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THE HISTORY OF AN APPROPRIATION

The cooperation between Paul F. Lazarsfeld and Theodor W. Adorno in the Radio Research Project was just one of those cases where émigré German-speaking social scientists got involved in some sort of cooperation after they had come to stay in the United States. In Central Europe, co-authorship of publications by German-speaking social scientists was rare in the first third of the twentieth century, and the project as a specific form of research organization was no less unusual, if not unknown. The predominant form of publications was the single-author monograph, with the same pattern holding true for journal articles. The only existing forms of cooperative production were the *festschrift* and the proceedings of academic conferences, but even in these cases the common grounds did not extend in most instances beyond the book's hardcover. Textbooks written by more than one author appeared only when the original author had died and the textbook was updated by one of his followers.

In the United States, change had set in as early as right after the First World War, and with time the differences with the conditions of scholarship in Central Europe became all the more marked. The first sociological book that was written by two authors who became famous was published in 1918: William I. Thomas and Florian Znaniecki's *The Polish Peasant* (Thomas and Znaniecki 1918–20). However, this cooperation did not yet result from a 'project' as a novel form of research organization, but came about because Thomas needed a Polish-speaking partner for his study of Polish immigrants' documents. Other publications written by two authors soon followed: W.I. Thomas and Dorothy Swaine Thomas, authors of *The Child in America* (Thomas and Thomas, 1928), and Robert Lynd and Helen Lynd, authors of *Middletown* (Lynd and Lynd 1929), were married couples, with husbands sovereignly ignoring the alphabetic order of (first) names. Double authorship gradually gained right of place in American publications. There were textbooks such as Park and Burgess (Park and Burgess 1921); first monographs resulting from teacher-student cooperation – such as Sorokin's study on social

mobility (Sorokin 1959) or the migration study by Thomas, Park and Miller (Thomas *et al.* 1921); and large-scale cooperative efforts such as *Recent Social Trends* (President's Research Committee on Social Trends and Mitchell 1934), in which a special board was appointed to take care of the design and general management while responsibility for individual contributions remained with the authors. At the same time, author duos also began to contribute to journals.

With the rising number of cooperative publications that differed from the customary omnibus volumes by the facts that several authors were involved in the overall product and individual contributions were hard to identify, there was also a rising need to solve the problem of which order to adopt for citing the collaborators' names. Where the authors were married couples, decisions were probably reached without further discussion, and where both partners were fully aware of their status difference, discussions will have been similarly unlikely. But the less obvious the status difference, the more urgent the question of who was to precede whom in the list of authors. In projects with more than two collaborators, this was often further complicated by the fact that it was impossible to identify each person's specific contributions to the final product.

This chapter is about one of the first social science publications that was produced and published by a group of authors with hardly any status differences: *The Authoritarian Personality*, cited most often simply as 'Adorno *et al.* 1950' (hereafter TAP for short). The aim of the analysis is not only to identify the contributions of the four, or rather six, authors, but to go into some further questions, as well. What was the institutional context of this study, how was the project managed, what are its outstanding innovations with respect to methodology and content, and, of course, what was the reception of this widely appreciated cooperative effort, including its selective translation into German?

According to Lepsius (Lepsius 1981b), the specific contribution of the social scientists evicted by the Nazi regime consists in their contributions to a theory of totalitarianism. Table 6.1 shows the works referred to by Lepsius and their reception in the social sciences since 1981 (due to the absence of citation indexes, there was no way of establishing reception before this date). A small number of the works referred to by Lepsius is still frequently cited. Considering the unusual fact that even forty years after their publication, these works are still appreciated (which is amazing in itself given the alleged shortening of the 'half-life period' of scientific publications – 'half-life periods' of more than ten years are considered highly unusual), the impact of the social science analyses of totalitarianism proposed by emigrants can indeed be said to be uninterrupted. However, for the entire period in question, only two hundred and twelve articles referring to 'totalitarianism' in their titles have been registered in the journals covered by the SSCI (see last line in Table 6.1), which suggests that there was more to these works than their analysis of totalitarianism. The three books with the highest citation rates – Schumpeter 1942, Adorno *et al.*

Table 6.1 Citations of Selected Studies on Totalitarianism by German Émigrés, Social Science Citation Index 1981–2003

Author(s)	Book	First edition	No. of quotations in SSCI
Schumpeter, Joseph A.	<i>Capitalism, Socialism and Democracy</i>	1942	2018
Adorno, Theodor W. et al.	<i>The Authoritarian Personality</i>	1950	1421
Popper, Karl R.	<i>Open Society and its Enemies</i>	1945	504
Horkheimer, Max and Adorno, Theodor W.	<i>Dialektik der Aufklärung</i>	1944	482
Arendt, Hannah	<i>The Origins of Totalitarianism</i>	1951	471
Fromm, Erich	<i>Escape from Freedom</i>	1941	469
Hayek, Friedrich A.	<i>Road to Serfdom</i>	1944	440
Horney, Karen	<i>Neurotic Personality of Our Time</i>	1937	221
Mannheim, Karl	<i>Man and Society in an Age of Reconstruction</i>	1941	110
Neumann, Franz	<i>Behemoth</i>	1943	83
Voegelin, Eric	<i>Order and History</i>	1956	44
Mannheim, Karl	<i>Diagnosis of our Time</i>	1943	33
Fraenkel, Ernst	<i>The Dual State</i>	1941	27
Mannheim, Karl	<i>Mensch und Gesellschaft im Zeitalter des Umbaus</i>	1935	15
Rauschnig, Hermann	<i>Die Revolution des Nihilismus</i>	1938	14
Lederer, Emil	<i>State of the Masses</i>	1940	13
Plessner, Helmut	<i>Die verspäte Nation</i>	1959	13
Drucker, Peter F.	<i>The End of Economic Man</i>	1939	11
Neumann, Sigmund	<i>Permanent Revolution</i>	1942	10
Röpke, Wilhelm	<i>Civitas humana</i>	1944	5
Borkenau, Franz	<i>The Totalitarian Enemy</i>	1939	4
Loewe, Adolf	<i>The Price of Liberty</i>	1937	3
Mises, Ludwig von	<i>Omnipotent Government</i>	1944	2
Heimann, Eduard	<i>Communism, Fascism, and Democracy</i>	1938	1
Loewenstein, Karl	<i>Hitler's Germany</i>	1940	1
Rüstow, Alexander	<i>Ortsbestimmung der Gegenwart</i>	1951–7	1
Röpke, Wilhelm	<i>Die Gesellschaftskrisis der Gegenwart</i>	1942	1
Articles with 'totalitarianism' in title			304

1950 and Popper 1945 – are prototypical examples of works whose role can be described as seminal for the forming of schools or discourses. But of these only TAP is a publication that builds on empirical research.

AN ANNOUNCEMENT

The first issue of the last volume of the *Zeitschrift für Sozialforschung* included a number of articles from the Radio Research Project that were expected, by Horkheimer, to impress American readers and, by Lazarsfeld in his function as a kind of guest editor, to help reconcile empiricists such as himself with critical theorists.¹ After the last article that deals with the subject of communication, there is a new section, 'Notes on Institute Activities', containing, besides some introductory remarks, a synopsis of a planned study on anti-Semitism. Horkheimer, who signed the note, informed the reader that a research project on anti-Semitism had been elaborated by his Institut für Sozialforschung a year previously but had been suspended since the United States, due to the international situation, was facing problems of an otherwise urgent nature. Anti-Semitism was considered by most people to be a relapse into the Dark Ages. The institute, in contrast, meant to show that anti-Semitism constituted a danger that was inherent to contemporary culture. In these introductory remarks on the 'special nature of the project', there is more than one statement that strikes one as rather peculiar, as illustrated by the following quotation:

Several new hypotheses will be presented which are the result of former studies of the Institute, such as that progressive modern thought has an ambivalent attitude toward the concept of human rights, that the persecution of the aristocrats in the French Revolution bears a resemblance to anti-Semitism in modern Germany, that the foreign rather than the German masses are the spectators for whom German pogroms are arranged. (Horkheimer 1941: 124)

Horkheimer further wrote that in their effort to identify the deeper mechanisms of anti-Semitism, they were bound to insist on facts that would not meet with approval by Jewish people. For the sake of scientific truth, however, these had to be accepted; nor would they refrain from maintaining that the ideas of freedom, progress and enlightenment were nothing but catchphrases in modern society since the fascists were saying the same things. It would be easy to criticize these and other statements of the synopsis, but what is really remarkable is that, at the time, these invectives and speculations were not only jotted down on paper but were published with the aim of garnering support, which suggests that this type of reasoning could well be expected to meet with approval.

Besides studies in the history of ideas, two empirical projects were planned: a typology of anti-Semites and an experimental part. Horkheimer argued that success in the fight against anti-Semitism essentially depended on the ability to distinguish between the various types of anti-Semites, which was hard to do in daily life. Experimental settings that were as close as possible to real-life situations would be used to develop a typology. Furthermore, the project would try to provide insights into regional and social differences in the occurrence of anti-Semitism. What Horkheimer had in mind at the time was not the kind of experiments that were later conducted by Stanley Milgram and, as is

well known, were explicitly conceived as a sequel to TAP, but the production of films that were to be shown to various audiences.² First, participants' responses to the film would be observed during the presentation, then interviews would be conducted and, finally, participants would be asked to write down their impressions. The following example may give an idea of the plan:

A film will be made, showing boys of 12 to 15 at play. An argument and a fight ensue. The relation of guilt and innocence is difficult to untangle. The scene ends, however, with one boy thrashed by the others. Two versions of the film will be made. In one, the thrashed boy will be played by a Gentile, in the other by a Jew. Another variation will be introduced by showing each of these versions with two different dramatis personae. In one version, the thrashed boy will bear a Jewish name, and in the other a Christian name. Thus the film will be shown in four different combinations:

- 1) The thrashed boy is a Gentile with a Gentile name.
- 2) " " " " " " " " Jewish "
- 3) " " " " " Jew " " Gentile "
- 4) " " " " " " " " Jewish ". (Horkheimer 1941: 142)

The almost naive pleasure with which standard procedures of empirical social research are thus written down in so many words shows that Adorno had after all learned a number of things during his participation in the Radio Research Project: the need to specify the stimulus, the formation of control groups, the hidden observation of audience responses and the techniques of the indirect interview are presented with the typical pride of the new convert.

The publication of the synopsis primarily served the purpose of having something that could be presented to potential sponsors. However, Horkheimer himself was handling the matter rather nonchalantly, a fact which historians concerned with the institute attribute to his intention to reduce the number of its collaborators and to wholly dedicate himself, with the remaining staff, to his philosophical concerns (Wiggershaus 1986: 307–13). By the intermediary of Paul Oppenheim, who was a personal acquaintance of his and enthusiastic about the synopsis, he tried to win over the latter's cousin, Max Warburg, as a sponsor for the Anti-Semitism Project.³

Those members of staff who risked being sacked, and primarily Franz Neumann, saw a chance to secure their position at the institute by establishing contacts with some representatives of the American Jewish Committee (AJC). During these talks, it soon became evident that the sum of \$50,000 (\$720,000 in 2010) they had initially hoped for was unrealistic. The project proposal was reduced in substance as well as scope, and in the autumn of 1942 expectations for funding were downsized to a \$10,000 subsidy (\$144,000 in 2010), to be matched by money from the institute's own funds.⁴ Rather than focus on the ever-expanding parts that were conceived of in terms of a history of ideas, there would now be only four sub-projects:

- (1) A section on the political role of anti-Semitism
- (2) A section on current socio-psychological mechanisms utilizing anti-Semitism as their spear-head

- (3) A section on the position of labor as regards anti-Semitism
- (4) A crucial concluding section, drawing on all of the above and looking ahead to the post-war world.⁵

The outline is clearly marked by the hand of Neumann and is rather instructive with respect to the degree to which the state of social science methodology was taken into account, since there is nothing to suggest that the planned study of the 'attitude of different groups' of labourers would involve attitude measurement on the individual level. Neumann intended to analyze the programmes of various groups in the labour movement as if it was possible to extrapolate from the programmatic declarations of the elites to the common members' attitudes:

Both elements [lack of a well organized political labor movement and the presence of a considerable Jewish element within the working class] together make for a prevalence of a variety of attitudes and for the lack of a well defined general policy. Analysis is needed of how the organizational structure and the racial and social composition of the different American unions influence their stand in the Jewish question.

As for the study in social psychology, the synopsis explains that its aim was to analyse anti-Semitic literature and other sources such as jokes, 'moving pictures, cartoons and other iconographical material', in order to identify types of anti-Semites, as illustrated by a 'preliminary classification' that was also part of the published synopsis. Since preliminary talks with the AJC had shown that the latter was primarily interested in material that could be used for educational purposes and was unwilling to support the studies unless they would contribute to the fight against anti-Semitism, the types of anti-Semites were also presented as 'Types of popular responses to anti-Semitic propaganda'. The success of counter-measures was said to depend on how clearly the differences between the types were perceived.

On 21 October 1942, notwithstanding some reservations, the Administrative Committee of the AJC approved a 'grant' of \$10,000 (\$144,000 in 2010). Only after this decision had been made did a detailed critical review of the Anti-Semitism Project, written by historian Abraham G. Duker, reach the AJC. If the Committee had been able to take note of this statement before reaching its decision, the Project would not have stood much chance of being considered worth supporting.

The long interval between the date of approval and the signing of the contract thus seems to suggest that those in charge at the AJC, while holding themselves bound by their decision, had begun to have doubts. A phrase in the AJC's letter to the institute in which the agreements reached between the two parties were laid down suggests as much, since it stipulated that the institute had to prepare, within the first month, a 'general statement of the scope of the project, the fields to be investigated'.⁶ Requesting researchers to find out what they consider worth investigating in the first place was a rather obvious

divergence from the customary procedure, at the time, for awarding a research contract or approving a request for funding.

The objections raised by Duker, however, were such that an a posteriori concretization of the research project seemed inevitable. In his statement, the American historian noted nine 'major shortcomings' of the memorandum. First, the project was too strongly focused on Central and Western Europe, and particularly on Germany. However, in Eastern Europe the Jewish problem was somewhat different since in Poland, for instance, Jews were a national minority. Second, while the authors might well have a 'very good knowledge of general history', they were definitely out of their depth with Jewish history. Third, their 'predominantly' economic perspective led them to ignore certain important factors in the fight against anti-Semitism. Fourth, the authors' knowledge of the working class seemed deficient, leading them to overrate the role of the labour movement in the fight against anti-Semitism. No systematic distinction was drawn between ideologies and the 'individuals who profess them' – the American historian, unlike the authors who belonged to the sphere of the institute, was obviously familiar with recent social science literature that advocated attitude measurement on the individual level. Admitting only the Communists as a revolutionary party amounted to the 'willful ignoring of other revolutionary left-wing parties'. The observations on 'labor and Jews in the U.S.' were of too general a nature to be criticized at all. Fifth, although Duker's own approach as a historian was 'largely economic', he felt that the 'almost exclusively economic orientation' of the memorandum was an error. Due to this orientation, the authors failed to see the particular characteristics of anti-Semitism in cases where Jews constituted 'sizeable minorities', as well as where they had to be considered the 'spearhead of the cultural imperialism of the ruling groups'. Sixth, the part dealing with postwar anti-Semitism was 'nebulous', as well. No reference at all was made to Zionism and the mass emigration of Jews that was to be expected. The authors seemed to be disinterested in a 'specific solution of the Jewish problem'. One need not be a Jewish nationalist or a Zionist to realize that such a solution could well be realized within a democratic framework. Seventh, the relations between 'scholarly approach' and 'practical results' were particularly unclear with respect to postwar strategy. Eighth, 'many of the suppositions, particularly in the first part' were so clear and self-evident that it would be a waste of time to invest any more research efforts in this respect. Ninth, the authors 'have bitten more than they can chew'. Duker had – very roughly – counted no less than eighty-six individual research issues, and 'this in spite of the reservation given on page 2 that "the above examples are only a few of the situations that should be studied"'. Duker felt sure that in 1943 and, presumably, again in 1944, the AJC would be faced with 'new requests for a new subvention', a prediction that turned out to be true.⁷

THE ANTI-SEMITISM PROJECT, PART I

In the beginning of March 1943, the detailed negotiations were at last concluded, the delay being arguably due to internal AJC problems and a turnover

of AJC personnel. The agreement that was sent to the institute provided for a one-year duration and two locations for the Project.⁸ The New York group would be co-directed by Friedrich (now Frederick) Pollock and Robert MacIver, each dedicating part of their working time to the task. Originally, co-directorship had been proposed to Robert Lynd, MacIver's rival at the Columbia department of sociology who, however, had in the end declined for reasons of overwork. Lynd had been approached by Neumann, much to Horkheimer's annoyance, who had not been informed of this move. At the time, both Columbia professors were members of the institute's Advisory Committee and their names appeared, among many of others, on its official letterhead. The New York group was to have Leo Löwenthal as Pollock's assistant and full-time collaborator, as well as Arkadij Gurland and Paul Massing, also on a full-time basis. MacIver himself was to have two part-time assistants, one of whom was Isaque Graeber, the man who in the summer of 1941 had proposed to act as a fundraiser for the Anti-Semitism Project and to approach the relevant foundations and Jewish organizations.⁹

The California-based second group was to be directed by Horkheimer, with Adorno as his full-time assistant and two further assistants yet to be recruited. The list of prospective collaborators reveals that, at the time, social-psychological competence was not represented at the institute. After falling-out with Erich Fromm, no effort had been made to fill the void. Rather than hire new collaborators, the institute made do with a number of assistants who were confined to the institute's periphery and were recruited for tasks that were considered of secondary importance. Given that a general orientation to a social-psychological reinterpretation of some fundamental Freudian insights was one of the core competences of the institute, this is remarkable. The conclusion that suggests itself is that someone belonging to the institute's core group felt that Fromm's contribution could be substituted. It seems that Adorno thought himself up to the task, and was supported in this by Horkheimer.¹⁰

The contract specified that the institute was held to match AJC funds by its own means and that it was given one month in which to draw up a more detailed work programme, as mentioned above. From the very beginning, a one-year deadline was fixed after which six to ten reports had to be submitted, their publication being subject to an agreement between the contracting parties: 'No empirical data developed through the project are to be published without our specific consent.'

The terms of the contract were much more detailed and specific than those of, for instance, the Rockefeller Foundation, and this constellation corresponded to the rules that Adorno was later to denounce as characteristic of 'administrative research': 'The collecting of data ... for the benefit of ... planning agencies' (Adorno 1981: 303–4). That is, the contract unequivocally stated that the project was expected to be a 'useful undertaking' with the aim of being 'better prepared to forstall and to deal with the possible developments and manifestations in the area of anti-Semitism'. The only difference from the Radio Research Project that prompted Adorno to develop the concept of 'administrative

research' was that the planning agency was the 'Committee on Public Relations' of the AJC rather than the radio stations and some well-meaning *educators*.

Given the AJC's lack of experience with respect to the planning and conducting of social research, it was probably unable to foresee that it was completely unrealistic to plan for six to ten reports to be completed within just one year; and those among the institute's collaborators who might have been able to foresee this did not bother about realistic planning. In July 1943, Horkheimer admitted in a letter to Marcuse that 'the problem of Antisemitism is more complicated than I thought in the beginning'.¹¹ At any rate, meeting this arrangement would have meant that each of the five full-time and three part-time collaborators, backed up by their three directors, would have to come up with a report within just one year. Not surprisingly, after this year, only a fraction of the announced reports was completed.

After only half a year into the work of the Anti-Semitism Project, and just as Duker had predicted, Pollock approached the AJC to demand additional funds to extend the three studies. Due to the collaboration recently begun with R. Nevitt Sanford, enhanced support was mainly required for the interviews and the psychological experiments, and Pollock therefore asked for an additional \$5,500 (\$72,000 in 2010) to be dedicated to the project.¹² It seems that the request either failed to gain approval by, or was lost in, the AJC. At any rate, no trace of further insistence by the institute, on the one hand, or of AJC documents referring to the demand, on the other hand, could be found.

Meanwhile, Horkheimer had either become more interested in the Anti-Semitism Project or felt that the odds of being granted additional funds by the AJC were by now more in favour of the institute. At any rate, beginning in the autumn of 1943, he sought out social relations with people who could exercise a certain influence on AJC policies. A meeting brokered by psychologist Maurice J. Karpf with the Vice-President of the AJC, Morris D. Waldman, was particularly important. During a dinner given at his home by Karpf, who was in a leading position in a Jewish welfare organization in Los Angeles, Horkheimer and Adorno explained their previous work. Sometime later both wrote down their reflections in an aide-memoire, which they sent to Waldmann at the end of December 1943. What had been said during that dinner was, thus, transformed into a kind of interim report on the work completed by the institute after three-quarters of the project's duration. The New York group was investigating the following questions:

To what extent did the distribution of Jews in the different branches of German economic life affect the rise of National Socialism? What can we learn from the attitudes of the various strata of the German population towards the National Socialist measures against the Jews? What were the various concrete causes in domestic and foreign politics for the anti-Semitic steps taken by the Nazis in each year since 1933? Why were the policies of the German Centralverein ineffective, and how could they possibly have been improved? In addition, our friends in New York are studying the ways in which anti-Semitism might be used as a political instrument in post-war America. Attention is also devoted to the attitudes of the various Churches, and the possibilities and means of winning some of their forces over to our cause.¹³

In spite of the succinct wording, it can be said that the New York group was still proceeding along the lines of the traditional humanities and political science analyses that aimed to capture the attitudes of the various social strata in Germany; the same procedure seems to have been used to identify American religious groups. The above-mentioned tendency to view anti-Semitism in the Weimar Republic and Nazi Germany as a response to Jewish presence in various societal spheres has been maintained. The arrogant attitude towards the 'Centralverein' – Centralverein deutscher Staatsbürger jüdischen Glaubens (Central Association of German Citizens of Jewish Faith) – which had been founded in 1893 and was the only Jewish association tolerated by the Nazis after 1933, may be something of a surprise (it is not quite clear whether the planned study was to cover the period after 1933, as well). The double allocation of blame – presuming that the emergence of anti-Semitism in Germany was solely due to the intense Jewish presence in certain social milieus, and stating that the Jewish agencies had been inefficient in fighting anti-Semitism – echoes previous statements by Horkheimer on Jewish issues, now made into a dogma. Thus, in Horkheimer's 'Die Juden und Europa' (The Jews and Europe), there is the maxim – later often quoted in an abbreviated form – that whoever refused to talk about capitalism should abstain from talking about fascism. Those who liked to chant this sentence in the 1960s surely overlooked the one that follows it, with Horkheimer arguing in a crudely Marxist manner against the Jews as the forerunners of capitalism (Horkheimer 1939).

In the written version of his remarks of that evening, Horkheimer then turned to the work of the West Coast group where, in spite of the 'tremendous importance of economic and social tendencies', anti-Semitism was considered to be 'fundamentally a psychological phenomenon'. This was one of the 'destructive human attitudes' that were closely linked to other 'expressions of hatred' and was part of a neurotic syndrome that could with good reason be labelled as an 'anti-democratism'. The Jews and all the others for whom this was a threat were not the only ones to ignore the true nature of the destructive forces they were obeying, since this was true even for the anti-Semites and anti-democrats themselves. The totalitarian agitator knew instinctively how to utilize these forces. Those who wanted to fight anti-Semitism had to study these hidden psychological forces under the microscope, so to speak: 'Only when we know them as thoroughly as medical science knows the biological infectors of mankind, can we hope to discover effective remedies, to be translated into devices of education and other sectors of our cultural life.'

This certainly was a powerful way of putting things, but as an argument it was highly ambivalent. Horkheimer's initial statement that they did not want to downplay the social and economic role that was so pivotal to the work of the New York group is counterbalanced by a notion that refers to deeply rooted psychological traits, on the one hand, and chooses a name for them, on the other hand, which strongly suggests – no doubt even more so at the time than today – that this had something to do with each individual's free choice. Of course the 'ism' suffix has since then been so frequently misused that one

tends to forget that 'isms' originally described ideologies, or worldviews, to which one adhered, or refused to adhere, by one's own free will, a semantic change perfectly illustrated by the term 'anti-Semitism' itself. After all, the term started its career as a self-attribution of people who, while they probably did not want to be actually viewed as haters of Jews, still made no secret of their antagonism to them, as shown by organizations and parties that called themselves 'association of anti-Semites' or 'alliance of anti-Semites'. In the course of the twentieth century, the meaning was actually inverted, with the result that if today people say that they are not anti-Semites, this can indeed be taken as a clue to the existence of a distinct aversion to Jews. Today, almost anything to which one wants to attribute great importance, or of whose universal and widely received nature one is convinced, is dubbed an 'ism'. Thus, anti-Semitism was at least verbally, for the time being, identified as a current threat to American society. By adopting the rhetoric of containment and precautionary action that was natural to the AJC and other organizations of self-defence, Horkheimer was led to propose a theory to his clients that managed to present their fight against this threat as a task of universal historical scope, with only one thing remaining unclear, namely whether the discovery of the infectious disease would go along with the delivery of its remedy. The analogy with medical research with which Horkheimer let himself get carried away might have told him – if his only purpose had not been to find some catchy images – that the detection (of the causes) of an infectious disease by no means implies that the vaccine that would guarantee immunity against it is discovered in the same process. But the critical theorists were far from being daunted by this Promethean task.

Before giving the floor to his assistant Adorno, Horkheimer indulged for a certain time on his hobby horse, the experimental film, and then declared *en passant* that in Europe, and even in Germany, anti-Semitism was currently quite insignificant. The German population, terrorized by fascism, was craving democracy. However, after the end of the Second World War, anti-Semitism might easily be resurrected by 'a few seemingly unimportant steps', which could be prevented by preparing for them early enough: 'We are confident that the responsible military agencies can be made aware of the importance of these problems and directed to do the right things without putting an undue weight on the antisemitic aspect.'

Whatever this last half sentence is supposed to mean, it is clear at any rate that Horkheimer offered to act as an advisor to the future US military government in Germany. However, this self-commitment of critical theory to the new rulers – which, in spite of all the verbal distance taken from administrative research, did indeed come to pass – was still several years away.

Adorno's contribution was only half as long as that of his boss, and limited to a report on what the California group had so far accomplished. According to this report, radio addresses by a certain Christian fundamentalist – as one would probably call him today – preacher had been subjected to a detailed study by means of content analysis.¹⁴ The stimuli regularly applied by this

preacher were 'mostly of a psychological nature'. What the preacher, named Martin Luther Thomas, did was not so much address the objective concerns of his audience or try to rope them in for a concrete political programme, but manipulate 'their emotions and ... their largely unconscious desires which he attempts to lead into the channels of his organization'. Modern anti-Semitism was 'to a very large extent, a matter of conditioned reflexes rather than of aboriginal and spontaneous behaviours' (whether this was not already true for, say, the followers of the Wilhelminian court chaplain Adolf Stoecker at the end of the nineteenth century is an open question). Therefore it was particularly important to study both these 'conditions' and the "'reflexes'" which very often are but the automatic imitations of those stimuli'. A detailed study of this stimuli-reflex relation was necessary before any 'large scale field work' could be conducted. These future field studies, however, should not be limited to an investigation of the beliefs of the masses. This was not only difficult on the technical level, since respondents tended to follow a 'mechanism of "psychological censorship"', but in a deeper sense, as well, since fascism was not just an expression of what the masses were thinking:

Though the opinion of the majority certainly cannot be discounted, it is the ideology of the self-styled Fascist elite that is decisive. It moulds public opinion to a much larger degree than is the case in democratic movements. Hence the weight of the 'stimuli' produced by the 'leaders', and the importance of our knowledge of those stimuli, is disproportionately greater than we are led to believe by our democratic persuasions.

That the people who conducted public opinion polls were primarily interested in what was expressed by the respective majorities was one of the constants of empirical social research, as seen by Adorno. In 1943, however, he felt that the role of the 'opinion leaders' – he did not explicitly use this term from Lazarsfeld's study, but this is what he meant – was more important. The idea which, for the Lazarsfeld school, was closely associated with this, i.e. that the mass media did not directly influence their audience – not even in the case of fascist agitators – had not been adopted by Adorno before he left for the West Coast. His specific line of thought, i.e. that fascist agitators such as Thomas were able to influence their audience more deeply, is not enlarged upon in the aide-memoire, which says nothing of either the stimulus or the responses. The claim that this preacher was 'highly successful' in Southern California is all there is, allowing Adorno to speculate on the higher degree of anti-Semitism in Los Angeles.¹⁵

In a letter to his life-long friend Pollock, Horkheimer again referred to the reasoning he had relied on to convince the AJC's Vice-President: 'there should not be one Thomas study, but a series of similar analyses. To prepare the right antidote for a definite social illness, one needs even greater and better equipped socio-chemical laboratories, and at least as well-trained staffs as for the fight against physical illness or for the invention of new weapons in real warfare.'¹⁶

Coming from the very authors who never tired in their life-long criticism of positivist sociology for its naturalistic tendency, i.e. for dealing in the same way with the social and the natural world, the idea of socio-chemical laboratories for critical sociology seems rather incongruous. Horkheimer's desire to be able to direct a research organization like the one at Los Alamos, where at that time the atomic bomb was in the making, can hardly be taken seriously, and even less so considering his permanent alternation between megalomania and asceticism.

At least on one point, Adorno's worldview was more stable than that of the institute's Director, his senior by eight years, since even a quarter of a century later he seriously advocated putting all anti-Semites on the proverbial psychoanalyst's couch.¹⁷ Even in middle age, he still adhered to this scheme, albeit in a more modest form: it would suffice to provide them with a textbook.

At the end of February 1944, the institute submitted a report stating that it employed a total of twenty-two researchers, which was hardly consistent with the accounts presented in the course of the year, where only fourteen names had appeared. The grandiose claim of thirty-eight existing individual studies, the titles of which were listed in an appendix, does not stand up to closer inspection. In the said appendix, the 'studies' are classified according to whether they were 'completed', 'in the process of research' or even merely 'in the state of preliminary study'. Seven studies were included in the first category, seventeen were classified as works-in-progress and for the remaining twelve studies, a title was all there was. In trying to identify the seven studies that were said to have been completed by February 1944, one finds that none of them has ever appeared in print. However, given the inconsistencies between the list of collaborators and the list of studies, verification is all but easy (Table 6.2). Horkheimer's correspondence with the members of the institute abounds with clues suggesting that the studies that were said to have been completed consisted of no more than a bibliography or, worse, were in a state that made it seem advisable for Horkheimer to refrain from putting them forward at all. In June, then, the institute submitted a thousand-page brick to the AJC and considered this to be the 'report on the first year's work'.¹⁸

Among the full-time employees in the project, one does not find Graeber's name on the list of collaborators, while the names of some of those who had obviously done some kind of work for the project fail to turn up in the account sheets submitted to the AJC. Manuscripts that actually existed at the time, such as the one written by Else Frenkel-Brunswik on a preliminary study with a number of female Berkeley students, are not mentioned in the report although payment of wages to Frenkel-Brunswik had been made.¹⁹ All these inconsistencies are not addressed in the report, let alone explained. Similarly, there is no indication of what had become of the two sub-projects on anti-Semitism among American blue-collar workers that had initially been announced, or of the outlook on the 'post-war world' that had originally been advertised as a 'crucial concluding section'. In contrast, the tables were reversed, so to speak, and the institute pointed out to the AJC that the institute

Table 6.2 Collaborators of the First Anti-Semitism Project of the Institute of Social Research, made possible by a grant of the American Jewish Committee (AJC)

Collaborator	Location	Total Salary
Financed by AJC:		
Gurland, A.R., L., Ph.D.	NY	2,853
Graeber, Isaque	NY	2,145
Peck, George, Ph.D.	LA	1,575
Massing, Paul, Pol.Sc.D.	NY	1,226
MacIver, Robert, Ph.D.	NY	1,000
Dupont, Grete	LA	750
Simmel, Alice	LA	450
Total AJC costs		10,000
Financed by the Institute of Social Research:		
Löwenthal, Leo, Pol.Sc.D.	NY	4,800
Adorno, Theodor W., Ph.D.	LA	4,600
Horkheimer, Max Ph.D.	LA	2,400
Pollock, Frederick, Pol.Sc.D.	NY	2,400
Massing, Paul, Pol.Sc.D.	NY	1,560
Reinheimer, Jane, B.A.	LA	675
Brunswick, Else F., Ph.D.	Berkeley	600
Sanford, R. Nevitt, Ph.D.	Berkeley	350
Psych Dept UC Berkeley	Berkeley	500
Other costs		554
Total Institutes costs		18,439
Other Collaborators of the Institute:		
Edelheim, Margaret T., J.D.		
Freeman, Joseph, B.A.		
Kirchheimer, Otto, J.D.	NY	
Langerhans, Heinz, Ph.D.	NY	
Levinson, Daniel, Ph.D.	Berkeley	
Neumann, Franz, J.D.	NY	
Paechter, Henry M., Ph.D.	NY	
Porter, John, B.A.		
Roberts, Fred		
Tillich, Paul J., Ph.D.	NY	
Weil, Felix J., Pol.Sc.D.	LA	

had raised almost double the sum provided by the AJC. (This claim is incorrect since about half of the payments allegedly made by the institute were made to people who apparently were not working for the project, cf. Table 6.3). Moreover, the AJC was accused of having failed to fulfil its part of the cooperation with the institute:

The Committee did not find it possible to provide the kind of guidance, so vitally required, on specific aspects of the Project and its focusing on practical considerations. Requests of the Institute for intensive work-sessions with members of the Committee did not bear fruit; and we cannot be sure that any careful consideration has been given to either the suggestions or the actual studies made by the Institute.

Table 6.3 Studies of the Institute of Social Research during the First Anti-Semitism Project

Studies	Title (according to report 1944)	Presumed Author	Joint Meetings (Speaker)
I. The Danger	<p>'The unique character of antisemitism as an instrument in domestic and foreign politics'</p> <p>'Joseph E. McWilliams: An American disciple of Adolf Hitler'</p>		
II. The European Experience	<p>'A defense that failed: The policy of the Jewish Central-Verein'</p> <p>'Attitudes of civilians toward Nazi antisemitic policies, based on personal observations in a penitentiary, in a concentration camp, in a contractor's shop and in Berlin, discussion from 1933 to 1939'^a</p>	<p>Paul Massing</p> <p>Paul Massing, Heinz Langerhans, Heinz Paechter</p>	<p>'Analysis of the policies of the Centralverein deutscher Staatsbürger jüdischen Glaubens' (Gurland)</p>
III. American Antisemitic Agitators and their Followers	<p>'X from the West Coast'</p> <p>'George Allison Phelps'</p>	<p>Theodor W. Adorno</p> <p>Leo Löwenthal</p>	
IV. Potential Allies	'Catholicism and Anti-Judaism'	Otto Kirchheimer	'The Catholic Church and her Jewish Policy' (Kirchheimer)

Note: ^a In October 1943 *Der Aufbau* reported on a writing competition at the Institute on the theme 'Experiences with Nazi-Antisemitism'. Among the judges were: Manfred George, Max Horkheimer, Thomas Mann and Paul Tillich.

Still, for all their accusations of having been deprived of guidance by their sponsor, the institute seems to have had no doubts as to the continuation of their cooperation when this 'Note' was submitted to the AJC. Again, a number of rather vague projects were proposed for the next phase: an in-depth study of the 'nature of modern antisemitism, especially in its relation to the mind and emotions of modern man' was to be conducted, as well as an 'investigation into the roots of antisemitism among one or more specific social groups'. The AJC was invited to select those of the ongoing studies on the list in which they were most interested; a compendium on anti-democratic propaganda was to be published; and, last but not least, a research laboratory should be established 'for developing methods to measure the scope and depth of anti-semitism in the U.S.A. and to test in a continuous and practical way the efficacy of counter measures'. One outcome of this report was a seminar for both the AJC and the institute, offering sixteen lectures, between May and December 1943, by members of the institute as well as by close associates such as MacIver and Paul Tillich.

How was all this possible? Three factors suggest themselves as the most likely reasons: the turnover of personnel at the AJC; the negotiating skills of Horkheimer and his colleagues, who did their thoroughgoing best to exploit their cultural capital for maximum effect; and, finally, the AJC's lack of experience in the administration of research projects, combined with the strong appeal of social science research.

As a result of the high turnover rates among the AJC staff and the frequent reassignment of responsibilities among those AJC officers who were supposed to ensure the support, control and administration of the project, members of the institute presently found themselves in the role of having to fill in the new AJC officers on what the project was about in the first place.²⁰ This, of course, strengthened the institute's bargaining position, while the differences between this situation and the routines for controlling support measures that had been elaborated over the years by the RF, for instance, became all the more obvious. At the RF, a small group of officers was exclusively charged with the close control of ongoing projects, relying, among other things, on a system of written notes that had been elaborated for this purpose and that even after so many years allows readers who confront this material for the first time to get their bearings in a very short time. In contrast, such AJC documents as still exist remind one of those state bureaucracies that have long ago dispensed with the ideal of readability. What is laid down in writing is comprehensible only if one can draw on a considerable amount of prior knowledge.

In a world as confusing as this, poise, belief in one's own importance and a diffuse display of symbols of reputation – in short, impression management – tends to prevail over any other factor. The impression a person is able to convey in a specific situation outweighs his or her proven skills or achievements. When it came to acting on the stage of impression management, the institute indeed proved to be an expert player. Allusions to the prominent role the

scholars had had in pre-Hitlerian Germany and to the impressive, if somewhat nebulous, list of the institute's previous achievements in the field of social research were garnished with the names of prominent Americans who had supported their cause and now acted either out of solidarity with academics driven into exile or had themselves fallen prey to the critical theorists' impression management. All this was accompanied by emphatic demonstrations of economic independence which, due to the severe losses suffered by the various foundations on the stock market, was far from what it had been during the institute's first years of exile, it is true, but was nevertheless presented with an aplomb that would have done honour to a con man (Conwell and Sutherland 1937). (The resemblance of the manner adopted by the institute's negotiators with that of con men, who inveigle their victims into believing that they should be grateful even to have the opportunity to help the con men out of a momentary financial fix, is evoked quite deliberately in this context – a financial history of the institute would provide evidence for this down to the very details of book-keeping.) Of course, acting in this manner posed no difficulties for these sons of the German bourgeoisie. After all, Horkheimer and Pollock expertly knew how to maintain the impression, for their American interlocutors, that their sole desire was a truly altruistic one – namely, to ensure the continuation of fruitful work for their collaborators – rather than for their own private luxury, although moderation in their luxury was the last thing they would consider. While the institute complained to everyone who cared to listen that they would soon have to close up shop, Horkheimer was having a house built for himself in Pacific Palisades that well-befitted his rank and at the same time saw to it that Herbert Marcuse's and other collaborators' wages were cut.

Another factor that may have weighed in on their success with the AJC officers was that the great majority of the latter had either come to the United States as the young children of parents who had emigrated from Eastern Europe to the United States, or had been born in the United States shortly after their parent's immigration. Denigrated – also by members of the institute – as 'Eastern Jews' (i.e. Jews from Russia and Russian Poland), they were no match in terms of endowment with cultural capital for these sons of long-established German-Jewish families, notwithstanding their social advancement which in most cases was a result of their having graduated from a state college such as the well-known City College in New York.²¹

Finally, another reason for the AJC – no different in this regard from other Jewish organizations – to be set on establishing a research department of its own and on putting its trust in social research may have been that during the years right before and after the entry of the United States into the Second World War virtually every self-respecting public and private organization was doing just this. The prestige of the social sciences arguably has never been higher than in those years. The successful election forecasts of the opinion research institutes contributed to this as much as the research done by the Chicago School of Sociology, the incipient industrial and work sociology of the

Human Relations School, and – more to the point for Jewish organizations – the orchestrated success of the study directed by Gunnar Myrdal on racial discrimination in the United States.²²

In mid-June, 1944, the new AJC Executive Vice-President, John Slawson, unwilling to reject Pollock's and Horkheimer's argument that in the first year of their 'collaboration' the institute had spent more money on the Anti-Semitism Project than the AJC, granted a preliminary \$2,500 (\$30,000 in 2010) to enable them to go on with their work.

A NEW RESEARCH DIRECTOR AT THE AJC

John Slawson, Horkheimer's junior by one year, was six years old when he came from the Ukraine to the United States and, like many other new immigrants, had climbed the social steps that enabled him to graduate from Columbia University as a psychologist in 1927. He then started working for various Jewish organizations, and in 1943 became the AJC's Executive Vice-President. The AJC was not the only Jewish self-defence organization, and it was reputed to be not only the most conservative of them, but also the one where German Jews were in charge. The ascent of 'Eastern Jew' Slawson, however, shows that there were exceptions. In the spring of 1944, at his suggestion, a group of scholars met with officers of Jewish organizations in New York to discuss the AJC's future activities in the field of research on, and in the fight against, anti-Semitism. The Institute of Social Research was represented by Horkheimer and Adorno. Other participants in the two-day meeting were Gordon Allport, who in the next decade became the uncontested authority for social-psychological research on prejudice (Allport 1997); Talcott Parsons, who at the time was deeply engaged in projects doing research on Nazism (Gerhardt 1993; Gerhardt 2002); as well as John Dollard, Edward Bribing, Kurt Lewin and Rensis Likert, among others.²³ A similar conference, although not initiated by the AJC, was held soon afterwards on the West Coast, with most of its participants being exiled psychologists and psychoanalysts (Simmel 1993).

Horkheimer had the opportunity to quickly establish a relationship with Slawson that was favourable for his own interests as well as those of the institute. In June 1944, Horkheimer addressed several letters to Slawson that abounded in flattering allusions to their elective intellectual affinities. He tried to talk Slawson out of his desire to see the final report on the work of the first year and, at the same time, to persuade him to found a new institute:

If science is really to be mobilized against the menace we face, a research organization should be created with a minimum annual budget of at least \$ 100,000.-. For this purpose we could use either an already established institute like ours, which is recognized as a tax exempt educational organization, or a new set-up like the Institute for Research in Social Prejudice, as conceived by one of the friends of the Committee.²⁴

A few days later Horkheimer was invited to join the Scientific Department of the AJC as a 'research consultant in domestic defense'. This, however, was too fast a move for Horkheimer, and he once more tried to gain time before making his choice. In the end, he accepted the offer and on 1 November 1944 took up his new function as an employee of the AJC, eleven-and-a-half years after he had lost his position as a university professor at the University of Frankfurt. The document that records Horkheimer's appointment to the AJC states that 'Doctor Horkheimer and his staff of scientists will conduct their experiments, tests and surveys under the supervision of Dr John Slawson.'²⁵

In a letter to Adorno, Horkheimer described his working day as follows:

Most of the time, I am sitting in a rather nice office of the Committee reading some of the myriads of brochures that have been scattered all over the world in the course of the last year. I share the room with a secretary who is charged with taking my dictations and in her free time works for Dr. S[lawson]. This free time takes up most of her day since I very rarely dictate anything. My main activity is to attend meetings and have talks with individual members of the staff to whom I politely point out my opinion on the myriads. My negotiations in view of having an assistant have, for the time being, led to the part-time employment of one Dr. Kornhauser whom I got to know via Lazarsfeld. He has an excellent reputation as a public opinion expert. He is to work with me as of December 1st. During the first months we plan to elaborate a maximum research program and already initiate a number of studies on the effects of the Committee production. Furthermore, Hertha Lazarsfeld [i.e. Herta Herzog] has agreed to devote her evenings and her Saturdays to the testing of radio programs.²⁶

Arthur Kornhauser was the Chicago professor of psychology who had been Paul Lazarsfeld's co-author for the latter's first article on psychological market research after his transfer to the United States, a cooperation that continued in subsequent years. That Herta Herzog had only the evenings and the weekends to dispose of had to do with her job at the advertising and marketing firm of McCann-Erickson. The recruitment of these two collaborators strongly suggests that Horkheimer intended to make use of the 'like-dislike studies' so abhorred by Adorno in order to investigate the effects of the brochures and the radio programmes designed by the AJC.

Horkheimer's AJC activities predominantly consisted of the tasks that are usually assigned to the director of the social science research department of an organization worried about the effectiveness of its products. That the outfit in question was a not-for-profit organization such as the AJC did not mean that there was no need to optimize the resources involved and no need to have a research department of their own to do just this by testing the effectiveness of their 'products' before they were released. Horkheimer's own account to the Committee on Scientific Research of the AJC, which was something like a sub-committee of the board that was supervising the new department, says so quite unequivocally:

(1) *The testing of the Committee's projects:* Pamphlets, speeches, broadcasts etc. are to be tested in such a way that we shall endeavour to ascertain whether they are good or bad in their practical effect, and how they can be improved.

(2) *Group research*: Various strata of the population, such as labor or war veterans, or the Irish Catholic group, are to be studied by trained members of the group to interview their colleagues and neighbours, to discuss the results with our field workers and then to test out certain counter-measures.

(3) *Memoranda on practical questions*: The department should prepare theoretical analyses on important subjects with which the Committee is concerned ... for example, of ... a possible Jewish state in Palestine ... The foregoing projects should produce results within a year or less.²⁷

More time would be needed to push on with the two other tasks: sociological and psychological studies designed to identify prejudiced and non-prejudiced individuals and to submit them to follow-up interviews and detailed tests to find out how propaganda could be improved. This also included preparing and testing 'new weapons' for this purpose. The self-surrender of the critical theorist to the practices of administrative research went even further than this, since Horkheimer informed Adorno right after having entered into office that he considered, 'rounding up a group of psychologists and sociologists, to meet once a month and discuss our ongoing work ... I am thinking of four or five analysts, plus Lazarsfeld, Merton and one or two others from the faculty' of Columbia University.²⁸

He repeatedly presented this proposition to his superiors together with complaints about his lack of collaborators and the need to change this situation straight away. As a matter of fact, the department rose to a respectable size, and its collaborators included not only social scientists who would later become quite renowned, such as Marie Jahoda and Alvin Gouldner, but also, and primarily, junior American researchers such as Eunice Cooper, Genevieve Knupfer, Helen Schneider and Marion Schneider, who had undertaken their practical social science training with Lazarsfeld at the Office of Radio Research.²⁹ Besides Horkheimer, or his later placeholder, the associate director Samuel H. Flowerman, at least three other social scientists were working at the department at any given time. Their work consisted mainly in supervising the major projects approved by the AJC and in organizing the operations of minor research assignments which, however, were usually conducted 'outside'. In the first years, Lazarsfeld's BASR served as the department's test station. In a detailed memorandum submitted to Slawson in 1947 to justify his demands for pay raises, Horkheimer also gave a retrospective account of the beginnings of his activities in New York:

It was the Department's task to integrate the outside studies into a concerted effort of various teams. This required the exchange of staff members between New York and the projects, as well as trips of some of our experts. I was the more inclined to advocate this policy as it would have enabled us to gradually transfer certain parts of the projects to the Bureau of Applied Social Research which is the leading agency in some important methods of our research. Even now I consider the intensive personal cooperation between the Department, the various projects, Dr. Lazarsfeld and myself one of the most pertinent presuppositions for the success of our work.³⁰

Horkheimer's role was more similar to that of a department head of, for instance, the Rockefeller Foundation than to Lazarsfeld's role in the BASR. Unlike the latter, there was no need for him to bother with fundraising and with acquiring commercial commissions in order to be able to employ a troop of collaborators. As a department head, Horkheimer had to act within a battlefield consisting of the AJC boards, which were his superiors (he was expected to report to them on proposals that were submitted for approval, and to receive instructions by them); his collaborators in the department; and the external projects in Berkeley, Chicago and New York City. Unlike Lazarsfeld, Horkheimer was not allowed to make strategy decisions. He was a senior executive who could try to impose certain preferences of his own within his organization. While Lazarsfeld's role was more like that of an entrepreneur, Horkheimer's status resembled that of a member of a bureaucratic structure with its familiar command lines and office intrigues. Both, however, had a material safety net. For the former, this was a position, albeit poorly paid, as an associate professor at Columbia University; for the latter it was his access to foundation funds (dependent on the capital market) on which he could draw to have himself rather liberally supported.

Now, nothing would be more misleading than to conclude from all this that Horkheimer had changed sides, so to speak, and had agreed to be a science bureaucrat. His letters to Adorno at the time clearly show that he was far from enjoying his new task. But why, then, had he agreed to return to New York, sharing an office with a secretary and doing work he detested? Two reasons suggest themselves: negotiating with the AJC, he had failed to win them over for his plans, i.e. to have them fund the institute as an independent contractor, but he had, at the same time, so strongly insisted on his commitment to seeing this work through that he could not very well refuse the offer to take up the position of a research consultant without forfeiting his credibility. Here again, it is instructive to compare his situation with that of Lazarsfeld who, in 1937, as the head of the fragile Newark Center, was offered a transfer to the well-endowed Princeton Radio Research Project. Lazarsfeld, as shown in the preceding chapter, had from the very start of the negotiations sought to maintain his institutional independence whereas Horkheimer chose to try to temporarily secure financial support for his institute by accepting the position of an AJC employee.

Second, during the one year of the institute's more or less autonomous work on its own Anti-Semitism Project, Horkheimer must have realized that, all in all, the result would not really be up to scratch. Most of the collaborators of the years 1943–4 were either no longer available, including former collaborators Otto Kirchheimer, Neumann and Marcuse, who had been relegated to Washington, DC or had turned out to be incapable of going beyond the conventional analyses conducted in the humanities and in history, such as Gurland, and Massing and Heinz Langerhans. The strategy of reducing the New York branch to a mere organizational shell, which Horkheimer had pursued independently from his relations to the AJC, had now produced an

unwanted side-effect: there were no collaborators whom Horkheimer, Pollock and Adorno, who was steadily drawing closer to the hub of the circle, thought capable of conducting the kind of research that would help the institute to be appreciated by the Americans.³¹ Even though they more or less looked down on the American social scientists, they were well aware that for reputation's sake they had to produce (or have others produce) something that met the prevailing expectations and standards.

As a result of this constellation, Horkheimer found himself in a dual role that was novel and unusual for him. Formally, he was a medium-level employee of the AJC with superiors and inferiors and a more or less clearly stated work assignment that included treating the Institute of Social Research as a contractor. His heart, however, was still almost exclusively in the institute whose director he remained, which provided him with an additional income, and whose positioning and material safety he tried to promote. He and his colleagues of the institute's inner circle were no doubt much more aware of this position as a broker than his superiors and collaborators at the department.

The institute's grandiose promises of the preceding years could only be kept if it reverted to its previous policy, namely, hiring external experts to conduct and analyse empirical studies, as it had done with Lazarsfeld for the empirical part of the *Studien über Autorität und Familie* (Authority and the Family) (Horkheimer 1936) and with Mirra Komarovsky for the institute's only extensive American publication thus far, *The Unemployed Man and his Family* (Komarovsky 1940). But in 1944, hiring low-cost high-competence staff was not that easy anymore, as the experiences that had led Lazarsfeld to suggest that the Rockefeller Foundation suspend the Radio Research Project until the end of the war had clearly shown. The institute's own resources no longer sufficed to support the only asset they still had in the United States market: the Berkeley Public Opinion Study, the group around Nevitt Sanford, Daniel Levinson and Else Frenkel-Brunswik, who had written a voluminous 1,000-page text that had aroused greatest interest at the AJC. The institute had obtained this contribution for very little money (less than 10 per cent of the money paid by the institute was dedicated to Sanford's group, see Table 6.3). Unless Horkheimer was prepared to let go of this chance to succeed on the American academic market, he had to swallow the pill and try to use, at least temporarily, his own position at the AJC headquarters to make sure that things went the way towards the consolidation of the institute's position between the AJC and the Berkeley group.³²

The Berkeley group which, like Lazarsfeld, adhered to an epistemological orientation that was contrary to the anti-positivism of critical theory, seems to have been as inexperienced as they were disinterested in anything that had to do with research organization. Anti-Semitism as a research concern had not come to Sanford of his own initiative. In the spring of 1943, one Mr Blumenthau, a New York-based Jewish proprietor of a theatre, had donated the sum of \$500 (\$6,100 in 2010) to the University of Berkeley to enable it to conduct a study

on anti-Semitism (Levinson 1992). The university's administration approached the psychology department, which charged Sanford – still an associate professor – with the project, who in turn hired graduate student Daniel Levinson as his collaborator. Since the donation alone did not cover the costs incurred, the psychology department added to it from its own funds. This tiny project, the goal of which (as defined by the donor and remembered by Levinson) was to do away with anti-Semitism once and for all ('a rather grandiose idea'), resulted in the publication of the first anti-Semitism scale (Levinson and Sanford 1944). Both authors as well as other Berkeley psychologists felt that it would be interesting to go on working on the issue and, thus, came into contact with Horkheimer. However, the Berkeley group was so far away from the New York centre with its foundation headquarters and generous Jewish organizations that Horkheimer found it possible to interpose himself and the institute as an intermediary and to prevent Sanford and Slawson from communicating directly. The Berkeley group wanted to continue the psychological studies they felt to be promising. They needed some money for this, and the institute held it out to them. As long as the funds were coming in, the Berkeley psychologists could not care less about who acted as an intermediary.

Finally, Horkheimer used his stay in New York, which he kept considering to be temporary (in a letter to Adorno he refers to the 'nine months that we expect this to last'), to reactivate his contacts with Lazarsfeld and other social scientists, and exploit their expertise. Given that Lazarsfeld and his colleagues at the university, as well as the psychoanalysts Horkheimer intended to consult, were more than occupied with their own work, neither of them would interfere with the institute's plans: 'When I get back [to California], however, everything and not only the Committee issues should be ordered to the point of not having to bother about anything but our most immediate concerns. Thinking of this time gives me the courage in this horrible bustle and all the false talk and false actions to which it compels me.'

In the end, the nine months became many years of work for the AJC. Although after more than a year Horkheimer retreated to California, allegedly for reasons of health, he remained on the AJC pay roll until his return to Frankfurt in 1949. At first he retained a still princely, if reduced, yearly income of \$6,000 (\$70,000 in 2010)³³ and, after that, as a 'West Coast Consultant' with a meagre support of \$1,000 (\$11,600 in 2010).³⁴ And, what was most important, he remained in the AJC Scientific Department's corridors of power. His collaborators and superiors in New York were made to believe that his absence was merely temporary and that, once his health was restored, he would return to New York. Actually, he returned to New York only as a visitor, since there was always some doctor ready to certify that for health reasons it would be highly unwise for him to return to the East Coast metropolis.³⁵ During his absence, the associate director, Samuel H. Flowerman, and the permanent secretary of the department, Marie Jahoda, both of whom Horkheimer had selected before his departure, organized the work to the satisfaction of all

concerned, and regularly asked about his health as well as for his suggestions in research matters. They continued to pass off his absence as a temporary vacation. After a move by a number of AJC officers to have him dismissed had failed, his position was formally downsized to that of a Chief Research Consultant for the AJC: 'finally ... it (should) be made clear to Dr. Horkheimer that he does not have the authority to initiate new projects without the approval of the New York staff and the Committee on Scientific Research'.³⁶ What Horkheimer was deprived of was a competence which, after one year, he was not much interested in anymore. Initiating further projects probably was the very last thing Horkheimer wanted to do. Securing those that existed was something he could do from Pacific Palisades.

By the turn of the year 1944–5, the first Anti-Semitism Project had been definitely buried. In the following months and years, only four of the various studies that had initially been envisaged were continued, often in a thoroughly revised form. Paul Massing, drawing on the various manuscripts on the history of anti-Semitism in Germany, took on the task of writing at least one volume on anti-Semitism in Wilhelmine Germany for the *Studies in Prejudice*. And at the end of 1940 Leo Löwenthal, who in the meantime had worked for Lazarsfeld's Bureau of Applied Social Research and the Office of Strategic Service,³⁷ began to compile, in cooperation with Norbert Guterman, some of the content analyses of the speeches given by anti-Semitic agitators, which made up another volume of this series. Both books were published in 1949 as the first two volumes of the *Studies in Prejudice*.

The third study to be continued was an investigation of anti-Semitism among American blue-collar workers for which the institute, thanks to Gurland's commitment and Daniel Bell's help, had meanwhile found a different sponsor, the Jewish Labor Committee.³⁸ For Gurland, this was a way to secure his job at the institute, while Bell, who in the years before had been one of the young Americans who copy-edited the texts of the members of the institute, was interested in helping the institute. Lazarsfeld was soon enlisted as an expert to help conduct the study. Although a final report was – after many delays for various reasons – completed, the typescript of the report has never been published.

The fourth study was TAP (Adorno *et al.* 1950). Here, the original typology eventually found its home. The two other volumes of the *Studies in Prejudice* had not even been conceived before Horkheimer took up his work at the AJC. Bruno Bettelheim and Morris Janowitz's study on anti-Semitism among war veterans, initially intended as a parallel to the Berkeley study, soon evolved to be a study in its own right (Bettelheim and Janowitz 1950) – a development that may have benefited from the fact that Horkheimer had quickly lost interest in exercising any influence on its content. A key actor in this transformation of the initial Anti-Semitism Project into the five-volume series of the *Studies in Prejudice* was the Advisory Council referred to in the very letter by Horkheimer to Adorno that has already been quoted above.

AN ADVISORY COUNCIL – FROM CONSULTATION TO RESIGNATION

In the first half of the year 1945, the Advisory Council met once every month on a Saturday afternoon. It initially consisted of eight members, but was later enlarged to include a number of additional members and, in certain cases, special guests. Advisors were paid \$50 (\$590 in 2010) for each meeting they attended. The core group, that is, all those who attended every meeting, consisted of Horkheimer as the chairman; Nathan W. Ackerman, a New York psychoanalyst; Vienna-born psychoanalyst Ernst Kris, who at the time was participating in a large-scale project on German propaganda that was financed by the Rockefeller Foundation; the two Columbia sociologists Lazarsfeld and Merton; psychologist Gardner Murphy, also from Columbia; and anthropologist Margaret Mead. The second meeting already included Herta Herzog and social psychologist Solomon Asch, who at the time was affiliated with the New School for Social Research in New York. Otto Klineberg and Rudolphe Lowenstein were among those members who attended irregularly. Among the guests, Leo Löwenthal was the most regular one, but Frederick Pollock also frequently attended the meetings, whereas Adorno, Siegfried Kracauer, Gregory Bateson, Bruno Bettelheim, Bernard Berelson, C.W. Mills and a number of others appeared only once.

At the first meeting of the 'Advisory Council to Dr Horkheimer', as it is officially called in the minutes, Horkheimer explained the state of affairs and what the advisors were expected to do. The success of his activity, he said, would depend on whether he succeeded in getting the necessary support and whether the projects conducted by the social scientists of the AJC would meet with approval by their peers. Kurt Lewin had accepted a similar function at the American Jewish Congress and intended to commit himself to the study of group dynamics. Horkheimer, in contrast, declared himself to be more interested in the study of 'individual attitudes'.³⁹ The work plans of the new department would be submitted for discussion to the Advisory Council.

Surprisingly, the first project to be discussed was Horkheimer's hobby horse of many years, the experimental film. He once again explained the design of the study and the film which, as a result of discussions with Siegfried Kracauer and Hans Richter, had been modified. It now featured a white Christian American, a Jew, a Black and a Briton, who were involved in an accident that an unprejudiced spectator might construe as the wilful wounding of a handicapped person. The film was to be shown to different groups of spectators. Follow-up interviews would be conducted to find out whether the four types were judged differently. Horkheimer then wanted to immediately discuss the questions to be asked after the film had been shown, but was stopped by Merton and Murphy who felt that they should first come to a clear definition of what exactly was to be measured. Was it to be the intensity of the prejudices held towards various ethnic groups or the occurrence of prejudice in specific situations? Merton felt the second option to be more promising since it allowed them to observe which persons responded by latent anti-Semitism in which of the situations represented. Kris pointed out that another of

Horkheimer's ideas was no less problematic, since he wanted to interview the spectators under the pretext that he was interested in the reliability of their testimony. It should be admitted, however, that people's responses tended to be different according to whether they were in the role of a witness or in the role of a spectator.

Horkheimer then wanted to discuss the design of the questionnaire that was to be used. Its first question would be, 'How did it really happen?' followed by, 'Is the Jew really guilty?' The others, however, rather than deal with these technicalities, raised further fundamental questions. Thus, Mead worried that the film experiment itself might contribute to the deepening of prejudice. No Jew should be shown attacking a one-legged person. Murphy doubted whether it made sense to lay open the actors' moral misconduct in order to find out something about prejudice in general. With each new contribution to the discussion, the number of influences to be considered increased. While Horkheimer again tried to get advice on what this film could measure, Lazarsfeld, who had definitely taken to the idea, suggested that the film should be used for exploratory purposes and not for obtaining easy results such as whether anti-Semitism was stronger among blue-collar workers than among the middle classes, or on the West Coast than on the East Coast. Finally, Merton advocated pre-testing the film to gain more insight into all these questions. At the time, the BASR was conducting studies for the United States Department of Defense to investigate the impact of films, using both the Program Analyzer and focused interviews.

The second part of the meeting was devoted to a discussion of the current state, and further outlook, of the Berkeley group studies. Sanford and Levinson had so far used questionnaires to interview students on ethnocentrism, chauvinism and other kinds of prejudice, and had then done case studies for an in-depth investigation of the extreme groups. For this, the Rorschach test and the Thematic Apperception Test (TAT) had been used, and Horkheimer wanted his advisors to tell him if these were really the best tests available. Murphy suggested the additional use of story-completion tests such as those employed by Frederic Charles Bartlett.

Other ongoing projects were a survey of young children who could not yet have any prejudices at all, and a study on the stereotypes of fascist agitators. For the latter, Horkheimer raised the question of whether it made sense to create a stereotype of an agitator that would make people laugh. Thereupon, Mead firmly objected to anything that could create enmities, while Lazarsfeld found the idea quite appealing, which earned him a rebuke by Mead. Merton argued that what mattered for the impact of propaganda and for people's adherence to prejudice was never just conformity with people's beliefs, but also group affiliation and the adherence to, or refusal of, beliefs by their reference groups. If the leader of a group that propagated crass forms of prejudice was accidentally or intentionally eliminated, the prejudices held by the group often enough disappeared with him. Merton knew what he was talking about since at the time he was also acting as an advisor to Stouffer's study,

The American Soldier, which later induced him to write his well-known works on reference groups.

At subsequent meetings, some members of the Advisory Council who misinterpreted the role they had been assigned also presented research projects they thought worth pursuing. This was not quite what Horkheimer had hoped to get from his advisors, but in the end, the *Studies in Prejudice* came to benefit from this, since at least one of its volumes originated in a proposal that had been submitted to the Advisory Council. This was one of two projects proposed by Nathan Ackerman, i.e. a study on anti-Semitic prejudice among the clients of psychoanalysts. With Jahoda as a co-author, Ackerman published this study in 1950 under the title of *Anti-Semitism and Emotional Disorder*.

The Advisory Council indeed discussed the film experiment at another of their meetings, but Horkheimer pushed for it mainly within the AJC. Screenwriters were recruited and potential directors discussed, but in the end it was never realized.⁴⁰ It passed away as peacefully as the 'Handbook of Fascist Agitators' and a number of other institute projects that had at some point simply been forgotten by their protagonists.⁴¹ By contrast, cooperation with third parties made for continuity in the institute's research projects. Besides the Chicago Study by Janowitz, Bettelheim and Edward Shils, this was especially true for the work of the Berkeley group, which managed to remain unfazed by Horkheimer's changeable moods. The documents kept in the AJC Archive include many project proposals that, while they almost always had Horkheimer's support, ultimately came to nothing. What Horkheimer actually had in mind, and consistently discussed in the Advisory Council over a considerable period, came down to the idea of being able to publish a definitive work that 'might become as influential as Gunnar Myrdal's work "The American Dilemma"'.⁴² What this was to be in terms of content, however, changed with each meeting, as did its estimated costs.⁴³ For a certain time, Horkheimer considered writing, with Robert MacIver and Gordon Allport, 'a standard book based on our present knowledge of the nature, motivation and extent of prejudice in general and antisemitism in particular',⁴⁴ but he soon lost interest in this, as well. Allport, who proved to be more persistent, published just this definitive work on prejudice, i.e. *The Nature of Prejudice*, in 1954, for which, however, he had not needed Horkheimer's help (Allport 1997).

In early June 1945, the Advisory Council held its last meeting before its summer recess, and was not called back until after Horkheimer's relocation to Los Angeles. The members of the Scientific Advisory Council, as it was now called, unanimously declared 'that their service had not been sufficiently utilized and (they) would welcome a reorganization that gave them a more active part in the work of the Department'.⁴⁵ An agreement was reached, stating that it was unreasonable to go on operating in the same style as before and that members should be better informed on what the department intended to do in order to enable them to be more specific in their suggestions

for improvement. Until Jahoda's resignation, the Advisory Council functioned in an individualized form, i.e. its former members were called upon as consultants.⁴⁶

After Horkheimer's departure from New York, some of the collaborators he had left behind suddenly seem to have seen his work style in a different light. After some hesitation, Marie Jahoda, who was his assistant at the time and felt bound to loyalty to him for more than one reason, since Horkheimer's efforts had helped to free her from detention in Vienna in 1937, finally gathered the courage to write a letter voicing her discontent with the man who had so hastily retreated back West. Due to Flowerman's administrative skills, she wrote, the department now was a 'proper and orderly working outfit', but Horkheimer's actions of the previous week had more than once roused her feelings to the point of 'committing an illoyalty to you'. Since his departure, these feelings had even intensified. Her impression was that Horkheimer did not employ her in a way that corresponded to her capabilities. Her admiration for him as a philosopher was unbroken, but his competences as a methodologist had raised certain doubts in her. Before going into the details of her criticism, taking the experimental film as an example, she wrote a sentence that made Horkheimer explode: 'The movie was a brilliant idea that took everybody in when you explained it, using all the nice tricks of your personality that ... could make you get away with murder'.⁴⁷

Jahoda's message was clear: Horkheimer's skills in convincing others of an idea largely exceeded his skills in implementing this idea. This, however, was something he refused to listen to, and even less so when it came from a woman who was his subordinate. His response, seven days later, was a proper dressing-down:

Our difference in scientific matters is as evident as the difference between Hegelian logic and an orderly working outfit cleansed of philosophical sloppiness ... It is downright inadmissible, however, for you to lecture me and, in doing so, not only to deliberately conform to the situation, long since denounced by more far-sighted persons, of the modern sociological employee who has to limit himself to pseudo-exact paraphernalia if he wants to avoid being dismissed by his client, but also to pass it off, for yourself as well as for me, as intellectual integrity, responsibility, and incorruptibility. You will not expect me to engage in a controversy over this (my inability of realizing my own research intentions). I refrain from humiliating you by drawing your attention to a number of reflections which, if you would take the time for it, you will undoubtedly come upon by yourself.⁴⁸

One might be content to point out that it was Jahoda who, in the end, was borne out by history, since it is well known that the experimental film never saw the light of day. But aside from the mere facts, the actions that followed upon this correspondence are highly significant in a quite different respect. Horkheimer responded to the well-intentioned criticism not only with all the arrogance he commanded but also instructed others how to act in this matter. Before sending his letter to Jahoda, he conferred with his intimate friend

Pollock and, having sent it, he instructed Pollock via Löwenthal how further to deal with Jahoda, namely, to prevent her from resigning. The whole affair not only shows that Horkheimer devoted a sizeable part of his life time to the art of high-level office intrigue but also that he was simply incapable of assessing his own competencies. Pollock and Löwenthal, who probably did not stop one second to reflect on what Jahoda may have meant, presently agreed with Horkheimer that Jahoda was a case of 'masochist rigidity'.⁴⁹

There is more than one case of an author whose fame would have remained unblemished but for the papers he left behind, papers that so harshly disavow him and the image one expects to find, or constructs, on reading his works. Adorno and Horkheimer doubtlessly belong to this category. While the former seems to have anticipated this and, therefore, stipulated that access to his legacy be blocked until fifty years after his widow's death, Horkheimer seems not to have harboured any such doubts. Access to the documents in the Max Horkheimer Archive has been free for many a long year, and with the publication of his *Gesammelte Schriften* (Collected Writings), all the embarrassing details are there for everyone to read. Here, the art of networking in terms of arrangements and intrigues, exercised on an almost daily basis, and the contemptuous judgements of others that verge on social denunciation, are bound up with a specific attitude of grandeur and an almost paranoiac suspicion of everyone and everything. After the Holocaust, all this coalesced into a habitus of persecution that made Horkheimer all but unfit to cope with reality, as he was no longer able to assess the true dimensions of the danger. When, around 1960, anti-Semitic graffiti began to spring up in Germany, Horkheimer panicked. For several months he desperately tried to re-obtain the United States citizenship he had lost (there was a ruling at the time that deprived anybody of his or her US citizenship who was not US-born and had spent too long a time living outside the United States). In order to be re-granted US citizenship, he wrote letter after letter describing his activities after his return to Frankfurt as if he had been a member of the United States occupation army.

While Horkheimer's reaction in 1960 may after all be interpreted in terms of a post-traumatic syndrome, the same cannot be said of a similar reaction shown by Adorno some time before. In 1954, Richard Christie and Marie Jahoda were co-editing a volume dedicated to a critical review of TAP, to be published as part of the series *Continuities in Social Research*. Jahoda sent the manuscripts of the contributions to Adorno and Horkheimer, asking for their opinion. One of these contributions was a shock for Adorno. Edward Shils, a long-term collaborator of the *Studies in Prejudice* that in the end were published solely under the names of Bettelheim and Janowitz (Bettelheim and Janowitz 1950), attacked TAP because of its failure to include leftwing authoritarianism (Shils 1981). This criticism was as justified as it was unfair, since when TAP had been in the making, i.e. between 1944 and 1947, focusing on rightwing anti-Semitic anti-democrats indeed made sense. In 1953, denouncing the failure of TAP to discuss leftwing authoritarianism could easily be construed as a gesture of submissiveness on the part of the critic to the

zeitgeist of the so-called McCarthy era – and was arguably seen as such by a majority of readers. Adorno responded to this criticism by sending a number of drafts to Jahoda in her capacity of an editor, proposing changes to be made in her introduction and in Shils's text. Jahoda integrated these changes in an attenuated form, while Shils refused to have his text interfered with in any way.⁵⁰ Adorno, who was staying in California at the time, wrote to Horkheimer that he had the 'definite feeling that I should be out [of the United States] before the book edited by charming Mizzi is published' (quoted in Wiggershaus 1986: 518) – as if the book edited by Christie and Jahoda was a testimony given before the House Un-American Activities Committee rather than a contribution to academic social science discourse which, of course, was no field of interest for the McCarthyites.

THE ANTI-SEMITISM PROJECT, PART II

At the end of 1945, the large-scale anti-Semitism project that the institute had pursued since 1940 had definitely collapsed. Thanks to Horkheimer's one-year presence in New York and Adorno's insistent letter writing, prospects for the Berkeley project were not bad at all. In the spring of 1945, it had been granted \$17,500 (\$206,000 in 2010) to be spread over two years, by the AJC, and thus disposed of as much money as the initial Anti-Semitism Project with its eighty-seven sub-studies. And this was only the beginning of their financial support. By the end of the project, the AJC had invested almost \$170,000 (\$1.5 million in 2010) in the *Studies on Prejudice*, \$52,000 (\$458,000 in 2010) of which had been spent on TAP.⁵¹

R. Nevitt Sanford, born in 1908, and Daniel Levinson, born in 1920, were affiliated with the psychology department of the University of California. So was Galicia-born Else Frenkel-Brunswik, born in 1909 and a student of the Böhlers. However, due to the so-called Anti-Nepotism Regulations that were in force at California universities at the time, and to the fact that her husband, Egon Brunswik, had obtained a regular appointment at the department when his Rockefeller fellowship was terminated, her position was only that of a research assistant. These three were not only interested in psychoanalysis but had undergone psychoanalysis themselves. Back in Vienna, Else Frenkel-Brunswik's analyst had been Ernst Kris, but the negative attitude of her teachers, the Bühler couple, to psychoanalysis had at that time prevented her from pursuing this line of research. Berkeley, by contrast, was much more favourably disposed towards psychoanalysis, as can be deduced from the very fact that Erik Erikson was at the time affiliated with the Psychology Department (Friedman 1999). It was no doubt their common concern with psychoanalysis that brought the three of them together as well as into contact with professional psychoanalysts, among them many emigrants such as Siegfried Bernfeld and Otto Fenichel. Via German emigrant psychoanalyst Ernst Simmel they came into contact with Horkheimer who was probably initially more impressed by the evidence of their affinities to psychoanalysis than by any specific

psychological competences the three of them might possess. For Frenkel, due to her Vienna studies, these competences were in the field of Karl Bühler's 'Denkpsychologie' (cognitive psychology/psychology of thinking) and, primarily, in child psychology and psychological life course research, to which Frenkel had contributed in a leading role under the direction of Charlotte Bühler. Also due to her Vienna activities, Frenkel-Brunswik was profoundly interested in the philosophy of the Vienna Circle which she felt to be superior to psychoanalysis on a theoretical level and which she later relied upon to reconstruct Freudian theory in terms of the philosophy of science (Paier 1996). During the first years of her stay in California, Frenkel-Brunswik worked on a large study on *Motivation and Behaviour* (Frenkel-Brunswik 1942)⁵² with the aim of elucidating the link between drive structure and behavioural manifestations. In the academic year of 1942/3, a fellowship of the Social Science Research Council allowed her to benefit from an extensive study stay in Chicago, where she worked with Chicago-based anthropologist Robert Redfield and sociologist Herbert Blumer, as well as in Harvard where she was in contact with psychologists such as Henry A. Murray, the inventor of the Thematic Apperception Tests (TAT), and anthropologists such as Clyde Kluckhohn (Paier 1996: 36–7). During her Harvard stay, Frenkel-Brunswik came into contact with the emerging 'Culture and Personality Approach' which she subsequently enhanced in her discussions with anthropologist Alfred Kroeber in Berkeley. The small anti-Semitism study thus easily fitted in the Berkeley research programme, and after Frenkel's return from her fellowship, the Sanford, Levinson and Frenkel-Brunswik decided to go on working on this issue as the Berkeley Public Opinion Study group.

Sanford's works pertained to the field of the psychology of personality. Together with his student Levinson he worked on a scale for assessing anti-Semitic attitudes, giving precedence to his junior partner when it came to the names under which the scale was published (Levinson and Sanford 1944).

While this scale was indeed the first of those that would be used in TAP, it was by no means a scientific innovation. Since Louis L. Thurstone's article 'Attitudes can be measured' (Thurstone 1928), published in the *American Journal of Sociology*, and a monograph with a similar title published the following year (Thurstone and Chave 1929), a technique of attitude measurement existed that was based on scaling. Rensis Likert (Likert 1932; cf. Allport 1968–9) then contributed the variation that has been named after him, differing from Thurstone's in that respondents were required to indicate grades of approval or refusal, which proved to be primarily advantageous for statistical analysis. The theoretical concept of attitude that was a precondition for this type of procedure had been formulated most stringently by Thomas and Znaniecki, who defined it as the 'state of mind of the individual toward a value' (Thomas and Znaniecki 1918–20: 21). In the decade between Thomas and Znaniecki's and Thurstone's publications, the empirical recording of attitudes evolved from a technique that was applied more or less intuitively to an elaborate research routine. The two sociologists had addressed each of these

'attitudes' as an isolated factor, but soon scales were developed where the researchers themselves defined the intervals between items. The most famous of these scales was Bogardus's 'social distance scale' (Bogardus 1925) where respondents had to report on the degree of proximity they would tolerate for the members of an alien social group: from refusing them entry into the country to working at the same establishment or having the same occupation, to being a member of the same association, to letting them marry a close relative. These *a priori* scales – called thus because the researchers were the only ones to define the intervals between items – were followed by Thurstone's scales where items were ordered according to the judgements of a maximum number of 'judges'. These 'judges' were asked to order a very large number of opinions on a specific issue that had been collected from newspapers, books, opinion polls, etc. Respondents were then presented with the resulting scales and were asked to indicate the statements with which they agreed or not. In the 1940s, these scales were further adapted for various social groups, mostly by way of a pre-test conducted with a small sample of the group to be studied. In contrast to their predecessors, the Thurstone scales provided an adequate basis for the more sophisticated statistical methods that were to be used for analysing measurement results.⁵³ The Guttman scale was a later development and had no part in TAP.

At the same time, Lazarsfeld was concerned with problems of index construction. Therefore, a short outline of the differences and the similarities between these two procedures seems necessary. For constructing an index, a number of traits – measuring behaviour, in most cases – are identified, and respondents are given one point for each hit. The overall number of points is the index value. Lazarsfeld used a simple example, i.e. socio-economic status, which can be determined by possession of a home, possession of a car, presence of a telephone at the home and college education. Respondents can score from 0 to *n* points, and no connection between individual variables needs to be assumed (Horwitz and Smith 1955: 74). At the time, Lazarsfeld was interested in detecting the connections between various indices that had been constructed independently from each other, leading him to the concept of the 'interchangeability of indices'. Researchers working with scales, by contrast, assume that the individual items of a scale are degrees on a dimension. This goes to show that the choice of the survey tool also has an effect on the questions to be solved (or considered interesting). Scales require detailed work on the intervals between items while Lazarsfeld's indices of that time resulted in a rough division, fourfold in most cases, and required more attention to be paid to the correlations between the various indices.

With regard to the content of the attitudes to be measured, Sanford and Levinson were no pioneers, either. Attempts to measure political attitudes and prejudice and, in several cases, even to identify fascist attitudes, had been made before them. As early as in 1925, Henry Moore, drawing on a questionnaire of twenty items, had published a paper on radical and conservative attitudes among undergraduates at Dartmouth College and Yale and Columbia

Universities. He had subsequently tried to link the results to intelligence, neuro-muscular constellations and other factors (Moore 1925), but in the present context this is of minor importance. What is more to the point is the history of attempts to assess specifically fascist attitudes. The first publication dealing with this matter is by Ross Stagner (Stagner 1936a), who used Thurstone scales to assess the affinity of American college students in the Midwest to German or Italian fascism. His item list already included a number of neutral items, and his analysis also considered sex differences and the social context, which was measured by indicating the college the respondent was attending. What is more, Stagner did something that Adorno and Horkheimer later thought was their very own invention: he grouped the extreme cases of those who had obtained particularly high or particularly low scores, respectively. Stagner's results suggest that 'high scorers' tend to refuse workers' unions, feel superior to blue-collar workers, show no sympathy for the disadvantaged and the unemployed, and largely identify with middle-class values. With respect to an improved version for adults, however, the author pointed out that a distinction should be made between emotionally charged stereotypes and 'true attitudes' (Stagner 1936b).

Raskin and Cook (Raskin and Cook 1938), directly building on Stagner, tried to eliminate the problem of stereotypes from their assessment of basal attitudes. Katz and Cantril (Katz and Cantril 1940) and Edwards (Edwards 1941), based on their finding that strong signal words provided a 'frame of reference' (Allport 1940) and affected response behaviour, addressed the question of how to measure 'unlabeled fascist attitudes'. Finally, Abraham H. Maslow published a treatise on 'The authoritarian character structure' (Maslow 1943) in which he tried to break down the concept of the authoritarian character structure that was pivotal for Erich Fromm, a former member of the Institute of Social Research, as well as for the Frankfurt School, into its psychological elements while steering clear of the risk of over-psychologization. Maslow discussed worldview, tendency to hierarchy, general tendency to classify people as superior or inferior, striving for power, hostility, hatred, prejudice and many more traits as evidence for the authoritarian character (drawing these distinctions from Arthur Koestler's *Darkness at Noon*, the impressive description of the mindset of the accused in the Moscow show trials). If the authors of TAP had taken into account this latter aspect, they would have been spared the accusations of one-sidedness – i.e. of having ignored left-wing authoritarianism (cf. Shils 1981).

These few indications must suffice here to point out something that Sanford and Levinson would no doubt have acknowledged, that their work certainly was up to the state of the art of contemporary psychological research, but it was by no means more than that.⁵⁴ The work they had done before they were invited by Horkheimer to join TAP project was a prototypical contribution to an ongoing cumulative psychological exploration of prejudice. Their original achievement was the construction of a scale for assessing anti-Semitism which up to then had not been given a great deal of attention. The few

socio-psychological or sociological interpretations of the phenomenon of anti-Semitism that were not embedded in the larger context of comparative studies on attitudes towards ethnic minorities had been proposed by authors who, for the most part, were either psychoanalysts themselves (Fenichel, Freud, Fromm, Reich, Simmel) or were influenced by Freud's teachings (Harold Lasswell). Frenkel-Brunswik's case studies enhanced the psychology of personality, which was nearing its peak at the time, by a number of new facets, and perfectly fitted into this psychoanalytic frame of reasoning.

From the very beginning, the encounter between the group around Horkheimer and the one around Sanford must have taken place in a friendly atmosphere, for no derogatory comments on the Berkeley group are found in the letters of the members of the Institute of Social Research at the time, which is highly unusual, and something of a surprise. After all, the members of the Berkeley group, in spite of their interest in psychoanalysis and their political affinities with the concerns of the institute, were primarily academic psychologists, which should have triggered some adverse reflexes in Adorno if, in 1944, he still adhered to the anti-psychological views he had expressed when arriving in New York in 1939. But in the meantime, Horkheimer and Adorno had discovered psychology as an effective conceptual framework that assured a certain congruency with traditional thought even though 'psychology' for them still was something rather different from academic psychology, as shown by a letter which Horkheimer wrote to Marcuse at the beginning of the alignment with the Berkeley group: 'I don't believe in psychology as in a means to solve a problem of such seriousness. I did not change a bit my skepticism towards that discipline. Also, the term psychology as I use it in the project stands for anthropology and anthropology for the theory of man as he has developed under the conditions of antagonistic society.'⁵⁵

What Horkheimer and Adorno wanted, as noted by Wiggershaus, was 'to be able to promote, under the cover of a traditional terminology, a maximum of their own thought' (Wiggershaus 1986: 398). Their own thought, in this case, was the attempt to embed the studies in a larger framework of social theory and in the typology that had been published as early as in 1941 but had apparently more or less fallen into oblivion in the meantime. Both concerns were in line with those of the group around Sanford, and they ultimately resigned themselves to having Adorno's collaboration forced upon them.⁵⁶ One may assume that in matters of social theory, they felt inferior to the German exiles, if only for the latter's permanent arrogation of competence in this respect, while in the field of their own competence they were actually facing an unsolved problem for which they needed all the help they could get.

The help that the critical theorists could give on the theoretical level did not consist of what at that time was gradually emerging as the mainstream of sociological theory, i.e. the attempt to attain a higher degree of generalization by the systematic conceptual reconstruction of empirically verifiable connections – for which Merton, with his 'middle range theories', provided the programmatic catchword (Merton 1949). Horkheimer and Adorno adhered

to Hegelian-Marxist thought and produced interpretations of the contemporary era, albeit with a stronger orientation to the history of ideas than to the Marxian model of class conflict and capitalist exploitation, as is evident from their best-known text of those years, the *Dialectic of Enlightenment* (*Dialektik der Aufklärung*) (Horkheimer and Adorno 1969). Referring to anthropology, as the two authors of the *Dialectic of Enlightenment* did, arguably provided another bridge to the Berkeley group, although it is obvious that their understanding of anthropology was something quite different. Sometimes, even misunderstandings may serve to establish a shared vision of the world and of the problems to be addressed. Levinson remembered that Adorno 'represented sociology much more fully than any of us could' (Levinson 1992). It can be supposed that not even Adorno thought of himself as the embodiment of the sociology of his time, but making others believe just this was something else altogether.

Attitude measurement by scales regularly faces the problem of what to do with the data so painstakingly collected. After all, all they collected were the frequencies of respondents' agreement to each of the items. How to come to a content-based characterization of the empirical distribution of the 'scores' obtained by a group under investigation was the key problem of academic psychologists at the time – and the issue of how to empirically identify the types of anti-Semites that had been constructed in 1941, and to establish their actual occurrence, was a problem Adorno could not help but be interested in if the final product of their concerted efforts was to succeed on the academic market. This opportunity to benefit from each other's contributions to their respective problems seems to have been the basis for the fruitful collaboration between these partners who, on the face of it, were not really meant for each other.

An additional incitement to intensive cooperation was provided by the idea, probably coming from Horkheimer and Adorno, that anti-Semitism should not be treated as an isolated phenomenon but should be embedded in the larger context of 'anti-democratism'. Exploring the latter implied that the dimensions that characterized it had to be identified, and that adequate scales had to be constructed. This challenge was enthusiastically taken up by Sanford & Co. since it offered them the welcome opportunity to apply a technique they had successfully developed for a different case to other phenomena as well. Thus Levinson, who was primarily interested in working at the scales, spent a great deal of energy on their improvement and sophistication.

Another congruence between the two sides of this unequal joint venture was that both of them wanted to do more than just establish distributions of certain attitudes. Horkheimer & Co. were not inclined to attribute much importance to what they disparagingly called measuring the opinions of the majority. They were not interested in what was fascinating Lazarsfeld, their partner of so many years, namely looking for causal factors that could be isolated and would allow him to break down the apparently uniform groups of

the population into distinct sub-groups, and engaging in the follow-up of divergent cases in order to find factors of influence that had so far been overlooked. This was something which Horkheimer and Adorno, provided they took notice of it at all, considered to be too atomistic an approach. Their own key concern was with holistic models such as those that, due to Fromm's contributions, could already be found in *Autorität und Familie*. Fromm and, following him, the institute's other collaborators were bent on identifying the specific social character of their epoch, an undertaking that was in line with their veneration of psychoanalysis as a critique of contemporary civilization. Here, Frenkel-Brunswik's line of research fitted in especially well since in her Vienna time she had participated in the local psychological life-course research and after her emigration to California had increasingly turned to the psychology of personality. Both research concerns required a concept positing that personality traits remained more or less robust over time. Thus, the psychology of personality might have provided some substantiation of Adorno's typology. At the same time, it helped them to get rid of the problem that Merton had pointed out in Horkheimer's Advisory Council: the definition of the situation. That is, the same person might show different attitudes and beliefs in different situations or, at any rate, give different answers in an attitude assessment to questions that had no consequences on the social level. With the reference group theory, Merton provided a tool for this which, however, was ignored not only by the authors of TAP but also by most of the others who were conducting studies on prejudice.

The extension of the scales that were used to measure anti-democratic attitudes soon led to complications. The original scale consisted of nothing but negative comments on Jews, because it was assumed that this would help to bypass the mental resistance that caused respondents to abide by the norm of not speaking disparagingly of an ethnic group. In the next step, a sophistication of the tool for assessing anti-Semitism was attempted. Sanford and Levinson came up with the idea to subdivide their original anti-Semitism scale into sub-scales. This, they reasoned, would allow them to identify different types of anti-Semites. This line was later abandoned but is found in Levinson's contributions to TAP. On reading these passages, one cannot help thinking that all this was in reality just some method-crazy psychologists tinkering around.

Another modification, namely the attempt to assess anti-Semitism without using a single phrase in the questionnaire that said anything at all about Jews, resulted in a nearly aporetic situation for the Berkeley group. In their discussions and statements, this approach was called the 'indirect tool'.⁵⁷ The idea behind this is tempting indeed: if genuine anti-Semites tended, for reasons of social conformity, politeness, fear or whatever, to keep quiet about their true feelings in a survey situation, all one needed to do was to construct a tool that allowed the researchers to penetrate to their true values. This could be achieved, or so they felt, by conceiving of anti-Semitism as embedded in a set of more general attitudes, a syndrome of something more deeply rooted in an

individual's personality. In other words, what they tried to do was to identify latent structures, an undertaking radically different, it is true, from Lazarsfeld's cross-table efforts. The F- (or Fascism-) scale that was subsequently constructed, however, was validated on the basis of its correspondence with other scales measuring anti-Semitism, ethnocentrism, and political and economic conservatism, as well as with the clinical interviews, which suggests some circularity. The indirect F-scale instrument was enhanced by statements which could be expected to meet with strong approval by those respondents who would not voice their anti-Semitic attitudes in direct measurements and in-depth interviews.

FURTHER ATTEMPTS AT A TYPOLOGY

During the long time it took to construct these instruments, all those involved were perfectly aware that it would be advantageous if they could find a way to identify different types of anti-Semites. This was the field in which Adorno felt at home and, therefore, he made every effort to contribute to it. In the summer of 1945, he presented two drafts. An undated thirteen-page memorandum states that in order to identify different types of anti-Semites, he had worked his way through 'about 40 of the detailed case interviews', i.e. those in-depth interviews that constitute the top and the bottom quarters.⁵⁸ In terms of content, Adorno sought to reconcile his typological ambitions with the previously established dichotomy of 'high scorers' and 'low scorers'.⁵⁹

The drawbacks of this typology are evident. First, the types are not clearly delimited from one another. Second, in an effort to distinguish between the analytical levels of psychodynamic roots, criteria for identifying the type and reflections on therapy, Adorno inserted subheadings, but was inconsistent in doing so. Speculations about the psychodynamic roots, i.e. what might be described as theory, keep alternating with listings of empirical indicators. Third, the dimensions of the typology are not clearly labelled, and the possible variants of any of these dimensions are not exhaustively treated. Adorno seems to have worked through the interviews in a way that was loosely connected to psychoanalysis, trying to link them up with the typology he had constructed in 1945. His abstractions, rather than resulting from his examination of the material, actually preceded it. He neither worked out genuine ideal types in the rigorous Weberian way nor used Lazarsfeld's techniques to construct a typology.

On the whole, Horkheimer was enthusiastic about Adorno's summer writing efforts, dubbing the manuscript as 'one of the most important steps in the Berkeley study' and advocating its distribution to all the other AJC projects.⁶⁰ But in spite of this praise for the man who was waiting in Los Angeles for his co-author of the big theory book to return, Horkheimer did not hold back with his criticism in the rest of his letter. What it ultimately comes down to is a completely different perspective on anti-Semitism, which makes it worth

dealing with in the present context. Horkheimer started with the authoritative statement that, 'if, in this current period, one is born into an average Gentile family one does not have to be a "type" in order to be an antisemite. One simply learns to speak disrespectfully of Jews as one would learn to curse, tell dirty jokes, drink heavily, or to rage about taxes and strikes.'

Anti-Semitism, he went on to argue, was a 'cultural pattern of modern society'. If this was true, it would cut the ground from under any approach to anti-Semitism that relied on the concept of an anti-Semitic personality as an identifiable part of the population, one different from other personality types that are less, or not at all, anti-Semitic. In the final analysis, his perspective implied that in a world in which 'the average human being, who lives in reaching distance of the ideological machinery of mass domination' everyone could without exception turn into an anti-Semite at certain times and under circumstances yet to be specified. Thus, the difference was not to be found in individual personalities but in different degrees of an anti-Semitic culture that could unleash an anti-Semitic mob whenever the forces of mass domination felt like it. Horkheimer did not further elaborate on what would, then, be the decisive point, i.e. whether in a situation like a pogrom all would join in or whether specific groups of individuals would abstain from or even protest against it. If the latter was conceivable, research on individual differences would again be useful – provided, however, that it was capable of identifying pro-democratic or anti-anti-Semitic types.

Horkheimer then actually suggested that Adorno should abandon his typology for another one that had been constructed by Horkheimer himself. In the present context, however, this is of minor importance since nothing came of it, although it does tell us something about the relational dynamics between the two critical theorists. Two further suggestions of Horkheimer's were, in contrast, acted on by Adorno. He eliminated the parts dealing with therapy, and he wrote the introduction to TAP Horkheimer had insisted on, specifying the status of typologies in their 'general theory'.

In his introductory pages to Chapter Nineteen of TAP, Adorno sought to defend the typological approach. He explicitly referred to Gordon Allport and David Boder (both in Harriman *et al.* 1946) who had pointed out that psychologists such as Erich Jaensch became instrumental to Nazi extinction policies on account of the concept of 'Gegentypus' (anti-type). Adorno's answer to this was that psychologists such as Jaensch were expressing the 'stereopathic mentality' that was one of the 'basic constituents of the potentially fascist character' (Adorno *et al.* 1950: 746). With regard to the refusal of American psychologists to consider types as an adequate research tool, Adorno stressed that typologies had been constructed by such irreproachable authors as Ernst Kretschmer, C.G. Jung and Sigmund Freud, and that those were the models to follow, rather than Jaensch, whose perspective was 'quasi-biological'.⁶¹ Using typologies was justified if three conditions were met.

First, classifying people according to types was nothing that could be done 'neatly statistically', or in terms of ideal types, but had to rely on 'mixtures': 'Our types are justified only if we succeed in organizing, under the name of each type, a number of traits and dispositions, in bringing them into a context which shows unity of meaning in those traits' (Adorno *et al.* 1950: 749).

Second, a typology had to be critical, that is, the standardization of people itself had to be seen as a social function. This obscure formulation can be supposed to mean that a valid typology was tantamount to a conceptualized form of respondents' self-standardizations: 'Here lies the ultimate principle of our whole typology. Its major dichotomy lies in the question of whether a person is standardized himself and thinks in a standardized way, or whether he is truly individualized and opposes standardization in the sphere of human experience' (Adorno *et al.* 1950: 749). Adorno went on to say that the tendency to self-standardization was also found in 'low scorers', and that the stronger this tendency, the more clearly the latter 'unwittingly' expressed 'the fascist potential within themselves'. In a footnote, Adorno specified that his types were real types rather than logical classes. While the three other main authors of TAP stuck to the rules of attitude measurement in their construal of the opposition of 'low scorers' and 'high scorers', i.e. took low scores of agreement to the statements of a scale as evidence for a low degree of adherence to the respective attitude (anything else being just absurd and disavowing the whole effort), Adorno insisted until the end, i.e. until the publication of TAP, that he had a deeper insight into the personality of the potential fascist than the three experts in psychological measurement with whom he had cooperated for years.

The third criterion to be met by a typology, Adorno wrote, is a pragmatic one, i.e. the need to make simple distinctions such as, for example, the distinction between the Romanic and the Gothic styles in art history. In order to make any sense at all of these deliberations, it should be kept in mind that in the initial synopsis of 1941 the typological classification always referred to social positions, as well. My impression is that Adorno came up with the third criterion because it allowed him to reintroduce this aspect of his original beliefs. An orientation to social group membership is 'easier' than the multidimensional psychological reflections that characterized his previous attempts.

The typology that was published in TAP differs from its predecessors in that the two extreme groups of 'low scorers' and 'high scorers' are used after all. Further changes are documented in Table 6.4, in an effort of mine to document the development of the typology across its four stages. It is not hard to see that the project came up against a number of problems. Some of the initial types just disappeared at later stages, others were split and, most notably, new ones kept popping up.

Table 6.4 Development of the Typology of the Anti-Semite

ZfS 1941	Memo Types	Memo Counter-types	Typescript Sep.45	TAP
A1 Born			D1 Idiosyncratic	
A2 Religious-philosophical				
A3 Back-woods or sectarian				
A4 Vanquished competitor			D2 Surface resentment	H1 Surface resentment (≠ L1)
A5 Well-bred			D3 Conventional	H2 Conventional
A6 Condottiere			D4 Rebel (≠ D5)	H4 Rebel and psychopath
A7 Jew-baiter				
A8 Fascist-political			D5 Manipulative (≠ D4)	H6 Manipulative (≠ L4)
A9 Jew-lover ► B1-B6	B1 Genuine liberal ► L5	C1- Leader ► D8		
	B2 Ticket progressive ► L1	C2- Manipulative ► D5		L1 Rigid Low Scorer (≠ H1) ^a
	B3 Devout Christian	C3- Conventional ► D3		
	B4 Conscientious ► L2	C4- Oedipal ► D7		L2 Protesting Low Scorer (≠ H3)
	B5 Easy going type ► L4	C5- Follower ► D9		L4 Easy Going Low Scorer (≠ H6)
	B6 Impulse ridden ► L3	C6- Tough guy ► D6		L3 Impulsive Low Scorer (≠ psychopathic high scorer)
	B7 Paranoid non-Antisemite	C7- Paranoid ► D10	D10 Paranoid	H5 Crank ^b
			D6 Tough Guy ► H4	
			D7 Oedipal	H3 Authoritarian (≠ L2)
			D8 Leader	
			D9 Follower	
				L5 Genuine Liberal (≠ A9)

Note: The columns represent the four stages of the development of the typology (column 3 is related to column 2 and gives the opposite type [*Gegentypen*]). The letters should be read as follows: Letters A to D stand for the three drafts, the numbers represent the order in which Adorno mentions them in the different drafts. In TAP (*The Authoritarian Personality*, Adorno et al. 1950) (right column), Adorno made a new differentiation and relates the types to the two extreme groups (H for high scorer and L for low scorer). In each line one finds from left to right the fitting types. No data to the right indicate that there were no follow-ups to find. Follow-ups in different lines are indicated with ►. In TAP one finds hints towards *Gegentypen*, here represented in parentheses (≠ Xn).

^a In a draft of the table of contents of TAP from July 1947 this type was named 'Ticket "Low"'. AJC records, b. 19: Studies in Prejudice, YIVO.

^b In a draft of the table of content of TAP from July 1947 this type was named 'frustrated'. AJC records, b. 19: Studies in Prejudice, YIVO.

It seems justified to say that Adorno failed in his attempt at a typological identification of anti-Semitism. First, it remains unclear at each of these stages which dimensions were taken into account in the typology; and in those cases where these dimensions can after all be identified they are rarely exploited exhaustively. The initial ordering along a quasi-stratified dimension was abandoned for a complex structure of types and counter-types that are mainly described in psychoanalytical terms. And in the end, Adorno seems to have been converted to the social-psychological conception that takes into account the fact that the attitudes measured differ in intensity. Four of the five types of 'low scorers', however, differ from each other only in the way they were labelled while the fifth one, the 'genuine liberal', is an ad hoc addition. Similarly, the central, i.e. 'authoritarian', type is a last-minute addition, as it were, to the typology and, with regard to content, does not go beyond what Fromm had written about the authoritarian-masochistic character as early as in 1936 (Fromm 1936: 110ff.) Considering that almost from the start the aim had been to speak of a fascist rather than an 'authoritarian' personality (Wiggershaus 1986: 458), the belated appearance of the authoritarian type is not really surprising. If, however, the authoritarian type originally was the fascist one, the question arises how the results of the F-scale could be reconciled with Adorno's typology. It is obvious that 'high scorers' on the F-scale are 'fascists', but what, then, are all the other types of 'high scorers' in Adorno's typology? Adorno himself added to the confusion by proposing, in a long memorandum on the 'book plan' that was found among the papers of the AJC Department, to bridge a major gap by introducing a section on anti-Semitism among Jewish people:

This chapter could be linked organically with the total book plan. By discussing AS among Jews and the importance of the Fascist character among them, we could demonstrate concretely to what extent AS is irrational, that is to say, largely independent of actual interest and also that it is psychologically conditioned, not due to belonging *objectively* to this or that national or racial group.⁶²

Second, Adorno's types and syndromes do not match the description of the dimensions of the authoritarian personality, which appear to be primarily based on Frenkel-Brunswick's contributions. Frenkel's aim was to construct a personality model by using variables that would allow researchers to empirically identify individual traits and, in a next step, to construct relatively stable personality types based on combinations of these traits. While she was very resourceful in her isolation of personality traits, her construction of personality types was less satisfactory. As a result of the lack of alternatives, her types do not go beyond the opposition of the prejudiced and authoritarian (or fascist) personality versus the non-prejudiced personality, i.e. they are just another way of labelling the contrast of 'low scorers' and 'high scorers'. The large variety of variables, however, would no doubt have allowed her to construct other types, as well. Table 6.5 shows the variables

Table 6.5 Variables of The Authoritarian Personality and Dimensions

Variable	Definition	F-Scale			Frenkel-Brunswik	
					Personality Trait	Definition of the prejudiced
					of the prejudiced of those with no prejudice	
Conventionalism	Rigid adherence to conventional, middle-class values	Conventionalism	Genuineness	Adoption of conventional values and rules		
Authoritarian submission	Submissive, uncritical attitude toward idealized moral authorities of the in-group	Rigidity	Flexibility	The avoidance of ambiguities, lack of individuation, superficial adjustment		
Authoritarian aggression	Tendency to be on the lookout for, and to condemn, reject, and punish people who violate conventional values					
Anti-intraception	Opposition to the subjective, the imaginative, the tender-minded	Repression	Awareness	Repression or awareness of unacceptable tendencies and impulses in himself		
Superstition and stereotypy	The belief in mystical determinants of the individual's fate; the disposition to think in rigid categories					
Power and 'toughness'	Preoccupation with the dominant-submission, strong-weak, leader-follower dimension; identification with power figures; overemphasis upon conventional attributes of ego; exaggerated assertion of strength and toughness	Power	Love-orientation	Admiration of, and search for, power		
Destructiveness and cynicism	Generalized hostility, vilification of the human					
Projectivity	The disposition to believe that wild and dangerous things go on in the world; the projection outwards of unconscious emotional impulses	Externalization	Internalization	The mechanism of projection, by which much of what cannot be accepted as part of one's ego is externalized		
Sex	Exaggerated concern with sexual 'goings-on'					

Source: Adorno *et al.* 1950: 228 and 474–6.

that were used in the 'F-scale,' with the four co-authors of this chapter claiming that, 'these variables were thought of as going together to form a single syndrome, a more or less enduring structure in the person that renders him receptive to antidemocratic propaganda. One might say, therefore, that the F scale attempts to measure the potentially antidemocratic personality' (Adorno *et al.* 1950: 228).

The variables of the authoritarian personality are opposed to the personality traits, as shown in the three columns on the right-hand side of Table 6.5, that Frenkel-Brunswick considered worth mentioning in the chapter of which she was the only author. There is little correspondence between these two sets of variables. The attempt to obtain a more differentiated scale of personality types had failed to produce any workable results and was deferred in favour of a multifaceted description of the traits of the two basic types.

Third, over the course of the years, Adorno's methodological justification of his typology kept changing in ways that remain incomprehensible, and where arguments were presented, they do not hold up under examination. The typology was said to be based on ideal types, or to capture real types, or – a frequent reference – to consist of transitions and mixed types.

Fourth, there is a technical objection, as well. Starting with the second typology, Adorno based his work on the clinical interviews conducted by the Berkeley project. That he himself, as far as we know, never conducted a single interview would not be objectionable as such. But considering how the clinical interviews came into being, it is obvious that the material, as Hyman and Sheatsly have convincingly shown, is subject to a strong interviewer effect (Hyman and Sheatsly 1981). The interviewers were aware of the results respondents had obtained in attitude measurements and had been instructed to systematically draw on this prior knowledge in conducting their interviews. Given the lack of technical recording devices, the content of these clinical interviews had to be noted from memory, and there is every indication that there was a large margin for interpretation – too large a margin to allow for so finely tuned an interpretation.

Fifth, one might add that using a typology that consisted of eleven types to analyse eighty interviews is a misconception, since it is too detailed to allow for a grouping of cases. Significantly, each of Adorno's types is illustrated by just one case. While it may just be possible to construct one type relying on a single case, doing so for eleven types seems highly improbable. Adorno's types, thus, are no more than eleven cases that have been given pompous names.

(NO) DISPUTE ABOUT 'CREDIT'?

Since Adorno lived in Los Angeles where he could direct a number of collaborators and was close to Horkheimer, he did not participate in the work of the Berkeley group. Levinson estimated that during the two-and-a-half years of

concentrated work on the project, there were at most eight to ten meetings a year with Adorno. In an interview conducted in 1992, Marie Jahoda, who attended one of these meetings as a delegate of the New York headquarters, described Adorno's performance:

I remember, at the time of the 'Authoritarian Personality' in America – I was in New York working at the American Jewish Committee, and Horkheimer was the boss, also in New York, (and) he said: 'I think you should go to California, have a look at what they are doing down there.' So he sent me there as a kind of spy or something. I remember that we had a meeting at Else Frenkel-Brunswik's house, and that Adorno kept lecturing; there was Sanford and Levinson and Else and me, and Adorno paced up and down in this large elegant salon – there was an armchair in the center, and when he passed there, he always bumped against it. Then he made a little bow saying: 'Excuse me, please!' to the armchair (laughs), went on, and on his way back, this happened all over again. He, he was really crazy.⁶³

Given that all her life Jahoda was anything but a gossip (this disposition being poorly developed in her, or deliberately kept at bay, so much so that her autobiography suffers from it), her judgement surely has a certain weight. Nevertheless, in an interview, Levinson insisted that 'we always liked Teddy [i.e. Adorno]'. In California, for the year 1945 and following, the unlikely case of successful cooperation between people who, on the face of it, do not fit in well with one another, seems to have come true.

There were frictions and, later, a conflict of the kind that are likely to occur in almost any cooperation that lasts for a number of years. But with regard to the institute, these did not result in the kind of defamation that Lazarsfeld was subjected to in the context of the Radio Research Project. The reason is that in this cooperative work the institute could not withdraw back to anything that could be said to be irreconcilable with 'administrative research', as had been the case with Adorno's music theory. For the institute, the only way to solve the conflict with its cooperative partners was to try to claim as its own as much as possible of the Berkeley group's achievements.

The first conflict flared up when Sanford & Co. discovered that, during most of his AJC time, Horkheimer had made the local officers believe that they were just a group of subaltern assistants. After Horkheimer had retreated to his new house in Pacific Palisades, his deputy, Samuel Flowerman, considered it his duty to visit the various projects that were now supposed to flourish under his direction in order to gain a clear picture of what they were doing. In the course of these visits he came to Berkeley in January 1943. What the psychologists gathered from their visitor prompted Sanford to write two letters to the Director of the AJC, Slawson, in which he tried to get things straight. In these letters, Sanford described how the cooperation between him and his colleagues, on the one hand, and Horkheimer and the institute, on the other hand, had come about, a story with which the reader is already familiar. In 1943, Sanford and Levinson had started to do research on anti-Semitism,

quite independently from the institute. They had then, via Simmel, come into contact with Horkheimer who had opened up prospects of cooperation and, more importantly, of funding for their work. This, however, had at first failed to materialize. Thereupon Sanford had continued working with the modest means of his department. In November 1944, that is, about the time when Horkheimer started to work at the AJC, 'all techniques and instruments which have since been used' had been ready:

Our greatest indebtedness to Dr. Horkheimer is for undertaking to convince people to whom we were unknown that our approach was a sound one. In this circumstance it seemed to us natural that there should be some lack of clarity concerning the origin of the ideas, methods and results. Since Dr. Horkheimer was acting as the representative of our researches we never felt called upon to claim credit for our role – the role of the Berkeley Public Opinion Study – in the overall undertaking.

There is more than one point that is remarkable in this. The contact Horkheimer established between Berkeley and the AJC was only an indirect one, as is well known. In the process, he seems to have described the Berkeley group as some kind of rear vassals of the institute, and he also seems to have passed off, at least to Flowerman, certain achievements that actually were those of the Berkeley group as those of the institute. Furthermore, it becomes evident – and later passages of this letter unequivocally say so – that Adorno was not a member of the Berkeley Public Opinion Study but only a cooperating partner, about whom Sanford went on to write:

We have particularly valued Dr. Adorno's deep understanding of Fascism and of the broader contexts within which anti-Semitism has to be considered, his grasp of important concepts in contemporary social science, and his belief – so much like our own – in the integration of psychoanalysis, sociology and the best in academic social psychology ... We would like the continued collaboration of Dr. Adorno.⁶⁴

There is no need to argue here about how deep Adorno's understanding of fascism really was. The previous remarks should have enabled the reader to come to his own judgement in this matter.

Horkheimer was immediately informed of Flowerman's visit to Berkeley and Sanford's letter, and his reaction shows that Sanford had indeed hit on a sore point. To Leo Löwenthal, whom Horkheimer had sent a copy of Sanford's letter instructing him at the same time to destroy the copy and not to let Flowerman know that he knew about it, he wrote a wordy complaint about the badness of the world. Sanford, he said, seemed to think that the institute and he himself 'must have received tremendous amounts of money' (which for the first phase in 1943/4 can hardly be denied, see Table 6.2) and that in the previous year, when Horkheimer was sitting in his AJC office, too little funding had been channelled to 'his [i.e. Sanford's] racket'. Horkheimer, in his typical manner,

exploited this episode in order to distil some general insights on the United States:

In this country you can get along and make headway if you develop great activity. Even in the intellectual branches, there are plenty of opportunities and the money lies so to speak in the street. There is only one condition: that you subscribe to action for action's sake without any reservation. It is not so much the content of your work which counts, but the quantity and even more than that: human relations.⁶⁵

However, in writing this, he was not thinking of himself but of Sanford! Flowerman, who had gotten wind of all this and largely shared Horkheimer's view of the matter, confidentially approached Frenkel-Brunswik and implored her to make sure that the existing ruling according to which the institute was the AJC's 'agent' in California was accepted by Berkeley. As soon as the 'operational part of the project ceases,' Adorno's usefulness would become evident.⁶⁶

Although Horkheimer and Adorno must have been aware from the winter of 1946, at the latest, that there was an impending conflict about the question of priority, this conflict strangely enough never really broke out. The reason is to be found in the rivalry between Frenkel-Brunswik and Sanford. Concerning the publication of TAP there surely was a debate about the order in which to present the authors' names, but in the end the three authors accepted the alphabetical order, being listed after Adorno. They even conceded priority to Adorno for the chapter on the F-scale, which was signed by all four of them. Adorno most likely had contributed little more than the F-word to this chapter but nevertheless set great store by coming first (Wiggershaus 1986: 457).

In the case of publications by more than one author, there are multiple solutions for the issue of how to order the names, none of which is binding in a normative sense. The easiest one, but by no means the one most often used, is the alphabetical order. Another one is an ordering which reflects how much each of the authors contributed to the overall product, and a third one is an ordering according to the age, status or prestige each author has so far acquired, i.e. reliance on the Matthew effect (Merton 1973: 439–59; Merton 1988). Other authors decide by lot or opt for a reverse alphabetical order. As yet, hardly an author will have felt entirely indifferent about the issue, not even those who choose to turn it into ridicule, such as the trio of 'Alpha, Beta, Gamow' that was in reality just one author (Watson 2002).

Horkheimer and Adorno were anything but disinterested in priority issues, as shown by the ordering of names for the *Dialectic of Enlightenment*, where the one that was superior by status and age came first and the one who presumably contributed more to the success of the fragmentary work had to be content with the second place. Author duos, however, have the great

advantage that, with only two names, both of them will be cited as a rule, just as in the case of the *Dialectic of Enlightenment*. The critical number is three or more, since in these cases references to sources, etc., are mostly in the form of 'XY *et al.*' or 'XY and collaborators'.

The issue of how to order authors' names for a collective work is only the most visible manifestation of the struggle for recognition among scholars. There is more than ample evidence that the four main TAP authors were very much aware of the issue of 'credits', and that a certain amount of infighting was going on about it. Levinson recalled that Frenkel-Brunswik and Sanford quarrelled about priority, i.e. the question of who had contributed what, and how much of it. Sanford himself had given priority to Levinson, who at the time had not even graduated, for the first publication that resulted from their cooperative work. But he could at the same time be sure that his name would always be cited, as well, and that, to the initiated, this would suggest that the senior researcher was also likely to have had a greater share in the intellectual output. When the Sanford-Levinson duo became a trio, debates soon set in between those two who were more or less equal in terms of age and status. Levinson, retrospectively, was very open about this:

As soon as we [three] were seen as a project and we were in a larger world, then there were questions of credit. Each of them [i.e. Frenkel and Sanford] felt that they have made an important contribution, and at times felt that the other one minimized or neglected it. But I would say there's a sociology of this, which is that the world around us wanted to know who was the real hero of this, who was the intellectual God of this, the source, and there were those who said it was Nevitt [Sanford], and those it was Else [Frenkel-Brunswik]. Nobody said it was Teddy [Adorno] or me ... (Levinson 1992)

In the late 1940s, Else Frenkel-Brunswik had more publications than Sanford who, however, topped Frenkel-Brunswik with respect to status, since he held a regular university position while Frenkel-Brunswik had to settle for the position of a research associate. The activities of the Berkeley group were communicated among psychologists long before they published their book, and the project team was invited to contribute to more than one publication. In most cases, a single author wrote about some aspects of the results. But when they were supposed to produce a summary contribution to a collected volume of recent works in social psychology, the three were sure that Adorno would have no part in it, but Frenkel-Brunswik and Sanford locked horns with one another about the ordering of names. According to Levinson, who was much younger and had no voice in this, the decision in favour of an alphabetical order was not reached for reasons of conventionality, but because it expressed the order of intellectual achievements. Sanford could put forward neither his role as a founder nor that as a director against Frenkel-Brunswik's disproportionately larger contribution to the content.

Table 6.6 Authors' Contributions to TAP by Chapters and Pages

Author	Contribution		Contribution	
	%	no. pages	%	no. chapters
Adorno, Theodor W.	19	178	17	4
Frenkel-Brunswik, Else	21	195	22	5
Levinson, Daniel J.	22	213	17	4
Sanford, R. Nevitt	7	67	13	3
Aron, Betty	6	55	4	1
Morrow, William	8	73	4	1
Levinson, Maria Hertz	8	79	4	1
Sanford, Adorno, Frenkel, Levinson	9	88	4	1
Unlabelled	3	32	13	3
	100%	948	100%	23

Table 6.6 shows that, when TAP was published, the authors who were further down, alphabetically speaking, would have had more than one reason to object to Adorno's prime place in the citation. Given the number of chapters and the number of pages, Frenkel-Brunswik and Levinson certainly topped, or were even with, Adorno. Since Co-Director Sanford had contributed relatively little to the book as such, an ordering according to the quantity of writing, and the quantity of intellectual input that went along with it, i.e. Frenkel-Brunswik, Levinson, Adorno, Sanford, would have more or less reflected the facts but would have made Sanford's position appear less important than he was ready to accept. To be the last one in the alphabet is something quite different from being the last one on an authors' list that was ordered according to other criteria. Any attempt to relegate Adorno to the end of this achievement list would have failed because of his resistance as well as the resistance of those whom he would have been able to mobilize. If Theodor Wiesengrund (Adorno) had not changed his name even before his naturalization in the United States in 1943 and thereby catapulted himself to the top of the alphabet, the order would have corresponded to the actual contributions, and Adorno would have stood hardly a chance to protest against this. That the alphabetic ordering was ultimately chosen was only due to the fact that Frenkel-Brunswik and Sanford could not agree on which of them had made the larger contribution to the study – the rejoicing third who became the first was Adorno.

In the spring of 1950, TAP was published as the fifth and last volume of the series *Studies in Prejudice*, and for the institute Adorno's top position on the list of authors must have been something like the crowning victory in a long battle for priority. In the preceding years, there had in fact been a number of conflicts about the issue of what part the institute had had in the work compiled in the *Studies in Prejudice*. The key question here was who was to be

presented as their editor. Finally, a compromise was reached that was satisfactory for both sides: editorship was credited to Max Horkheimer and Samuel H. Flowerman. The short-term director and long-term adviser of the Department of Scientific Research, and his successor, who had been in office five times longer than he but was inferior to him in terms of age and status, both represented the AJC. But to third parties, Horkheimer could also present himself as the Director of the Institute of Social Research who was cooperating with a minor AJC officer. When the first reviews of the first two volumes appeared, the AJC became aware of Horkheimer's double strategy. In a long letter, Flowerman, Horkheimer's successor in the AJC Department of Scientific Research, confronted him about this:

These items [reviews and reports] were discussed yesterday at a Staff Policy Committee meeting where I found myself on the defensive against allegations that a common element was present – and another common element absent – in each of these items: the Institute of Social Research is invariably mentioned; the Department of Scientific Research of the American Jewish Committee is invariably omitted ... it was pointed out that these studies were launched while you were on the staff of the American Jewish Committee ... that the books by Massing and Lowenthal were completely financed by the American Jewish Committee; that Massing was not even a regular member of the Institute but was hired to do this special job ... the [American Jewish] Committee likewise paid for editorial work on the books ... and completely subsidized the publication ... The inference was made that in both book reviews, in the sociology journal, and in the McWilliams book, Institute personnel were in a position of influence and that therefore the omission of reference to the Department of Scientific Research ... could not be regarded as accidental. The further inference was made that the chief aim of the Institute has been to seek its own self-aggrandizement even at the expense of other groups.⁶⁷

Horkheimer was asked to immediately clear up the accusations and to make sure that in the future due reference was made to the role of the AJC. Contrary to his usual conduct, the addressee took his time before responding. In a letter to Adorno, who at the time was in Germany, the reason for this delay is explained, as were Horkheimer's countermeasures:

There was a number of reasons for me (to go to San Francisco). Given the fact that the Slawsons and the Flowermen keep foaming at the mouth because in the first reviews of the Series in the New York Times the Institute was highlighted rather than the Committee, I felt that the thing to do was to renew relations with Sanford and Brunswik by a personal visit – without, however, mentioning the affair itself. If the Committee wants to conspire, it would probably not help anyway, but ... I did it so as not to feel I neglected something.⁶⁸

Horkheimer's response to Flowerman was a strange medley of servility and arrogance. He started out by denying that he or anybody else from the Institute had influenced the reviewers. But, then, he launched a counterattack by listing those publications and those articles in the press that mentioned the AJC

but not the Institute of Social Research. And, finally, he proposed to Flowerman that, in the beginning, the AJC had been rather fearful that there might be too much publicity. This letter, as well as others, also tells us that the AJC refused to abide by Horkheimer's request that the institute be cited on the jacket or the cover of TAP. In a letter to Slawson, Horkheimer rather brazenly argued that since the group no longer existed, there was no need any more to cite the Berkeley Public Opinion Study.⁶⁹

As a result of the high visibility that the Frankfurt School temporarily enjoyed among the German-language audience, the history of the institute was not only written down but was also, and no less so, informed by self-portrayals, speeches, lectures, conferences, interviews and other forms of oral records. Among the published self-portrayals, Adorno's 'Wissenschaftliche Erfahrungen in Amerika' (Experiencing Research in America) (Adorno 1981) is a key contribution. The study of the first historian of the Frankfurt School, Martin Jay, which was published in English in 1973 and in German three years later, had a lasting impact on the American and, after the German version was published, on the German perceptions of the Frankfurt School (Jay 1973). Jay built on information by those members of the institute who were still living at the time, as well as on the selective exploitation of the writings of those who were already dead. Rolf Wiggershaus's even more detailed history of the Frankfurt School, it is true, addressed some of the more problematic aspects of the history of the institute as seen by insiders; but while he provided many more historical details than Jay, this did not do much to modify the institute's by now well established image (Wiggershaus 1986). All the historical accounts of the institute are almost exclusively based on the papers left behind by former members of the institute, and the prime source for this is the Max Horkheimer Archive, which not only houses the papers of its namesake but also a lot of material by others who had temporarily been affiliated with the institute.

By the late 1970s, the legend of the years of exile of the Institute of Social Research was firmly established, and was told more or less as follows:⁷⁰ the Frankfurt Institut für Sozialforschung, thanks to the foresight of its directors, had emigrated in good time, its independent funding allowing its members to settle first in New York, then in California, and to go on with their theoretical work as well as to support other emigrants. Its financial independency enabled the institute to keep its distance from the corrupting business of American social research. After some time, on their own initiative, they decided to address American issues. When reports on the almost complete extermination of European Jewish life began to register with the more general American public, they were charged by an honourable Jewish defence association to do a research project that investigated the fascist potential of an outwardly democratic society and, five years after the end of the war, delivered a five-volume report that provided evidence that anti-Semitism was a widespread phenomenon and a danger to democracy in the United States, as well. Having accomplished this task of enlightening the people, and having published a

philosophical manifesto, the *Dialectic of Enlightenment*, the key figures of the institute turned to a new challenge: re-establishing the institute in Frankfurt and participating in the democratic reconstruction of German universities and German society. Their transatlantic luggage included a suitcase with brand-new methods in social research, which they made it their job to use and to teach. Only when they could no longer deny that German social scientists were more interested in stabilizing capitalist exploitation than in an emancipatory overcoming of it, did they turn away from sociology and re-turn to philosophy.

Since there was no complete translation of TAP, this legend was all the easier to propagate. Horkheimer was initially interested in having a German translation and, in 1952, had Frankfurt students do a rough translation of all five volumes.⁷¹ (Many years later, this version was distributed in a pirate edition.) Since the AJC was unwilling to subsidize the German edition, Horkheimer lost interest. Later, he and Adorno seem to have actually done their best to prevent the publication of a complete German edition of TAP. Rather, German readers were provided merely with a partial edition of less than one-fifth of the text. And after what has been described in the present chapter, it will come as no surprise that this translation included only those chapters where Adorno was either the author or had successfully laid claim to being included in the authors' quartet.