sociology (Emirbayer & Johnson 2008) that in their sensitivity to contexts and their interest in empirical studies are compatible with what historians do and indeed have signalled interest in interdisciplinary collaboration (Beckert 2010).

However, there are also obstacles. One would have to mention limited opportunity structures that result from the precarious situation of younger scientists and their dependence on older peers within the discipline. Individuals intent on having a career still have to make the appropriate disciplinary connections and acquaintances. Disciplinary networks provide a great deal of »social gravitation« (Black 1998: 126f.) in disciplinary

fields, arresting researchers' trajectories and opportunities of involvement, at the start of their careers and beyond. While this is the norm, these forces are felt stronger the scarcer resources are. Besides, the social sciences are not the rising disciplines that would appear to be the powerful partner for an interdisciplinary cooperation, and, from the perspective of sociology, neither is history. The rise of rational-choice sociology since the 1980s illustrates that many sociologists consider economics as a more attractive partner, a trend that has also been visible in historical sociology (Goldthorpe 1991; Kieser & Hechter 1998). With respect to both history and sociology, the situation is different to the 1960s and 1970s, when the disciplines appeared as congenial in their scientific and intellectual missions. Finally, there seems to be a lack of shared intrinsic motivation that carried former scientific movements to some degree. During the ascent of social history, it was Marxism, which had this popular appeal that motivated history students to analyze the anonymous processes that generated social inequality and conflict. At the height of cultural history, Foucault fulfilled the same function when his books nurtured an excitement about understanding the power of discourses (Foucault 1989; 2001; 2003). While much of the actual historical work carried out in reference to these two thinkers contained only extracts of their ideas, they were highly important as they rallied scientists in a camp and pitted them against the other, older and established faction. As it seems necessary to have figures like Marx and Foucault to create something like a radical consensus to fuel intrinsic motivation, the question is: What is the work that may have a similar inspiring effect today?

Towards autonomous interdisciplinarity?

Our sketch of oppositional interdisciplinarity underlines the stability of disciplinary boundaries. These boundaries do not only compartmentalize research, they are also flexible enough to bring interdisciplinarians back into the fold and involve them in reproducing the structure of the discipline. Nevertheless, interdisciplinary fields in social and historical science do exist. Science and technology studies (STS) are the prime example of this (Jasanoff et al. 2002; Hackett et al. 2007). Behavioral genetics, or genocide studies are further cases in point. At the interface of

history and the social sciences, historical social science, social science history or historical sociology are denominations for an interdisciplinary project with a variety of life cycles dating back to the early days of both academic disciplines. There is an agile community with several associations and networks and at least one regular journal with considerable reputation among both historians and sociologists on a global level (*Social Science History*). Each of the reincarnations and life cycles of historical social science appears to have produced a set of researchers importing some extradisciplinary interest in sociology or history respectively back into their disciplinary departments. Research ›inter‹ history and sociology has tended to be drawn back into disciplinary gravitation, and oppositional interdisciplinarity has remained the dominant mode. This, however, may change, if an interdisciplinary field between history and sociology becomes independent of the disciplines in the same sense that, for example, STS has over the last thirty years or so turned into a field in which researchers from different disciplinary backgrounds come together and stay for a large share if not the remainder of their academic career.

In assessing the possible emergence of autonomous interdisciplinarity ›inter‹ history and sociology, we will present a few systematic ideas and discuss the potential for interdisciplinary engagements coalescing into an independent field in three steps. We will subsequently discuss problems of attracting and committing participants, of arresting their trajectories, and of producing field-specific symbolic capital.

Attracting and committing participants

Any field is a structure of interrelating positions, and therefore, the first step towards establishing a field is to rally participants. Participants need to be brought into reiterated interaction with one another in order for positions to emerge which in turn attract position-takings (Emirbayer & Johnson 2008: 14-17). With respect to interdisciplinary cooperation, participants need to be drawn into social exchange, which are often challenging, and may often turn out to be tiresome and frustrating. We may initially distinguish various strategies of recruiting them with respect to

the kind of capital that is being employed in making interdisciplinary engagement attractive.

One – and perhaps the initially most salient – strategy of attracting historians and sociologists to interdisciplinary communication is, of course, by providing them with access to funds. The easiest way to kick-start a potential interdisciplinary field would be to create several professorships, about ten tenure-track positions in reputable academic institutions, for example, and make interdisciplinary communication an enforceable part of their job descriptions. Historically, something along these lines has happened en miniature in the foundation of the Department of History in Bielefeld in the 1970s. Other institutions followed suit once historical social science had become a household name. Even though the autonomy of academic institutions (not to mention the economic conditions which affect these institutions quite selectively) makes it illusory to coordinate the allocation and assignment of positions, it may still be possible to reinforce interdisciplinary communication locally and wait for the migration of participants to diffuse interdisciplinary issues and resources later on across larger networks of scholars. Young academics may be a particularly valuable investment that is relatively inexpensive to assign and maintain. David Edge's memory of arriving in Edinburgh in 1966 to found the Science Studies Unit and to be »shown my bare office: no phone, no books, no bibliographical resources, no files, no staff – indeed, it was tempting to think, *no subject*!« (cf. Edge 2002: 3; emphasis by the author), is a reminder that little resources committed over a longer period of time may sometimes be a more rewarding investment in interdisciplinary cooperation than spending large sums on spectacular conferences.

Establishing a journal such as *InterDisciplines* may be considered a strategy of attracting scientists by offering scientific capital in the currency of citable publications. At this moment, there is still comparatively little bureaucratization in both history and sociology in attributing merits for publications and translating formal merits into public funding. In a setting in which the informal reputation of a journal rather than a carefully calculated impact factor determines the value of the articles published in

it, an upcoming journal still has a chance of offering publication opportunities which are deemed valuable as such. The value of journal articles as a form of symbolic capital is determined by the standing of the journal within collectives in which valuation takes place. A new journal cannot create these collectives; it has to find them. Tracking them down involves uncertainties about whether respective collectives of authors and audiences even exist, and, if they exist, whether they are willing to accept and support a new project which may interfere with competing runs of publications that have already been scheduled (e.g. the Social Science History journal). More generally, we have come to wonder to what extent symbolic capital can be offered at all by opportunities to invest into interdisciplinary communication if that manifestation of symbolic capital that is critical for a historian or sociologist to make or continue her or his career is ultimately accumulated and distributed within the disciplines.

If a nascent interdisciplinary field – and, as we have briefly shown in the last section, the one in question has been nascent for half a century at least – is unable to provide incentives in the form of its own field-specific scientific capital, it needs to borrow scientific capital from the academic disciplines. Its agents may, for example, try to motivate some »scientific stars« and »top producers« (Collins 1998: 42-44) to engage in interdisciplinary communication, and hope to draw other participants in. Mixed strategies of providing both economic and scientific capital in attracting participants are perhaps the most promising ones at the current stage of collaboration among historians and sociologists. Mixed strategies of attraction allow a gradual transformation of economic into scientific capital: keynote speakers are paid to give talks and contribute papers, projects are funded to commit research activities, students are given grants to engage in interdisciplinary contexts, conferences are held to put issues on the map (and provide opportunities for travelling and visiting, meeting colleagues and friends, and so on).

Especially in employing mixed strategies, it may seem self-evident to involve disciplinary ›stars‹ in order to attract sufficient attention and justify the existence of an interdisciplinary initiative in the eyes of colleagues

within the disciplines: if ›stars‹ and ›top producers‹ engage in interdisciplinary communication, it has to be worthwhile. The problem is, of course, that ›stars‹ are stars by virtue of the stratification within their disciplines, and, just like ›top producers‹, they have a vested interest in defending their disciplinary capital, and this derives from the structure of their academic disciplines as they stand. Renegades from the established disciplines, on the other hand, may be more easily drawn in once they are presented with opportunities to compete with the high-status participants. One particular problem is that potential participants in interdisciplinary communication who are neither incumbents nor challengers in their disciplines but still have valuable input to offer to the interdisciplinary discourse may initially be all but invisible. Yet they may be the ones providing the critical mass for establishing an interdisciplinary field, and bring about processes of stratification, which do not merely duplicate the stratification of other fields.

Devaluating disciplinary resources, generating social capital

There is one grave problem with the funding of participants on a fixed-term basis: participants go back to where more permanent sources of income and opportunities are. How many of the graduate students at the BGHS (the institution supporting this journal) have actually committed themselves to interdisciplinary projects, and how many of these will extend their commitments beyond finishing their PhDs? The example of the Science Studies Unit in Edinburgh illustrates the value of longer-term institutional investments, but even this institution has lost its administrative independence at the University of Edinburgh in the new millennium. In the light of this, we had better take into account a world in which long-term investments are largely determined by the established structure of the academic disciplines, with interdisciplinary engagements largely confined to the peculiar life-cycle of ›projects‹ – organizational units which die by their own hand.

In a context such as this, participants' trajectories can only be arrested on a longer-term basis by other motivations than the quest for economic capital. As we acknowledged earlier, intrinsic motivation cannot be dis-

counted altogether, but the history of interdisciplinarity ›inter‹ history and sociology has shown us that it is a somewhat unreliable ally, as it is based on individual habitus resulting from idiosyncratic trajectories. If we assume that there is as yet no interdisciplinary capital genuine to our nascent field, then there is little that can be done to prevent the exploitation of interdisciplinary opportunities by participants with little long-term interest in interdisciplinary cooperation, and little structural leverage to counter the exploitation of interdisciplinary funding by the established disciplines. Short of interdisciplinary capital sui generis there is, though, at least one strategy of committing participants in the medium term, and this is the strategy of ›devaluating disciplinary capital‹ within the field, a strategy scientists rarely consider, and one which has, to our knowledge, never been systematically investigated in prior studies of interdisciplinarity. Far from attracting able sociologists and historians indiscriminately to the interdisciplinary field, devaluating disciplinary capital is a strategy economizing on the population of scientists drawn into interdisciplinary communication to start with, particularly by discouraging exploitive engagements. Participants who shy away from »How do you know?« questions, or those which are easily frustrated by repeatedly confronting skepticism towards their disciplinary wisdom may thus be kept away from congesting the interdisciplinary discourse. Those who are willing to engage in basic deliberations about the very substance matter of their disciplines and who are willing to lay bare (and often, to find out again for themselves) why and how they know what they know, may thus be reinforced by finding like-minded colleagues whom they will like to meet again.

Devaluation of disciplinary capital played an important part in the emergence of STS as a field in the late 1970s. The proclamation of the »strong programme« in the sociology of scientific knowledge (Bloor 1976) was, by the sociological standards of its time, a somewhat simplistic program of causally explaining scientific knowledge by reference to social structures and ›interests‹ (cf. Woolgar 1981). The program was broadened to include the explanation of technology (MacKenzie & Wajcman 1985), and, following a ›logic of outflanking‹ typical for the sociology of science

(Bourdieu 2004: 8), it was subsequently attacked quite fiercely by several parties (e.g. Slezak 1989; Latour 1999; Kemp 2005). The significance of the strong program was not that it would have articulated ground-breaking sociology, but that it cleared a space in which interaction between sociologists, philosophers and historians of science and technology, and, yes, scientists and engineers could evolve. It may take just a small group of radical sceptics to bring about a devaluation of disciplinary resources, if only in the sense that established knowledge is pressed to justify itself and make explicit that which within the discipline has been taken for granted. In sociology of the late 1970s, and, more specifically, in the sociology of science, this was the position of structural-functionalism (cf. Bourdieu 2004: 11-14), and to this day, the Mertonian approach representative of this position has remained marginal. The Mertonian approach represented a sociology of science that fit well with sociology's self-concept as a specialized academic discipline. Its devaluation in the »hybrid region where all sociologists are philosophers and all philosophers are sociologists« (Bourdieu 2004: 8) – or, for that matter, physicists, engineers, or biologists – might initially have been tentative, but within STS, it turned out to be final.[2]

In attracting participants selectively, devaluating disciplinary capital is a strategy of drawing in those who are willing to give up preaching to the converted and intrinsically motivated to discuss interdisciplinary without a disciplinary safety net. If a gradual remigration of these participants to their disciplines is to be averted though, the nascent interdisciplinary field needs to establish some initial gravity by accumulating and selectively distributing incentives before a form of symbolic capital genuine to its own discourse can be produced and distributed. Apart from economic capital, social capital – capital inherent in participants' social relations (Lin 2001: 19) – may be the initially most accessible resource. Devaluating disciplinary capital may provide a strategy of committing the

2 Steven Shapin's (1995: 297) remark that »the over-publicized ›warfare‹ between SSK and the ›Mertonians‹ was, in fact, but a brief early episode in the career of the field« illustrates the perception of the »old« sociology of science by STS protagonists.

attention of participants to articulating, discussing and negotiating each others' positions within interdisciplinary communication, and discourage appeals to disciplinary authority. It may help to refocus participants' attention not on what is authoritatively known, but on what their partners may understand and offer as an alternative interpretation of established disciplinary wisdom. In the same way as STS turned »opening the black boxes« of scientific knowledge and technological artefacts into a programmatic concern (cf. Pinch 1992; Hård 1994: 549-553; MacKenzie 2005), historians and sociologists could also learn from one another how to unpack the knowledge respectively stabilized within their disciplines. The critical resource able to commit participants to this kind of learning process is the social capital generated by repeatedly meeting like-minded colleagues with whom interacting is mutually rewarding. The devaluation of disciplinary resource allows the creation of social capital by:

(a) Increasing the probability that participants interested in interdisciplinary engagement will find congenial partners: scientists who are unlikely to frustrate them by responding to inquiries by recourse to authorities they are unlikely to recognize, let alone would like to simply surrender to.

(b) Increasing the probability that reputation will be gained and accumulated by participants in terms of successful engagements in interdisciplinary communication, which may be a first step toward generating a social gravity intrinsic to the field, and to creating symbolic capital genuine to it.

The main problem, of course, is to achieve devaluation. This might be easier in situations in which it can be assumed that participants have an interest in keeping conversations on a pleasant level (cf. Frost & Jean 2003: 137). This, however, is more difficult in scientific discourse in which the pacifying veil of mutual co-presence is lifted. Historians and sociologists tend to engage in scientific discourse fully armed, employing all types of symbolic capital at their disposal, while the use and reproduction of social capital is bracketed in the name of neutrality and truth. In contrast to this, interdisciplinarity can flourish in encounters where the professional stakes are lowered so that participants meet as equals, at

least for the duration of the meeting. Georg Simmel has labelled this mode of social exchange »sociability«, or the »play form of association« (Simmel 1910/1949). And just as sociability has the potential to anticipate social change as it leads people into encounters where differences in social standing are excluded, this temporary and non-binding form of interaction may serve as a laboratory for interdisciplinarity. Conversing with scientists from other fields in »free play« could inspire new ways of thinking and lead on to interdisciplinary commitments.

Generating symbolic capital

As suggested earlier, the autonomy of a field is represented in its strongest form by the existence of a particular form of symbolic capital that is genuine to and can only be generated within the field. A field built solely on social capital would turn out to be a network in which participants interact because they like each other, as in friendship ties and groups. In such a network, positions would inextricably be bound to the individual persons occupying them. But sustaining autonomous interdisciplinarity would require a field in which thematic interests could be reproduced through various cohorts of participants, and which could tolerate a considerable regular passage and fluctuation of participants. Positions and position-takings in such a field would need to be generally accessible to outsiders, while participation would simultaneously need to be restricted to those outsiders willing to respect the level of field-specific discourse. In generating a field-specific form of symbolic capital, social exchange within a field-specific discourse can gradually be calibrated toward a respective mixture of accessibility and selectiveness: outsiders are drawn into the field by their desire for a share of this symbolic capital, and in order to do so, they have to develop a »feel for the game« (Bourdieu 1990: 66f.), to read the literature, learn to recognize authors and positions, imagine position-takings and learn to perform them (Bourdieu 2004: 45-55).

Symbolic capital is a special form of cultural capital conferring upon its bearer specific chances of symbolic domination (cf. Bourdieu 1991b: 72-76). It is a »set of distinctive properties which exist in and through the

perception of agents endowed with the adequate categories of perception, categories which are acquired in particular through experience of the structure of the distribution of this capital within the social space or a particular social microcosm such as the scientific field« (Bourdieu 2004: 55). Thus the existence of symbolic capital genuine to a field is not represented by a specific type of truth, theory or paradigm able to integrate field-specific knowledge, but by an unequal distribution of chances of symbolic recognition resulting from exposure to field-specific discourse and interaction. This, again, is why arresting participants' trajectories is crucial for autonomous interdisciplinarity to gradually emerge. In the mode of oppositional interdisciplinarity, discourse among historians and sociologist continues to be dominated by symbolic capital imported from the disciplines, yet this very fact also establishes a discourse that is accessible to everybody with the respective kind of training, providing opportunities to anybody able to mobilize and employ symbolic capital effectively. Access can be universalized because the ability to realize access is regulated by the acquisition of a specifically trained academic habitus (cf. Bourdieu 2004: 51-53). The reproduction of symbolic capital cannot be separated from providing opportunities of training and acquisition, and the generation of a field-specific form requires the existence of field-specific discourse regulating the socialization of participants over years and decades, and across institutions, schools, and paradigms. Only the perpetuation of academic discourse is able to gradually turn the production of symbolic capital into a long-term collective enterprise transcending the individual investments and claims of specific scholars, institutions, or theories. Any scientific field needs to reassure the reproduction of a field-specific discourse that is indifferent to the comings and goings of theories, paradigms, institutions, and people. This speaks for establishing specialized peer-review journals – rather than the need for some representative theory or paradigm[3] – as a primary requirement for

3 The idea that some paradigm in the sense of Kuhn (1962) or some general theory about a common object of interest to which the collective attention of researchers is directed within a field is often taken to represent the existence and autonomy of scientific fields, cf. for example, the study by Harty and Modell (1991) about what they consider has been

the emergence and reproduction of a scientific field. In STS, these are journals like *Social Studies of Science* and *Science, Technology, & Human Values*. In the field in question these are *The Journal of Historical Sociology* and *Social Science History*, *Social History*, the *Journal of Social History* and a few other journals with a more regional readership, like the German *Geschichte und Gesellschaft* and the *Historical Social Research* (HSR).

But the mere existence of journals does, of course, also not suffice to gain and hold the attention of participants, as there have to be issues considered interesting enough to sustain interdisciplinary communication, and deemed complex enough to require participants to read a lot of text and do a large amount of research in order to engage with them. All problems of manufacturing interdisciplinary capital can be addressed as particular expressions of the more general problem of motivating and sustaining interdisciplinary communication. The first and perhaps most difficult problem of generating interdisciplinary capital is finding issues worth engaging with on a more sustained basis during the initial stage at which the production of research problems tends to be monopolized by the disciplines. Again, scientific capital initially has to be borrowed from the disciplines, and issues deemed intrinsically interesting will be those valuable by disciplinary standards, with some spread of interests deriving from individual habitus and idiosyncratic trajectories across the fields.

The second problem of producing interdisciplinary capital is to transform these issues from disciplinary into interdisciplinary ones. The gradual devaluation of disciplinary capital is, again, crucial to bringing about such a transformation. An appreciation of skepticism and radical statements is necessary, and one may need to accept, and maybe even deliberately provoke respective conflicts in order to raise the interest in interdisciplinary communication. Interdisciplinary conflict may then help to solve the third problem in producing interdisciplinary capital, which is the stabilization of positions among which symbolic capital genuine to the interdisciplinary field can be distributed and continuously redistri-

an ultimately unsuccessful attempt at establishing conflict resolution research as an interdisciplinary field.

buted. An unequal distribution of symbolic capital is required in order to draw participants into efforts at position-taking. In other words, one cannot have a genuine form of symbolic capital without a genuine form of stratification. The first ›stars‹ of autonomous interdisciplinarity will probably be those lucky radicals whose positions can successfully be stabilized in stratification of positions independent of disciplinary reputation.

An indicator of a completed cycle of producing symbolic capital is the recognition of this capital as genuine to a field-specific discourse. Strategically speaking, one way of accelerating the cycle up to that point may be the early infusion of reflexivity into this discourse. STS had a sustained engagement with reflexivity issues in the 1980s (Woolgar 1988), but the question of the reflexivity, and more particularly the question of the possibility of a science of science, had already been an intrinsic aspect of the early articulation of the strong program by Bloor (1976).[4] This first issue of *InterDisciplines* could be caught out for trying to present itself as part of an academic lineage and tradition, which it seeks to reflexively articulate as constituting a field in nucleus. This is neither an innocent demonstration of respect, nor a purely academic exercise in identifying an intellectual heritage. Establishing a journal is always a political act in the sense that it tries to affect the distribution of opportunities for mobilizing symbolic capital, and conjuring up a tradition is also a rally to form an interdisciplinary coalition. Participants can only be mobilized by claiming that symbolic capital is there for the taking, and talking reflexively about symbolic capital may be considered as a peculiar way of overstating its value. Talking reflexively about symbolic capital in the early stages of an interdisciplinary field is, in other words, an exercise in reification. Reflexive interdisciplinary communication reifies a yet uncertain value, a value that can only be realized once the other problems of generating gravitation towards autonomous interdisciplinarity – of

4 Bourdieu (2004: 18-21) is highly critical of the potential for reflexivity inherent in the strong programme, and in STS more generally, cf. Carroll (2006) for counter-criticism, and Kim (2009) for a defense of Bourdieu.

attracting participants and arresting their trajectories – have successfully been overcome.

Concluding remarks on levels of interdisciplinarity

Interdisciplinarity has had a history of being indiscriminately and in many cases, perhaps, prematurely celebrated by its advocates. Less often, if ever, have historians and sociologists interested in cooperation ›inter‹ disciplines asked themselves what kind of interdisciplinary research in what kind of setting would generally be desirable to them, and how respective preferences would correspond to what they expect to gain from their interdisciplinary engagements. Our differentiation of oppositional and autonomous interdisciplinarity poses the question of which level of interdisciplinarity would be appropriate for historians and sociologists moving ›inter‹ disciplines. The implications of this differentiation are not at all purely academic, they are political and institutional, and they refer to questions of organizational politics and power, of »modifying the rules of profit distribution« (Bourdieu 2004: 9). Addressing questions of who can benefit in what respect from interdisciplinary collaboration should, we think, be a central concern for sociologists and historians considering long-term investments into cooperative efforts.

Contradictory sources of symbolic capital are particularly serviceable in challenging intellectual incumbents, forcing them to take sides. Both history and sociology appear to have internalized the associated mechanisms of intellectual dispute, perpetuating and institutionalizing the respective tensions – quantitative vs. qualitative research, functionalism vs. conflict sociology, systems theory vs. rational choice approaches, social vs. cultural history, and so on. The problem with internalizing disputes in this way is that chances of renegotiating – not to speak of resolving – the boundaries are given up by incorporating them into field-specific distributions. In oppositional interdisciplinarity, the respective intellectual coalitions are likely to be reproduced on an interdisciplinary basis with, for example, ›qualitative‹ sociologists cooperating exclusively with ›qualitative‹ historians. Autonomous interdisciplinarity may be more likely to generate issues and coalitions that the disciplines are unfamiliar

with. One might question whether sociology would have had a debate about hybrid identities and non-human kinds of agency without the works of Bruno Latour (1987, 1993, 2005) and their impact on STS and beyond, challenging sociological thinking from without. The example of ›stars‹ in STS like David Bloor, Bruno Latour, or Donald MacKenzie also illustrates that autonomous interdisciplinarity neither prohibits the incidence of convergences nor the voluntary migration of issues and researchers back to the established disciplines. In the new millennium, a larger group of STS scholars has turned to exploring financial markets, easily transcending the state of the art in economic sociology in this field of research (e.g. Knorr Cetina & Bruegger 2002; MacKenzie & Millo 2003; Knorr Cetina & Preda 2005). »I have finally discovered in writing this book the conditions under which I could be proud of being called a sociologist«, Bruno Latour (2005) has written lately. As in the case of STS, autonomous interdisciplinarity may bring about long-term cycles of disciplinary change while oppositional interdisciplinarity is more likely to be associated with punctuating disciplinary equilibria which subsequently quickly normalize, as with social history in the 1970s.

The major obstacle in bringing about autonomous interdisciplinarity appears to be the devaluation of disciplinary capital. This devaluation will tend to be resisted by incumbents within the disciplines – not only because they will want to defend individual chances of symbolic domination and their prior investments into the ›disciplinary stake‹ (Cambrosio & Keating 1983), but also because this devaluation involves a downgrading of what they have learned and habitualized (cf. Bauer 1990; Pollak & Harshav 1988). Criticisms of STS waged by representatives of the elder Mertonian paradigm in the sociology of science (e.g. Baber 1992; Shapin 1993) are good examples of such resistance. A devaluation of disciplinary capital within interdisciplinary cooperation ultimately results from establishing a form of symbolic capital genuine to autonomous interdisciplinarity, and we have argued earlier that a conscious effort at devaluation may be instrumental in bringing this form of interdisciplinarity about. But there is no proven social process of devaluation: Might interdisciplinary peer review of research articles – of sociological

papers by historians, of historiographic papers by sociologists – do the job? Or may other, more responsive types of contributions, for example within symposium-style exchanges, institutionalized forms of trading several rounds of replies and responses, be more effective than the traditional research article in producing an intrinsic social gravitation to discourses suspending disciplinary authorities? A journal positioned ›inter‹ history and sociology offers a space to experiment with devaluation, and with academic discourse supporting it.

In this essay, we could do little more than present some very general ideas about modes of interdisciplinary engagement. Levels of interdisciplinarity as we have set out here remain to be explored empirically, most effectively perhaps in terms of a relational understanding of interdisciplinary cooperation (cf. Vandenberghe 1999; Kim 2009; Emirbayer 1997), but also with a view to the formation of disciplines and the institutionalization of academic boundaries. The differentiation of oppositional and autonomous interdisciplinarity is a differentiation of how historians and sociologists find partners in cooperation, how they relate to and interact with one another, how contacts are kept up or abandoned, how and by what means relationships are negotiated and cultivated, and how participants' access to symbolic capital is selectively affected (cf. Bourdieu 2004: 33f.; De Nooy 2003). The agents of autonomous interdisciplinarity, if indeed any are willing to come forward, will need social skill in Fligstein's (2001) sense, i.e. an ability to induce cooperation in others, especially those others with which they do not share a disciplinary background, and maybe not even a methodological orientation. The agents of oppositional interdisciplinarity, on the other hand, can basically focus on economizing their disciplinary habitus towards accommodating interdisciplinary issues episodically. We at least think that those interested in bringing about more regular and sustained forms of interdisciplinary cooperation among historians and sociologists deserve more support than they have been getting up until now. We sincerely hope this new journal will turn out to be a particular asset to them.

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History and sociology – the First Century

From Ranke to Weber

Hartmann Tyrell

Two prefatory remarks

I would like to start with two introductory remarks before I come to my topic.

1. My *first* remark is a historical one concerning the University of Bielefeld. Let me remind you that when our two faculties were founded, the relation between sociology and history as well as their collaboration was an important issue. First, you know of course that the foundation of the two faculties was connected with the launching of two new journals, the *Zeitschrift für Soziologie* and *Geschichte und Gesellschaft,* which have been published since 1972 and 1976 respectively, the latter then and now explicitly understanding itself as being of interdisciplinary nature. It is already in the very first issue that we find Niklas Luhmann reviewing Wolfgang Schluchter's book *Aspekte bürokratischer Herrschaft*. But this is not the only proof of the interdisciplinary nature of the enterprise of the two faculties. In 1972 the relation between history and sociology was the object of two large collections of essays with a programmatic thrust. In both cases a scholar from Bielefeld acted as editor, and prominent members of the two faculties contributed to both volumes. Among these I count Norbert Elias, who already had good connections with Bielefeld and later, in 1978, moved to the Center for Interdisciplinary Research (*Zentrum für interdisziplänre Forschung,* henceforth ZiF) to live in Bielefeld for some years. I am thinking of the two volumes of Hans-Ulrich Wehler, the editor of *Geschichte und Soziologie*, published in the widely appreciated NWB series (*Neue Wissenschaftliche Bibliothek*), and of Peter Christian Ludz, the editor of *Soziologie und Sozialgeschichte*, published as special issue No 16 of the *Kölner Zeitschrift für Soziologie und Sozialpsychologie.* At that time, Ludz was

the political scientist at the Bielefeld Faculty of Sociology. Both volumes were a promise, and maybe the time has come to make good on this promise.

I hope you will not be too disappointed when I leave the 1970s behind and continue instead with the childhood of both our disciplines, that is with historicism and Ranke on the one hand, and Dilthey and the beginnings of sociology as a discipline in the 1890s on the other – all this with a primary focus and emphasis on the development in Germany. Of course I will not ignore Max Weber, but I am saving him till the end.

To add to your possible disappointment, I have to further admit that I have little intention to discuss what is known as ›Historical Sociology‹, especially in the Anglo-Saxon world, with the only exception of Weber, who stands, after all, for the genealogists of ›Historical Sociology‹, i.e. at the beginning of the discipline. I will also stay out of the lively and programmatic debates about social history versus cultural history, about society and/or culture, macro and micro, structure and agency and so on. These debates – not hidden from the sociologists – have been conducted for two decades by the historical sciences. The Bielefeld *Historische Sozialwissenschaft* has been strongly involved in these controversies. But I want to draw your attention to a research programme which already in the 1880s eagerly promoted the »study of society and history« and which integrated the notion of culture in a certain way. The sociologist feels attracted to it because of the emphasis it puts on the question of social differentiation (cf. Tyrell 2008: 107ff.). And I think social differentiation might be a good topic for the collaboration between historians and sociologists.

I could dwell much longer on the list of things I am going to ignore, but let me stop here and move on to my second remark.

2. After World War II, German sociology declared itself, as René König put it, a *Gegenwartswissenschaft*: rooted in »modern society«, belonging to it and oriented towards »knowledge of the present«. In the same sense Helmut Schelsky spoke of sociology's »categorical concern for the present« and said its main task would be the »diagnosis of the times« (*Zeitdiagnose*).

Confronted with the accelerated modernization of modernity – to use a jargon term – sociology has since then limited its curiosity mainly to the horizon of its respective present and moves on within it. ›Historical consciousness‹ has thus fallen by the wayside as well as the search for contact with the historical sciences. What was then left to the latter – under the name of ›contemporary history‹ – was the collection of all the forgotten diagnoses of the times, of the sociological descriptions of past societal presents.

But – and König and Schelsky were well aware of this – there is also a need to speak of a »categorical concern for the present« felt by the sociologists living around 1900. Think of Simmel's *Philosophie des Geldes* (1978, originally 1900), of Durkheim's empirical study on suicide (1951, originally 1897), which was nothing less than a sociological pathology of modernity, or remember Max Weber's plan of an empirical, sociological study on newspapers and associations (*Zeitungs- und Vereinsenquete*), not to mention what he and Sombart dubbed »modern capitalism« whose »irreversibility« was precisely the point. In the editorial of the newly founded *Archiv für Sozialwissenschaft und Sozialpolitik* (1904) we read »that capitalism is a result of a historical development which we cannot undo and which we have to accept unconditionally«.

But in Germany and »under the influence of historicism« (Otto Gerhard Oexle) on the »offspring of the modern European cultural world« – as Weber said – the concern for the present turned inevitably towards the historical and raised the question of where this »modern world« came from. How did something so improbable become possible? »What nexus of circumstances« led to this outcome – at first in a particular corner of the world, but later with global consequences? Since then this question has never been raised in the same passionate vein, with the same claim and perspective on universal history. It was »Max Weber the sociologist« as well as »Max Weber the historian« who put forward this question. To be sure, in view of such questioning one could think that the sociology of knowlegde came into play here. But I will leave this aside. With regard to what seperates us from Weber I will only mention one point: to use Hans Freyer's words, it is not only remarkable that sociology in this case

was incomparably »saturated with historical knowledge and historical sense«, what I find even more noteworthy is that the political and historical world of antiquity especially is always ›present‹ as a conveniently available model for interpretation and comparison. When Weber speaks of the imperialism of the European national states, at the same time he is speaking about the Delian League.

Even if since 1900 »the light of great cultural problems« has moved on, and sociology – facing the complexities and the speed of modernity – cannot help being overwhelmingly chained to modernity, obsessed by actuality and oblivious to history, it is in my opinion nevertheless worthwhile looking back to the period when sociology and history coincided. I will stick primarily to the classic sociological triad of Simmel, Durkheim, and Weber. Each one of these three has a totally different view of the relation between sociology and history, and only Weber constitutes an unquestionable case of coincidence (cf. Gosh 2008; Firsching & Tyrell 2009).

Ranke and Weber

I am now going to discuss an example of asynchronic historical-sociological collaboration as well as a case of the sociological elaboration and continuation of an historical discovery and insight. Participants in this collaboration were Ranke and Weber. As you can't go wrong with Ranke I start with Ranke, more precisely with his early essay of 1833 »The Great Powers« (*Die großen Mächte*). Here Ranke places in a systematic context an idea that he had implicitly mentioned before in his preface to his first work *Geschichte der romanischen und germanischen Völker von 1494 bis 1535*, published in 1824. Ranke imparts to us a discovery, namely that there exists a European system of competing states which continues throughout the centuries and which always reverts into a state of equilibrium despite repeated efforts of this or that »great power« to establish supremacy. One could call this »restoration«. Ranke's finding is confirmed and made interesting by contrasting it in two ways: First, there is a contrast with the contingencies of historical events, to what imposes itself on the observer »at first sight«. Despite the changing historical ac-

tors and circumstances there is – I quote – no »chaotic tumult, warring, and planless succession of states and peoples«.[1] Instead, the power constellations remain constant in a peculiar way (Ranke 1973: 86).

Secondly, Ranke uses this finding against the contemporary discourses (Pankoke 1984: 1004ff.) on ›the social‹, on the ›social movement‹ and societal discontinuities, as they had dominated the writings of intellectuals since the French Revolution, both the conservative or counter-revolutionary and the liberal ones. What we see at work here intellectually preceded sociology whose existence as a specific scientific discipline dates back only to the 1890s (cf. Tyrell 1995). This is what Ranke had to say on the matter:

> It is almost generally held that our times tend towards, and are capable only of, dissolution. Their only significance lies in the fact that they are putting an end to the unifying or shackling institutions left over from the Middle Ages. They are striding towards this goal with the certainty of an innate impulse. It is the end-product of all great events and discoveries, of our entire civilization, in fact. It also explains the irresistible inclinations to democratic ideas and institutions, which of necessity produces all the great changes which we are witnessing. It is a general movement, in which France merely preceded the other countries. All this is an opinion which can of course lead to the gloomiest prospects for the future. We believe, however, that it cannot be supported by the truth of the facts (Ranke 1973: 98f.).

I have no intention whatsoever to condemn Ranke's reactionary sentiments or to add this statement to the long list of anti-sociological remarks with which fledgling sociology was repeatedly confronted by historians. His criticism of the assumed ›irresistibility‹ and ›necessity‹ of the ›social movement‹ was not wrong. Besides, he had a strong argument, one that – so to speak – lasted till 1945. But I do not want to dwell on this point. Instead, I will show how Max Weber, sociologist, social economist and historian of universal history, took up and expanded the

1 »[…] kein zufälliges Durcheinanderstürmen, Übereinanderherfallen, Nacheinanderfolgen der Staaten und Völker« (Ranke 1973: 86).

Rankean argument, thereby strongly affirming Ranke's discovery, while placing it in a different context.

Once again I am afraid you will have to endure a lenghty quotation, in this case one from a section of *Wirtschaft und Gesellschaft* about »mercantilism«, an economic policy which not only spares capital and capital owners, but actively favors them.

> There are two reasons for the fact that ›mercantilism‹ at the beginning of modern history had a specific character and specific effects […]: (1) the political structure of the competing states and their economies – this will be discussed later –, and (2) the novel structure of emergent modern capitalism, especially industrial capitalism which was unknown to antiquity and in the long run profited greatly from state protection. At any rate, from that time dates that European competitive struggle between large, approximately equal and purely political structures which has had a global impact. It is well known that this political competition has remained one of the most important motives of the capitalist protectionism that emerged then and today continues in different forms. Neither the trade nor the monetary policies of the modern states – those policies most closely linked to the essential interests of the present economic system – can be understood without this peculiar political competition and ›equilibrium‹ among the European states during the past five hundred years – a phenomenon which Ranke in his first work recognized as the world-historical distinctiveness of this era (Weber 1978: 353f.).

I will offer only three short comments on the way Weber expanded Ranke's discovery. As for the first, he did it with a view to his social economics (*Sozialökonomik*). Social economics in the Weberian sense was a research programme which originated from the argument with historical materialism. It relates non-economic social fields, i.e. politics, law, religion and so on, to economics and then asks in what way they are relevant for economics, or in how far they are conditioned by economics. In our case the question is: How did the competition between European states – as a purely political constellation – influence the formation and evolution of »modern industrial capitalism«? As we have already seen, for Weber, this influence was considerable. In *Protestant Ethic* (Weber 1930,

originally 1905), too, Weber dealt with the economic relevance of non-economic givens, in that case religious ones. As for my second comment, in his remarks on Ranke Weber revealed himself as an author who is committed to the idea of social differentiation. We can see this because of his presuppositon that there is a difference between politics and economics, a distinction that can and must be made. Suffice it to point to his expression »the purely political«. Of course Weber recognized also the »purely religious« or the »purely legal« and so on. Moreover, it is well known that he attributed different processes of rationalization to all these fields in the long run of history, and these processes underline the autonomy (*Eigengesetzlichkeit*) of law, politics, and science. As for my third comment, we encounter Weber as a world historian and comparative scholar. When Weber highlights the »world historical specificity« of the plurality and competion of European states, he always does so from a global perspective and by comparison. You will never find that Weber speaks simply of ›modernity‹, all the time he is speaking about the peculiarities of the occident. It is no coincidence that Weber refers to Ranke's idea, above all in his study on China (Weber 1920: 348f., 394f.). After the »period of the battling empires« China emerged as a unified empire and is thereby precisely the classic contrast to Europe. After Max Weber and later Otto Hintze, this »Eurasian« argument was often taken up when the *europäischer Sonderweg* or the »European miracle« was discussed (Jones 1981). Besides, the sociologist cannot avoid pointing to competition as a ›social form‹. Georg Simmel's great *Soziologie* (2009, originally 1908) offered a splendid account of this phenomenon (Tyrell 2007). So much for the affiliation of Ranke with Weber.

The *Arbeitsgebiet der Geschichte* and Wilhelm Dilthey's bringing together of society and history

I am not about to enter into the issues of historicism or of Ranke's famous slogan according to which each epoch is »immediate to God«. What I am going to discuss subsequently is the scope of the – to use Dietrich Schäfer's words – *Arbeitsgebiet* of that academic discipline which, since the 1830s, has successfully styled itself the ›science of history‹ (*Geschichtswissenschaft*). As for its scope, the first question must be whose

history is to be dealt with. Right from the outset we encounter answers which center around the idea of ›humanity‹ or ›humankind‹, and ›world history‹ is thought to be the framework. One humankind, one world, one history: the singular was reigning. Any exclusion of these or those nations/peoples or world regions would be incompatible with this approach, at least in principle. »History is universal by its very nature«, said Ranke, whose own notion of ›world history‹ still heavily depended on ›God‹, in contradistinction to ›the world‹. It is remarkable that Ranke very early on confined himself on the ›inner-worldly‹ level explicitly to the »Romanic and Germanic nations«. He emphasized their ›unity‹ and their association with each other in their respective developments (*gemeinschaftliche Entwickelung*). This unity, however, had its social environment. Before Ranke turned to the history of those nations, he cast a quick look at their »external enterprises«, their expansionism. Ranke mentioned three stages of those ›enterprises‹: the migration of nations, the crusades and colonialism (*Pflanzungen in fremden Weltteilen*). So, what we are confronted with here is, as Hans Freyer called it, the »world history of Europe« (*Weltgeschichte Europas*).

We find similar thoughts when we focus on time. Following the logic of historicism, there is – after having set oneself apart from the most immediate present – absolutely no temporal limit to what historians might become interested in. What belongs to the realm of history, is human life virtually »at all times and in all places«, and in all expressions of life, too, insofar as human remnants are detectable. Antiquarian interests do not know any limits. At some point, however, the distant past will turn into the subject of biology. On the other hand, the science of history in fact confined itself undoubtedly to high, i.e. literate cultures, especially the Mediterranean one. This might be inferred from what we call the period prior to the (early) high cultures. ›Pre-history‹ (*Vor- und Frühgeschichte*) is merely an extrinsic denomination from subsequent time. As far as I know, historians were in opposition to sociology, but never minded the establishment of an academic ›ethnology‹ (*Völkerkunde*) which would be concerned with illiterate cultures. In addition, the subject matter which was being dealt with was a narrow one. Of course, historicism was far

from excluding any sector of human activity from its scope. In fact, however, its main business was politics. To conclude: The 19th century type of the science of history boasted of its »illimitable« subject – as Ranke called it. Within this illimitable realm, however, it dwelled in fact only upon a comparatively small sector, the borders of which were kept more or less open.

The question now is: How does a science whose concern is illimitable fit into the whole range of sciences? How does it fit in with its neighbouring sciences? If the establishment of the science of history is due to the 19th century, how was coexistence possible between this particular science and the other ones? I am certainly not the person to tackle this problem properly. Two points, however, are striking.

First, the science of history certainly has not become the overall science with the broadest scope possible (*Integrationswissenschaft mit größtmöglicher Reichweite*, Manfred Wüstemeyer). Instead – during »the century of the nation states« – it became a science which focused on the fates of nations and states and, in addition, cultivated the history of ideas, oriented towards a sequence of different epochs.

Second, the German success story of historicism from the 1830s on consisted in its having put pressure on several neighbouring disciplines to reinvent themselves and to become historical disciplines as well (Otto Gerhard Oexle). This holds true for jurisprudence, economics, theology, the philological disciplines and the history of art. At the end of the century, Wilhelm Dilthey granted these historicized disciplines a scientific and epistemological status in their own right. He dubbed them *Geisteswissenschaften*, translated now as ›human sciences‹, in contradistinction to the *Naturwissenschaften*, the ›natural sciences‹.

Dilthey is, as you see, the very man who opened up a grand vista of cooperation between history and sociology. In 1883, his *Introduction to the Geisteswissenschaften* (Dilthey 1962) presented itself in the subtitle as an »attempt at the foundation of the study of society and history« (*Versuch einer Grundlegung für das Studium der Gesellschaft und der Geschichte*). History on its own and in the singular would not do any longer. Let us put this with

Shakespeare: »Society is the happiness of life!« It is important that the notion of ›society‹, which Dilthey linked to the notion of ›history‹ and which accompanied it, did not exclude the notion of ›state‹, as it did, for instance, for Robert von Mohl. Rather, Dilthey's notion of ›society‹ included the political order. It signified a certain whole, a social totality, and the notion of ›history‹ likewise signified a whole, a chronologically structured totality.

As for the subject matter of the *Geisteswissenschaften*, Dilthey talked about a »historical-social reality« (*geschichtlich-gesellschaftliche Wirklichkeit*). The earlier focus on the fates of nations, states and their epochs was dismissed here, for the notion of ›society‹ was combined with the idea of *differentiation*. What was brought home to his readers through this shift of focus was, beyond the political order, the social fields or spheres of law, economics, religion, arts and science, their respective evolutions, their separation from one another, their increasing autonomy. Dilthey used the expression ›cultural systems‹ (*Kultursysteme*). The *Geisteswissenschaften* themselves are seen as being reactive to the differentiation of such cultural systems in order to grasp this very process on a theoretical level.

> The differentiation of the particular social sciences thus did not come about by means of the theoretical intellect and its efforts to approach the socio-historical world as an object to be investigated by means of methodological analysis. Rather, the differentiation was brought about by life itself. Whenever a distinct sphere of social influences was formed and that sphere yielded a set of facts to which the activity of the individual was oriented, the conditions were present under which a theory could arise. The vast process of the differentiation of society, in which its marvellously complex structures have arisen, contained in itself both the conditions and the demands that allowed each sphere of life that had achieved a relative independence to be reflected in a theory.[2]

2 »Die Aussonderung der Einzelwissenschaften der Gesellschaft vollzog sich […] nicht durch einen Kunstgriff des theoretischen Verstandes, welcher das Problem der Tatsache der geschichtlich-gesellschaftlichen Welt durch eine methodische Zerlegung des zu untersuchenden Objektes zu lösen unternommen hätte: das Leben selber vollbrachte sie. Sooft die

As if this quotation were not already sufficient, I should once again like to let Dilthey speak for himself in order to spell out the idea of differentiation. There is a passage of great emphasis to bear in mind. In this passage, we hear about something »sublime«:

> What a process of differentiation in which Roman law split off the sphere of civil law, in which the medieval Church helped the religious sphere to gain full autonomy. From the activities which serve man's reign over nature to the highest creations of religion and art, the human spirit has always worked on separation, on the formation of these systems, on the development of the society's outer organisation (i.e. the state). An idea not less sublime than that which natural science can design of the origin and structure of cosmos. While individuals come and go, each of them is nonetheless a carrier and co-developer of the immense building of socio-historical reality.[3]

Ausscheidung eines gesellschaftlichen Wirkungskreises eintrat und dieser eine Anordnung von Tatsachen hervorbrachte, auf welche die Tätigkeit des Individuums sich bezog, waren die Bedingungen da, unter denen eine Theorie entstehen konnte. So trug der große Differenzierungsprozeß der Gesellschaft […] in sich selber die Bedingungen und zugleich die Bedürfnisse, vermöge deren die Abspiegelung eines jeden relativ selbständig gewordenen Lebenskreises derselben in einer Theorie sich vollzog« (Dilthey 1962: 39).

3 »Welch ein Vorgang von Differenzierung, in welchem das römische Recht die Privatrechtssphäre absonderte, die mittelalterliche Kirche der religiösen Sphäre zu voller Selbständigkeit verhalf! Von den Veranstaltungen ab, welche der Herrschaft des Menschen über die Natur dienen, bis zu den höchsten Gebilden der Religion und Kunst arbeitete sich so der Geist beständig an Scheidung, Gestaltung dieser Systeme, an der Entwicklung der äußeren Organisation der Gesellschaft. Ein Bild nicht weniger erhaben als jedes, das Naturforschen von Entstehung und Bau des Kosmos entwerfen kann: während die Individuen kommen und gehen, ist doch jedes von ihnen Träger und Mitbildner an diesem ungeheuren Bau der geschichtlich-gesellschaftlichen Wirklichkeit« (Dilthey 1962: 87).

You will not find any research program superior to this noble and cogent vision of how the sciences of history and sociology may be bound together.

When I say ›sociology‹, I mean a science which centers around the notions of ›society‹ and ›differentiation‹. That type of sociology, however, which Dilthey had in mind in his own time, i.e. a sociology à la Comte and Spencer, was *not* accepted by him in the circle of the *Geisteswissenschaften*. The socio-historical roots of that type of sociology lay – Dilthey was very clear about this – »in the upheavals of European society since the last third of the 18th century« (Dilthey 1962: 90). What excluded that type of sociology from being a *Geisteswissenschaft* was its nomothetic approach (*Generalisierungswut*). The fundamental duality of sciences from the point of view of German historicism – with history and sociology not only being separated, but located on opposite sides of that duality – starts here, too. Furthermore, the idolatry of progress was something that Dilthey disapproved of under the title of *Geschichtsphilosophie*. Sociology and ›philosophy of history‹ in that sense were later on mentioned like twins. Still, Dilthey did not oppose Simmel's sociology which started – unquestionably in the vein of that of Spencer's – with a book on social differentiation (Simmel 1890). Dilthey had no difficulties with the »historical school of national economics«. There was room for what this school had to contribute to an elaboration of the ›social question‹ within the range of his Geisteswissenschaften. We have got Gustav Schmoller's own testimony to this in his positive review of Dilthey's »Introduction«.

Sociology since the 1890s

In the second half of the 19th century, especially towards its end, we experience in Germany on the part of the historical and political sciences an astonishing hardening both against the attempt to put the social and the material on the intellectual agenda, and against a disjunctive way of thinking which was keen on separating state and society, and particularly against socialist ideas. In this context one has – regarding the relation between historical and social sciences – spoken of the »German schism« (*deutsches Schisma*). Representative of this situation was Treitschke's fierce

criticism of Robert von Mohl's project of a *Gesellschaftswissenschaft* in its own right and in opposition to political science (*Staatswissenschaft*). Treitschke insisted on *unity*: »the state is one nation [*volk*] unified in living together. There is not even a distinction of reason between nation and state in concept.« With similar fierceness the historian Dietrich Schäfer opposed the project of a *Kulturgeschichte*, proposed by Eberhard Gothein, who let himself be inspired by Dilthey and Jacob Burckhardt. Gothein had no intention of disputing the role of the state, but he wanted the ›cultural systems‹ of law, economics, religion and so on to be considered in their own right, too. But Schäfer rejected any intrusion of such »a study of society« into the genuine field of history (*eigentliches Arbeitsgebiet der Geschichte*), whereas he insisted on the scientific primacy of political historiography. It was due to Dietrich Schäfer's blatantly anti-Semitic assessment that Georg Simmel was not given a professorship in Heidelberg. I prefer to keep silent on the horrors of the infamous *Lamprechtstreit* in the 1890s and the ferocious attacks by historians on Sombart's book *Modern Capitalism* (1902). It hardly comes as a surprise that the disputatious political historians had a strong dislike for sociology. For the purpose of illustration I will only mention the word *Wortmaskenverleihinstitut*, which you will, I hope, forgive me for not even trying to translate.

It is now time to talk about the ›sociology‹ which in the 1890s took energetic steps to become a science and discipline by its own legitimacy. Some evidence for this is offered by the projected journals of the time. There is Durkheim's review *L'Année sociologique*, which got under way in 1898, but also Georg Simmel's failed plan to initiate an international and polyglot quarterly with the title *Zeitschrift für Soziologie*. For years it was one of his most favourite projects (Rol 2009). What is, first, interesting for us is that the sociological developments of that time were scarcely affected by the already mentioned horrors of the *Lamprechtstreit*, raging among the historians, although the dispute turned on the question of ›the social‹ and collective conditions. There are reasons for this which amongst other things are connected with the ›internationality‹ of the sociology of that time. Indeed, the last decade of the 19th century was the most international by far in the history of the discipline, and there were

probably also quantative reasons to account for that. Another explanation would be that sociology did not primarily try to distinguish itself from history. It was not history as an already established discipline with which the newcomer, conscious of its own and different merits, wanted to be of equal standing; for Durkheim it was psychology.

Sociology's ambition to be acknowledged as a science and an autonomous discipline was, especially in Germany, tantamount to separating itself from the narrow notion of the ›social‹, as it was connected with the ›social question‹ or socialism, and to giving the notion a more general and broader meaning. Simmel achieved this by means of his category of ›interaction‹ (*Wechselwirkung*), and it was characteristic that he planned his future journal to be free of all contents concerning »practical social policy«. Of course, this did not preclude a whole and important chapter in his *Soziologie* (2009) from being dedicated to »the poor«. The interaction type of sociality was not Durkheim's cup of tea, what he was interested in was ›the social‹ as something collective or as a society which integrates everything social. But thereby sociology had added another problem, one of the kind that – as I have mentioned before – also applied to the historical sciences, although with a more temporal emphasis. If sociology declares itself to be qualified to treat ›the social‹, everything social, it burdens itself, to use Ranke's expression, with the illimitability of its scope on the one hand, and on the other with the problem of the extent to which it is compatible with the other human and social sciences which also deal with social conditions, like law, politics or economics. How did both authors face the problem? I will now briefly sketch the very different solutions proposed by Durkheim and Simmel, and I will do this with regard to the historical sciences. One thing, however, is for sure: none of these solutions took refuge under the roof which Dilthey erected for the study of society and history.

As for Simmel, the solution he found in 1894 for »the problem of sociology« and which he thought to be internationally acceptable was a modest one, carefully fitted to the question of how sociology could peacefully coexist with other scientific disciplines. He settled on a less ambitious notion of society, and it was one of his main concerns not to

intrude »into the subject matters of established sciences« (Eberhard Gothein). Sociology does not lay claim to subject matters of its own, to content which can only be adressed by it, instead it is a science of »second order«, a »processing« science which deals with »the results of historical research, of anthropology, statistics and psychology as if they were only semi-manufactured products«.

Such processing is achieved by means of the distinction between content and form, so that the analysis of the forms of social interaction (i.e. competition, power, division of labour etc.) is the core business of sociology. As all this is well-known, I can leave it at this point. Just one final remark: Simmel's sociology has certainly made its impact, but there were almost no direct successors to it.

Regarding historiography, whose methodology Simmel treated separately, I will make only three points: Firstly, there is plenty of historical material in Simmel's sociology, material he extracted from relevant literature but used mainly for illustrative purposes. This almost playful approach to history was rather resented by his contemporaries. There was one thing he could have done to give his sociology a stronger historical turn, but which he did only rarely: he could have historicized the forms of interaction in a more prominent way. To go in this direction might have been a promising enterprise. Secondly, formal sociology does not allow what Durkheim and Weber took for granted: the elaboration of a sociology of law, religion or the family and the description of longterm processes in the respective fields. For this we only find scattered hints in Simmel, and it takes a certain effort to put them together. Of course this does not apply to the *Philosophie des Geldes*, although the book was not intended to offer a sociology of economics. Thirdly, the sociology of forms puts the conception of social differentiation back to second rank, there remains, however, enough of it, especially in the *Philosophie des Geldes*. Here the historian could reap a rich harvest.

Now let's turn to Durkheim and the paradoxical case of an author who has the reputation of being an ahistorical theorist, but whose sociological school has nevertheless left its marks on the historical sciences; in this context I am thinking especially of Marc Bloch and the *école des annales*.

And most importantly, Durkheim was the student of a historian whose main concern was the history of the institutions. I am speaking of Fustel de Coulanges whose famous *La Cité antique*, published in 1864, deeply influenced his student. Even in his late sociology of religion Durkheim cannot deny his intellectual roots. Furthermore, Durkheim was a sociologist who – with regard to society as a nation – attributed an important task to history: »Its function is to place societies in the state of remembering their past; this is the eminent form of the collective memory«.

On the other hand, conspiciously the Durkheim School avoided any contact with historiography by turning to ethnology, a tendency which has increased since the turn of the century. Durkheim's book on religion dealt with the natives of Australia. Far removed from »religious evolution« or the history of religion, the natives were meant to bear witness to »an essential and perennial aspect of mankind«. You see: the relation of Durkheimian sociology to history is manifold, but it remains nevertheless detached. For the rest, Durkheim was so convinced of his sociological mission that he refused to accept history and sociology as two different and autonomously coexisting disciplines. In the case of religious studies, too, he proclaimed »that history ceases to be itself and becomes a branch of sociology. It merges with dynamic sociology.«

Max Weber once again

Nevertheless, Max Weber is a totally different cup of tea, here sociology and history become unified in one person. Which does not mean that they merge, on the contrary, they remain distinguished from each other, but they are also each other's complement. It was only in his last decade that Weber understood himself as a sociologist, and he continued to put social economics first (cf. Tyrell 1994). If he believed that there was one discipline which sociology had to be distinguished from, this discipline surely was history.

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Concluding remarks

History, sociology, theory and the fallacy of misplaced abstractness

Stephen Mennell[1]

Although I have taught joint courses with historians,[2] I have usually had to exist in a sealed container labelled »sociology«. At my present university, the very mention of the word historiography by a sociologist is enough to send the historians screaming for the exit door. Looking back, I think all along I have been a historian *manqué*. That has certainly been true since my work on food more than two decades ago and my more recent work on America (Mennell 1985; 2007). People often say my work reads more like history than sociology. I deny that, but it is symptomatic of an endemic difficulty. Since the Second World War, sociologists have »retreated into the present«,[3] preoccupied with hodiecentric,[4] static empirical studies of contemporary society, often with immediate short-term questions of public policy in mind. All sociology, in my view, needs to be historically informed. The absence of a broader historical perspective means that sociological research too often has a very short shelf life. Historians, on the other hand, have often pursued detailed

1 This is a revised version of the transcript of substantially impromptu remarks at the close of the conference.

2 Notably at the University of Exeter, UK, in the 1980s with Colin Jones, now President of the Royal Historical Society, and at Monash University, Australia, with Graeme Davidson.

3 Norbert Elias, The retreat of sociologists into the present, in Elias 2009a: 107-126, originally published in German in 1983, and in English in 1987.

4 Hodiecentric: a useful word meaning present-centred, coined by the Dutch sociologist Goudsblom 1977.

archival research – in many cases for similarly short-term periods, though in the past rather than the present – loudly proclaiming themselves practical empiricists to whom »theory« is irrelevant. One is tempted to recall Keynes's famous remark that »Practical men, who believe themselves to be quite exempt from any intellectual influences, are usually the slaves of some defunct economist«. »Atheoretical« historians are usually using some theoretical assumptions of which they are unaware. For that reason, much historical research too has a short shelf life, and apparently needs to be rewritten frequently, in the light of whatever each wave of contemporary preoccupations brings along (Elias 2006a).

In this Annual Seminar, the main lines of debate seem not to have been between history and sociology at all but between different theoretical slants and, to some extent, different substantive interests among sociologists and historians. What most worried me was that many participants were showing signs of anxiety in their use of »theory« in their research. Of course, I welcome the idea that theory is useful in the writing of history and sociology. A good theory is rather like a road map: it shows you how things are connected, how they are related to each other. But a theory, like a map, has to be capable of modification in the light of changing empirical evidence. A theory is not just a conceptual scheme. I did not intend to make this an autobiographical article, but I came to the conclusion that maybe I had better talk a little about my own intellectual journey through sociology and history.

Some quasi-autobiographical reflections

I am within weeks of hitting the age of retirement, and perhaps that turns one's thoughts back to the beginnings of one's academic career. Old men forget, but they don't forget very much. I took a degree in economics at Cambridge and then immediately won what was called a Frank Knox Fellowship to Harvard. It gave me the run of the university, though in practice I settled down in the old Department of Social Relations. It was an utterly star-studded department in the mid 1960s. What had drawn me there was the remarkable reputation of Talcott Parsons, who dominated the world of sociology in a way that no sociologist, no

sociological theorist, has dominated it since. I actually came to know Parsons reasonably well, albeit for only a short period. He was a very nice man, but he did not have a lot of small talk; he found that I had a great deal, so he gravitated towards me. I became the resident expert on Parsons's theory among my cohort of graduate students. Yet in spite of the fact that I found it beautiful and aesthetically pleasing and all that, I had this nagging feeling, what's it all for? I could not really make any connection between it and any sociological research that I might actually want to undertake. Yes, I could see how I could write a book about Parsons, or about sociological theory in general, and indeed that is what I soon did (Mennell 1974).[5] And of course that is the way sociology has developed, with »theory« as a self-contained speciality – may I say as an »autopoietic system«? – in its own right, with no relation at all to what most sociologists are doing most of the time.

Towards the end of my year at Harvard, this problem was crystallised for me in an incident that deserves to be better known. The German sociologist of religion, Rainer Baum, who was a few years ahead of me, was very much an adept, an acolyte, of Parsons, and the story was that he went to see Parsons and he said »Professor Parsons, isn't it true that your system of four functional exigencies – Goal Attainment, Adaptation, Integration and Pattern Maintenance – can be used to analyse any system of any kind of social system, personality system, or cultural system?« »Yes,« replied Parsons, »it can«. »Then« asked Baum, »what about the Holy Trinity? It's only got three bits.« Parsons, so I heard, invented the fourth bit of the Holy Trinity to make it fit his system. Jesus was in the Adaptation box, the Holy Spirit in the Integration box, and God the Father in the Pattern Maintenance box. »The fourth bit«, said Parsons, was »the Human Spirit«, which he fitted into the Goal Attainment box. So Parsons's theoretical framework proved capable of fixing the mistakes in the Nicene Creed. You will think this is a joke; actually I thought it was a joke. It was one of those gossipy things that went around among

5 My wife and I also translated from French Guy Rocher's excellent *Talcott Parsons and American Sociology* (Nelson 1975).

the graduate students. I thought someone must be a really creative satirist. Then, a couple of days later, Parsons toddled into the room and said, »I've had a new idea« and out it came: Parsons's doctrine of the Holy Quadrumvirate. Years afterwards I found he'd actually published this idea.

This is a good illustration of theory for theory's sake, of the »social theory« industry, of forcing reality into one's abstract categories. »Social theory« is a specialist activity with great prestige. Among sociologists, the »theorists« are the members of the House of Lords, speaking metaphorically (or now, in the case of Tony Giddens, literally).

Nor, coming back to Parsons, was the Holy Trinity episode an isolated aberration, as can be seen if you look at his two more historically-orientated small books, one of which had just been published when I was at Harvard, while the next one was being circulated for comment among us (Parsons 1966; 1971). There you will find again how Parsons thought: »So we've got to think about world history. Let's fit it into our four boxes«. So, yes, you've got it: Russia goes into the Adaptation box, America goes into the Goal-Attainment box, and so on. It's nonsense. The moral of this story is that you will never understand the doctrine of the Holy Trinity, or the development of human societies, by fitting them into abstract boxes.

Parsons was very fond of referring to Alfred North Whitehead's concept of the »fallacy of misplaced concreteness« (Whitehead 1997 [1925]: 51). That is the fallacy of believing that because we have a word or a concept, there must be something real »out there« corresponding to our concept. But Parsons never seems to have considered the opposite fallacy in concept formation, the fallacy of misplaced abstractness.[6] The fallacy of mis-

6 If Whitehead will always be credited with labelling the former fallacy, it is less clear who coined the latter term. I have been using the term for many years, without any clear notion of where I acquired it. The earliest use in print that I have found is by Daniel Bell in his reply to a review by Peter Berger of Bell's *The Coming of Post-Industrial Society*; see Reply by Daniel Bell, *Contemporary Sociology* (1974) vol. 3, issue 2: 108*n*.

placed abstractness may be defined as the assumption that we can know in the abstract, in the general, what we do not know in the particular. I think that is probably valid in physics, in Heisenberg's »Uncertainty Principle«, but I am not convinced that it is a relevant idea in the social sciences and history.

Let me give you a little concrete example of over-abstraction. At this conference I have been carrying out a modest observation project in the style of Erving Goffman. Just go around the corner behind me and look at the little abstract symbol on the Gentlemen's toilet. It's not very clear at first glance which sex it means, and first of all I hesitated. I went a few steps further on, to look at the sign on what proved to be the Ladies', which did look like a female. I then watched several other men do the same thing: go to the Ladies', see the symbol on the Ladies', then go back because that clarified the meaning of the abstract symbol on the Gents'. Now, the point is if a symbol is so abstract that it doesn't even tell you what door to go through to the loo, it isn't a lot of use.

But back to my autobiography: that year, 1966-1967, we first-year graduate students spent a whole seminar discussing the so-called macro-micro problem. The macro-micro problem concerns the relationship between macroscopic sociological theories on the one hand and microscopic or interactionist social theories on the other, and the fact that there seems to be a gap between them. Of course, this problem is quite obviously also just a variant of several other common chicken-and-egg static conceptualisations in sociological theory: »action« *versus* »structure« in Parsons's day, or »agency« *versus* »structure« as it later became in the hands of people like Giddens, or »individual« *versus* »society«, and so on. At Harvard we did not discuss only Parsons; we also spent a lot of time discussing a then brand-new book by Peter Blau, *Exchange and Power in Social Life*, which was actually one of the foundation stones of what later became »rational choice theory« in sociology (Blau 1964). As a recent graduate in economics I thought it was pretty much nonsensical; I knew enough economics to think this was not remotely convincing as economic theory. When Blau tried to bridge from his essentially interactionist rational choice theory at the micro level to the macro level – as

he claimed to be able to do – in practice it drove him back into the arms of the tired old ideas of shared norms and values, back to Parsons. Thirty years later, in 1997, I went to the American Sociological Association Annual Meeting in Toronto, and the overall theme chosen by that year's president was the macro-micro problem. I can tell you that the American sociologists had not advanced one inch in the three decades from 1967 to 1997.

George Homans, another of the great Harvard stars of those days, once said to me, rather provocatively, »Talcott is a great empirical sociologist, but he's no good at theory«. The same went for Peter Blau, who did great empirical work. I came to the same conclusion as Homans, and my early exposure to Parsons's theories served me as a kind of vaccination. I had a built-in resistance when other »system builders« in the same tradition came along later: Giddens's »structuration theory«, or even Habermas's »theory of communicative action« – although I would admit that Habermas's enterprise is on an altogether higher intellectual plane than Giddens's. As a prominent British sociologist said about Giddens, »There *is* no theory of structuration, he merely re-describes the problem using a different set of jargon.« As graduate students you must beware: you have to be on guard against such nostrums. Think of Thorstein Veblen's *Theory of the Leisure Class*, with its account of conspicuous consumption and competitive status display. Something similar is involved in the domain of »grand theory« in the social scientific world. »Theory« carries great prestige, and to have a bigger and better theory is a badge of very high status. It seems to me that a lot of theory building in sociology has been driven by that kind of social mechanism of competition. My own mentor, Norbert Elias – of whom more in a moment – had a nice image concerning the relevance of theory, both in sociology and history. He used the Greek myth about Hercules fighting the giant Antaeus. Hercules repeatedly struck great blows, knocking Antaeus to the ground, but each time Antaeus picked himself up and fought back, until finally it dawned on Hercules that Antaeus regained his strength through his feet touching the earth. And so Hercules picked up Antaeus, holding him up in the air with his feet clear of the ground, until Antaeus' strength ebbed

away. The analogy is that theories that are so far up in the air lose their power and become less useful and forceful (Elias 2008a: 77; see Graves 1955: 509).

Thus far, I may have given the impression that I am simply anti-theoretical, like old-style historians. Nothing is actually further from the truth. I consider myself a theoretical maximalist in the writing of history. All history – including traditional narrative history, concerning political events, for example – needs to be informed by sociology, and equally all sociology needs to be informed by history. It is not just a matter for historical sociology, social history, and perhaps intellectual history. History and sociology are equally relevant for the traditional narrative to political and diplomatic history. I found myself thinking of another amusing story, a famous remark by British Prime Minister Harold Macmillan (Prime Minister 1957-1963). When he was in retirement, an interviewer asked him »Mr Macmillan, what was the greatest difficulty you faced as Prime Minister?« Macmillan replied »Events, dear boy, events«. The point of the story is that if we are to write convincing narratives of the past we need to be able to explain why events were unforeseen at the time, and theory is essential if we are to have the 20:20 hindsight for which historians and sociologists tend to be famous. We are essentially proponents of retrospective wisdom. You can't be wise without having some kind of theory; the question is, what kind of theory?

Elias's critique of sociology and history

Well, to continue the autobiography simply as a way of joining up a theoretical argument, I returned from America and started teaching sociological theory at a British university, the University of Exeter, where I was still worrying about the macro-micro problem. I read Georg Simmel on »the significance of numbers in social life« (Simmel 1950) [1908]), and he seemed to be pointing in a certain direction, but I couldn't actually get very much further by myself. About four years later, quite by accident, I came into contact with Norbert Elias. He was then virtually unknown in Britain, and he was only just beginning to be famous in Germany and the

Netherlands. International celebrity came to Elias very near the end of his life.

Here is a strong connection with Bielefeld. Norbert Elias was Permanent Fellow-in-Residence at the Center for Interdisciplinary Research (*Zentrum für interdisziplinäre Forschung,* ZiF). Indeed he remains the only person to have had permanent status of this kind at the ZiF. As I recall, Elias was quite a significant intellectual presence here on the campus at Bielefeld. But why am I telling you this? – why is Elias relevant for the problems of history and sociology that are now being taken up anew by the Bielefeld Graduate School in History and Sociology?

I met Elias because by chance I was asked to translate Elias's little book *Was ist Soziologie?*[7] At first I couldn't really see what the book was about. It seemed very eccentric and obscure. It certainly wasn't the accessible introduction to sociology for first-year undergraduates that the publishers seemed to be expecting. And then I reached chapter 3, entitled *Spielmodelle*, or »games models«, and suddenly the penny dropped. Elias doesn't even call it a theory; he just says that this series of models are didactic or heuristic models. But that modest little chapter seems to me to be worth more than ten tons of philosophoidal writing on the macro-micro and agency-structure problems. Obviously I can't summarise the argument in full, save to say that the series of models shows how the interweaving or *Verflechtung* of people's intentional actions produces processes that none of them has intended. This tendency towards the production of the unintended consequences through the interweaving of intentional actions is increased as the number of participants increases – that is the insight that Simmel was groping towards. But it also increases as the power ratios or balances of power between the participants – individuals or groups of individuals – become relatively more equal. The more relatively equal the balances of power, the more there emerge structured but unintended and unplanned processes. Elias shows how it becomes more difficult for participants to put together a realistic picture, to have a map of the game in which they are involved. This links

7 For a fuller account of this chapter of accidents, see Mennell 2006.

through, then, to problems of knowledge and ideology. Models that are couched in terms of the intentions of individuals tend to be associated with right-wing ideologies. Models that say we are all subject to the force of history tend to be on the leftward side of the ideological continuum. If you haven't read Elias – and I get the feeling that, in spite him having been a major presence for some years in Bielfeld a quarter of a century or so ago, he is not now much remembered among historians and sociologists here – I recommend that any historian or sociologist to start with chapter three of *Was ist Soziologie?* And then you should pay some attention to the more profound development of these ideas in more difficult books like *Involvement and Detachment* and *The Society of Individuals* (Elias 2007; 2010). Of course you can also read the more famous *Über den Prozess der Zivilisation* and *Die höfische Gesellschaft* (Elias 2000; Elias 2006b). But they are *empirical-theoretical* investigations, the subject of legitimate debate among historians and sociologists concerning actual empirical evidence as well as the theoretical explanations they offer. For understanding the place of theory in history and sociology, I think the other books that I mentioned are actually more important.

So, what is the essence of Elias's critique of sociological concepts? Again, it is useful to explain this biographically – that is, to trace the roots of Elias's intellectual stance in his own early career. His views on concepts and theory go all the way back to a profound disagreement that he had with his supervisor as a graduate student of philosophy in Breslau, the neo-Kantian Richard Hönigswald. The disagreement centred on the Kantian idea that the brain is, so to speak, hard-wired with fundamental categories: space, time causality, the categorical imperative in morality, and so on. Elias thought that this was entirely implausible and argued, even as a twenty-one year-old Dr. phil. student, that these categories of thought could only be understood as the outcome of the growth of knowledge over many generations – a long inter-generational learning process, as he would later call it. Hönigswald refused to give him his doctorate unless he tore out the last three or four pages of his thesis, and they are lost. The thesis exists – it was rediscovered in the library of what is now the University of Wrocław, but it is minus the crucial last few

pages (Elias 2006c [1922]). The formulation found in the early doctoral thesis is perhaps a little rough and ready, but you can see which way he is heading, and indeed he developed these ideas in progressively more refined versions for the rest of his life. His argument is that the mainstream of western philosophy has worked with an assumption of what he calls *homo clausus*, the model of an individual as a closed person. It can be seen very clearly in Descartes's *cogito ergo sum*. Elias's interpretation is that *homo clausus* is not merely a philosophical error; rather, it corresponds to something real: it was a mode of self-experience that was becoming more common in the European Renaissance. That intellectuals like Descartes had begun to think in this way was a symptom of something real happening in society. Something important and enduring too, for it found its way into the dominant stream of philosophical thinking right down to the present day – and philosophers are influential among intellectuals in general. But the sense that one is a *homo clausus* is not a universal mode of self-experience. One way of looking at Elias's *magnum opus*, *Über den Prozess der Zivilisation*, is to see it as showing in a very empirical way, through the development of social standards of manners and feelings, *how* people came to think of themselves as isolated individuals. He speaks of an

> invisible wall of affects which seems now to rise between one human body and another, repelling and separating, the wall which is often perceptible today at the mere approach of something that has been in contact with the mouth or hands of someone else, and which manifests itself as embarrassment at the mere sight of many bodily functions of others, and often at their mere mention, or as a feeling of shame when one's own functions are exposed to the gaze of others (Elias 2000: 60).

And yet, coming back to the philosophical mainstream, this is really the point: the image of the human being as a *homo clausus*, which runs from Descartes through Kant to the present day, is the starting point for endless circular discussions on agency and structure and the individual and society and the macro-micro problem and so on. It is also, I would say, the root from which spring such dichotomous distinctions as that of language versus practice – one of many »static dualisms« as Elias calls them.

As Elias himself puts it in one of his essays »Action theorists mentally dissect social contexts into seemingly independent actions of many seemingly independent single people. The utility of statistical investigations is based on this fictious dissection« (Elias 2008b: 93). I actually disagree with him to some extent about the value of statistics, but there is sometimes an uncritical assumption of *homo clausus* underlying statistical work.

What I especially want to argue is that Elias represented quite a radical break. The break with the neo-Kantian or mainstream tradition, on the part of Elias and a small number of others, stands in contrast to Talcott Parsons, who in his intellectual autobiography quite explicitly says that the foundations of his theory were laid in his study of Kant at Amherst as an undergraduate (Parsons 1970). Jürgen Habermas is also quite clearly in the Kantian tradition, along with Karl-Otto Apel[8] and many other people who have been influential in the area of social theory. Giddens doesn't actually say he is a Kantian, but Hermínio Martins has described him as the ventriloquist of the *Zeitgeist*, and the ventriloquist of the *Zeitgeist* is not likely to make a bold and radical departure from the mainstream. (Such a ventroloquist is more likely to make a lot of money out of articulating the mainstream just before people realise it needs articulating!) The whole phenomenological stream in sociological theory – including Berger and Luckmann on the one hand and the ethnomethodologists on the other – constitute another manifestation of the mainstream. And, paradoxically, latter-day systems theorists like Luhmann, despite apparently having little place for »the individual« in their thinking, seem to me to stand in the same tradition simply because they cannot escape the static dualism of »individual« versus »society«.

Only a minority of theorists reject that central Kantian tradition. Besides Elias, Pierre Bourdieu must be mentioned; he begins his famous book *Distinction* with an »anti-Kantian theory of the judgement of taste« (Bour-

8 Apel and Elias were fellow members of the research group on utopias at the ZiF in 1980-1981, cf. Norbert Elias, Appendix I: Note on Kant's solipsistic doubt, in Elias 2009b: 288-9, especially the note on p. 289.

dieu 1984). And the work of Bruno Latour in social studies of science would be another example. The point is, to put it simply, that it is necessary always to think in terms not of *homo clausus* but of *homines aperti* – not a »closed person« but »open people«, in the plural. One has to start from interdependent people because factually there is no such thing as an isolated human being severed from interdependences with other human beings.

I did not intend to deliver a lecture about Elias any more than I planned to give you my intellectual autobiography. Yet, listening to discussions of »theory« in this conference, I could not help but find Elias's critique of concept formation in the human sciences highly relevant (Elias 1978). His critique of sociological concepts has two main elements. One target is *homo clausus*. His other target is *Zustandsreduktion* – a term which, after lengthy arguments with him, I translated as »process reduction« rather than »state reduction«, because he is arguing that our normal way of thinking – if you like, our default setting – is to look at a process of change but then try to reduce it to static concepts. He bases this partly on a famous theory associated with the anthropologist Edwin Sapir and the linguist Benjamin Lee Whorf, the so-called Sapir-Whorf hypothesis (Whorf 1956). The argument in a nutshell, is that what Whorf calls the »Standard Average European« (SAE) languages have a tendency to indicate process by the use of a noun, a static term, plus a verb. So we have little verbal tics like »the wind blows« – but of course as soon as we stop to think about it we know that there is no such thing as a wind without the blowing. The wind *is* the blowing. And it is easy to think of sociological ideas where people try to use a static concept and then with the help of a verb make it move. It's very tricky to avoid this, actually. I'll give you one example of a vacuous concept that everyone uses – modernity. I think it is totally unusable, but it is very widely used. Even as an undergraduate I was told to be careful of even modern*isation* let alone modern*ity*. Similarly, concepts like »fields« and »spheres« and »systems« and »sub-systems« all smack to my mind of *Zustandsreduktion*. Sociologists, like little boys, seem to enjoy playing with a Meccano set of concepts that they can bolt together in various ways to form buildings and

machines. I realise I'm sticking my neck out to have it chopped off in this centre of Luhmaniac affiliations, but I really don't think this is a very profitable way of writing history in particular.

Finding and using process concepts can be tricky, because we only have SAE at our disposal. Even if we use process terminology, SAE constantly exerts a pressure towards turning them into something static. Thus, for instance, in the hands of Kant, the word *Zivilisation* denoted a process. And Elias wished to retain that sense when he spoke of *der Prozess der Zivilisation*. But already, by the time he wrote his book, in everyday usage civilisation had become a noun, something static and finished. Yet there is no such thing as an uncivilised society, and there is no such thing as a perfectly civilised society. There is no zero point, and no end point; all you can do is think in terms of vectors. All societies have some rules about the things that Elias was discussing – eating, spitting, undressing and going to the toilet and so on. All that can be observed is whether and in what direction the rules – the social standards prevalent at any given time or place – are changing.[9] That is what I mean by thinking in terms of a vector. Alternatively, in mathematical terms perhaps it is a little bit like thinking of the first differential in calculus, looking at the rate of change.

What do process theories look like? Well, there is one major category of process theories in the human sciences, those stemming from the Marxist tradition. I suppose that reflects Marx's original intellectual debt to Hegel as opposed to Kant. But even the Marxist tradition has been subject from time to time to this kind of process-reduction and systematisation. Louis Althusser's theories seem to have amounted to Parsons with the sign changed from positive to negative. Although few people have read Althusser since he murdered his wife, it certainly is still worth reading that wonderful essay by Edward P. Thompson, *The poverty*

9 Which is why the five volumes of Hans-Peter Duerr's polemic against Elias, published under the overall title of *Der Mythos vom Zivilisationsprozeße* completely misses the point because it is just a random display of static evidence, cf. Goudsblom & Mennell 1997.

of theory, or the orrery of errors (in Thompson 1978: 193-397), in which he compares Althusser's system to a mechanical device with the planets circulating around the sun, and moons moving around the planets; you turn a handle and everything revolves in predestinate orbits. Edward P. Thompson seemed to me to be in many respects an excellent model of the writing of history from a processual point of view. He fully recognised the process-reducing inclinations of modern sociologists:

> Sociologists who have stopped the time-machine and, with a good deal of conceptual huffing and puffing, have gone down into the engine-room to look, tell us that nowhere at all have they been able to locate and classify a class. They can only find a multitude of people with different occupations, incomes, status-hierarchies and the rest. Of course they are right, since class is not this or that part of the machine, but *the way the machine works* once it is set in motion – not this interest and that interest, but the *friction* of interests – the movement itself, the heat, the thundering noise [… C]lass itself is not a thing, it is a happening (Thompson 1978: 85, italics in original).

The twin critiques of the *homo clausus* assumption and of process-reducing concepts offer an escape route from the endless circularity of agency-structure debates. They also point to a key point of processual thinking that Goudsblom sums up in one sentence: »The unintended consequences of people's intentional actions become the unintended conditions of further actions« (Goudsblom 1977). So, there is a need for a theory, yet I've just launched a very rude attack on a whole tranche of famous theorists. What kind of theory is useful to sociologists and to historians?

The need for theory

I think I can distinguish between two kinds of historical sociology or the use of sociological ideas that are useful in the writing of history. One of them I call »the sociology of the past«, which involves taking a modern sociological idea and using it in interpreting historical evidence. An example is the use of Stan Cohen's famous book *Folk Devils and Moral Panics* (Cohen 1972). It was a study of the »Mods« and the »Rockers«, who

were two groups of young people who rode Vespa scooters and motorbikes respectively in an outbreak of juvenile exuberance in the 1960s; the Great British Public thought that the end of civilisation had come just because two different gangs were fighting in seaside resorts. But the idea of the »moral panic« has since been applied quite widely in historical writing, being applied notably in understanding the Salem witch trials in late 17th century Massachusetts. Another example is Keith Thomas's use of modern anthropological ideas about magic in his famous book *Religion and the Decline of Magic* (Thomas 1971). A third example is Kai Erikson's *Wayward Puritans* (Erikson 1966), in which he used what was then the latest thinking in sociological theory about »deviant behaviour« to interpret evidence of the life of the early Puritan settlers in New England. This book has dated somewhat, because »deviancy theory« at that time was heavily influenced by Parsons, taking for granted assumptions about »shared norms and values« from which people deviate. The fact that Erikson was using deviancy theory as it stood then in the 1960s does actually weaken the durability of the historical writing, which is itself still quite interesting. This points to the danger that theories often date more quickly than the historical evidence they are used to explain. (That being said, it should be acknowledged that Erikson's book also made a contribution to sociological theory; there is always a two-way flow yielding insights of value both to history and to sociology.)

The other kind of theoretically informed history writing, in which I myself try to indulge, is what may be called the »sociology of the long term«. I think it is quite difficult to do, and rarely yields a nice compact research topic for a PhD sociologist. And historians have tended to be frightened off this by the writings especially of Sir Karl Popper, who denied the possibility of a theoretical history and identified the attempt to formulate one with incipient totalitarianism.[10] Many notable social scientists, such as my teacher John Goldthorpe in Britain or Hartmut Esser in Germany,

10 Popper 1945; 1957. See also Norbert Elias's critique of Popper, On the Creed of a Nominalist: Observations on Popper's »The Logic of Scientific Discovery«, and »Science or sciences«? Contribution to a Debate with Reality-Blind Philosophers«, in Elias 2009b: 161-211.

became convinced Popperians, and came to distrust all uses of history in sociology. One problem is that Popper set up physics – classical physics – as a model for all sciences, and this gave many social scientists an inferiority complex. They suffer from what has been called »physics envy« (by analogy to Freud's »penis envy«).

Yet that is surely nonsense. It is certainly possible to discover patterns in long-term historical processes. Most obviously is the case of the division of labour, a very long-term process that has experienced some fluctuations, but has basically continued in a steady direction throughout human history. Other examples, in which long-term trends are subject to more marked fluctuations, are civilising processes on the one hand and state formation and the dissolution of states on the other, both of them investigated (and related to each other) in Elias's *Über den Prozess der Zivilisation*.

In the classical philosophy of science, prediction can be divided into two parts: *pro*diction and *retro*diction. Prodiction actually means prediction in the ordinary sense: predicting the future. Retrodiction means the intellectual prediction of what we might find in the evidence of the past when we come to investigate it. It is possible to predict – on the basis of our present knowledge and our theory – what is going to be found in historic archives, for example. Popper was ideologically opposed to any attempt at large-scale prodiction of the future. I think we can sometimes do that to a limited extent. The science fiction writer Arthur C. Clarke was famous for predicting some scientific breakthroughs long before they happened, such as television broadcasts from geo-stationary satellites; he did not just guess at random, but rather knew a great deal about science, and that together with a novelist's imagination enabled him to make some strikingly bold prodictions. But sociologists and historians are on rather stronger ground when it comes to retrodiction; we are often accused of having retrospective wisdom. Even that may be extremely useful, however. Take the example of the current banking crisis. History, sociology and economics (if economists are so inclined) can help to explain to politicians how we got to where we are and why we went wrong. There are links to be made to different kinds of speculation and different

episodes. The knowledge that we generate ought to enable people to avoid making the same mistakes again – though of course it often does not. I have recently been reading Galbraith's *The Great Crash 1929*, which is a best seller once again. It is quite gripping, because the parallels between the idiocies of the late 1920s and the even grander idiocies of today are really very obvious. Our present masters of the universe have discovered much more sophisticated ways to make the same mistakes. Galbraith, in a preface written for a new edition in 1975, rightly claims that there is merit in keeping alive the memory of the greatest cycle of speculative boom and bust since the South Sea Bubble. For a generation after 1929, politicians and businessmen remembered the crash and avoided the mistakes. But, by the 1960s »this memory had dimmed« and »almost everything described in this book had reappeared« (Galbraith 1975 [1954]: 9). By the first decade of the 21st century total amnesia had set in among the people with the power to cause economic catastrophes. One must, of course, always ask the classic question *Cui bono*? There are reasons for forgetting. A lot of people made a lot of money out of forgetting the lessons of 1929. As Karl Deutsch remarked, power is »the ability to talk instead of listen [and] the ability to afford not to learn« (Deutsch 1963: 111).

It may only be a satisfaction to us as historians and sociologists, but it does give some kind of intellectual satisfaction to be able to explain where they went wrong, what the players in this disaster did not know. Probably the bankers do not want to know it, but it is some sort of intellectual satisfaction to us to be able to point out that Adam Smith was fully conscious of the need for government and aware that the markets did not entirely work on their own: the hidden hand operated within boundaries set by law and government. But the extreme laissez-faire reading of Smith has come to dominate. Much more recently, but still more than half a century ago, Karl Polanyi in his book *The Great Transformation* made some points that now seem utterly relevant (Polanyi 1944). He demonstrated vividly that markets are always embedded in wider social arrangements. He showed how the Gold Standard was a convention, a tacit agreement between governments and bankers to fol-

low a system of rigid rules, which imposed a kind of intense self-restraint upon them, but threw the burden of adjustment to the trade cycle on to the mass of the workers through vicious cycles of unemployment.[11] Polanyi's book was on the reading list when I was an economics student more than forty years ago, but people didn't bother to read it because Polanyi seemed to be telling them things that everybody knew. Well it now turns out that not everybody any longer did know, and *The Great Transformation*, like *The Great Crash*, is attracting new readers today.

I would argue that historians and sociologists, working together, could provide a useful critique for policy makers. I don't mean »critique« in quite the sense that the Frankfurt school meant critique – not an ideological critique, although that might come about as well – but a practically useful critique, a sort of social historical equivalent to psychoanalysis perhaps. That could be far more useful, but less comfortable, than much of the current limited and myopic social research for which governments will pay large sums of money – and thus determine our intellectual agenda for us. (That is a *cri de coeur* from the sociologists probably more than from the historians.)

The maximalist programme for history and sociology

I said at the beginning of this talk that I am an advocate of the maximalist programme for history and sociology, a proponent of the univer-

11 A certain parallel can be seen in the consequences of the Bretton Woods agreement, which are now attracting renewed and widespread interest. Contrary to popular myth (at least in Britain), it was not John Maynard Keynes's proposal that was accepted, but rather the American model, which was perhaps intended primarily to avoid the terrible traumas of the Great Depression ever occurring again in America. It made the dollar the world's reserve currency, and the USA the world's banker. In the medium term, that brought enormous benefits to the living standards of Americans, but, like the Gold Standard, threw the burden of adjustment to trade cycles on to the poor: the poorer parts of the world in this case. In the longer term, it appears to be highly destabilising for the USA, see Liaquat Ahamed, The Future of Global Finance, *New York Times*, 20 September 2009.

sal relevance of sociology and theory – at least if it's the right kind of theory – for the writing of history, as well as the relevance of history for writing sociology. I recently glanced again at John Hall's book *Powers and Liberties*, in the introduction to which he tells a story about Oxford history as it was in the 1960s. He relates:

> Whilst an undergraduate at Oxford studying history, a close friend was asked to write an essay on the social origins of the Third Reich. He spent a week reading about unemployment, working class authoritarianism, inflation, reparations, lower middle class anti-communist attitudes and the like and produced an essay with these factors very much in mind. His tutor pounced, delighted at this deliberate hoax, designed to teach a lesson, delighted that it had worked so well. What was the lesson? There *were* no social origins of the Third Reich, the tutor insisted, merely particular moves made by specific actors, especially Von Papen, and these political manoeuvrings were the real cause for Hitler's accession to power (Hall 1985: 1).

Of course, there's no need to explain to this audience what nonsense that is, but there is a grain of truth in it as well, because social processes, social factors are ultimately the product of the interweaving of the plans and intentions of many people. Again, to quote Elias »underlying all intended interactions of human beings is their unintended interdependence«.[12] So, obviously, whatever theory we are using there is a need to explore factual interdependences, the power balances that link people and groups in whatever time and place we are studying. Sometimes we may be able to explain how there arise the unanticipated »events«, to echo Macmillan's aphorism. Again, a couplet that I like from Elias:

> From plans arising yet unplanned
> By purpose moved yet purposeless (Elias 1991: 64).

12 Norbert Elias, Sociology and psychiatry, in Elias 2009a: 175.

So we need to study the power ratios, go back to the game models and the proposition that the more equal the power ratios and the longer the chains of interdependence, the less planned the overall process is – yet it can be made intelligible.

Actually, what I have just said is probably obvious to historians; historians do it all the time without necessarily conceptualising it in those terms. That's what history consists of, but sociologists don't always see these things so clearly, particularly if they read too much contemporary social theory. We also need to look at knowledge and assumptions and perceptions. Knowledge: the theme of one of the workshops at this conference (although the convenors wondered whether it was quite the right term – but it will do for the moment). Obviously, the further back we go in time as historians the more important it is, and the more difficult it is to understand what people knew. The less can it be taken for granted that we know what people knew. That is clearly the case in medieval and early modern European history, for example, where increasingly the need for the ethnographic skills developed by anthropologists for the study of non-European societies are seen as relevant. But Darwin's bicentenary in February 2009 reminds us how difficult it is now, looking back to what seems like only yesterday, the mid-nineteenth century, for us to understand why even the Victorians thought as they did. Why were theological modes of thought and knowledge, and the use of them against natural scientists, so much more marked than we can conceive now (except among the very numerous American hillbillies, of course)? Or take the whole question of »rational choice« theory. It really is the greatest nonsense, but it seems to work in the short term for so many things. Yes, human beings always had the intellectual equipment to pursue their goals rationally, but the bigger problem is to understand what they wanted and why they wanted it in the more distant past.[13] For

13 Another example from Elias is relevant: his discussion in *The Court Society* of »court rationality«, which from the point of view of rational bourgeois rationality looks irrational. They spent money that they didn't have, they spent – rationally in their terms – in order to maintain a rank and to consume in relation to their rank rather than trimming their expenditure to their resources.

all its glitter, it seems to me that rational choice theory is still vulnerable to Lord Macaulay's jibe that what the theory really amounts to is the proposition that »a man had rather do what a man had rather do« (Macaulay 1889 [1829]: 180). Translating that into more modern English: a person will do what he wants to do. The historical and sociological task is to explain why and what »he had rather do«, and that is not something to be taken for granted.

Let me conclude with another case study, on the question of knowledge and perception. In the last chapter of my most recent book, *The American Civilising Process*, I look at the effects of unequal power ratios on Americans' perception of themselves and the wider world. It seems to be a general principle that the more unequal the power ratio between two parties, the more the perception on the part of the more powerful party becomes distorted. You can obviously reach this principle from Hegel's famous discussion of the master-slave relationship, but I actually reached it through a study of a refuge for battered wives in Amsterdam written by two Dutch friends (van Stolk & Wouters 1983). What they found was that if you asked the wives who had been beaten up by their men to write a character sketch of their man, they could give a lot of detail about the men's personal idiosyncrasies, behaviour, what got them riled, and so on. If you asked the men to describe their women, all they could do was to speak in terms of stereotypes of »the little woman«. This seems to be a principle of wide application. For example, I am a British person living in Ireland. The Irish know everything there is to know about the goings on in their neighbouring more powerful country, Britain. Talk to a Brit, they still have old out-of-date stereotypes of Ireland – people riding around on donkeys and so on. In the case of the USA, it seems to me that Americans always see themselves as the champions of democracy, even though history shows their record to be distinctly mixed. The key element that has shaped American character in the very long term, I would argue, is that America has continuously become more powerful vis-à-vis its neighbours. The Pilgrim Fathers appear to have been briefly dependent on the Indians, but the New England settlers were very shortly fighting the Indians in King Philip's War, and there followed westward

expansion in which Native Americans were almost wiped out, and then the gradual emergence of the USA as a world great power. Now the whole world watches America – following its elections in great detail for instance – but Americans' perception of the wider world is not reciprocal. It is as though they are sitting in a brightly lit social psychological laboratory. We, on the other hand, are sitting in a dimly lit observation room behind a one-way mirror, and when they look towards us they only see their own reflections. I give you a concrete example: why do the Iranians hate the Americans? Americans tend to be very much puzzled by that, thinking »they must be evil people if they don't like us«. But if you ask even a highly educated American whether he or she knows anything about Mohammed Mossadeq, the answer is likely to be, »Who?« Mossadeq was the democratically elected prime minister of Iran in 1951-1953, who was overthrown in a *coup d'état* arranged by the British and the CIA, in the interests of preserving Anglo-American control of Iran's oilfields.[14] The Shah's murderous regime was reinstalled and not overthrown until the Iranian Revolution of 1979. Ask any Iranian about Mossadeq and they know all about the coup of 1953. A similar asymmetry could be found between the United States and any number of countries in Latin America.

A further small extension of this line of argument throws light on the consequences of the end of the Cold War. The collapse of the Soviet bloc in 1989-1990 at first led only to intellectual triumphalism in America, like Fukuyama's notorious essay *The End of History* (Fukuyama 1989). But the removal of a major external constraint upon the behaviour of the USA – just like the reduction of regulatory control over the bankers mentioned before – had consequences in gradually changing behaviour. Just like a small child, bankers or American presidents began to explore what they could get away with. Eventually, the administration of George

14 It is symptomatic in 2009/2010 that the Wikipedia entry on Mossadeq had a notice at the top reading »The neutrality of this article is disputed. Please see the discussion on the talk page.« The talk page reveals that some American readers found the factual history of this episode unpalatable.

W. Bush decided it could get away with a war of choice in the invasion of Iraq.

What I'm saying is that this hypothesis about the connection between unequal power ratios and perceptions that lack congruence with reality is a fairly simple sociological idea – only a middle range theory – but it seems to me to be quite illuminating and of wide application. It helps to explain quite a range of things. But it is not a huge great theoretical system, a crate full of boxes within boxes within boxes.

Conclusion

In conclusion, I'd like to make an observation based on my experience of doctoral students in sociology. My own students, particularly at the Masters level, but also at the doctoral level, now seem to think that it is uncool to commit yourself to a particular theoretical perspective. They may have picked up this idea from teachers of my generation, who remember »the war of the schools« which virtually destroyed British sociology, maybe German sociology at times as well, when departments blew up and were closed down in British universities because staff felt passionately committed to one or other »school« of theory – ranging from positivistic survey research through the 57 varieties of Marxism to phenomenology and ethnomethodology. Sometimes it went to the point where they could not speak to each other at all. Boom! – departments of sociology exploded. Thank goodness those days are past. But one consequence seems to be that students are hesitant to take a theory and explore it and see how it fits and how it can be developed and how it can be modified. Instead what they tend to do in the introduction to their thesis is say to themselves, »Oh, you've got to have a theory chapter, right?« So, whatever their specific empirical topic, they write a list: what Habermas says about it, what Bourdieu says about it, what Foucault says about it, what Elias says about it, what (if you are German) Luhmann says about it, what Uncle Tom Cobley and All say about it. And when they have written the list, they disappear into their data and that is the last you hear of any theoretical perspective. I do not think that is the way to proceed. I think you should let your empirical research lead you to a

relevant theory with the potential to gear into empirical evidence – to make connections for you. It should not be something up in the air like Anteaus being held clear of the ground by Hercules. It has to be some hypothesis, some question, some insight that has quite clear implications for what you might expect to find in your archive or other source of evidence. Then pursue the theory, develop it, use it, and if necessary reject it in due course. At least you are using theory, whereas so many of my students think it's a sort of exercise to be written at the beginning of the thesis and then to be forgotten about. It's probably not a danger here, because you all seem to be well and truly dug into the lasting influence of Niklas Luhmann. I hope I've explained why I'm sceptical about grand theory, but still think that theory is essential both in sociological and historical research.

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Conference Report

Diskursanalyse in der Wissenschaftsgeschichte

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Diskursanalyse in der Wissenschaftsgeschichte (Discourse analysis in the history of science). 2. Internationale Tagung zur historischen Diskursanalyse, University of Zürich, 27th-28th of May 2010, hosted by Franz X. Eder (Wien), Achim Landwehr (Mainz/Düsseldorf), Jürgen Martschukat (Erfurt) and Philipp Sarasin (Zürich), organized by Peter-Paul Bänziger, Mirjam Bugmann, Pascal Germann and Philipp Sarasin

It seems that in the history of science, discourse analysis has passed its apex of popularity. Although the concept of discourse coined by Michel Foucault is still in use, historians of science today favour other methods which are more likely associated with the iconic or practical turn. This development provokes a range of questions: Are discourse-analytical perspectives so common and self-evident today that they do not need to be explicated any more? Or have they become outdated? And how can historical discourse analysis contribute to the historization of science? These questions were to be debated at a conference held at the University of Zürich under the auspices of Franz X. Eder, Achim Landwehr, Jürgen Martschukat and Philipp Sarasin. The objective of the conference was to discuss the assumptions outlined above and to present new forms of historical discourse analysis. Hence, its focus should have been on recent methodology – a goal that the conference in most parts failed to achieve, as the majority of papers presented empirical findings or considerations on discourse theory rather than considerations on methods.

Introduction

Philipp Sarasin (Zürich) opened up the conference with a concise introduction on the characteristics of Foucauldian discourse analysis and the

alleged withdrawal of the history of science from this method. As Sarasin pointed out, discourse analysis was developed in the context of the history of science, but refused to contribute to narrations about progress, ›great men‹ and discoveries. Furthermore, there were similarities to other constructivist concepts and theories such as Ludwik Fleck's thought-styles or Thomas S. Kuhn's paradigm shift that have been widely acknowledged. Yet it is hard to say what historical discourse analysis exactly is. Sarasin outlined five characteristics: the denial of the subject as an active and willing founder of his or her world; emphasis on the power of the discourse to produce reality; a focus on disruptions rather than continuities; the analysis of conditions that allow for certain statements while suppressing or excluding others; the assumption that discourses are »copy machines«. While most of these points are common, the last aspect highlights a specific characteristic of Foucauldian discourse analysis by using a stunning metaphor: Discourses, as Sarasin pointed out, produce objects just in the same way as copy machines do. Copy machines can reproduce reprints over and over again, but neither do they have an intention while doing so nor do they produce identical copies. By introducing this metaphor, Sarasin added an aspect that is widely neglected in historical discourse analysis – and was, unfortunately, not paid any further attention to in the course of the conference: Discourses are material structures and they work technically. Subsequently Sarasin dealt with the turn towards objects instead of discourses in the history of science. He pointed out that, inspired by Bruno Latour's Actor-Network-Theory and Hans-Jörg Rheinberger's suggestions to investigate into experimental systems, it has become common to analyze images and practices at laboratories. Here Sarasin pointed out a striking gap: By ignoring discourses, spaces are treated as being free from them. Instead, there is a strong focus on evidence, presence and »things as such« – just as if they were really there and as if they would appear before ones eyes if one did not consider discourses. According to Sarasin, this development causes risks: First, there was a positivism of ›pure‹ description; second, media were seen as having material logics that determine things.

Basic questions of Foucauldian discourse analysis

The first section of the conference dealt with basic questions of Foucauldian discourse analysis. Looking at Foucault's works on literature and painting, *Roberto Nigro* (Zürich) discussed the relationship between the sayable and the visible. He thus showed how discourse analysis could be connected to the iconic turn. Nigro concentrated on Foucault's analytical handling of pictures. The philosopher's interest in pictures was defined by their function. He utilized analyses of pictures to fix his view on epochs. According to Nigro, Foucault perceived pictures not as representations but as diagrams. What is more, discourses and pictorial outlines are seen as having their own way of being, but still they correlate. The discussion centred on the relationship between the sayable and the visible. Furthermore it was hotly debated what the invisible was and how it could be studied. It was stressed that Foucault, being positivistic, argued for analysing existing – that is: visible – structures.

Maximilian Schochow (Leipzig) took a look at the role of figures of crisis in historical discourse analysis. He discussed disruptions and continuities as seen by Foucault, concentrating on figures of change. Schochow pointed out that Cervantes' *Don Quixote* and de Sade's *Justine und Juliette* can be seen as symptoms of change and disruption: They heralded new phenomena. Schochow explicated his concept of figures of crisis in historical discourses by presenting a case study. During a period of change that emerged in the 16th century, a person perceived as being female who had had sexual intercourse with an other woman and had worn men's clothes was regarded by some as a woman, whereas others perceived her/him as a hermaphrodite after examining her/his body. Schochow's argument that this was a figure of crisis representing a dramatic change of knowledge was questioned by the conference's participants. In sum, they argued for emphasizing simultaneity rather than disruption. Moreover, the analysis should concentrate on continuities that accompany changes and, in addition, take a look at the social contexts of discourses.

Aleksander Miłosz Zielinski (Bern) talked about the role of epistemes in post-structural history of science. He presented a case study on the struggle between Gabriel Tarde and Emile Durkheim, at their time fierce

opponents in social theory. According to Zielinski, Durkheim outdated Tarde as a leading sociologist because he represented an episteme that was more up to date: While Tarde tried to constitute sociology on the basis of a larger historical-philosophical system, Durkheim established sociology as a distinct discipline and promoted the virtualization of thinking. Zielinski tried to situate the change of epistemes in a broad socio-historical context. He paralleled it with changes in transport and media systems, starting with the invention of the printing press and the ›discovery of America‹ in the 15th century, considering the effects of railway systems in the 19th century and the broad scope of communication in the late modern age. Participants criticized this historical *tour d'horizon* as simplifying too much and suggested to concentrate on concrete cases. With regard to epistemes it was asked to consider that, firstly, different epistemes could appear parallel in history; secondly, that Foucault increasingly favoured to look at discourses rather than epistemes; and, thirdly, that this category was useful only for explaining broad developments. Still, discourse analysis is usually not called into question on the historical macro level, but with regard to its implementation in laboratory studies.

Production of alterity

The second section was dedicated to the production of alterity. *Birgit Stammberger* (Vechta/Lüneburg) argued for a kind of discourse analysis in the history of science that should be extended by a perspective on gender. Looking at the history of 19th century monsters she argued that discursive constructions should not be played off against the materiality of the body – and vice versa. The monster was a bodily phenomenon, but it only appeared within discursive formations. In the discussion the focus on monsters was called into question, as it was considered to be well explored. There was an argument about the fact that some topics and aspects attract a lot of research interest while others – such as diversity in and of discourses on monsters – are neglected. At this point the important general question about the historian's own perspective was raised. The allegation was formulated that one only takes those discourses or fragments into account which suit one's own position.

Also looking at an empirical case, *Cécile Stehrenberger* (Zürich) analyzed the strategies of the Instituto de Estudios Africanos to generate knowledge and located it in the context of the history of science. From the 1940s to the 1960s a network of Spanish experts from various disciplines investigated the physical and mental characteristics of the *indígenas* in Equatorial Guinea. To gain a wide range of results, they applied different strategies. These were tied to political aims such as the progress of colonial policy and often contradicted each another. Adopting a micro-historical perspective, Stehrenberger illustrated how scientific knowledge was generated and diffused in a specific political-historical situation. Participants approved of that perspective as well as the focus on the production of knowledge within a network: By analyzing a dispositive – that is interwoven discourses, institutions and practices – it is possible to identify the character of specific discourses. That is its social and political contexts, the situation of the actors involved, the impacts of research instruments and methods on outcomes, the effects of media that are applied to disseminate findings, and how all these factors are inter- or co-related. Stehrenberger's ironic presentation was criticised for producing a colonial view on the historical objects she had investigated. It was thus argued that historians have to be very careful not to adopt a colonial view on their historical objects.

Sexuality and economy

Combining perspectives on sexuality and economy, as the third section did, is not odd: In the modern age both these fields are distinctively characterized by the imperative of productivity. *Mike Laufenberg*'s (Berlin) depiction of Foucault's discourse analysis as a historical ontology was not concerned with methodology but with theory. Laufenberg postulated that Foucault's concept of discourse was mostly perceived as epistemological. But, as he argued, in his works on sexuality Foucault also considers historical conditions and feasibility. On this basis, Laufenberg drafted a historical-epistemological concept of discourse, concentrating on debates on scientific discourses on sexuality and sexual subjectivity. As he pointed out, sexuality emerges from a heterogeneous constellation of power: It is not just an effect of the *sciencia sexualis* but also about the

ethical question of how to lead one's life. As Laufenberg was dealing with theory rather than methodology, the question was raised as how to combine his approach with discourse analysis. Laufenberg argued that the integration of subjects revealed the limits of discourse analysis. He emphasized that subjects were not simply determined by discourses but that they have the ability to carry out changes. He stressed that objects and subjects modify each other.

Ute Tellmann (Basel) took a look at the history of modern economy and thus brought a field into focus that until now has been underrepresented in historical discourse analysis. Perceiving economy as an epistemic object, she explored the reconfiguration of this object in the early 20th century. Tellmann proposed to take an archaeological view on economy and argued that this view reveals a crucial turning point at around 1930: Since then, money was not perceived any longer »as a medium of representation but as a time machine«. Consequently, there was a new emphasis on loans and thus future and measuring time, a »break-in of time in the discourse of economy«, as Tellmann concluded.

Life Sciences

The last section was about life sciences – like the history of sexuality a well established object of historical discourse analysis. *Heiko Stoff* (Braunschweig) advocated a combination of discourse analysis and Actor-Network-Theory while looking at the history of research in active pharmaceutical ingredients (*Wirkstoffe)*. In a historical perspective he outlined methods and concepts that were applied to analyze how industries, states and sciences cooperated in producing knowledge. According to Stoff, methods such as community research could explain how trilateral networks work, but they do not help to analyze how things were constituted. The concept of translation that was introduced by Actor-Network-Theory allowed for looking at the state, industry and science interpreting problems in the same way, while the concept of the dispositive was useful to study research in active pharmaceutical ingredients, because it emphasizes problematization and mobilization. In addition, dispositives produce options as well as things that can be analyzed. However, it was

not really clear for the audience how Actor-Network-Theory and discourse analysis are related to each other.

Lisa Malich (Berlin) criticized the preoccupation with change and discontinuities of discourses as well as their perception as closed topical or disciplinary entities. In critical distance to this, she proposed to integrate Michel Serres's concept of folded time into historical discourse analysis. Malich tried to prove that through this concept multi-temporal and overlapping fragments originating in different historical periods can be theorized and investigated. She illustrated this idea with the discourse of mood swings during pregnancies. In this example, discursive fragments deriving from various periods coexist and overlap. In the discussion it was stressed that until now the dimension of time has been underexposed in the research on historical discourses. Still the question was left open why the concept of folded time should be preferred to Foucault's archaeology of knowledge, which also enables the historian to trace different fragments occurring at the same time.

Julia Diekämper (Bremen) shifted the focus from discourses in science to discourses in the media. She argued that scientific knowledge was generated not so much at laboratories than in the course of public negotiations in mass media. Looking at press coverage on preimplantation diagnostics in *Der Spiegel* and *Die Zeit*, she explicated how different discourses dating back to different times have recently been circulating. As Diekämper pointed out, the »ethic of healing« competes with the »holiness of life«. Moreover, mass media do not simply offer advice and information but set norms that have effects on self-technologies. With her neat analysis, Diekämper gave insight into recent discourses on preimplantation diagnostics. Unfortunately, like most other papers, there was no reflection on methods. Diekämper could have brought in a new perspective by discussing how exactly the media produce discourses and how this production could be investigated in terms of discourse analyses. She also ignored the question of how knowledge circulating in the media affects science. If she had covered this aspect, she could have enriched discussions on historical discourses, since historical research still concen-

trates on the popularization of scientific knowledge through the media and neglects possible influence in the other direction.

The last paper presented a combination of empirical findings and reflections on methodology. *Jens Elberfeld* (Bielefeld) lamented that by stressing the local situation and contingency of the production of knowledge, laboratory studies fail to adopt a critical view on society. Presenting the history of the Biological Computer Laboratory of the University of Illinois, founded in 1958 and closed down in 1974, he argued that discourses »don't stop at the steps of laboratories«. On the contrary, laboratories have to be seen as material results of historical discourses and thus as a part of complex power relationships. That is why the genealogy of knowledge of the laboratory and the production of knowledge at the laboratory need to be analyzed. It would thus be possible to »bring society back in«. The disputants acknowledged that the analysis of knowledge was missing with laboratory studies. But how can knowledge circulating within laboratories be discourse-analytically analyzed? Elberfeld suggested that laboratory-books documenting works and processes could be a valuable source.

Synopsis, final discussion, assessment

Achim Landwehr (Mainz/Düsseldorf) presented a synopsis and a comment. From his point of view, discourse analysis is still flourishing, but studies are just not labelled as discourse analyses anymore. Landwehr highlighted that in the history of science questions about power and society have indeed been dropped. Most speakers had critically pointed at Rheinberger, Latour and the works of those who had stepped into their footprints, noting that they used theories and methods that did not take the presence and effects of discourses into account. Landwehr proposed to integrate the topics dealt with in the history of science into discourse analysis. Indeed, this would be a way to bring discourse analysis back in and at the same time open up new fields of research in the history of discourses. But Landwehr, on the other hand, also wondered what could be achieved by discourse analysis. He opposed the reproduction of grand narratives. Instead, he favoured »complexifying« histo-

rical processes and pointing out relationships between different elements – the latter a classical demand in discourse analysis as coined by Foucault. Furthermore, Landwehr, with a reference to Wittgenstein, urged to »make the visible visible« by applying discourse analysis and to reformulate theory through empiricism.

In a final discussion the question of why discourse analysis has passed the apex of popularity in the history of science was taken up again. There was a consensus on the thesis that it is not favoured anymore because of political reasons: Politics and society ask and provide means for research on ›reality‹, there is a »hunger for the real«. What is more, only utilizable knowledge is accepted. Historical discourse analysis with all its premises and aims is completely opposed to these demands: It is not about what the world is like but how it has been made; it does not provide narrations that help to keep up identities but deconstructs them; it does not simplify but complexifies history, and that is: our lives. Thus, historical discourse analysis at its very heart is a critical method that calls into question the world that is usually taken for granted. And that is why it is a useful method that should by no means be neglected.

The conference, which attracted a great number of attendants who lively joined the discussions, showed that discourse analysis is not outdated and that obviously there is an interest in historical discourse analysis. But although the hosts and organizers of the conference did their best to create a good atmosphere by choosing an attractive locality and producing a coherent event by connecting the sections' papers, they did not achieve their main objective. First of all, most papers dealt with topics that are very common in historical discourse analysis. And, more important, there were hardly any reflections on historical discourse analysis as a method.

It seems urgent to ask why historians and researchers from neighboring disciplines are rather prepared to present their findings from discourse analysis than to scrutinizing and developing this (heterogeneous) method. The answer would by no means be one-dimensional. Still, the key element causing the reluctance in challenging discourse analysis by those who practice it might be the unchallenged perception of the person who

is regarded as its founder: Foucault's statements about discourses are widely adopted in a rather uncritical way. They are either treated as initial points that stimulate research or they are referred to as confirmations of empirical findings. Hence, Foucault is treated as an authority, although his thinking and his ›works‹ should be perceived – strictly speaking from the point of discourse theory – as effects of historical discourses. To deal with Foucault more critically would lead to challenging and developing historical discourse analysis: His writings on discourses are, as everybody knows, only a tool box. These tools can be rearranged and supplemented: by looking at the way that institutions work, scientists generate knowledge and media effect meaning.

Was war Bielefeld?

Eine ideengeschichtliche Nachfrage. Edited by Sonja Asal and Stephan Schlak – Bookreview

Axel C. Hüntelmann

The collected volume harks back to a symposium in Weimar in February 2007, held on the occasion of the introduction of the first issue of the *Zeitschrift für Ideengeschichte*. The different contributions try to figure out what »Bielefeld« was. »Bielefeld« neither refers to the medium-sized town in East Westphalia in Western Germany nor to Bielefeld University but to a certain strand in humanities, well-known since the 1970s for its theory-driven and interdisciplinary (historical) research, the so-called Bielefeld School. »Bielefeld« was not a consistent school building but a heterogenous set of theoretical and methodological approaches, rather a social construction than reality, rather an idea and a state initially attributed by others. Beyond this, the editors emphasize that »Bielefeld« was more than an attributed state associated with certain ideas and semantics: As one of several reform universities, »Bielefeld« was a cipher for the academic and intellectual condition of the West German state. In accordance to Hans-Ulrich Wehler, one of the most prominent representitives of the »Bielefeld School«, the editors of the volume focus on the heyday of social history from the early 1970s and the foundation of Bielefeld University up to 1989 respectively.

To understand the rise of Bielefeld University as the centre of West-German historiography and sociology, the authors analyze the horizon of expectations of the 1960s and the basic ideas of reform. Like at an academic laboratory, at newly founded Bielefeld University, the ideas of Humboldt ought to combined with the training requirements of a modern industrialized society. Despite many of the reform plans becoming already obsolet during the implementation phase, the microcosm of Bie-

lefeld University became the »secret capital of theoretical work« and of humanities. Despite the »style of thought« (p. 9) comprised different theoretical orientations and offered an academic home for completely different outstanding humanities scholars like Niklas Luhmann, Hans-Ulrich Wehler, Reinhart Koselleck and Hartmut von Hentig, the volume asks if there had been a collective thought style, unifying scholars and students across the different humanities disciplines. Which idea, which sociological, intellectual and political ideas did shape the foundation of Bielefeld University and then lead to the label »Bielefeld School«. The different range of essays, varying between objective analysis, subjective retrospection and private impressions illustrate the foundation of Bielefeld University and the then following two decades, the theoretical concepts and paradigms.

Hermann Lübbe's contribution sketches the scientific and political context of the foundation of Bielefeld University, the original idea of an elite university and the contingent developments that led straight away to adjusting these plans to the real needs of a West German university. Lübbe describes the foundation of Bielefeld University in the context of the general expansion of universities and mainly apart from the metropolises in the provinces. Newly founded universities did not become full-scale universities but universities with a special profile and a functional differentiation, compensated by the assignment of extra ressources. Helmut Schelsky further developed this concept to that of a research university for the eduction of elites: small student groups and an perfect professor-student relation, the regular shift between teaching and research and interdisciplinary exchange – which was intended to become reality in Bielefeld. Schelsky mainly managed the foundation of the university in Bielefeld, but shortly after its implementation the elite reform-university was caught up by reality. There was criticism of the elite concept, and there were demands for equality and equal opportunities at universites. Mainly the rising number of students in the 1970s led to an adjustment to the needs of a mass university. Lübbe appreciates Schelsky's merits with the foundation of a specialized research university and the Center

for Interdisciplinary Research (Zentrum für interdisziplinäre Forschung ZiF) and the appointment of excellent scientists as professors.

Clemens Albrecht comes to a contrary result in his essay about Schelsky and the spiritual physiognomy of political conversion. He presents his reflections while conducting some exams about a »thought reflex«, a specific thought style of West Germany that he called the »Bielefeld-Syndrome«, generated from a spiritual physiognomy of a certain political conversion. According to Albrecht, the foundation of Schelsky's reflection on higher education and science policies was social reality. Concepts of science and eduction had been adjusted to the needs of modern society. As part of industrial society, culture means education towards functional objectivity – in contrast to Friedrich H. Tenbruck, who saw a need for a distanced science that enshrines the potential for the development of alternatives and cultural self-stabilisation in a scientificated society. Albrecht calls Schelsky's concept, the adjustment of an idea to social reality, the »Bielefeld-Syndrome«. In its pahological form, the openness towards approved methods is tranformed into hypostatized adjustments of reality. Albrecht deduces the keenness on adjustments in regard to reality to Schelsky's own past during the NS-era and his successful political conversion after the war. This conversion leads to preemptive obedience not only towards old and new authorities after the war but also towards new social realities that were condensed into terms like »Modern« or »Industrial Society«, »Globalisation«, »World Society« or »Knowledge Society«. These ideas became a normative bondage for the scientific community. This thought style, originated by politically converted intellectuals like Schelsky, has been adapted in Bielefeld and became ubiquitary then. Even more, this thought style has become a specific pathology of West Germany and has created a political style characterized by Albrecht as »adaptive modernisation« which these days comes to its limits.

Another leading figure besides Schelsky was the educationalist Harmut von Hentig who defended Schelsky's ideas and achievements. In the social upheavels of the 1960s as a social reality, Hentig argues against the »Bielefeld Syndrome« that Humboldt's idea of »solitude and freedom« as

keywords for autonomous research had been implemented as best as possible by Schelsky. He transformed necessary change into administrable reform. With the implementation of the university, the relation between idea and reality, the position of the idea, had been considerably improved. Rather, Hentig criticized that Schelsky's ideas of a research university, the premises for change, would never have been improved, would never have been realized if the given opportunities had not been grasped.

Interestingly, the three contributions vary in their judgement on Schelsky's committment and accordingly in their judgement on the foundation of Bielefeld University. Lübbe and Hentig, who were both themselves engaged in the foundation of Bielefeld University, defend Schelsky, while Albrecht, a follower of Tenbruck, critizes Schelskys for infecting Bielefeld and West Germany with the »Bielefeld-Syndrome«. Although one might argue in support of or against Albrecht's hypothesis, it is interesting that Albrecht continues a former discussion between Schelsky and Tenbruck. Independently, if one agrees with Albrecht's hypothesis about the »Bielefeld Syndrome«, one could ask if Albrecht is going too far. In his short essay he could not explain convincingly why this syndrome is typical only for Bielefeld and not for other reform universities like Regensburg, Bochum, Konstanz or Bremen. Political converts, as his description of Schelsky – probably not that prominent – might be engaged also in the founding committees of the other reform universities. Moreover, I would say that Albrecht overestimates the influence of Bielefeld. He stated that starting from Bielefeld the pathological form of that thought style has created a political style defined as »adaptive modernisation«. I would argue just the other way round that the idea of a modern, functionally differentiated society had become manifest by the foundation of Bielefeld University, and also that the foundation of Bielefeld was part of an »adaptive modernisation«.

Jürgen Oelkers in his essay analyses the educational reform of Hartmut von Hentig. Oelkers differentiates the two concepts of education, using the example of texts by John Dewey and Robert M. Hutchins. Dewey's pragmatic, empiricism-based pedagogy was observed at an early labo-

ratory school that was founded at the end of the 19th century and affiliated to the university of Chicago. Experiences were seen as part of the learning process, anticipating communal life in society, and they were seen as an »embryonic society«. Hutchins represented the concept of a humanistic idea of education which comprises a holistic personal development within the process of enculturation. Hentig synthesized both ideas: a humanistic education at a laboratory school that was affiliated to Bielefeld University. Furthermore, the foundation of an »Oberstufen-Kolleg« should interlock school and university. While Oelkers considers the laboratory school as a link between Paidea and pragmatism a great success, the idea of the »Oberstufen-Kolleg« was not realized as originally planned, and the »Kolleg« was transformed into a school comparable to the Senior High School.

The two essays of Markus Krajewski about the »intimicy of coding« and of Jürgen Kaube about Luhmanns slip box fit very well together. Both focus on the function of Luhmann's »Zettelkasten« and the interrelation between man and »machine«. Krajewski analyses the materiality of Luhmanns system of notes and which library-orientated and informational techniques of data handling and information processing were used: the slip of paper and the cards, the case, the writing utensils, as well as the system of notes and record-taking, tagging and the system of references. Krajewski cop a look at the self-description of the system, its design and the aesthetics of the production of annotations to highlight the internal communication and the interaction between man and »machine«. The self-referential system developed into an independent existence. In some way, Luhmann was dependent on his »Zettelkasten«, and the productivity of the »Zettelkasten« became itself an label for systems theory. On the one hand, the slip box made Luhmann independent of the library; on the other hand the »Zettelkasten« in his great extent tied Luhmann to the slip box and Bielefeld. The »Zettelkasten«, Luhmann was cited, was a reduction to built up complexity. While Krajewski focusses on the materiality and the communication processes, Kaube traces back the history of the slip box and he sketches how the »Zettelkasten« worked.

Two leading figures of the »Bielefeld School« were Hans-Ulrich Wehler and Reinhart Koselleck. In regard to the search for a collective thought style in Bielefeld as it is announced in the introduction, Frank Becker tries to identify – beside the well-known differences between Wehler and Koselleck – a common programme of the »Bielefeld School«. The differences between the two scholars start already with their different ways of socialisation and their careers that influenced also their historical work. Beyond all epistemological and contentual differences, they were connected by their interest in theoretical questions, their embeddedness in an integral social history, their aversion against narrative elements in history. In common had both the importance of the decades around 1800 as a boundary.

The essay of Wolfgang Braungart deals with the architecture and the buildings of Bielefeld University. As many others in this volume, Braungart appreciates the reform efforts along a modern university in an industrialized society, on the other hand he articulates discomfort with the cool and pedestrian rationality and functionality that is materialized in the university building. The university was intended as a closed system and planned as one university building, linked by Braungart to the concept of »the whole house« (*das ganze Haus*). All faculties, training and research should be accomodated in one building under one roof. All main university facilities like the cafeteria, the lecture halls or the library should be accessible via the main hall. The whole infrastructure, the arrangement of the seminar rooms and the offices was rationally planed within an alpha-numerical order. The concept of the university building provided a functional arrangement of all facilities, a training and research zone, flexibility and variability for future purposes and was supposed to leave the possibility for micro- and macro-expansions. Moreover, the architecture of the building and its surroundings should encourage a stimulating communication between students and lecturers, and the building should contribute to a stimulation and humanisation of the academic world of work. But this encouraging becomes a coercion to communicate, and finally Braungart doubts that the building invites to communication or even invites to stay. »Communication is everything.

The main thing is motion […] Arrival is impossible; being home not allowed. No place. Nowhere« (p. 60). Braungart did not like the university building. Usually, modern architecture becomes ever more ugly and unbearable when getting older (p. 37), and only from a wider distance one might like to look at the building, even if still one can see the similarity to a well-fortified castle (p. 52). Meanwhile, the building has become a restructuring case – and Braungart leaves open if this also refers to the concept of Bielefeld University as such.

In the last two essays, by Gustav Seibt and Valentin Groebner, the authors reflect on their own experiences during their studies and their time as doctoral students in Bielefeld. They try to memorize »what Bielefeld was like«. Gustav Seibt came to Bielefeld for two semesters, mainly to hear Koselleck, in autumn 1983. His first impressions of Bielefeld were depressing and he felt better only during the summer months 1984. He travelled from Rome to a dusty Bielefeld, »the air smelled like Waldsterben«. He stayed in a little appartment in a crowded house, on the central market square a so-called »die in« was happening, people lying like dead on the square, demonstrating against the NATO Double-Track Decision. He attended classes by Reinhart Koselleck, Niklas Luhmann and Karl Heinz Bohrer and discussed the ongoing political issues during a winter of moral uproar, followed by a lovely summer. What was left was the memory of the dawn of a new (political) era. According to Seibt, the remains of the depressing 1970s had gone and the 1980s started. The next ›contemporary witness‹, Valentin Groebner, came a few years later, in spring 1989 to write his doctoral thesis. He memorized the affinity resp. the fixation to theory. Bielefeld was the sound of multi-clause sentences saturated with theory, with references to keywords like »class«, »civil society« or »state«; the layout of tables and their representations in curves, pie charts, diagrams; and the ritual in the different colloquia with harsh discussions and the pride to be a »Bielefelder«. In Bielefeld, theory was *a sine qua non*. Bielefeld was finally less a place than a mode of self-placing within historiography.

The essays range from historical treatises, saturated with footnotes, to personal memories. Nearly all essays answer the question of »What was

Bielefeld« in their own, subjective way. Unfortunately the arrangement of the contributions is sometimes confusing: What is the narrative that links one article with the next one? Between Lübbe and Albrecht, both were linked by refering to Schelsky, the contribution by Braungart looks misplaced, and also the contributions of Hentig and Oelkers should have been arranged together. The collection shows the heterogenous ideas that where summarized under the label of »Bielefeld«. A certain strand in sociology, philosophy, history and pedagogy and literary studies – in short: humanities – that was often connotated with the use of theory and abstractness. Even more, Bielefeld was a space with a productive atmosphere and outcome, due to (or despite) a rationalized and functional organisation of work space and working facilities described very vividly in the article by Krasjewski. On the other hand, most of the articles describe »Bielefeld« in an ambivalent, if not in a critical or negative way (like Braungart, Albrecht, Becker, Seibt and Groebner). Whatever Bielefeld was: it was ambivalent and caused controverse emotions. »Bielefeld« was evidently linked and restricted to West German society, as many authors mentioned.

»Bielefeld« was well-known for the use of theory and a vivid and critical discussions. But some of the authors took umbrage at the often harsh style of criticism in Bielefeld. Wehler's ductus was combative (p. 102), his critisism of other methods, like cultural-, everyday- or intellectual history, was very polemic (p. 103). Sometimes the negative judgement on the »Bielefeld School« in some articles seems to be a continuation of a former discussion, but under opposite signs. Under the label of intellectual history, now social history is judged on. One gets the impression that still there are resentiments left between social or intellectual history.

»What was Bielefeld?« Was Bielefeld the »Bielefeld School«, or was »Bielefeld« identified with the style of thought, described in Albrecht's study. It is unclear if the editors meant »Bielefeld« or »Bielefeld School« when talking about Bielefeld. Would Hans-Ulrich Wehler, who would doubtless assign himself to the »Bielefeld School«, also think of Hartmut von Hentig as a schoolmate? Insofar, most of the articles refered to »Bielefeld« in a wider sense. But when talking about »Bielefeld«, many other

disciplines were dismissed: economies, law and the natural sciences that would all define themselves under the umbrella of interdisciplinarity – that had become a label for »Bielefeld«, institutionalized in the Center for Interdisciplinary Research. Here it would have been very interesting to see how these blind spots fit to the concept of »Bielefeld«.

Although the short introduction tried to embrace the varying articles and to focus them on the question: »What was Bielefeld«, the answer is not yet clear – some of the contributors did not even answer this question. The essays by Hentig or Oelkers eather contribute to a history of the foundation of the university or to the history of their discipline. Lübbe tries to figure out the idea behind »Bielefeld«; Becker focuses on the respective ideas of Wehler and Koselleck; Groebner frequently gives answers to the question of what Bielefeld might be; or Albrecht answers the question explicitly by a *tour de raison*, from Schelsky up to today. Finally Groebner's appraisal of »Bielefeld« as a mode of self-placing at the place of Bielefeld seems to be the best answer to the initial question. And if Bielefeld is a thought style, what has happened to this thought style during the last decades? What might Bielefeld be today?

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