

**“How can we tell it to the children?”
A Deliberation at the Institute of Social Research: January 1941**

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Abstract: To introduce an archival protocol of a “Debate about methods in the social sciences, especially the conception of social science method represented by the Institute,” held on January 17, 1941 at the Institute of Social Research in New York, the article focuses on certain conflicts in substance and terms of discourse among member of the Institute, with special emphasis on Franz Neumann’s distinctive approaches, notwithstanding his professed loyalty to Max Horkheimer’s theory. These are seen to arise not only from Neumann’s assignment as bargaining agent for the Institute and his distinctive relations with American colleagues, but also from their different orientations to the conflicted legacies of Weimar.

Key Words: Adorno, exile, foundations, Horkheimer, Marx, methodology, negotiations, Neumann, theory, Weimar

Much has been written about the transmission of the Weimar intellectual legacy to the United States through the activities of the 1930s emigrants, especially to the United States. Yet it is clear that Weimar was the scene of numerous conflicts among contesting tendencies that tended to be mutually shaped by the terms of their confrontations. Its legacies were bitterly contested. Re-contextualized within the American intellectual scene, where controversies had different configurations, these contested legacies took on new meanings, if they were understood at all. Such changes could be stimulating to the exiles as well as to their hosts, but they could also lead to mystification or to loss of depth, with esotericism contending with popularization. At a general level, for example, the rich and complex German debate between the proponents of *Wissenschaft* and *Bildung* as the prime mission of academic studies was adapted to the American conflicts between the defenders of the practical orientations of land-grant universities and the upholders of liberal arts, with dubious consequences for the clarity of the American discourse and for the depth of the other, which had figured importantly in wider debates about the character of theory and the constitution of knowledge (Kettler and Lauer, 2005; and Kettler and Wheatland, 2004). Within a narrower circle, similar discontinuities affected the attempts to translate the complex Weimar debates about Karl Marx, whose legacy had been debated in Weimar by a generation of intellectuals exemplified by figures like Max Adler, Karl Korsch, Georg Lukács, and Karl Mannheim and whose sophisticated and antithetical readings were equally remote from the ideological simplifications of the contending Weimar political parties or the range of possibilities entertained by American “progressive” thinkers. In New York, the capital of intellectual exile, the attempts to continue working through the contested legacy of Marx in a setting where the possibility of such work was in considerable measure dependent on the possibility of

having it recognized if not accepted by persons influential in American universities and funding agencies, there were two centers of contrasting émigré efforts to build on Marx, the New School for Social Research and the Institute for Social Research. Both have been the subjects of much study, but there has also been much myth-making, especially in the case of the Horkheimer group and its students, whose retrospective accounts are often led to underestimate the elements of internal uncertainty and conflict by the prescriptive model that portrays them as a “school” (Wheatland, 2009). The document we are introducing with these remarks provides a snapshot of the complex dynamics of this intellectual formation.

The Protocol of the “Debate about methods in the social sciences, especially the conception of social science method represented by the Institute,” held on January 17, 1941 at the Institute of Social Research in New York, documents a critical moment in the history of the Institute, as it struggled to find outside funding for research projects that would keep the group together after Max Horkheimer’s departure for the West Coast. Seen in a wider context, however, the protocol refers not only to joint deliberations on the pragmatic and tactical problems of presenting their work so as to increase their chances of gaining grants, but also to the participants’ differing understandings of the gap to be bridged between work consistent with their commonly avowed theoretical approach and the requirements of the social-scientific strategies sanctioned by American funding agencies, notably the Rockefeller Foundation.

Until the 1937 recession and the flood of new solicitations from desperate intellectual exiles after 1938, the Horkheimer group had been unique among exile scholars in being largely self-supporting,¹ with the supplementary help of housing provided by Columbia University. Accordingly they had felt free to carry out a program on their own terms, essentially continuous with their earlier work in Frankfurt, Paris and Geneva, and to publish their studies in their closely held German-language periodical. Within these constraints, they were eager to gain a constituency among American scholars, a number of whom allowed their names to be listed as an advisory committee; and they also cultivated as best they could their necessary Columbia connections, whose legitimation was very important (Wheatland, 2009). In addition to numerous philosophical inquiries, their work included, as it had done in Europe, research projects that rested on interview materials, but the treatment of these empirical elements was not oriented to American practice, it was not reflected upon in the theoretical articles that were the mainstay of the journal, and this phase of their work had been in any case under the supervision of the psychoanalyst, Erich Fromm, who had been led to break with the group in 1939. The economic plight of the Institute precipitated the decisions that proved unacceptable to precisely this member of the group, the one who had the most extensive American connections and fewest inhibitions about joint projects with American colleagues (Wheatland, 2009).

¹ In a letter to Nicholas Murray Butler written on January 25, 1935, Max Horkheimer claims an annual income of \$100,000, derived from two funds established by Hermann Weill at the founding of the Institute in Germany and extricated in good time from Germany. *Columbia University Archives* (formerly known as the “Columbiana Collection”), file folder 549/7-8.

The consequences of Fromm's loss to the Institute are implicit in the protocol to be reproduced below, as are the ambiguous marks of Franz Neumann's effort to take Fromm's place as promoter of research open to American scholars. By January 1941, when the debate took place, there was evidently a residual endowment sufficient to support the work of the five or six scholars closest to Max Horkheimer and a provisional plan to move the core group to California, where Horkheimer had decided to relocate in 1941, at least partly on medical advice. His prime hope was to devote himself, in collaboration with Theodor W. Adorno, to the philosophical underpinnings of the "critical theory" whose maxims he had been publishing in a series of articles.² These writings expanded on the legacy of Weimar post-Marxist disputes, which had sought to create a philosophically grounded alternative to Reformist, Orthodox, or Communist versions of Marxism, a contest whose terms and aims were essentially unknown among American scholars and intellectuals, who addressed a quite different state of the question.

At the same time, there was also a certain conditional willingness among the members of the Institute to widen its research constituency and reputation in the United States, an impulse that was unsurprisingly strongest among those who would find themselves separated from the Institute if this could not be achieved. No one illustrates this better than Franz L. Neumann, a comparative newcomer to the group but an important figure in the 1941 debates, who had initially been brought from England to provide legal advice and related administrative tasks, and who had been given a year's notice of cessation of his salaried status as early as September of 1939. From that moment, Neumann had been devising proposals for funded research for himself and playing a leading role in generating and promoting proposals for group research that began to be developed in the Institute in 1939.³ Certainly at one with the Institute group about the primacy of the Marxist legacy for social understanding and about his rejection of the laborist social democratic modes of appropriating Marx with which he was associated during the Weimar years, he differed precisely because he had come to this point by a quite different route and because he had a number of quite different, less philosophical questions to answer.

Unlike Fromm, whose access to American academic circles derived first of all from widespread local curiosity about the psychoanalytical approach and from his ingenuity in adapting it to questions about social change, Neumann drew strength from several sources. First, he possessed unique credentials as a result of his doctoral studies with Harold Laski at the London School of Economics, who was highly regarded by many American academics in politics and law. Second, he had rare and sought-after expertise about law and industrial organization in Germany. Above all, however, he was distinguished by his ingenious ability to turn encounters of many kinds into bargaining situa-

² See Horkheimer (1935a), Horkheimer (1935b), Horkheimer (1937a), Horkheimer (1937b), and Horkheimer (1940).

³ Letter from Franz L. Neumann to Max Horkheimer, September 24, 1939. *Max Horkheimer Archiv* (hereafter cited as *MHA*) at the Universitätsbibliothek, Wolfgang Goethe Universität, Frankfurt-am-Main, VI, 30, 124-6. He gives notice of research proposals to the Spelman Fund of the Rockefeller Foundation (Labor Law) and the Guggenheim Foundation (Natural Law).

tions, as well as his skills as negotiator. He had been a labor lawyer before his exile from Germany, and the special character of collective bargaining in that field is that contested situations of many kinds have first to be defined as negotiable, and that the aspects to be made subject to negotiation, as well as the parties to be included, are themselves a matter for a kind of meta-negotiation before and after. Since the collectivities involved are also commonly subject to internal dissent, especially as negotiations proceed, it is also a feature of this process that the parties must intermittently negotiate among themselves to define and then to adjust their bargaining positions. The recourse to intermediaries and the management of various alliances further complicates this style of action. Neumann's disposition to adapt the style to intellectual relations enhanced his value to the Institute, but for such core figures as Horkheimer himself and Adorno it also clashed with the deeply rooted preferences for a humanistic style that depended more on aristocratic authority to resolve conflicts. It is not far-fetched to say that if Neumann sought to import into the January discussion a model of internal bargaining preliminary to collective bargaining, notably the determination of a starting point for negotiations with "Americans," as well the limits of flexibility as to what may be put "on the table", most of the others preferred to see the event as a consultation of a leader with his liegemen, with the question of humanistically sanctioned authority always uppermost (Kettler, 2007).

A retrospective and much simplified rendition of this bargaining dimension may be found in a lecture that Neumann gave a dozen years later on the receptions and adaptations that led to the "happy solution" that the American universities provided to his cohort of émigré social scientists (Neumann, 1953). Bred to history and theory, Neumann says, the German émigrés initially disparaged the empiricism and pragmatism of American scholarship, and they were confronted with a choice rather than a simple welcome. Some exiles attempted to make a total change, according to Neumann, to become intellectually like the Americans, as they saw them. Others simply maintained their previous positions and sought converts—or accepted the status of recluse. Here he may have been thinking of a characterization that Paul Tillich, in a document known to Neumann, clearly intended to apply to the Horkheimer Group, when he spoke of a "group [that] thinks it likely that spiritual life during the next historical period will have to be rescued by esoteric groups from destruction by liberal skepticism, anarchic disintegration, and authoritarian suppression."⁴ From Neumann's point of view, however, the optimal strategy had come to be one of attempted "integration" between the two cultures. To explain this possibility, as it applied in the social and political sciences, he essays a rather simple historical characterization of the contested legacies from Germany.

The starting points are, first, the traditions of scholarship and, second, the great systems achieved during the long nineteenth century, Kant, Hegel and Marx, as well as their counter-systems in the work of Nietzsche and Freud. In the universities, however, both Kant and Hegel were transmuted into conservative stereotypes, remote from actual

⁴ (Tillich, 1938). For the exchanges between Tillich and Neumann (speaking for the Institute) on this and related issue, see (Kettler, 2008).

conditions, while Marx and Freud were simply excluded. Nietzsche was turned into his own opposite, Neumann says. The great achievements of the universities were in history and law, which could be done by book learning and speculation, without reference to social and political reality. Social and political science were thus outside the university, except for Max Weber, whose actual empirical work, like his emphasis on the social responsibility of scholars, was neglected in favor of the much more uncertain preoccupations with methodology. It is only in the United States, Neumann asserts in an aside, that Max Weber comes into his own. There were some great social and political scientists in Germany in the middle of the nineteenth century, whose work in fact inspired the first political scientists in America, but this Liberal current succumbed to Bismarck after unification, and Liberalism was reduced to the defense of private rights in the *Rechtsstaat*. Jurisprudence replaced the political and social science that had inquired as well into the sources of law in relations of power. In short, the émigré political scholars could find in the American universities the focus on training social and political scientists concerned with the reform of society and politics, which was missing in the German universities after Bismarck.

Neumann contends that persons like himself, trained in the German tradition, were able in turn to achieve two things. First, they brought skepticism about the ability of social science to engineer change. In making this point, Neumann does not mean to disown the radical projections of Marx and similar European trends, although he contents himself with a certain ambiguity, but to question engineering models of social transformation. Most important, he claims that the insistence by himself and his cohort on a theoretical framing of empirical research averts four capital dangers in the American pattern of social science. First, there is the overstressing of data at the expense of context and especially the historical frame. Second, according to Neumann, there is the transformation of the scholar into a functionary, constrained by the techniques of data collection. And finally, citing a consideration that played an important part in his own early life as researcher in America, there is the dependence of the scholar on funding sources. It is not only an opportunity but also an obligation, Neumann says, for the émigré scholars to bring their backgrounds to bear on minimizing these threats. In return, American social and political science teaches them a “concern with and analysis of the brute facts of life.” That is a mutually beneficial bargain, Neumann thinks, although he does not at this point use the language of negotiations that is elsewhere so pervasive in his discourse on these subjects. “Integration” is in effect a fair deal, and the success of the intellectual emigration is the result less of an immediately compatible environment, as had been argued by Hans Speier (Speier [1937] 1952), than of a promising setting for negotiations.

Neumann’s concept of a “theoretical framing of empirical inquiry” is hardly self-explanatory, however. In the setting of a benign retrospective, he was not constrained to offer an explanation, but the difficulties in the way of any such “integration” for Neumann as well as for others associated with Horkheimer’s Institute in New York appear clearly in the internal discussion whose transcript we are publishing in translation in *Eleventh Seal*, a unique moment of collective reflection on the relationship between

the theoretical inquiries that engage the philosopher, Horkheimer and his closest associate, Theodor W. Adorno, and the “social research” that belonged to the mission of the Institute, both in its institutional terms of reference and in the collective (and individual) searches for recognition in a scientific culture that required expertise, usually measured by the expansion of empirical knowledge, as a condition of legitimacy. The immediate occasion was the prospect of submitting a major funding proposal to the Rockefeller Foundation, which had never supported the Institute, although it was open to a competing group of exiled social studies investigators at the New School⁵; and the discussion consequently fluctuated between rhetorical questions about ways to state their case to the foundations without a demoralizing breach of trust, on the one hand, and, on the other, substantive questions about the relations between theory and “research” in the view of participants. At the same time, as noted earlier, the dynamics of the event are conditioned by a certain largely tacit conflict about the kind of discussion they were having and the priority of aims that the alternatives entailed.

Horkheimer set the terms of the discussion by characterizing the small and uncertain opening he thought available for securing support from the American foundations. They are evidently no longer satisfied with “empiricism” alone, he noted, but increasingly recognize the importance of “theoretical viewpoints.” Despite a continuing skepticism among most social scientists, accordingly, the foundations are ready to consider the theory the Institute might bring. In fact, he thinks, they are even being invited to propose a methodology that might serve as a “model.” Yet American social science in general—and presumably the evaluators for the foundations—also insists that theoretical claims are “hypotheses” that require verification by empirical research, a methodological conception that is antithetical to that of the Institute. The primary question of the consultation is whether the group can explain its method so as to overcome this obstacle and to seize this opportunity.⁶ Simple adaptation to American ways is not an option,

⁵ For support of the New School Social Science programs, see *The Rockefeller Foundation Annual Report 1940*, 262-3 and, especially *The Rockefeller Foundation Annual Report 1941*, 234-5. It may be that preliminary reports of the grants to the New School helped to persuade the Institute group of the possible openings. In the end, the largest award was made to a study of labor in Germany and Russia, a topic close to Neumann’s own share of the collaborative Institute proposal.

⁶ Although the debate kept returning to the problematic place of “theory” in American sociological research, it is striking that the discussants in fact largely disregarded where that question stood by the end of 1940. This is especially striking in view of the fact that both Horkheimer and Neumann were acquainted with Louis Wirth since 1937, at least, and that Wirth was an exceptionally influential sociologist at the University of Chicago who had devoted himself to expanding the role of sociological theory in empirical sociological research since his Rockefeller year in Germany in 1930. He is best known as the editor of Karl Mannheim’s *Ideology and Utopia* (1936), but he was no less interested in bringing Mannheim’s theoretical opposite, Leopold von Wiese, into American discussions, and he arranged for a year-long visit by him in 1935-1936, after it was clear that von Wiese would not become an exile from Nazi Germany. And Wirth was especially active in the wider-ranging effort initiated in 1940 by the Social Science Research Council under the heading of *Critiques of Research in the Social Sciences*, highlighted by advocacy of qualitative research by Herbert Blumer and others. It is possible that reports of this ferment were behind Horkheimer’s opening statement, yet the debate avoids an encounter with the new trends, except for the figures that Neumann introduces in the discussion, and the debate certainly remains distant from such works as Talcott Parson’s attempt to integrate Weber and Durkheim into American sociology in his *Theory of Social*

although the discussion shows that there are enough ambiguities in both the shared theory and in their understanding of American practices to permit differences in the views of the boundaries that may not be crossed.

Two group projects are in the background, both designed in the course of the preceding year with a view to external funding and both subjected to signs of trouble. The first was initiated by Horkheimer and involved a kind of structural analysis of the anti-Semitic belief-system based on leading anti-Semitic texts, to be applied for identification of the character (Wheatland, 2009). For obvious reasons, this project looked for its funding to Jewish organizations, notably the American Jewish Committee, and the original scheme was scheduled for a complete reworking after clear signals that it would not be supported because remote from needed and useable information.⁷ The topic intended for the Rockefeller Foundation was an analysis of National Socialist Germany—with both genealogical and structural approaches under consideration—and the succession of project proposals devoted to this subject occupy at least six of the members of the Institute during 1940 and early 1941, as well being the principal focus of Neumann's efforts during this time, both as planner and as promoter.⁸ It would be only a slight exaggeration to

Action, which had already appeared in 1937 (Parsons, 1937). Interestingly, Horkheimer had received a six-page hand-written briefing paper on American sociology in 1938, in which it is reported that the widening scope of social problems in the United States was propelling sociology from its narrowly focused empirical studies, oriented to social engineering problems, towards Marxism and other structural and theoretical approaches. (MHA I, 22- 372-375) The author was Edward Shils, an assistant of Wirth and a social theorist, who had in fact done most of the work on Wirth's Mannheim translation, as well drafting as the script of Wirth's defense of Mannheim at the Sociological Research Association in 1938; and who went on to be Parson's collaborator. (Kettler and Meja, 1995) Horkheimer had the text translated into German, presumably for study by the group. Yet almost none of this is reflected in the repeated characterizations of American sociology.

⁷ Max Horkheimer to Franz L. Neumann, July 10, 1940 (MHA VI, 30, 117-8). Neumann was skeptical about Horkheimer's hopes of establishing the Institute as a major center for studies of anti-Semitism, in view of established expertise elsewhere, even after the focus shifted to a study of German policies and possible counter-measures; and he forcefully questioned the saliency of the topic as well after Theodor W. Adorno prepared a new proposal in the course of the summer of 1940. Franz L. Neumann to Theodor W. Adorno (cc: Max Horkheimer), August 14, 1940 (MHA VI, 1A, 21-23). Neumann's argumentative manner in this correspondence and elsewhere belongs to the bargaining style since it presupposes the mutual recognition that constitutes the relationship and expects to get as good as it gives. In terms of the humanistic style of competing authorities, his manner appears arrogant. Once the decision was made to pursue the revised project, however, Neumann played an important part in attempting to reopen the possibilities of AJC funding, but the effort failed and, as a group project, the topic was put aside in the course of 1941. Although a methodological manifesto that could be taken as the authoritative statement of conclusions drawn (by Horkheimer) from the January "Debates" was published as a sort of preface to the final draft of the early anti-Semitism proposal (Institute for Social Research, 1941). This text in fact appears first in a late version of the Germany study, to be discussed below, and it does not suggest that the anti-Semitism study was on the agenda of the participants in January 1941.

⁸ By the end of the Summer of 1940, there were two distinct project designs under development and the subjects of consultations with American scholars. A longer view of the origins of National Socialism went under the name of "German Economy, Politics and Culture, 1900-1933" and was to contain sections on the German economy, political history, labor movement, philosophy, and culture (Literature, Theater, Motion Pictures, and Music). A 55-page prospectus was ready in June. An alternative approach focused more narrowly on "The Collapse of German Democracy and the Expansion of National Socialism," and was

say that the January meeting was called to discuss a project in which Neumann played the leading role, and that it was designed both to see how its funding could be facilitated and how improper compromises could be prevented. It was not a simple matter to be viewed as Fromm's replacement as apostle to the gentiles.

About a year earlier, Neumann had spoken on the distinctive approach of the Institute at a round table discussion called "The Social Sciences, One or Many," chaired by Wesley C. Mitchell at the University of Chicago celebration of the tenth anniversary of its Social Science Research Building (December 1-2, 1939). In his report on the event to Horkheimer, Neumann notes that the discussants included both Mitchell and Robert Lynd, and he reproduces his own remarks from memory. Opening with a declaration that the Institute has the integration of the social sciences as its "very aim," he insisted that this implies a search for a "comprehensive theory of the rise of modern society, its structure and its prospective development." (MHA IX, 57a, 4b) This requires in turn the collaboration between a philosophical examination "of the traditional concepts and methods of the social sciences" and the recognition that "sociological work can only be fruitful if it is historical," citing Robert Lynd's *Knowledge for What?* as a supporting text. He concludes, "We are guided by the view which Adam Smith's Classical Economy and its critics held, namely that only on the basis of an all embracing theory of society can the specialization of the social sciences be overcome and unity among them be established." Neumann's use of Adam Smith (and such critics as Adam Ferguson) as a surrogate for Marx when talking with "Americans" was a source of amusement to his students a dozen years later.

Strikingly, Horkheimer's opening statement at the 1941 internal discussion is twice interrupted by Neumann, who would without an understanding of the context appear to be marginal to this discussion since his contributions to the Institute's work had been ra-

built around a diagnostic examination of six key areas and designed to yield a structural analysis of National Socialist Germany. Although Neumann reported greater interest in the second of these approaches among the social scientists he consulted, a number of them political scientists, Horkheimer gained the collaboration of Eugene N. Anderson in December of 1940 to serve as co-director for purposes of the application to the Rockefeller Foundation. Anderson was an intellectual historian of nineteenth-century German nationalism, whose interests can be gauged by an article he published in mid-1941 (Anderson, 1941). The less political design was progressively refined until it was submitted to the Rockefeller Foundation as "Cultural Aspects of National Socialism" in late February of 1941. Neumann continued to promote some variant of the Germany project for many months after the Rockefeller rejection, "negotiating" with influential academics and officials, aiming at other foundations. Neumann's own political and economic structural analysis of National Socialist Germany, *Behemoth*, was published in 1942, written without the disciplining internal consultations of Institute projects, but bearing many of the marks of the project he had sought to promote. As far as the correspondence shows, Neumann asked Horkheimer's advice on only one topic, which happened to be his definition of "bureaucracy," and in that instance, he disregarded Horkheimer's objections. See Neumann to Horkheimer, August 5, 1941 (MHA VI. 30, 48); and Horkheimer to Neumann, August 13, 1941 (MHA VI. 30, 40-43). Cp. Franz L. Neumann, *Behemoth* (1944) 368-9. Horkheimer in turn complimented Neumann in due form when the book appeared, but he also wrote a stern critique, which he did not mail. Horkheimer to Neumann, June 2, 1942 (MHA VI.30, 346-350). For detailed discussion, see David Kettler and Thomas Wheatland, *Franz Neumann as Political Intellectual*. Forthcoming.

ther unreflective about methodological issues. And, as noted, he was under intense pressure to find employment elsewhere, having been given a year's notice of dismissal from the salaried staff of the Institute more than a year before the date of this meeting. Yet his early intervention and distinctive role make it clear that the discussion was very much his business. As soon as Horkheimer says that they are expected to supply an explanation of their method, Neumann interjects that the explanation must not appear Marxist, and when Horkheimer refers to the empirical testing of hypotheses expected of them, Neumann moves the discussion towards an examination of this conception, maintaining that the social scientists do not know how to discover hypotheses but see them as grounded simply in preferences. As he proceeds, however, it becomes clear that he does not consider this "general consensus" to be immovable, but rather a position with regard to which there is also discontent, as witness the high regard for Veblen, as well as the writings of Max Lerner and—especially—Robert A. Lynd, whose critique of "positivism" in his recent *Knowledge for What?* he deems especially significant, perhaps because of Lynd's past reputation as an empirical researcher skeptical about "theory" (Wheatland, 2009).

Throughout the debate, Neumann is the participant who is most focused on avoiding grandiose formulations that deny all recognition to the Americans and thereby render them impossible as bargaining partners. He insists on defining the situation as a challenge to effective advocacy. At least twice, he responds to sophisticated articulations of critical theory maxims—one each by Horkheimer and Adorno—by coolly enacting the skeptical responses to be expected from Americans, as if they merited an answer, and the third time, after a proclamation of lofty principle by H. Weill, he insists that the task remains that of communicating their special qualities to the Americans, rather than reassuring themselves. Not surprisingly, then, Neumann is also the only participant whose statements are at times expressly opposed by others, once by Horkheimer and the other time by Adorno, although he pledges his allegiance to the common theoretical program. It seems that he was prepared to take risks in order to develop a negotiable bargaining position, focused on American proponents of "historical method" rather than on sociologists,⁹ an orientation that most of the others considered dangerous.

The third most active participant in the discussion, then, is T.W. Adorno, whose relationship to American social science and the prospects of any sort of mutual understanding are also the most antagonistic. While he concedes that the *avant garde* among American social scientists are no longer satisfied with merely amassing information, he also insists that their inability to comprehend genuine theory—as distinct from the hypothesis-verification model of Positivism—is too deeply rooted for any mediation. The Institute's task is to explain the pervasiveness of this uncritical and unhistorical conception and thereby to "roll up the postulates of positivism." Herbert Marcuse takes active part as well, but he appears closer to Adorno than to Neumann, who is known as his closest friend, in denying that there are any grounds for comprehension in the orientation that

⁹ For later developments of this strategy during Neumann's years at OSS, see Müller (2010). Cf. the attachment.

brings Americans to social science inquiry. They simply lack the “experience” that defines the state of the question and shapes the understanding of the Horkheimer group, a characterization that Horkheimer escalates to the contention that Americans and others grounded in bourgeois society are in fact altogether incapable of experience, in any meaningful sense of the term.

The most revealing series of exchanges in the discussion opens with a surprisingly diplomatic concession by Horkheimer to the effect that there would not be a difference in kind between an explication of their own method and that of American researchers, but merely one of precision, a puzzling assertion that is immediately followed by a proposal to examine the difference between themselves and “someone we find especially alien.” It is hard to credit or explain this qualified opening, since the rest of the statement lays out a position that Neumann properly calls in effect incommunicable to an American who does not already share the theory to which Horkheimer refers and to which Neumann also pledges his allegiance. Horkheimer begins with the assertion that they would never posit and empirically test a hypothesis if addressed with a question that their own theory could show to be falsely posed. To illustrate his point he turns to a question on a topic that figures largely in their proposed research on Germany and that will later lead to substantive disagreements between himself and Neumann, disagreements that may have been already prefigured in their discussions. The question is whether bureaucracy is a new form of rule. While the “alien” sociologist (who is an American) would turn the components of this question into a hypothesis and seek out new information to (dis)confirm it, Horkheimer and his associates would ask instead, he claims, whether something like bureaucracy could in fact exercise power when it is not and cannot be a ruling class, since it is not related to the economic relations in the way their theory shows to be decisive. The phenomena of bureaucratization have to be accounted for in relation to the historical development of the principal classes. Asserting that there would be no occasion to “collect an assortment of new facts” he concludes that “no problem of method could occur” because they have a well developed theory of society, which “the Americans”—no talk now of only those most alien—lack.

Neumann replies, as noted, that an American, not accepting this theory, would simply ask them to show why this theory is correct. Marcuse insists that the “state of the question” is grounded in the group’s “experience,” which no Positivist would have available. The economist, Henryk Grossmann, asserts their reliance on a theory of class society but then acknowledges rather naively that they are also unable to say how far this theory is true, except insofar as they call on the authority of Marx or on their historical experience. Horkheimer insists that the search for “evidence” leads in a circle, presumably because the formulation of the question defines what shall count as evidence. At this point, he takes up Marcuse’s rather phenomenological—or even Heideggerian—call for understanding grounded in decisive experiences, which renders the call for proofs irrelevant. Adorno presses on with a proposal to use their theory to expose the fear at the base of the demand for evidence, and Horkheimer proposes that it all about a profound fear that “humanity” hangs in the balance if empirical criteria for truth are compromised. Adorno sums up the critical theory perspective and Neumann promptly counters

in his acerbic manner: "That is all very interesting, but a vicious circle for an American." He follows this up with a proposal that they expand on their earlier public posture that they are distinctive by virtue of their effort to integrate the social sciences by adding the element of historicity, "which the Americans do not do," and separating themselves from sociology.

Horkheimer rejects Neumann's proposal as failing to reveal their "distinctive spirit," and he insists that they must openly profess their view that truth cannot be verified, a position that Neumann provocatively likens to the openness of Americans to intuitions that are not falsified by the fact that they cannot be verified. Horkheimer's categorical reply to Neumann's overall pressure to find a way of "telling it to the children" is the assertion that "the demand for communicability is best satisfied by Hitler" because of the faulty structure of the "experiences of most humans." Neumann's role in the discussion effectively ends at that point, and the principal accommodation to the initial question about utilizing the seeming opening provided by the foundations has to do with the idea of working on a small topic in a way that opens a view to the "entire line of approach." In the end, Horkheimer avows once again the distinctive character of their theory, which is tied to practice in a way that "we will not be able to say." "This is what makes these people so uneasy," he concludes proudly. In the end, the session appears to end with a profession of faith—and a certain denial of recognition to the "people" with whom negotiations would have to be conducted—rather than a bargaining position.

Yet that characterization simplifies a fluid situation. In fact, a methodological statement that would not have taken the form it did without the January debate was prepared afterwards as preface to the project on "Cultural Aspects of National Socialism" for its submission to the Rockefeller Foundation. After the rejection of the submission to the Rockefeller Foundation, this methodological statement was published in the Institute's *Studies in Philosophy and Social Science* as a preliminary to a documentation of the anti-Semitism prospectus, which is said to be postponed due to matters more urgent from the standpoint of American interests (see Max Horkheimer "Notes on Institute Activities," *Zeitschrift für Sozialforschung*, vol. 9, no. 1 (April 1941), 121-123).

The methodological statement comprises four theses, of which it is reassuringly said at the outset that "none of these theses will be treated as dogmas once the actual research is carried through." Two of the four recall suggestions that were put aside when Neumann advanced them at the January session. The first announces that "concepts are historically formed,... concretized in a theoretical analysis, and related to the whole of the historical process. In the second thesis, the argument expounding the claim that "concepts are critically formed" resembles the approach of Robert Lynd in *Knowledge for What*, so highly prized by Neumann:

Social theory may be able to circumvent a skeptical spurning of value judgments without succumbing to normative dogmatism. This may be accomplished by relating social institutions and activities to the values they themselves set forth as their standards and ideals.... The ambivalent relation between prevailing values and the social context forces the catego-

ries of social theory to become critical and thus to reflect the actual rift between the social reality and the values it posits.”

The remaining theses more nearly resemble attempts to reformulate in “American” language the holistic conceptions emphasized by Adorno and Horkheimer in the earlier discussion, inasmuch as “induction” is redefined:

Categories have to be formed through a process of induction that is the reverse of the traditional inductive method which verified its hypotheses by collecting individual experiences until they attained the weight of universal laws. Induction in social theory, *per contra*, should seek the universal within the particular, not above or beyond it, and, instead of moving from one particular to another and then to the heights of abstraction, should delve deeper and deeper into the particular and discover the universal law therein.

This is not the occasion to assess this formulation of the methodological program. The present point is simply to suggest that the outcome of the process we have examined confirms our initial claim that the Institute in New York hardly conformed to the general opinion of a unified “school,” and that the intervention of participants like Neumann opened questions that could not be answered *ex cathedra*. The children—or their guardians—sometimes talked back.

When the Institute, with its studies of anti-Semitism and “the authoritarian personality,” did hit upon a track that gained it both funding and recognition as making a contribution to American social research, it was for work that drew more on the current that Fromm had tapped, the contested legacy of Freud, than on their unresolved collective attempts to deepen and communicate their encounters with the contested legacy of Marxism (Ziege, 2009; Wheatland, 2009). The contributions to the latter development in America came at a later time from the heterodox efforts of Marcuse and, to a lesser extent, Neumann, as well as from the extraordinary backwash, two decades after the debates reproduced below, of the work that Horkheimer and his closest associates did after their return to Germany.

Debate about Methods in the Social Sciences

Especially the Conception of Social Science Method for which the Institute stands

INSTITUTE FOR SOCIAL RESEARCH

Seminar of 17 January 1941

Horkheimer: Today, the empiricists say that it is impossible to continue in a purely empiricist way. It is necessary to draw on theoretical points of view. On the other hand, there are people who totally reject empiricism. Now the American foundations want to see projects done that can serve as models for a methodology different from that which has been applied in this country up to the present. We are expected to supply a brief explanation of the way in which we understand the method of the social sciences.

Neumann: It is especially important that the explanation be formulated so as not to be Marxist.

Horkheimer: A widespread conception goes like this: We poor Americans may be industrious, know a lot, and possess good methods, but we have no great theoretical ideas. You Europeans arrive with your noses in the air and act as if you knew everything. What we expect from you are both the theoretical viewpoints that you have brought and their subjection to empirical testing—e.g. your conception of class struggle. Undertake research and show us that the class struggle does in fact have a decisive importance in the interpretation of contemporary social happenings. But these researches should go beyond the mere collection of materials. I believe (H.) that this point of view contains an error about the method.

Neumann: The general consensus is that it is necessary to have a working hypothesis, but it is not known how this can be discovered. That is [taken to be] a question of preference and attitude.

Horkheimer: You are completely right that this is not our method. What is our method then? I would not deny that we also approach a given material with certain conceptions. This does not distinguish us from most Americans, although there are many Americans who have no conceptions beyond the method, but simply a research program.

Gumperz: We should not make things too easy for ourselves. This is not the case with leaders in the field.

Horkheimer: Quite likely not psychologically, but the[ir] theory shows that [the conception] is not uncovered. There is contempt for the necessity adhering to the contents of the hypothesis, a necessity that applies beyond the sphere of the investigation.

F. Weil: Don't Americans simply reject all hypotheses and demand "unbiased" approaches to their research?

Adorno: I think that it is as Gumperz says. The avant garde discuss hypotheses but the normal American "research-men" are supposed to approach the matter "unbiased" and they reject hypotheses.

Neumann: This trend is not preponderant any more.

Adorno: In the field of the social sciences, it certainly remains the case. Another American trend now cultivated is what is called content analysis, according to which one first analyzes the stimuli that influence the subjects. In my opinion, the theoretical approach does not go any further.

Neumann: cites the example of Thorsten Veblen: the great interest in him contradicts Adorno's thesis.

Adorno: Veblen is considered a heretic.

Gumperz: Not any more. Veblen has become an academic deity in his lifetime, if also in a much revised and softened version, while he was earlier prevented from teaching.

Horkheimer: As far as I understand the theory that is generally accepted in this and other instances, it is the case that one has a hypothesis and attempts to order the facts on this foundation and that one then takes the instances that run against the hypothesis and reconstructs it accordingly.

Neumann: That is a widely prevalent trend. [Robert] Lynd's "Knowledge for What" already represents a declaration of war against it. The thesis is: nothing is gained through hypotheses. That is a positivistic method. It is necessary to extrapolate a value system out of the tendencies in American society.

Gumperz: ...but this is nothing but a repetition of Veblen's theory.

Pollock: [Wesley C.] Mitchell has spoken vehemently against it.¹

Gumperz: ...but absolutely affirmed it [?] in his essays, etc.

Horkheimer: So how do things really look?

Neumann: offers an essay of [Max] Lerner as example: to construe contemporary problems so as to let the structure appear.¹

Grossmann: We are still faced with the task of formulating our method.

Gumperz: This cannot be done without confrontation with the other methods.

H. Weill: Every scientist has a longing to achieve understanding [*Erkenntnis*], but he is also, on the other hand, bound to the findings [*Erkenntnis*] of his researches. There can be no research without a yearning to know [*Wissensbegierde*].¹

Horkheimer: Whatever we may work out as our method will also be contained in the method of American researchers. It is impossible to make a strict division. The question is only whether we reach a more precise and better determination of our method than other people who have thought about method. I will try to set forth very simply the difference between the way we approach an inquiry and the way in which someone we find especially alien does so. It would never occur to us to construct a hypothesis because we find a quite specific state of the question [*Fragestellung*] already given. The question is posed: is bureaucracy in fact a new form of rule? We do not say then that bureaucracy is the form of rule and go on from there. We would rather revert to certain

conceptions of society that we already possess and ask ourselves whether it is at all possible to say that something like bureaucracy can exercise actual power? Or: is bureaucracy a class? And we would probably tend to say that what is bureaucracy must be understood first of all as the transformational process of the ruling class during the past fifty years, and that what is [properly] called a ruling class is linked to the conception of economic relations that we already possess. The new facts enter into the inquiry in quite a different way. We would not take the trouble to collect an assortment of new facts but rather ask ourselves what is the concept of bureaucracy when you fill it with historical contents and hold it up against the experiences of the past fifty years. We can do that because we have a certain theory that unites us. The Americans lack a stock of theory on which they could draw. That also explains the helplessness whenever the question of defining a theme arises. The problem of method only comes up when there is not an extensive supply of understanding [*Erkenntnis*] already available. (Conflict problem). If we have a definite conception of what society is and what its tendencies are, no problem of method would arise if the question were posed whether a rule by bureaucracy could occur. -- It seems to me, in sum, that one can conclude, first of all, that inquiries and the methods applied essentially depend on the extent to which a well developed theory of society is already at hand.

Neumann: That is fully in accord with my own views. The next difficulty is the difficulty of gaining an understanding. The objection will be made: what is correct about the theory on which you base yourselves? To come to such an understanding with the American who does not accept the theory is very difficult.

Marcuse: The state of the question [*Fragestellung*] is presented to us, so to speak, in the light of a certain experience. This experience is not however the experience on which the Positivists would call. What is the distinctive experience, then, on which we draw? What is it that we have already learned prior to this statement of the question [*Fragestellung*]?¹

Grossman: We have a theory of a class society erected upon profit. If we take that as our point of departure, we see the question clearly. How far this is true, we are also unable to say. We can either answer with Marx or we can say on the basis of historical experiences that this is substantiated, for example, by the class struggle.

Horkheimer: If you show up with evidence, you inevitably end up in a circle, since the evidence will inevitably contain elements that are just as uncertain. As soon as you arrive at decisive experiences, the other refuses to go along. Bourgeois society could almost be identified by the fact that the people share only the most pitiful impressions. But it is a matter of understanding, and when things are understood no one asks for proofs any more. Wherever structured experience is at issue [in our relations with these others], mutual understanding is cut off.

Adorno: So the outcome is the following situation: although we cannot get out of the circle of fact and hypothesis, and should thus not even let ourselves be ensnared by this issue, our theory can contribute, under appropriate circumstances, to our understanding of what determines the horizon of inquiry within which such questions of factual

provability assert themselves. We are in a position to say what it means that people view that which can be comprehended by evidence as the last and ultimate. While we cannot by virtue of critical theory take on the burden of proof, we can start from there to roll up the postulates of Positivism.

Horkheimer: Why is proof demanded? What is their fear? It is this: "If we no longer have the forcible separation of fact and hypothesis, humanism goes up in smoke. Humanity is at stake because it is necessary to be upright in science. One cannot treat as valid anything that is simply asserted. In contemporary society where there is no truth, we must be careful to avoid a return to the abandonment of criteria."

Adorno: Since we do not go along with this separation, we need have no fear of the fact. As dialecticians, we can come to an agreement with positivists on one point. What we have to do is not somehow to approach reality with ready-made hypotheses or systems in order to verify them. I would take up the proposed concepts anew, analyze them, and realize that the concept has indeed been defined but not thought through, being subject rather to a historical presupposition. I would carefully scrutinize all the concepts that emerge until the concepts themselves become fluid. What is new in this is that with such a critique we know what our aim is. It makes a big difference, however, if I begin in the sense of a hypothesis or finished theory, or if I am able to think so as to mobilize the force of theory. It is the driving force.

Neumann: That is all very convincing, but a vicious circle for an American.

Marcuse: We are supposed to say in three pages what our method looks like, but we have only criticized the others instead of examining our own method.

H.Weil: If we want to instruct people in the usefulness of our theory from our new observation point, it is best to state it as simply as possible, as if anyone could do it. Science is something--whether one wants it to be so or not--that can only thrive on a large scale, and if we want to enter into the game we can accomplish this only insofar as we think over the extent to which we are in a position to present it to the others so that they can understand it. It is a part of this that we hold certain concepts back [*festhalten*] for the sake of communications.¹

Neumann: This is not about working out our own method but about the question, "How do I tell this to the children?" Until now we have been satisfied to say that we seek to integrate all the social sciences. That does not suffice. The question is whether we can present our method so as to attack the hypothesis-fact problem. We distinguish ourselves from sociology in that we view all phenomena as historical phenomena, which the Americans do not do. We must emphasize that we are not engaged in sociological but in social-scientific work, and we must explain this. The difference is enormous, and we must show this.

Horkheimer: If we treat the problem as Neumann wants, something not quite fair will come out of it, in that we will again say something that in our opinion is not actually decisive. The others have a right to learn something of what makes up our distinctive spirit. (We do not understand history as [James Thomas] Shotwell does; we do not do soci-

ology but social sciences.) To bring out this unique element is very difficult, but it would be very nice if we could succeed in transmitting to the outside world something of the fact that in our opinion truth cannot be social-scientifically split into parts.

Gumperz: That truth is not verifiable.

Horkheimer: Once you begin with verifiability you may well say that worry about verifiability is a worry that often castrates the enterprise of knowledge [*Wissenschaft*]. For if I already split [it] into separate sciences, and then these sciences as well into separate elements... and beyond that one must also have thoughts and ideas.

Neumann: But the Americans also say that insights are not rendered false just because it is not possible to verify them.

Marcuse: First of all we must ask further how the experiences to which we appeal actually look.

Adorno: The project of showing how the distinction can be made already entails the theory as a whole. You can attempt such an undertaking, as long as you remain true to the dialectic, only if you refer such expressions to totality, even in the case of lies.

Horkheimer: The distinguishing feature of science is communicability. It seems as if at present that which is most false is also most communicable. The demand for comprehensibility is best satisfied by Hitler. The focus of the discussion is that it is said that one can sense a certain mistrust within the present-day social sciences. Is it possible to make some improvements in this? I wouldn't even open up the question of verifiability...it is quite secondary. The fact is that what is evil in fascism evidently rests in the fact that the experiences of most humans are faultily structured today, that truth and comprehensible messages are confused with one another, as well as immediate accessibility and truth. Communications are confused with contents in a mode that no longer requires thinking for oneself.

Seidenmann: The individual moments must be set forth. It would be the task to show which moments are comprehended and true within our own theory, and that the theory would be altogether impossible without such truth. Then it must be shown why history, for example, is essential to the theory to be presented.

Neumann: An anti-pluralist posture appears very important to me,

Horkheimer: The difference between sociology or the social sciences and what we do lies in that at least the American social sciences investigate recurrent happenings, and that they are in a certain sense natural sciences. For example, they are supposed to answer questions that cause problems in this society, which can in turn be solved by the intervention of administrators or the like, on the basis of the researches undertaken by the social sciences.

The problem of bureaucracy [for example]: one establishes the tendencies of the bureaucracy and shows how the rule of the bureaucracy can be contained.

Marcuse: Our experience sees reality in the light of these experiences. Our theory is essentially a theory of transformation [*Veränderung*].

Horkheimer: The distinction should be worked out.

F. Weil: But we would have to say something about our conception of history and the like.

Marcuse: A theory of transformation would have to be set against the theory of order.

Neumann: Order and transformation are not contradictories.

Marcuse: The concept of “social change” (sic) is a pure concept of order.

Neumann: Theory of transformation cannot be translated by “theory of change” (sic).

Horkheimer: In the concept of transformation there are two themes that we should hold apart: (1) Transformation in the sense of change (sic) and (2) dynamism, transformation without end.

Adorno: Method is really the substantive, complete understanding.

Horkheimer: What the American wants from us is that we work on a small topic in a way that reveals our entire line of approach. I believe that we can see something very positive in this. Investigations of small spheres of objects in which it can be shown that when I deal with a sphere this one aspect is illuminated without treating of the entire world.

Adorno: What distinguishes us from [social?] science is that, while it registers uncounted facts and codifies them and always has them at its disposal, it always in a sense forgets again what it has already possessed. It can repeatedly incorporate earlier results in a new investigation, but the manner in which it now thinks is indifferent to the fact that it has earlier come to know something. For us, in contrast, everything that once was there represents a continuity.

Horkheimer: We should present what we call theory in several decisive moments. Each individual inquiry should have a certain character as key to the total situation. Someone who reads it can then see that a challenge is being laid down here that is not satisfied in the average social sciences.

Pollock: Something must be said about the question of value freedom in science. This is taken to mean that every inquiry, regardless of its field, is equally justified, as long as it employs certain methods. We should take a stand on this. This question is long settled for us, but it is still at the center here.

Adorno: If we set forth our point of view, it is necessary to proceed in so differentiated a manner that it is true. We must say at this point that we reject value freedom as well as a science oriented to certain so-called values.

Horkheimer: We will not be able to say the most decisive thing: that we ultimately take the search for knowledge so seriously that the decision about our life and a shift in our entire life depend on it. That theory is connected to practice and that when our understanding is changed, our practice changes too. Practical and political seriousness still ad-

heres to science for us. The difference between the American and the European is that for us science is philosophy. It is possible to act on the basis of religious belief, purely subjective and unconnected value judgments, or theory and understanding. This is what makes these people so uneasy.