

The choice between market research and sociography, or: What happened to Lazarsfeld in the United States?

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Sous la direction de
Jacques Lautman et Bernard-Pierre Lécuyer

PAUL LAZARSFELD

(1901-1976)

La sociologie de Vienne à New York

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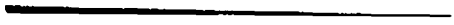
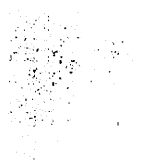
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***The Choice Between Market Research and Sociography,
Or : What happened to Lazarsfeld in the United States?****

Christian FLECK

Choisir entre études de marché et sociographie, ou : Qu'arriva-t-il à Lazarsfeld aux Etats-Unis? Dans cet article, je veux d'abord résumer les expériences de Lazarsfeld à Vienne puis reconstruire une partie du processus par lequel Lazarsfeld s'est transformé en sociologue américain. Comme les autres immigrants, il dut faire des choix entre diverses options d'affiliation à des disciplines plus ou moins établies, à leurs programmes, à leurs écoles et à leur champ de recherche, puis il dut s'adapter à un nouvel environnement, à une nouvelle culture et à de nouveaux modèles de réussite universitaire.

La carrière de Lazarsfeld à Vienne fut relativement brève. Il n'y a fait des recherches en sciences sociales que pendant cinq ou six ans. Auparavant il militait au sein du mouvement de jeunesse et du Parti Social Démocrate, puis il soutint une thèse de Doctorat en mathématique et participa aux séminaires d'un couple de psychologues nouvellement nommés, Karl et Charlotte Bühler. Les premiers articles qu'il écrivit montrent sa tentative d'associer marxisme et psychologie. Pour lui, la « conception marxiste du monde » offrait une interprétation des « grands événements » qui « apportait un nouvel éclairage sur le monde ». Son intérêt pour la psychologie grandit, renforcé par les travaux de sa mère sur la psychologie individuelle et par les croyances et les idées du milieu social auquel il appartenait. Ce n'est pas avant 1929 que l'on peut trouver des traces dans ses

* I am grateful to those who read and commented a first draft of this paper and to all with whom I had the opportunity to talk about Paul F. Lazarsfeld. Special thanks go to Robert K. Merton whose comments and copy-editing improved the paper. Daniel Bell's and David Riesman's detailed remarks rised more questions than I was able to answer. Part of the research was funded by a grant of the « Fonds zur Förderung der wissenschaftlichen Forschung » (P 10061).

écrits de certaines idées issues de toute évidence de la doctrine psychologique des Bühler. Ce n'est que lors de sa rencontre avec la psychologie des Bühler qu'il trouva un cadre théorique qui appelait par le fait à la synthèse entre statistique et psychologie. Durant la période pendant laquelle il fut assistant de Charlotte Bühler, entre 1928 et 1931, Lazarsfeld fut surtout responsable des analyses statistiques; il faisait aussi les comptes-rendus des publications américaines qu'elle recevait et enseignait la statistique. Bien que Charlotte Bühler eût de l'estime pour les capacités analytiques de Lazarsfeld, elle appréciait beaucoup moins son ambition marxiste. Au moment où Lazarsfeld avait intégré le cercle Bühler il n'avait encore jamais reçu de formation conventionnelle en science sociale ni n'avait expérimenté la routine quotidienne de la recherche scientifique.

La fondation en 1931 du Wirtschaftspsychologische Forschungsstelle permit à Lazarsfeld de prendre ses distances d'avec les Bühler sans rompre tous les liens avec eux. Comme la comparaison avec d'autres institutions de recherche en sciences sociales qui furent créées à la même époque le montre, sous bien des aspects le Forschungsstelle était une exception. Le plus gros problème auquel devait faire face ce nouvel institut était le financement de ses activités. On ignore si la décision de créer un institut indépendant fut prise avant ou après la visite au Département de psychologie d'un étudiant américain qui avait déclaré qu'aux Etats-Unis les études de marché constituaient une entreprise rentable. Ce que l'on sait, par contre, c'est que Lazarsfeld projetait de financer son institut en obtenant des contrats de travail auprès d'autres instituts. Bien évidemment, ce ne fut pas tâche facile dans un pays touché par les effets de la crise économique mondiale. Les efforts de Lazarsfeld ne connurent qu'un succès très bref, et on a des raisons de croire que c'est cette situation financière désastreuse qui conduisit le Forschungsstelle à projeter d'enquêter sur les conséquences socio-psychologiques du chômage et engendra Die Arbeitslosen von Marienthal. Ne parvenant pas à obtenir de contrats, le Forschungsstelle aurait pu se tourner vers des moyens de financement plus conventionnels. La Fondation Rockefeller, dont les Bühler étaient administrateurs, et le mouvement ouvrier étaient des choix évidents.

Marienthal se distingue très nettement des autres publications de Lazarsfeld et ses pairs durant cette période. Les préparatifs de l'enquête commencèrent à l'automne 1931, et le travail de terrain vers la fin de l'année. Un examen plus attentif de la méthodologie employée dans Marienthal mettrait en évidence la nouveauté de l'approche. Deux de ses aspects sont originaux : premièrement, parce qu'aujourd'hui on dirait de Marienthal que c'est une recherche-action; deuxièmement, parce que plusieurs moyens de collecte des données ont été utilisés et que des efforts ont été faits pour les associer. Marienthal est aussi exemplaire en ce qu'il ne dévia pas du principe selon lequel les méthodes et procédures employées devaient convenir à l'objet de la recherche.

Se limiter à lister les aspects cognitifs et institutionnels qui ont fait de Marienthal une enquête innovante n'en donnerait pas une image complète; ses aspects politiques et sociaux sont tout aussi significatifs. On pourrait soutenir en effet que

c'est l'intégration de la recherche dans le mouvement social-démocrate d'une part, et le fait que Marienthal était un village dont la population entière était au chômage d'autre part, qui permirent aux chercheurs de rendre compte de l'intégralité de ses conditions sociales, assurant ainsi le succès de l'enquête. Mais l'appartenance des chercheurs, comme leurs enquêtés, au mouvement social démocrate aida aussi à surmonter d'éventuelles difficultés.

Marienthal fut la seule grande enquête menée par le Forschungsstelle. Par son approche innovatrice dans laquelle austro-marxisme et psychologie sociale s'équilibraient, elle aurait bien valu qu'on la prolonge et l'approfondisse mais elle fut stoppée avant d'avoir reçu tout le crédit qu'elle aurait mérité. C'est de l'extérieur de la communauté scientifique que vint le plus mauvais coup porté à l'avenir du Forschungsstelle. En effet, peu avant la publication de Marienthal, le Parti national-socialiste était arrivé au pouvoir en Allemagne. Un an plus tard, le mouvement austro-marxiste était écrasé par l'Austro-fascisme. Toutes les organisations de gauche furent mises hors la loi. En tant qu'institut privé le Forschungsstelle ne fut pas touché directement par la suppression du mouvement social-démocrate; mais il le fut indirectement car le Conseil d'administration comprenait des représentants officiels du Parti social-démocrate. Il perdit ainsi beaucoup de ses soutiens et de ses sources de financement, la plupart des membres du Forschungsstelle ayant dû quitter l'Autriche entre 1933 et 1938.

Lazarsfeld arriva aux États-Unis comme simple boursier de la Fondation Rockefeller. Il se transforma en exilé lorsqu'il décida de ne plus retourner vivre à Vienne après que Dollfuss eut pris le pouvoir en Autriche. Peu de biographies rendent compte de la spécificité du cas de Lazarsfeld. Quand il était boursier à New York de 1933 à 1935 il se sentait reçu comme un invité par ses collègues américains. Mais après sa décision d'immigrer tout changea et Lazarsfeld vécut la vie des réfugiés pendant quelques mois.

Comme tout émigré il paraît avoir eu des difficultés à s'adapter à l'environnement intellectuel des exilés. Il marqua sa différence en tentant constamment de nouer des contacts avec ses nouveaux collègues. Il se rendit dans plusieurs universités pour y trouver des collaborations. L'une des premières relations qu'il établit lui permit de rencontrer les membres de l'École de Chicago; puis il prit contact avec Robert Lynd à Columbia. Personne parmi eux cependant ne fut intéressé par l'analyse méthodologique de son programme de recherche. Paul Lazarsfeld reçut donc plus de réponses positives dans le domaine des études de marché que de la communauté universitaire. C'est peut-être en partie par gratitude envers ses premiers soutiens amicaux qu'il y resta si longtemps.

C'est en jeune et brillant universitaire que Lazarsfeld arriva à New York, non en expert d'un domaine de recherche donné ou en représentant d'un style de recherche particulier. Je voudrais pourtant montrer que Lazarsfeld avait au moins deux spécialités à proposer à son auditoire américain : d'abord sa capacité à expliquer l'action et à découvrir les racines d'un processus de décision; ensuite un intérêt fort, bien que frustré, pour la méthodologie de ce qu'il est convenu d'appeler sociographie. Il avait aussi deux compétences techniques à offrir : son habi-

leté à créer les instituts de recherche, et son talent pour analyser les données quantitatives. Une seule de ces spécialités fut jugée intéressante, mais Lazarsfeld possédait les deux.

Le nombre des centres d'intérêt de Lazarsfeld et l'étendue de ses capacités lui permirent d'envoyer plusieurs ballons d'essai afin de déterminer dans quel domaine il pourrait le mieux être reconnu par la communauté des sciences sociales américaine. Pendant sa première année aux Etats-Unis il écrivit deux articles assez longs qui relataient ses expériences viennoises. Plus tard dans ses Mémoires il montre clairement que l'écho produit n'avait pas correspondu au message qu'il espérait faire passer ni à l'accueil qu'il espérait recevoir. Le plus célèbre de ces articles est « The Art of Asking Why » qui traite de trois des principes de base à la formulation des questionnaires. L'autre article n'existe que dans une version dactylographiée non publiée. Écrit en 1933, il s'intitulait « Principles of Sociography ». Lazarsfeld le proposa à Social Research, la revue de la New School for Social Research. Comme beaucoup des sociologues qui avaient dû quitter l'Allemagne après le printemps 1933 y travaillaient, Lazarsfeld avait espéré intéresser les éditeurs. Il le refusèrent. Dans la dernière partie de mon article je fais une analyse des « Principles of Sociography » où j'étudie la tentative de sociologie qualitative de Lazarsfeld, ses mérites et ses limites.

INTRODUCTION

Lazarsfeld arrived in New York in September 1933 at the age of 32. Half a year after the first German refugees reached the secure haven of the then New School for Social Research. Compared with these well-known German professors Lazarsfeld was at the time of his arrival a no-name scholar.¹ But, more important, Lazarsfeld was not a refugee; he came to the US as a one year Rockefeller Fellow and had promised to return to Vienna after the end of the scholarship. As you know, he was able to add a second year as a Rockefeller fellow and at the end of that year he decided to change his status but not his residence. In 1935 Lazarsfeld became an immigrant and a couple of years later an American citizen. Differing from the vast majority of refugees in this way Lazarsfeld could begin his career as a visitor, changing his intellectual orientation much more slowly than the refugees.

In this paper I want to reconstruct one part of this process of his becoming an American sociologist. Like other immigrants he had to make his choice between different options of affiliation to more or less well-established scientific disciplines, their programs, schools, and fields of research, and he had to adapt himself to the new environment, culture and patterns of academic achievement.

Lazarsfeld came to New York as a well-educated, bright young scholar, but not as a well-defined expert or representative of a distinct intellectual orientation or style of doing social research. I'd like to argue that Lazarsfeld could offer his American audience at least two different foci of interest : On one hand, his well-known interest in explaining action and discovering the roots of the decision-making process, and on the other, a strong but frustrated interest in the methodology of so-called sociography. And Lazarsfeld had two skills to offer : The ability to create research institutions and his competence in analyzing quantitative data. Only one of the foci found sufficient demand, but Lazarsfeld was able to apply both abilities.

I shall first and briefly summarize Lazarsfeld's Viennese experiences, without falling into sheer story-telling and reproducing the myths that have emerged from the anecdotes which Lazarsfeld loved to tell on different occasions. I shall then trace his first papers in the US, in an attempt to establish whether he was able to build on and evolve the methodology he had developed in Austria and examine the extent to which his ideas were accepted by the American scientific communities. Finally, I shall argue that Lazarsfeld lost sight of a promising way of clarifying social research practices.

LAZARSFELD'S MARXISM

Lazarsfeld's Viennese career was relatively short. He had worked in the social sciences only for five or six years. Before this he was active in the youth movement and in Austria's Social Democratic Workers Party; later, he finished a Ph.D. in mathematics and began to participate in the seminars of the newly appointed couple of psychologists, Karl and Charlotte Bühler. At this time – the end of the 1920s – Lazarsfeld shared the ideological views of the so-called Austromarxists and the psychological orientation of Alfred Adler.

Lazarsfeld's early written papers reflect his attempts to bring together Marxism and individual psychology. In 1927, he attended two « International Conferences of Socialist Individual Psychologists » and summarized the discussions in a very informative report.² His introduction tried to explain why there had been « frequent attempts in the last few months » to have socialism utilize psychology. To illustrate his point he used a metaphor that he was using again 42 years later : « Disappointed hopes had made many comrades withdraw from the active political struggle after the revolution (of 1918) » and « prompted them to try to trace the roots of the sad events in the soul of man. »³

Yet while Lazarsfeld in his 1969 memoir is content to recall that at the time he had « created a formula » which stated that « a fighting revolution requires

economics (Marx); a victorious revolution requires engineers (Russia); a defeated revolution calls for psychology (Vienna) »⁴ his 1927 article continues in a different tone :

« After a few years, Socialism had sufficiently recovered ... With new energy everyone can concentrate once again on the old struggle, even those who sought a temporary refuge in psychology and education. Understandably, they do not want to see the energies they have expended on their work in the previous years wasted, and so try to bring about a synthesis of labor movement and psychology. »⁵

For Lazarsfeld this attempt to create a synthesis is a « positive symptom of a renewed political awareness and readiness to continue the fight. » He never queries the argument that Marxism needs to be « placed on a psychological footing. » The « techniques of the class struggle » were « typical psychological efforts. »⁶ Psychology likewise played a decisive role in « outlining the possibilities » of « future programs of socialization and related plans » that had to be drawn up.

Lazarsfeld's Austro-Marxist conception of sociology is clearly revealed in this and other articles where he gives the Austro-Marxists credit for having developed a variant of Marxist sociology that could provide valid explanations of collective social processes. He feels however, that the Marxists have failed to outline a social psychology which could answer « the question of the organizability of the individual » and would define the « potential and scope of our actions ».⁷ Although Lazarsfeld's proposals remain on the whole programmatic, they clearly indicate the role he wants to have social psychology play. Summing up his views of the time we might modify his 1969 formula : psychology was the science of the imminent revolution.

LAZARSFELD AND THE BÜHLERS

The papers published by Lazarsfeld before 1929 throw light on his gradual approximation to the ideas of Karl and Charlotte Bühler. Before that, the main influence on his psychological thought came from Alfred Adler. Not until 1929 can we trace certain ideas in his writings that were obviously formulated in accord with the Böhlers' psychological doctrine.⁸ It seems no exaggeration to say that in those years Lazarsfeld's choice of theories depended on the political position adopted by their authors. He is convinced less by their psychological conceptions, than by their political attitudes, supporting those whom he evidently feels he can « trust » in the political sphere. Disliking the party officials he would rather support the leftist opposition, for example Otto

Rühle, or outsiders like Hendrik de Man, than be content with the centrist theoreticians of the Social Democratic Workers Party. That the Böhlers exerted a disciplining influence on his intellectual development was affirmed by Lazarsfeld in *Jugend und Beruf* (Youth and Occupation) :

« The author sees the vindication for this experiment in the fact that after having first worked for a decade with his friend Ludwig Wagner on the fascinating problems of the young generation and then having been in the fortunate position to have found the scientific method in Charlotte Bühler's work which allowed him to objectivize his experiences and to apply them. He strove to combine experiences and methods in a way which he hopes his long-standing friend and scientific mentor will find beneficial. »⁹

Lazarsfeld first mentions Karl Bühler in his article *Gemeinschaftserziehung* (Co-education, 1924), albeit only in the bibliography where he quotes Bühler's *Die geistige Entwicklung des Kindes*. In 1926, Lazarsfeld collaborated on a project coordinated by Charlotte Bühler that was entitled *Berufseinstellung des jugendlichen Arbeiters* (Occupational Attitudes of the Young Worker). The results of the study were not published until five years later, when it became part of *Jugend und Beruf*. They include Lazarsfeld's analysis of 1,100 questionnaires which a leading official of the social democratic youth organization had allowed him to use for what later became known as « secondary analysis. »

The survey was originally designed to provide information about the members of the *Sozialistische Arbeiterjugend* (Young Socialist Workers). The data were severely restricted in scope, consisting mainly of information on employment, job satisfaction, alternative job preferences, and ideals. Lazarsfeld's comments indicate that he realized the limitations of his analysis, regretting that neither father's occupation nor « reasons for the choice of the present job » were included in the questionnaire, and emphasizing that the survey was limited to « a certain type of young worker, the one affiliated to a local political organization. »¹⁰

Lazarsfeld's primary categories were therefore confined to gender and place of residence. The inclusion of the latter was sensible, he felt, because it allowed him to capture « the intellectualizing influence that industries exerted on the domestic environment of those surveyed. »¹¹ The main independent variable was taken as the increasing level of industrialization that not only lessened people's job satisfaction but affected their life aspirations in general. In his view, an « industrialized society » had fewer « primitive wishes for happiness », showed greater « skepticism », was more political, and « clearly and constantly reflected formative influences. ». In a sensitive analysis Lazarsfeld draws up a ten-part classification of people's life aspirations that he subsequently collapses into four « factors », before attempting to correlate

the changes in life-goals with age-groups. He thereby arrives at a developmental stage-model which he describes as « transition from one's personal economic distress to social sublimation ». ¹²

Lazarsfeld thus combined statistical analyses and political assessments, a decision he justified in his conclusion as follows : « At any rate, the findings should provoke discussion. And that is how it should be, since it is the function of statistics to make numbers speak and people act. » ¹³ It would be wrong, however, to assume that this study signalled the end of Lazarsfeld's socialist commitment and the beginning of a purely scientific career. Indeed, there is no evidence at all to suggest that this was the time at which he finally aligned himself with the Bühler school. (Thus he makes reference to Bühler).

That Paul Lazarsfeld was at the time beginning to tend more towards the Bühlers view is cogently illustrated by his brief study on *Körperliche und geistige Entwicklung* (Physical and Mental Development). Following Karl Bühler's « advice » as he states, ¹⁴ Lazarsfeld examines the extent to which these two developments are correlated. Different authors – amongst them the Adlerian individual psychologists – had come to very different conclusions. This little known treatise beautifully illustrates Lazarsfeld's intellectual development from the late 1920s onwards. Having worked under Alfred Adler he had a good grounding in statistical methods that he could now bring into play. Yet it was Bühler's advice which allowed him to fully utilize this knowledge by helping him formulate the right questions. Bühler's questions can be translated into a table with four sections which again allows Lazarsfeld to demonstrate his prowess as a statistician – one is tempted to say, on a higher level. While in his writings based on individual psychology, Lazarsfeld had rarely ventured beyond programmatic proposals (with the exception, perhaps, of his study on the occupational attitudes of young workers where his qualitative and classificatory interpretation is very sensitive, although the quantitative analysis remained rather rudimentary), the later influence of the Bühlers helped him prepare statistical analyses that were relevant in their content but successful in form.

Lazarsfeld's intellectual development during his Viennese years can, I believe, be explained as follows : In his early years, he had become acquainted with Marxist thoughts through experiencing them, to use his phrase. To his mind, the « Marxist conception of the world » offered an interpretation of the « great events » which « showed the world in a new light. » ¹⁵ He soon developed an interest in psychology, confirmed by his mother's work on individual psychology and by the beliefs and ideas of the social milieu to which he belonged. In addition, yet independently, he mastered the scientific formalisms which at first he was unable to apply to the hypotheses of Marxism and individual psychology because neither of these

doctrines was amenable to such an analytical approach. It was only when he encountered the Böhlers' psychology that he was introduced to a theoretical construct which virtually invited the synthesis of statistics and psychology.

It was obviously Lazarsfeld's skillful handling of statistics which persuaded the Böhlers to enlist his support – despite his being a mathematician and his training as an individual psychologist he was in both respects an outsider in their school. His first publications as one of Charlotte Bühler's collaborators furnish ample proof of this. Whenever Lazarsfeld includes quotations from work he had written in his pre-Bühler period to demonstrate some fundamental statistical procedures, he clearly indicates that he wishes to distance himself from the content of those analyses (remarking in formulary style « content of the table is completely irrelevant for the purpose of our discussion »¹⁶).

Charlotte Bühler has been described as an excellent organizer by her onetime former colleagues and students. Yet their admiration for her was not unqualified. A somewhat critical undertone makes it clear that Bühler's overbearing ambitions did not always leave her assistants sufficient scope to pursue their own interests and preferences.¹⁷ Years later Lazarsfeld, too, joins this account. In his memoir he writes :

« She [Charlotte Bühler] had a Prussian ability to organize the work activities of many people at many places. Some felt exploited by her, but I always appreciated her good training and help. »¹⁸

During his time as Charlotte Bühler's assistant between 1928 and 1931, Lazarsfeld was above all responsible for statistical analyses, reviewed American publications which came within her remit, and taught a course in statistics.

Although Charlotte Bühler appreciated Lazarsfeld's analytical abilities, she was definitely less supportive of his Marxist ambitions, as is evidenced by the history of « Occupational attitudes of young workers » which he first presented at Bühler's discussion circle. She was prepared to accept it, but only after Lazarsfeld had revised it to delete all political statements.

« But she [ie. Charlotte Bühler] objected strenuously to the tone in which the section on proletarian youth was written. I was, indeed, full of compassion, talking about exploitation by the bourgeois society and the hortative style of this section was quite different from the rest of the manuscript. I could not deny this fact, and finally rewrote it. None of the argument was omitted but the whole tone became descriptive and naturalistic, instead of critical. »¹⁹

The version which was finally published retains a number of passages which show that Charlotte Bühler and Lazarsfeld still differed on at least two issues. Strongly influenced by individual psychology, Lazarsfeld was

reluctant to discard its conceptual system completely. Moreover, he never felt fully at ease with Bühler's developmental stage-model or the life-span research which she had just initiated. Lazarsfeld defined adolescence in terms of actions and options and, not like Bühler, in terms of biological and idealistic concepts. « (...) Adolescence is characterized by the fact that (...) the individual must take decisions which determine the rest of his life, even though he does not yet have sufficient knowledge of himself or the reality of life on which to base these decisions. »²⁰ In his survey of existing literature on young people and their occupations, which Lazarsfeld probably did not complete until 1930/31 when he submitted it for publication of the book, Lazarsfeld's references to Charlotte Bühler's phase model are generally little more than polite gestures towards the head of department and editor of the series. He is quite explicit about attributing merely explorative value to life-span research.²¹

Looking back on the revised work, Lazarsfeld admitted that the task had made him realize that one could find a less critical formulation – « a more descriptive and naturalistic one » – without modifying the essential argument. This insight, he conceded in retrospect, had a long-term effect on him. The Marienthal study, for example, (although it was not written by Lazarsfeld himself) was equally purged of explicit political statements. Not only did his work under the Bühlers have a positive influence on Lazarsfeld's writing style and cognitive development, it also improved his organizational skills.

SUMMING UP THE VIENNESE YEARS

A close look at Lazarsfeld's publications in 1931 suggests that he was gradually beginning to dismiss Marxism at the theoretical level while remaining committed to its political aims. Before 1929, Lazarsfeld had been a professed Marxist (and individual psychologist) in all questions of theory. In 1931 he slowly emerges as someone who has freed himself from the bonds of Marxist theories and is now trying hard to present his arguments within the categorial framework of the Bühlers' psychology. At the same time, he remains true to his Socialist views, using them as the experiential basis for his research.

« Only the researcher who has firsthand experience of a problem so that his conceptual and methodological apparatus is derived through introspection, so to speak, and who, in spite of this personal involvement, possesses the scientific ruthlessness to translate the experience into data and verifiable formulae, or at least in statements about presumed links which are in principle amenable to this kind of analysis – only he will help us to gain a clearer view than we have now of the problems. »²²

However, we cannot ignore another personal aspect of his work style, if we are to comprehend Lazarsfeld's intellectual development fully. Marie Jahoda has described this facet of Lazarsfeld's individual style as « foxiness » (following Isaiah Berlin). By this she referred to a cognitive style and approach to work characteristic of people who know about many different things, thus differing markedly from the « hedgehog » who knows one great thing.²³ But historical circumstances required Lazarsfeld, she observed, to « put on the mask of the hedgehog ». The wide range of different projects Lazarsfeld engaged in the 1930s and his numerous activities in the 1920s provide excellent illustrations of his « foxy » style.²⁴

The Austromarxists offered Lazarsfeld a macrosociological orientation, in particular (a rather weak) theory on social discrimination, social stratification and power. Alfred Adler contributed some psychological insights to Lazarsfeld's point of view as well as an intensified conviction on the necessity of meliorating the lot of lower-class members.

At the time Lazarsfeld joined the Bühler circle he had not received any formal training in social science or experience with the everyday routines of scientific work. That worked to help him develop new and innovative perspectives on various topics.

CREATING RESEARCH INSTITUTIONS

As few historical surveys mention the *Wirtschaftspsychologische Forschungsstelle* and *Marienthal* are two of the more successful enterprises initiated by Austrian sociologists in the inter-war years. Nevertheless in spite of their great popularity, these two innovative projects are not always accurately represented. As we shall now see many traditional descriptions of those achievements are fraught with distortion, misinterpretations and omissions. These were mostly introduced by authors who accepted the best-known report about this episode at face value. In his *Memoir*, Lazarsfeld gave the following account :

« Slowly, my work as assistant at the university expanded, and I also taught courses in social and applied psychology. I received a small remuneration, by no means sufficient to give up my position in the Gymnasium. Still, my desire to shift entirely to the Psychological Institute increased, and around 1927 I got the idea that I would create a division of social psychology at the Institute. This would permit work on paid contracts, and from such sources I would get a small but adequate salary, in keeping with the generally low standard of living. The idea was realized in the form of an independent research center (*Wirtschaftspsychologische Forschungsstelle*, a term connoting broadly the application of psychology to social

and economic problems), of which Karl Bühler was the president. From then on, I directed the applied studies of this Center, and at the same time gave my courses at the University Institute and supervised dissertations. A number of students worked at the *Forschungsstelle*, and quite a few dissertations were based on data collected there. »²⁵

A word of caution is called for here. Interpreted too literally, Lazarsfeld's autobiography might easily give the wrong impression. What Lazarsfeld describes retrospectively as a number of independently taken decisions which were all successfully put into practice, is in fact the reconstruction of a « successful career » by a great scholar, who chooses to include his years as a research assistant in his recollections as a necessary albeit soon completed, stage in his career. Having been long established as a distinguished professor, Lazarsfeld forgets how difficult it was to overcome these first career hurdles.

Anyone with even a superficial knowledge of the Austrian university system as it was in the late 1920s is bound to be disturbed by this account.²⁶ How could a 26-year-old graduate in mathematics have possibly realized a plan that ran counter to all academic traditions and mechanisms of career advancement? A more critical look at Lazarsfeld's recollections in the light of historical evidence produces a rather different picture :

- Lazarsfeld's courses in statistics do not appear in any of the official lists of courses published by the university at the time,
- his name is not included in the list of staff members,
- no personal file of Lazarsfeld was found in the university archives,
- no division of social psychology was ever created at the University,
- Lazarsfeld could not have officially supervised student dissertations, and
- the *Forschungsstelle* was not officially established until November 1931.

This discrepancy between Lazarsfeld's autobiographical account and historical documents calls for a more detailed analysis of the events leading to the establishment of the *Forschungsstelle*.

For many authors the *Forschungsstelle* (research center) represented a new type of research institution. It was not directly attached to a university, yet it was still linked with one. This interpretation would be correct if the year of foundation quoted by former members of the *Forschungsstelle*'s staff in their memoirs were accurate, but there is no evidence to suggest that 1925, the date they all give, is the actual date. All contemporary publications and sources agree that the *Forschungsstelle* was not officially established until 1931. In *Konsumentenpsychologie* (entitled *Market Research in Austria* in its English translation), Hans Zeisel mentions the *Forschungsstelle* in his editorial note, referring to its foundation « two years ago ».²⁷ In his Bühler

Gedenkschrift Fadrus quotes from the 1931/32 annual report²⁸ which Karl Bühler had submitted to the *Pädagogisches Institut* of Vienna which also states that the *Forschungsstelle* was established during the period covered by the report. An *informal* group headed by Lazarsfeld had existed at the Psychological Institute since 1930, thus coinciding with Lazarsfeld's new interest in Bühler's psychology.²⁹ There is no contemporary indication that the grouping which was later to grow into the *Forschungsstelle* had existed before 1929.

If it is the case that the *Forschungsstelle* was founded only in 1931, then we must draw the necessary consequence and assign the title of « first research institute outside the university » to some other institution, one which is remarkably like the *Forschungsstelle*, but had been established earlier, in 1926. This was the *Österreichische Institut für Konjunkturforschung*, the « Austrian Institute for Business Cycle Research », which was headed by Ludwig Mises. Both institutes were organized like a society. They were presided over by a committee which included representatives from the Chamber of Commerce and the Chamber of Labor – anticipating the corporatistic structure of the Second Republic. Both institutes also had a board of trustees whose members included professors, senior civil servants, senior officials from professional organizations, and business people. And lastly, both institutions received funding from the Rockefeller Foundation.³⁰

We have no cogent evidence, other than the close personal links which existed between the two institutions, that Lazarsfeld's *Forschungsstelle* was an imitation of Mises' institute. Yet the close personal links between them, and even the physical proximity of the two institutions, as well as the chronology of their establishments, suggest that Lazarsfeld may have drawn inspiration from the already renowned Business Cycle Institute.

All staff members of the *Forschungsstelle* were of the same generation; none held senior university positions, as, for instance, a readership. These are just two of the special features that characterized the *Forschungsstelle*. They do not quite tally with Lazarsfeld's conclusions in his memoir that « the nature of the work (of a research institution) requires a more hierarchical relation among the participant professionals than is habitual in an academic department. »³¹ From the available reports as well as the objective data cited above we derive a very different view. The *Forschungsstelle* seems to have been organized along extremely egalitarian lines, with Lazarsfeld alone granted a special place as its leading intellectual authority. He later noted that all the research institutions that he headed over the years had been organized along much the same lines as the Socialist Youth clubs.³²

In those early years, Hans Zeisel, a childhood friend of Lazarsfeld, was the only staff member who did not belong to the Bühler circle. Indeed, his narrowly circumscribed role within the Bühler School seems to have been one main reason why Lazarsfeld sought to establish his own institution. A half-century later Lotte Schenk-Danzinger recalls : « Whenever we had any figures and numbers we never did the calculations ourselves but said : Listen, I've got the figures here, could you [Lazarsfeld] do the calculations and see what the outcome is. »³³ Lazarsfeld apparently found it impossible to fit his own social psychological interests³⁴ into the Bühlers' research program; nor did he evidently consider a career as an expert in statistics as an attractive prospect.

The foundation of the *Forschungsstelle*, allowed Lazarsfeld to distance himself from the Bühlers without severing all links with them. The *Forschungsstelle* itself is in many ways an anomaly, as a comparison of various social-science institutions which were set up at about the same time demonstrates. Most « founders » of these other institutions were on average ten years older than Lazarsfeld (average : founders of institutions : 43; of journals : 39), and they held higher positions within academia. Comparing these figures with the average age of professors at the time of their first appointment (40.1) we see that « founding » an institution was largely the reserve of professors soon after their appointment.³⁵

Had Lazarsfeld and the *Forschungsstelle* cut all links with Karl Bühler it would probably have been very difficult for them to raise money and to obtain research commissions. Bühler's appointment as the head of the institution gave a clear signal that he approved of the enterprise. This was an unusual decision for a head of a university department because normally they « had to » keep a tight rein on their followers and disciples which they are rarely prepared to relax. A further positive aspect of Karl Bühler's nomination as head of the *Forschungsstelle* was his reputation as a scientist which provided some counterbalance for the slight, or non-existent, standing of the institute's founders.

The greatest problem faced by the newly established institute was financing its activities. We cannot now ascertain whether the decision to found an independent institution was taken before or after the visit to the psychology department by an American student who mentioned that in the United States, market research was a profitable enterprise. We do know, however, that Lazarsfeld was planning to fund his institution through contract work for other organizations (Mises' *Konjunkturforschungsinstitut* got its money from the Chamber of Commerce, the official representative of Austria's entrepreneurs). Obviously, this was not an easy task in a country suffering the effects of the world economic crisis.

Lazarsfeld took a great personal interest in industrial psychology, which represented a further reason for establishing the *Forschungsstelle*.³⁶ A second factor seems to have been the survey amongst radio listeners which the group around Lazarsfeld were commissioned to conduct after Karl Bühler had conducted an experiment on radio in May 1931. Bühler had asked listeners to « guess » the personality of nine speakers whose voices they had heard on radio. A questionnaire included in the *Radio Vienna* program journal asked listeners to indicate the speakers' gender, occupation, appearance » overall self-confidence » and « personal appeal ». Around 3000 listeners responded. Lazarsfeld analyzed 1000 of the questionnaires within a record period, and published the « results » in the program journal. This successful cooperation between the Department of Psychology and *Ravag*,³⁷ the Austrian broadcasting company, might have led a few weeks later to *Ravag* commissioning a listener survey, and may have provided a further incentive for Lazarsfeld to establish the « *Forschungsstelle* ». ³⁸ Financial expectations, however, do not appear to have been met. Staff members were to recall later that « the financial situation was terrible. The money we got to carry out a survey was always spent long before the survey was completed. Then we obtained a new contract, and used the money to fund the previous commission ». ³⁹ A pattern, of course, which was repeated later in the Office of Radio Research and the Bureau of Applied Social Research.

Lazarsfeld's efforts did not enjoy a lasting success, and some evidence suggests that it was this dire financial situation which induced the *Forschungsstelle* to plan the study at Marienthal. Since only few contracts could be acquired in the marketplace this might have encouraged the *Forschungsstelle* to resort to the more conventional forms of research funding. The Rockefeller Fund, which was administered by the Bühlers, and the Labor Movement were two obvious choices. If both were to be won over as sponsors of a survey, a topic would have to be found that was of significance to both « worlds ». Lazarsfeld had originally contemplated carrying out a study on the leisure-time activities of the worker population who had recently been given more free time with the reduction of working hours. When Lazarsfeld discussed his plans with the intellectual leader of the Social Democratic movement, Otto Bauer, Bauer tried to convince Lazarsfeld that it was « silly » at a time of mass unemployment to conduct a study on leisure-time habits.⁴⁰ He also seems to have suggested the topic of unemployment and even to have mentioned Marienthal as a potential site for investigations.

MARIENTHAL

Compared with the other studies published by Lazarsfeld and his peers in that period *Marienthal* stands out even more distinctively. Preparations for the study started in the autumn of 1931, with fieldwork beginning towards the end of the year when Lotte Danzinger went to Marienthal to live in the community for six weeks.⁴¹ As stated in the preface of *Marienthal* « (...) contact with the population was facilitated » by Dr. Lotte Danzinger's preparatory work (...); she inspired the confidence to which we owe the copious biographical material ».⁴² Almost 60 years later Lotte Schenk-Danzinger recalled her somewhat mixed feelings about her work there :

« Well, I lived there for a while (ie. Marienthal) and did a number of interviews, but I really hated it. (...) I had a terrible, an awful room, really awful. That was for about a week, or perhaps ten days (...). I left the house in the morning and did a few interviews with different families, and then wrote them down in the afternoon, (...) you could not really write them down in the presence of the people because then they would have immediately stopped telling their stories, so you had to draw up the protocols from memory. »⁴³

Apparently there were trivial reasons for commissioning someone from the periphery of the *Forschungsstelle* to carry out the fieldwork. Jahoda was at the time completing her thesis and her final exams, Lazarsfeld was busy with the listeners' survey so that he could not leave his work at the Psychological Institute and the *Forschungsstelle* for any substantial time, while Hans Zeisel was working for a firm of solicitors in Vienna and was likewise unable to take an extended period of leave. We do not know how many students helped out occasionally, only that « ten psychologists » conducted the field-work.⁴⁴ That the three authors of *Marienthal*, who would later be primarily associated with the study, were only marginally involved at this stage was partly offset by staff meetings which were held once or twice a week and where « arrangements for the following days »⁴⁵ were made. This indicates that no definite research design had been worked out in advance and that possible methods and approaches were discovered only in the course of the study. A major advantage of the study is that the team was flexible and not routinized – even to the extent of issuing modified guidelines for the field-workers. Openness and the flexible responses to the specific requirements of the situation in their fieldwork are virtues open to few social researchers.

A further look at the methodology employed by *Marienthal* will perhaps highlight the novelty of their approach. This might best be discussed from two perspectives : First, methods which members of the team had used before (in their own investigations or other studies), and second, the categories in which these methods would fit today. As Table 1 illustrates, the method most

Table 1 :

Methods employed in the Marienthal study

NON-REACTIVE METHODS

- | | |
|--|--|
| 1. Official statistics and documents : | <i>Election results</i>
<i>Population statistics</i>
Complaints made to the Industrial Commission |
| 2. Analysis of documents | Account books
Library records (loans)
Subscriptions to newspapers
Membership figures of clubs
<i>Diaries</i> |
| 3. Observation | Measurement of walking speed |

REACTIVE METHODS

- | | |
|--|--|
| 1. Participant observation and action research | <i>Visits to families</i>
Clothing project
Medical consultation
Pattern design course
Girls' gymnastics course
Political Activity
<i>Parent Guidance</i> |
| 2. Expert reports | <i>Reports from teachers, parish-priest, town mayor, doctors, business people, officials from the clubs and organizations</i> |
| 3. Projective material | <i>School essays, essay competition</i> |
| 4. Tests | <i>Psychological tests</i> |
| 5. Written records | Family files (eg. records of meal)
Time sheets |
| 6. Direct interviews | <i>Life-histories</i> |

Note : Italics indicate that the methods had been used previously.

frequently employed today, *i.e.* direct surveys in which subjects are asked about their views and attitudes were little practiced at the time. When interviews were carried out at all, they followed quite different guidelines from those used today. Except for statistical information, the inclusion of which was to be expected, the researchers employed highly original methods of data collection which they had not encountered before, neither in their training nor in the relevant literature. The only methods they were familiar with, mainly from the surveys carried out by the *Verein für Sozialpolitik* (Association for Social Policy) were interviews of experts, the recording of life-histories, school essays and psychological tests (although the latter project had to be abandoned owing to a shortage of funds).

The methods used in *Marienthal* can be described as original in two respects : First, in today's terminology they would probably be classified as « action research » although, strictly speaking, this would be an incorrect description as the study did not primarily seek to activate the respondents politically. Action research, like communitarianism these days, ultimately implies that the researchers know what is « good » for the community they investigate. The research role is interventionist, with the investigators seeking to generate the social movement they feel the community lacks. The researchers in *Marienthal* subordinated their own objectives to the people's « needs ».

The Table also shows the « mixture of methods » used by the researchers. Efforts were made to employ various ways of collecting data or combinations of them. Again, we are probably justified in saying that their approach differs from most of today's practice. *Marienthal* was exemplary in its strict adherence to the principle that the methods and procedures employed should be appropriate to the object of the study. Lacking little or no precedents, they perform could not abide by traditional disciplinary strictures.

A half-century later Jahoda recalled that « the methods emerged as a result of the concentration on the problem, and not for their own sake ».⁴⁶ Even before *Marienthal* was published, writing at the texture of *Marienthal*, Zeisel had presented similar arguments to counter « criticism of our procedure ». He rejected suggestions that their research displayed « little uniformity from the point of view of any specialized science » and did not respect the « methodological barriers laboriously erected to keep psychology and sociology apart » by emphasizing « the special advantage » of the chosen approach, which « our design (...) did not want to adopt a single uniform perspective, but allowed us to give a unified description of the social phenomenon which the unemployed village of *Marienthal* represented, from the perspective of the problem. The methodological advantage of this approach is directly linked to the ultimately applied purpose of social science research : It wants to provide a basis for our actions ».⁴⁷

Because « 30 kg of material »⁴⁸ of the *Forschungsstelle* were lost after the arrest of Marie Jahoda in 1936 we must try to reconstruct the answer from the residual information. Lazarsfeld provides some clues to a possible answer in his introduction, where he discusses the problem of collecting the data : « (...) we made it a consistent point of our policy that none of our researchers should be in Marienthal as a mere reporter or outside observer. Everyone was to fit naturally into the communal life by participating in some activity generally useful to the community. »⁴⁹ Following the same line of argument, Zeisel underlines the importance of the American method of « unobtrusive observation » in the Afterword.⁵⁰ Contemporary readers of the study consequently felt that the greatest achievement of *Marienthal* was its « functional penetration » as Oeser called it.⁵¹

Participation in an activity useful to the community, I think, only becomes possible if several preconditions are met. First, researchers must oppose the trend towards ever more rigid demarcation lines in the work environment, and second, must be prepared to abandon their socially elevated and secure position and relinquish the role of objective observing scientist for reasons of *methodology*. This does not mean that they must regress to the kind of involved attitude in which personal involvement in the life of the community regularly overrides their observational role. The approach might best be described in the almost paradoxical way : The researchers temporarily join the social group they want to study. Acting the role of a new member of the group allows them to explain their presence to the group and to find a more detached role within the community in which they will be able to pursue their scientific interests. They must constantly balance one role against the other, yet this « immersion into the situation »⁵² gives them « firsthand information and compassionate understanding »⁵³ of the social life they are investigating. Once the fieldwork has been completed, this knowledge will help the participant observer to arrive at a more valid interpretation and description of the social realities. It is only when the collected material is being *assessed* that the process for which *Marienthal* is usually remembered, quantification, can start.

Participant observation in the *Marienthal* study could be begun, first because the research team had distanced themselves from the contemporary practice in the German-speaking countries where social scientists – provided they were at all interested in empirical research⁵⁴ – were primarily concerned with achieving a maximum of objectivity, for reasons of reputation. This type of detachment was described by Zeisel in a paper published simultaneously with *Marienthal*, which referred to « sociology » :

« Between the general overview which the statistical data of the contemporary administration network can give and the relatively abstract knowledge which science-based

sociology provides there is a gap in our knowledge of social events. We feel that it should be the task of sociographic methods to fill this gap. »⁵⁵

A second factor in their use of participation was the positive reception of the new behaviorism by the Bühler School. The new ideas were not allowed to ossify into sterile dogma, but inspired a certain methodological approach. Lazarsfeld's comment that the team tried « to illustrate the psychological aspect of unemployment using modern research methods » was therefore an apt description of their objectives.⁵⁶

To list only the cognitive and institutional aspects that made *Marienthal* an innovative study would be to create an incomplete picture; the political and social aspects of the study were just as relevant. In the appendix to *Marienthal* on the history of sociography Zeisel points out that several researchers had previously tried to employ the method of participant observation, but none had raised the question of the social preconditions for such an approach. Of course, researchers wanting to be more than reporters of facts or neutral observers in the community might not always be able to carry out their plan; and obviously, success or failure of their plan depends on more than their efforts alone. Resistance to their design and misunderstandings may contribute to its failure. *Marienthal* does not seem to have encountered such difficulties.⁵⁷ Indeed, one might argue that it was the integration of the research proposal into the Social-Democratic Labor Movement, as well as the fact that *Marienthal* was a village whose entire population had become unemployed, that allowed the researchers to circumscribe the social conditions which ensured the success of the investigation. Because everyone in the village had become a potential subject, selection of a group interested in the study, or establishing contacts with them, was not a problem. The Social Democratic background shared by the researchers and the majority of their respondents also helped them to overcome potential difficulties. The mutual respect of the social scientists and the Social Democrats encouraged their cooperation. The research team, for example, discussed their plans with the politician Otto Bauer. This prevented the politicians from taking a strictly instrumentalist and reserved view of the study, and the social scientists from adopting a supercilious and precocious attitude.⁵⁸

Marienthal was the only major study carried out by the *Forschungsstelle*. Its innovative approach, balancing Austro-Marxism against social psychology, which would have been well worth pursuing further, was halted before it had obtained the kind of currency it deserved. This, despite the very favourable first reactions to the publication of the study, a rather surprising response in view of relative anonymity of its authors. « Anonymous » is correct in this context in both senses of the word. The first edition did not give

the authors' names and indicated only that the *Forschungsstelle* had compiled and edited the study. Moreover, the authors were little known (Lazarsfeld), or wholly unknown (Jahoda and Zeisel) in the scientific community. However the publication of the study in a series of monographs edited by Karl Bühler probably helped it gain notice.⁵⁹

Most of the reviews are positive.⁶⁰ This is not surprising in the case of Käthe Leichter's detailed critique, Austria's leading female Social Democrat. The praise for the book by one of the most distinguished German sociologists, Leopold von Wiese, came unexpected. Although not without some idiosyncratic passages, his detailed review is particularly critical of those sections that differ markedly from his own modes of sociological inquiry. Wiese saw *Marienthal* as essentially a sociological study. It was thus regrettable that the authors failed to recognize this, even though they did not get « bogged down in psychological details. » Wiese also criticizes the authors' insistence that no conclusions should be drawn unless they could be backed by statistical evidence; that is too great a concession to the statisticians. « Fortunately », however, « they were not too strict in the application of this principle. » He exercises less restraint in his attack on the last chapter. Not only does he condemn the misspelling of proper names and the exclusion of certain schools of scholars – such as the German statisticians of the 18th century – but he is equally critical of the authors' claim that sociography proper was limited to investigations of working-class life. Wiese objected to Zeisel's criticism of Lynd's *Middletown*. What Zeisel had considered a flaw in the study, *i.e.* that it did not give sufficient attention to social and political problems, is for Wiese one of its assets.

An anonymous reviewer of *Marienthal* in *Sociology and Social Research* admits that the material is valuable but finds the « method of investigation questionable, because of its "breach of confidence" and expense of set-up ». ⁶¹ The reviewer holds that the researchers had bribed the population of Marienthal in order to obtain information (obviously insinuating base motives to the relief programs), and praises comparable American and English studies because they proved that a « trained observer was able to secure the subjects' cooperation by giving them truthful explanations in simple language. » Like other reviewers, he can find no immediate connection between the study itself and the history of sociography in the appendix. The other positive reviews discuss the contents of the book, in greater or lesser detail, but most of them are rather short and lacking an assessment.⁶²

All in all, *Marienthal* produced a considerable echo. Its reception was, however, not nearly as universal or enthusiastic as today's popular assumption has it.⁶³

THE *FORSCHUNGSSTELLE* DURING THE AUSTRO-FASCIST PERIOD

By far the worst and far-reaching impact on the future of the *Forschungsstelle* as well as on the reception of *Marienthal*, emanated from outside the science system. Shortly before *Marienthal* was published, the NSDAP had seized power over the *Reich*.⁶⁴ A year later, the Austrian Labour Movement was defeated by Austro-Fascism. Subsequently, all left-wing organisations were banned. As a privately organized institution, the *Forschungsstelle* was not directly affected by the suppression of the Social-Democratic movement.⁶⁵ It did have an indirect effect on the *Forschungsstelle*, however for its executive committee included official representatives of the Social-Democratic Party. Of course, it lost supporters and sources of funding.

Another type of problem resulted from Lazarsfeld and Karl Bühler, who was the chairman of the committee, falling out over Lazarsfeld's commercial leadership style of the institute.⁶⁶ After Lazarsfeld left for the United States, Hans Zeisel took over as the interim head of the *Forschungsstelle* in early 1934. Marie Jahoda and Gertrud Wagner became its scientific leaders, and all commercial matters were dealt with by a staff member especially recruited for this task. Towards the end of 1934, differences between this commercially minded man and the other members of the team led to the formal dissolution of the association. Jahoda, Wagner and a new commercial head subsequently founded a new association, the *Arbeitsgemeinschaft der Österreichischen Wirtschaftspsychologischen Forschungsstelle*. The new commercial organizer seems to have been rather successful at the beginning, as he was able to win commissions totalling AS 22,000 in the first six months. Despite this initial success, the *Forschungsstelle* was soon faced with more financial difficulties. In the spring of 1935, fears of imminent closure were temporarily allayed when the institute secured a loan from a private person.

Hopes were raised when their « silent partner » settled permanently in the United States in 1935. Paul Lazarsfeld had promised he would inform them of « all new developments in the area of market research »⁶⁷ and, as he recalls in retrospect, he recalls having made efforts to secure commissions for the *Forschungsstelle* and he did indeed succeed in persuading the exiled Frankfurt Institute for Social Research to commission the *Forschungsstelle* to carry out some research for them.⁶⁸ Their highly ambitious study of « Authority and Family » had suffered considerably when the Institute went into exile and in 1934 and in 1935 Max Horkheimer, the head of the Institute, tried to mend the disrupted links of cooperation. Numerous projects were proposed, two of which were eventually realized.⁶⁹ Lazarsfeld analysed data

for the Institute which had been collected by Käthe Leichter and Erich Fromm, and Marie Jahoda drew up a research report of which the chapter on history was included in the publication edited by the Institute of Sociology. In early 1936 it seemed as if the *Forschungsstelle* had managed to overcome its financial crisis. Marie Jahoda met Horkheimer in Paris who commissioned her to organize a study on the impact of unemployment on parental authority that was to be carried out by Käthe Leichter and Ludwig Wagner. Moreover, Horkheimer manifested his interest in Jahoda's own research project on « habits of thinking » and encouraged her to extend the range of data that she proposed to use as her basis. During a visit to Paris Jahoda also met the Secretary General of an international chain of department stores who promised financial support for the *Forschungsstelle*. A few weeks later, this was in fact arranged. The businessman joined the *Forschungsstelle* as a partner, replacing Lazarsfeld, who had been a nominal partner, and contributed a considerable sum of money.

THE PRINCIPLES OF SOCIOGRAPHY

From a history of science point of view, papers written shortly after their authors' arrival in countries of exile are particularly instructive as they show the scientists struggling to establish a foothold in an alien environment, and probing unknown territory in an attempt to find out which of their skills might be accepted by their new compatriots. We may assume that emigrés generally emphasized those qualifications which they considered their personal strengths. In this respect, the unpublished papers written in the early months of exile are of particular relevance because they provide evidence of presumably unproductive efforts.

Lazarsfeld's wide-ranging interests and skills allowed him to launch several probes in order to find out how he might best gain the respect of the American social science community. During his first year in the U.S., he wrote two fairly long papers, both of them summarizing his Viennese experiences. His later *Memoir* and comments on other occasions⁷⁰ clearly demonstrate that the resonance he produced did not coincide with the message he hoped to convey and which he also hoped would be welcomed. The best-known paper is « The Art of Asking Why », which discussed three principles underlying the formulation of questionnaires.⁷¹ The other paper exists only in an unpublished, typewritten version. Written in 1933, it was entitled *Principles of Sociography*, and Lazarsfeld submitted it to the journal of the *New School for Social Research*.⁷² Numerous social scientists who had been forced to leave Germany after the spring of 1933 were there working at

the New School and its University in Exile,⁷³ and Lazarsfeld therefore hoped that the editors would be interested in the paper.

In the introduction Lazarsfeld defines sociology as « all attempts to investigate social facts » which « present the object as completely as possible ». He lists a range of potential subject matters, thus proposing an approach which is diametrically opposed to later variable-based studies.

« (...) community surveys in which case it [the community] becomes an object in the sense of a social unit through the locality; investigations of school class, political parties and others which can in the narrow sense be considered as social units; market research and other investigations of consumption such as the use of what might be called "narcotics" and their like; investigation of social attitudes – Protestantism, Communism, etc. »

He stresses that « every sociologist, psychologist, or historian who is concerned with describing a definite field will be considered a sociographer ». He consequently believes that it is necessary « to present briefly what has been established as valid concerning the techniques of sociology » because without a clear account of « why one does it in the way one does » » the teaching of the method, and the discussion of the results is rendered much more difficult ».

In subsequent parts of the paper Lazarsfeld attempts to formulate a classification of social science data. He arranges the material in « pairs of opposites » along « five heuristic axes ».

- 1 subjective and objective data,
- 2 single data and statistics,
- 3 present and past data,
- 4 natural and experimental data,
- 5 elementary and complex units.

In his *Memoir* Lazarsfeld partly translates the types of data into present-day terminology. The first category is described as « objective observations » and « introspective reports », that is, all those data which are open to interpretation. The second category comprises « case studies » and « statistical information », in other words, the type of data that gives exact figures. The third category is defined as « contemporary information » and « information on earlier phases », by which Lazarsfeld does not mean historical data per se, but biographical depth of the investigated persons. Lazarsfeld retains the original terminology for the fourth category. He makes no mention at all of the fifth category, although the 1933 paper includes a detailed exemplification of what would be termed « first-order and second-order data »⁷⁴ in present-day parlance. More clearly than in the 1933 original

Lazarsfeld underlines in the *Memoir* the data collection aspect. He speaks now of « rules underlying the Viennese research tradition ». ⁷⁵

The *Memoir* emphasizes that this variates of data should be used as a heuristic for collecting data. Some of the more interesting observations in the 1933 paper on the quality of the individual types of data, their scope and the depth of the hypotheses founded on them, are unfortunately omitted. Lazarsfeld's sometimes vague and uncertain formulations in 1933 obviously required further elaboration. As an illustration of Lazarsfeld's critical use of data, however, the paper can still be considered recommended reading from which even experienced researchers will benefit.

We shall give just two examples to illustrate this claim. To exemplify the subjective-objective axis, Lazarsfeld cites a market survey which investigated people's choice of a breakfast beverage. One finding was that the reasons for the choice of tea differed from the reasons for others to choose coffee. Lazarsfeld consequently distinguished between extrinsic (*Ablauf*) and intrinsic (*Merkmal*) reasons (adding in a handwritten note : « attributes – influences »). Intrinsic reasons included all answers which referred to « the object itself » – such as « coffee is nutritious » « tastes better » and so on –, while under extrinsic reasons he classified all those responses which indicated the social acceptability of the drink, such as « advice of a friend » « influence of a trip » etc. From this Lazarsfeld concludes that it is not enough merely to ask which « object » people preferred, but that the reasons for their choice, irrespective of whether or not they proved accurate or adequate when put to the test, were of strategic importance : « (...) tea advertising ought to be based on detailed arguments for tea drinking, whereas coffee advertising could be based much more on the mere but continuous repetition of the brand name. » This example, derived from the « tradition » of searching for « the methodological equivalence of socialist voting and the buying of soap » ⁷⁶ depicts Lazarsfeld as a scholar who critically analyzed practical implications of subjects' responses.

The second example is of special significance in the context of research design. In his explanation of the fourth axis « natural versus experimental data » Lazarsfeld clearly indicates that natural (non-reactive) data are preferable and that experimental (elicited) data should be obtained only if the collection of natural data is too slow or if they are not available « in sufficient quantity ». Furthermore the collected data « leave various aspects of life untouched. » Even then he recommends the « intermediary gathering of data » by informants – « people who have excellent opportunities to make observations because of personal confidence they enjoy with their fellow inhabitants ». Such collaborators one has « to discover, to interest, and to train ». Reactive data should be collected only if it is « impossible to obtain (vital information) in a "natural" way ».

The paper clearly indicates Lazarsfeld's distrust of questionnaires and gives quite original reasons for this attitude. Some passages would fit neatly into a text such as C. W. Mills' unfavourable verdict on « abstract empiricism ». In a concluding paragraph Lazarsfeld stresses the differences between the European and the American way of doing social research :

« European students are inclined toward the use of natural data, while the Americans go in for the question situation. The numerous European researchers, those who have to do with school children's reports, would be more reliable if they had been supplemented with multiple choice questions and were allowed to make the choice. Even now the American experience demonstrate that very precise questions can be posed about the changes in family life and about the relationship between earlier life histories and present life situations. Through this the vagueness in German family investigations could undoubtedly be made stronger. »

In Part 2 of the paper Lazarsfeld turns to the « formulation of the experience », which, for him, includes both aspects of the analysis of sociographical data and the presentation of results. In this second section – which he would later describe as the more difficult chapter⁷⁷ – Lazarsfeld develops his concept of the « matrix formula ». In the *Memoir* the term is translated as an « integrating construct », because, as Lazarsfeld explains in a footnote « the term matrix has become identified with its use in algebra » and so he « prefer(s) the present translation ».

Lazarsfeld emphasizes three main aspects of the integrating construct or « matrix formula » : « (1) Where the values of such matrix formulas lie, (2) How they are arrived at, (3) What their logical structure is. » Lazarsfeld initially points out that anyone who has ever collected sociographic material or has heard others report their data, is familiar with the problem. « It is as if the sociographer had laid out the subject to be worked on in many discrete parts and had forgotten to put them together again. » Lazarsfeld hopes that his formula will offer a possibility to reduce the data and compress the information. To clarify his approach he gives two examples : « the purchase of ready-made men's clothing is a case of confidence in the quality of the material » and « (...) the (unemployed) workers (in Marienthal) found themselves in a condition of resignation. »

Secondly, Lazarsfeld claims that « the matrix formulas lead to action ». « If one has (...) a social academic approach to the question of unemployment, one will see that the most important thing is to give the people of the place an opportunity for activity in order to prevent further breakdown and to maintain their fitness over the period of unemployment. » He then goes on to say :

« If one is in a position of pure political power, one will reason in the following way : these are people to whom an appeal based on self-responsibility will on the whole not be

successfull [sic]. When I include them in my political plans, ... I must give them to a certain extent motives for activity (Aktionsprothesen). Continuing this line of thought, it is conceivable that such people are especially vulnerable to ... leader propaganda. »

Lazarsfeld sees the « pragmatic function of the leading formula (Leitformel) » as linked to Karl Bühler's linguistic model, claiming that the matrix formula functions like words : « (...) frequently objects in the outside [i.e. external] world become accessible only after we have given them names. »

Lazarsfeld concedes that at « the present state it is hardly possible to give general direction for the formulation of results, for the way to a matrix formula. » He limits therefore his discussion to three aspects :

« (a) at which stages in the research should conceptualization be made? (b) which data should the concept include? (c) from which experience should the concept[ual] picture be taken? »

For Lazarsfeld the advantage of an early selection of the concept was the greater degree of detailed discussion it permitted. If it was chosen later, it avoided the « danger of prejudice ». In the *Marienthal* study the « choice of the matrix-formula was made completely at the end of the research ». E.g. the procedure obviously resulted in making it impossible to put the hypothesis derived from the « central formula of resignation » in the *Marienthal* study to further tests.

« It would be consistent with the increasing picture of resignation to anticipate the following results : the unemployed read for the first time relatively meaningful books; they wish to use the "vacation" for improving themselves. With increasing time, the level of literature declines. When we drew our conclusions, we no longer had any possibility of checking them. In general, it is best to go ahead this way, by having the collaborators gathered around conference tables in close contact with an opportunity to examine the choice of the formula and the various possibilities of the material that has already come in. »

Lazarsfeld insists that the matrix formula should be derived from experience, so that we « can risk trusting to the "Magic Wand of Analogy" ».

Lazarsfeld finally adds a few comments on the « logical structure » of the matrix formulae, in which he returns to an idea that he had first proposed in a *Gedenkschrift* for Wilhelm Betz⁷⁸ and which is encountered again many years later in a review of the *American Soldier*.⁷⁹ To illustrate his point, he cites the survey of evening school students which had obviously interested him for some time :

« Let us assume we had expected that the manual workers among the students would mainly choose courses which were the farthest removed from their daily occupations. We

could well understand that people look for diversion in evening school. Actually, it was the other way round : the workers chose their courses very closely related to their daily work. Again, we understand immediately : the worker wants to further his occupational opportunities in evening school. What kind of a strange interpretation is this which fits contrary data as well. »

Lazarsfeld's explanation for this unsatisfactory outcome is that a single isolated datum was mapped onto a « model » (a term used here for « matrix formula »), and that our knowledge of people's attitudes towards their occupation is so vague that it is possible to conceive several different and equally valid models. He then adds several remarks on the role of « understanding » in the human and social sciences which suggest that Lazarsfeld believed that we claim to have understood someone or something if a procedure analogous to the matrix formula is employed.

« When we understand another individual, it only furnishes us with many discrete data. We synthesize the data in the form of a familiar model : in social life, we have in our own experience especially accessible model fields, and (...) particularly useful. But in principle the assertion "Mr. Meyer is sad", is already organized as a matrix formula just as the assertion that the existence of early capitalistic economy lay within the Puritan ethic. »

After having explicitly stated that « each formula is right which leads to new data » Lazarsfeld returns to the question whether the *Marienthal* formula, *i.e.* that the unemployed felt resignation, was perhaps applicable to other fields of research as well. After quoting from several other surveys of the effects of unemployment and a number of theoretical observations, he concludes :

« The main objection related to the thesis of the paralyzing effects of unemployment is naturally the view of turbulent or criminal incidents which were reported everywhere. I am inclined to surmise the following : Compared to the more infrequent but noisier cases of aggression, the great extent of inactivity escapes casual observers or social workers who are prepared to remedy the worst effects. As has been said, [the hypothesis] can only be reinforced when more material is available. The important point was to demonstrate once more even in the discussion [of] our two methodological propositions : that for any sociographic activity, it is necessary to collect data along all of the heuristic axes; and the creative act always consists in selecting and relating matrix formulas. »

It was probably the lack of success of this first attempt to gain a foothold in the American world of science which persuaded Lazarsfeld not to conduct further sociographic studies himself.

Lazarsfeld became an exile when he decided not to return to Vienna permanently after the Dollfuss regime had assumed power in Austria. He returned to Vienna in 1935 only to arrange for orderly removal of his personal

belongings to the United States. Most historiographies underestimate the peculiarity of Lazarsfeld's case. He lived in New York from 1933 to 1935 as a Rockefeller fellow and one can find in his papers and in oral history interviews strong hints that he thought to return to Vienna up to 1935. During his stay as a fellowship holder he himself felt welcomed by the American colleagues as a guest.⁸⁰ But after deciding to become an immigrant things changed and Lazarsfeld had to live the life of a refugee for a couple of months.

Like other émigré he seems to have faced difficulties in trying to adapt to the intellectual environments in exile. He differed from most émigré scholars by trying repeatedly to get in touch with his new colleagues. He visited different universities looking for possible cooperation. One of these early contacts with American social scientists brought him in contact with members of the Chicago school, while another relationship was established with Robert Lynd at Columbia. However neither were interested in a methodological analysis of their own research agendas. So it was that Paul Lazarsfeld found his first American resonance in the field of market research rather than in the university. It may be that he remained in this field for many years partly as a kind of gratitude for the early friendliness.

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NOTES

1. Comp. the article headlined « "Exiles" university opens here Oct. 1 – Lederer is among them » on the arrival of Emil Lederer in *New York Times*, August 19, 1933.
2. It was published by the youth organization of the Social Democrats, *Kinderfreunde*, in their journal *Die Sozialistische Erziehung*, (Lazarsfeld 1927a).
3. 1927a, 98.
4. 1982, 13. In a conversation with David Morrison dated 25 May 1973 Lazarsfeld cited the formula as a « joke », Morrison 1976, 129.
5. 1927a, 98.
6. 1927b, 1927d.
7. 1927b, 427.
8. He published two papers on occupational choice by Viennese high school students using statistical analysis without any reference to one of the psychological schools earlier : 1927 e, 1928a.
9. 1931b, 1. The tone of Lazarsfeld's acknowledgment reflects the academic style of the time and doesn't sound like the Americanized Lazarsfeld.
10. 1931a, 160.
11. 1931a, 161.
12. 1931a. Lazarsfeld's terminology was neither transparent nor was he using concepts and terms from the then mainstream psychology. The intuition behind these phrases seems to be the hypotheses that industrialisation and urbanisation reinforce individualism and the development of an independent judgment.
13. 1931a, 174.
14. 1929b, 803.
15. 1927d, 689. Later on Lazarsfeld explained that he « became a socialist by birth » (Sills 1979, 411). A sentence in the 1927 article illustrates Lazarsfeld's historical sense at that time. His generation, he wrote, would be the last one to experience the knowing of marxism in the « sense of an absolute new ».
16. 1928b, 247, note; 1929a, 1, note.
17. Comp. the interviews with Hetzer, Schenk-Danzinger, Jahoda, and Wagner by the author.
18. Lazarsfeld 1982, 25. See T. N. Clark's paper.
19. 1982, 24.
20. 1931b, 4; Lazarsfeld develops this action approach with reference to Karl Bühler *ibid.*, p. 28.

21. 1931b, 78.
22. 1931a, 63.
23. Jahoda 1979b, 3.
24. A complete list of Lazarsfeld's collaborations during his Viennese years does not exist. Some examples of his wide ranging interests include : Psychotechnisches Institut, Statistisches Amt der Stadt Wien, Frauenreferat der Wiener Arbeiterkammer, participation in different discussion groups inside the Social Democratic Party, study group for scientific cooperation with Ludwig Bertalanffy, Egon Brunswik, Rudolf Carnap, Herbert Feigl, Heinz Hartmann, Karl Polanyi, and Wilhelm Reich.
25. 1982, 15.
26. Surprisingly all authors follow the memories of Lazarsfeld, Zeisel and Jahoda and practically no one examines the historical sources. See Sills 1979, Oberschall 1981, Coleman 1981, Knoll et.al. 1981, Pollak 1981, Kern 1982, Neurath 1983, Coser 1984, and Wiggershaus 1986.
27. Zeisel 1933a, and 1934. See also Jahoda's statement in her criminal court hearing, Landesgericht Wien Akt.
28. Fadrus 1959, 11.
29. See Radermacher 1932.
30. Nemschak 1952, 12; Lazarsfeld-Jahoda & Zeisel 1933, VI.
31. 1982, 24f.
32. 1982, 25. Comp. interview Stehr : 1976. The Austrian writer Hilde Spiel reinforced this view at her speech at the Lazarsfeld Conference in Vienna 1988.
33. Schenk-Danzinger interview 1988.
34. A specific sociological interest was not in the center of Lazarsfeld's interest in these days. In 1931b, 20 he dismissed the investigation of modifying influences by social class because this would « trace to much into sociology. »
35. Comp. Fleck, 1990, 95-118.
36. 1927c. Comp. Jahoda 1927 and 1928.
37. 1931c and 1931d. See also Radermacher 1931.
38. The questionnaires were distributed in November 1931, exactly the time of the beginning of the Forschungsstelle. See *Hörerbefragung* 1931a, 1931b.
39. Jahoda 1979a, 118. See interview with Wagner 1985.
40. 1982, 352, n9. One can find a reminiscence of these interests in *Marienthal* : at the end of Zeisel « History of Sociography » one can find as a work in progress a study about « Über Freizeitverwendung ».
41. Interview with Schenk-Danzinger 1988.
42. Lazarsfeld-Jahoda & Zeisel 1933, Vf.
43. Interview with Schenk-Danzinger 1988.
44. 1932a, 148, where this number is reported. The group spent 120 days in Marienthal, so one can say that Schenk-Danzinger did one third of the field work.
45. Lazarsfeld-Jahoda & Zeisel 1933, 1.
46. Jahoda 1981.
47. Zeisel 1933b, 105.
48. Lazarsfeld-Jahoda & Zeisel 1933, 8.
49. *Ibid.*, 5.
50. *Ibid.*, 120.

51. Oeser 1937, 352.
52. Lazarsfeld-Jahoda & Zeisel 1933, 1.
53. Jahoda 1989, 4.
54. In the Afterword to *Marienthal* Zeisel mentioned only Andreas Walther, ignoring Rudolf Heberle, Leopold von Wiese, Theodor Geiger and Ferdinand Tönnies.
55. Zeisel 1933b, 96.
56. 1932a, 147.
57. In a 1988 interview Schenk-Danzinger remembers her resistance : « I cannot remember, I have suppressed the whole thing. But, my God, I don't have any trouble with the people there, they were really friendly. There was no one who rejected me. ... But I don't like to interrogate people, I always feel some hesitation... but first I was interested and then I feel disturb. They invited me to cooperate in the data analysis but I didn't want to to this. »
58. For an analysis of the unpolitical style of Marienthal see Fleck 1988, 352f.
59. The American audience obtained an impression of the Marienthal study through an article in *The Nation*, written by a visitor to Vienna (McMurry 1933).
60. Reviews appeared in the following journals : *Arbeit und Wirtschaft, Zeitschrift für Sozialforschung, Kölner Vierteljahresshefte für Soziologie, Archiv für die gesamte Psychologie, Jahrbücher für Nationalökonomie und Statistik, Reichsarbeitsblatt, Mensch en Maatschappij, Sociology and Social Research, Archivio italiano di psicologia, Revue de l'Institut de Sociologie, Freie Wohlfahrtspflege, Literarisches Centralblatt für Deutschland.*
61. *Sociology and Social Research* 18.1934, 77.
62. Two of the German reviewers mentioned criticism in the style of those days; for example, Richter 1934 criticized the lack of « volksbiologische » aspects.
63. The names of the authors were mentioned in only five of the reviews; the name of Bühler, the series editor, is mentioned six times. Lazarsfeld's name could be found only once. He is named only indirectly by his wife's name, Marie Jahoda-Lazarsfeld.
64. However, there is no evidence that « Marienthal » was burned during the Nazi book-burnings. See Fleck 1990, 230, n. 67.
65. This is surprising because other professional organizations were banned; for example, the neopositivistic « Verein Ernst Mach » (see Stadler 1982, 196ff.)
66. See Landesgericht Wien Akt Jahoda, sheet 81.
67. Statement Henrich Faludi, Bundespolizeidirektion Wien November 28, 1936, Landesgericht Wien Akt Jahoda, sheet 119.
68. See Wiggershaus 1986.
69. *Ibid.*, See also Dahms 1994.
70. 1982, 21f. and his preface to 1972.
71. It first appeared in *The National Marketing Review*, and is reprinted in 1972, 183-202.
72. Lazarsfeld wrote in his *Memoir* : « I thought the editors would be interested in a paper trying to link explicitly empirical work done in both countries. ... The original paper was refused and was never published. On reading [1969] it for the present purpose, I find it, and especially the examples contained in it, characteristic of the state of affairs in the early 1930s. » (1982, 353, n 19). The following quotations are from the original manuscript which Helga Nowotny has presented to the Archives for the History of Sociology in Austria.
73. See Krohn 1987.
74. Wilson 1982.
75. Lazarsfeld 1982, 22.

76. 1982, 19.

77. So in a memo to Patricia Kendall from December 11, 1947, (Lazarsfeld Archive University of Vienna).

78. 1932c, 166.

79. « The American Soldier : An Expository Review » *Public Opinion Quarterly* 12. 1949, 377-404.

80. See a similar description in Simmel *The Stranger*.