"The Radio, Adorno and The Panel." *A Transatlantic History of the Social Sciences: Robber Barons, the Third Reich and the Invention of Empirical Social Research*. Fleck, Christian. London: Bloomsbury Academic, 2011. 165–220. *Bloomsbury Collections*. Web. 26 Apr. 2023. <<http://dx.doi.org/10.5040/9781849662932.ch-005>>.

Downloaded from Bloomsbury Collections, www.bloomsburycollections.com, 26 April 2023, 03:01 UTC.

Copyright © Christian Fleck 2011. You may share this work for non-commercial purposes only, provided you give attribution to the copyright holder and the publisher, and provide a link to the Creative Commons licence.

5

THE RADIO, ADORNO AND THE PANEL

As the previous chapter has shown, Paul F. Lazarsfeld (hereafter PFL for short) was a high-reputation social science emigrant. Although his departure from Vienna in the autumn of 1933 was not directly related to the handing-over of power to the Nazis in Germany or the elimination of democracy in Austria, he has always been considered a member of the group of political refugees, and rightly so since sticking to his plan to return to Vienna would have been highly unreasonable. This chapter will describe the beginnings of PFL's academic career in the United States. However, rather than re-narrate his first years of scraping along in New York after his RF fellowship had run out (cf. Fleck and Stehr 2007), I will focus on his entry into the research project that smoothed the way to the top for him: the Princeton Radio Research Project (PRRP) which, from 1937 onward, gave him the opportunity to move from the margin to the centre of the American science system.

PFL joined a research project of which he had had no part in either the design or the acquisition. Initially, his position was that of the 'second (or third) in line', but when negotiations for continuation began after the first two years, he already acted as the one and only negotiator for the project, making his own proposals and having them complied with. In the third year, he succeeded in having the project transferred to Columbia University, where at age thirty-nine he was appointed to the Faculty of Political Science, first as a visiting lecturer and, as of September 1941, as an associate professor. Becoming established like this is a highly contingent undertaking that requires aspirants to show consideration in all areas, avoid antagonizing the sponsors, keep any promises that are made and accept whatever setbacks are imposed; in short, prove that they are reliable and agreeable partners in the complex interplay of academic forces. In the following, PFL's trajectory will be reconstructed from two closely interrelated points of view, one of them organizational, i.e. PFL's interaction with the various other actors, and the other one oriented to content, i.e. the development of techniques of radio research and their results. A special concern will be paid to the analysis of the cooperation between 'American

empiricist' PFL and 'European theorist' Theodor Wiesengrund Adorno. Unlike previous literature dealing with this cooperation, the following will show that Adorno did not fail because of PFL, but because of himself.

WANTED: RESEARCH DIRECTOR!

In the summer of 1937, Hadley Cantril and Frank Stanton, the designated co-directors of the research project that was due to start on 1 September, were in a tight spot: unable and unwilling to undertake the project themselves, they were looking for a replacement. Robert Lynd and George Gallup, independently from each other, recommended asking PFL, who was now running his own research centre at the University of Newark. For personal reasons but also as an emissary of Max Horkheimer's Institute of Social Research, PFL was back in Europe at the time. As a collaborator with the institute, he was charged with leading the European part of the study dealing with the erosion of authority as a result of unemployment and to represent the institute in Paris at a congress of neo-positivists (Dahms 1994: 227). It was there, arguably, that PFL first met Theodor Wiesengrund, who at the time did not yet call himself Adorno.¹

In August of the same year, PFL in Newark, was vacationing in the Austrian Alps when he received a telegram from Cantril: 'Would you accept full time position beginning September directing Rockefeller Radio Research. Salary seven thousand another thousand Herta. Assistantship two years sure possible four. Headquarters Princeton. Lynd advises acceptance. Cable answer or questions ..., Hadley.' PFL replied: 'Much interested wondering if some connection of project with Research Center possible suggest decision end of August or telephone call Vienna Stop please prepay return cables. Letter follows, thanks, Paul.'²

In a subsequent letter, Cantril gave more details of the planned research project. As Stanton had in the very last moment declined to act as a director, Cantril had turned to Lynd to ask him if he could recommend someone 'like Lazarsfeld'. Cantril had not thought of Lazarsfeld himself as a candidate since he had assumed that PFL wanted to stay in Newark. As to the substance of the research project, Cantril was not very explicit. It remains unclear what had caused the Rockefeller Foundation – and more specifically John Marshall³ as the officer in charge – to be 'quite excited' about this research project, but it may have to do with the fact that, given that the major educational value of the radio had become a special concern of John D. Rockefeller Jr, they were pleased to have found anyone at all who was ready to address the issue.

Rather than be more explicit about matters of content – 'you know my heart is not in this type of research as much as yours' – Cantril elaborated on the details of organizational issues. He and Stanton, as agreed upon with Marshall, would remain associate directors of the PRRP, providing the occasional idea but certainly not wishing 'to "boss" the job, especially if we could get someone of your caliber to run things'. As for PFL's wish to bring in his Newark Research Center, Cantril felt that the odds were rather against it. The PRRP

was one of those uniquely American grants, absurd in a way, that were awarded to a specific institution – in this case, Princeton University's School of Public and International Affairs. Nominally at least, project headquarters would have to be at Princeton, a requirement certainly backed by the university authorities. Could PFL not take a leave of absence from Newark, or reduce his teaching activities to a *pro forma* lecture? Cantril declared himself ready to shield this, assuring that nobody at Princeton or in the RF needed to know about it.

After the two years of directorship, which as Cantril saw it were very likely to become four years, it would be easy for PFL to secure another job or just return to Newark. Until then he could easily 'produce a couple of studies that would put your name at the top of the list'. Any publications from PRRP 'would be entirely yours'. Cantril was evidently not thinking of himself as an honorary author. But the recently appointed associate professor of psychology at Princeton University speculated on catching the European through his uncertainty of status: 'Your title throughout the job would be Research Associate in the School of Public and International Affairs at Princeton University. Doesn't that appeal to your bourgeois soul!' Finally, Cantril proposed to double the salary that Herta Herzog, PFL's second wife, was to receive as an assistant, and specified further financial and organizational benefits which included the fact that the Lazarsfelds and the Cantrils would be able to see more of each other.

However, in the summer of 1937, PFL no longer was a 'penniless immigrant' who had to be grateful for any job he was offered; he was already in a position to choose between several options. In a detailed letter he expedited from the Austrian mountains – 'it is a queer experience to sit in a mountain village and to discuss American research problems' – immediately after his telegram to Cantril (whose letter had not yet reached him at the time) he wrote that while he was certainly pleased at the offer, he nevertheless had to balance it against his own plans. Information on what was being offered him was much too sparse and, what was more, he was unwilling to abandon what he had set up in the course of the past year. By founding the Newark Research Center, he had pursued two aims: he had wanted to conduct the largest possible number of different studies in order to enhance his own methodological know-how, and he had tried to set up groups of junior scholars whom he wished to train in the kind of research he was interested in. While it seemed unproblematic to pursue the first issue within the PRRP – 'radio is a topic around which actually any kind of research methods can be tried out and can be applied satisfactorily' – he was unsure of whether, and how, a satisfactory solution could be found for the second issue. He wondered whether Cantril was aware that by that time students from other universities were coming to Newark on a regular basis to work with him on specific projects. The question, therefore, was whether Cantril felt that it would be possible to conduct at least part of the PRRP in a way 'that it could be used as a sort of training institution'.

This led to a third question that pertained to a strategy he had adopted for himself: while the Newark Research Center had indeed reached a level of consolidation that allowed it to function without him, his personal perspective was to link up all his research activities and publications with the centre's name. Founding a research centre was not something one did every day, and even if the University of Newark might not be a top address, at least his research centre might still become just this within another decade. It was an institutional investment he had no intention of writing off:

You see all comes back to an European attitude which might be not so easy to understand from your point of view. I feel strongly that I don't want to go ahead alone, that I want to stay for an institution and I try to build up an institution which is able and willing to stand for me. Of course, I will have to do very different things, less glorious but about the same way as you are a Professor in Harvard, then in Columbia, then in Princeton. But as my poise and my past and my name cannot compare with yours, I try to identify whatever I do with an institution which might after some time acquire the dignity which I myself for reason of destiny and maybe of personality can hardly aspire at.

After some further exchanges with Cantril, PFL accepted the offer of taking charge of the Princeton Radio Research Project. Cantril immediately reported on this welcome news to the Rockefeller Foundation as well as to the authorities of his university, who followed suit by appointing PFL a research associate as agreed.⁴

In one major point, PFL refused to give in to Cantril: he did not transfer to Princeton but continued to live in New York's Upper West Side, with his professional headquarters located on the other side of the Hudson River in the shabby industrial town of Newark. That said, for some time his postal address was 'Office of Radio Research, 203 Eno Hall, Princeton University, Princeton, New Jersey', and only the initiated addressed their letters to the 'Research Center, University of Newark, New Jersey'.⁵ The old brewery building that housed the Research Center suited PFL far better than any room in one of the red-brick buildings at Princeton's campus could have done. During his whole life in the United States, PFL, whose mother was a specialist in individual psychology, believed or at least tried to make others believe that he was the social odd-man-out, suffering from an inferiority complex. At Newark, he was the university President's chosen innovator while at Princeton he would have been reduced to being the bottom dog for one or the other recently appointed young professors and held to respect the academic pecking order. Another reason for PFL to not really transfer to the famous Ivy League university may have been that at the time, as part of a complex organization, he preferred having a protector of the highest possible rank. He may have felt that Cantril, being five years his junior, and notwithstanding his upper-class background, was not quite up to this role. The veiled hints dropped in the letter quoted above – 'my poise and my past and my name' – may also be read as saying that at Princeton, he feared coming up against anti-Semitic and anti-socialist prejudice.

In complying with PFL's demands, Cantril had gone much farther than he had initially planned, which probably had also to do with his own situation: the PRRP was located at Princeton's School of Public and International Affairs, and Cantril himself was affiliated with Princeton's Psychology Department. Any failure to get the project started as agreed would have resulted, only some months after his arrival, in a severe loss of face for the ambitious young professor and would have given rise to second thoughts not only in his own department. As for Rockefeller, to give up a project that had already been approved would jeopardize Cantril's chances of receiving RF support for future endeavours. By acting as he did, Cantril showed the sponsors that he was not prepared to slip out of a previous commitment as soon as something better presented itself yet made it his duty to ensure that the handing-over and transition were carried out in due form. After all, Cantril could be sure that in PFL he had found someone whose intellectual and organizational capacities were up to the task. And, finally, being an associate director of such a well-endowed project would boost his own reputation not only at the local level.

Ironically enough, the ex-Viennese succeeded in having Cantril sell to the sponsor one of those little departures from the truth that Cantril himself had suggested to PFL to pry him away from Newark. In a letter dealing with the start of the PRRP, Cantril informed John Marshall that after lengthy discussions an agreement had been reached with PFL about the administration of the project. The 'central office' would thus be located in the room provided by Princeton, housing a typist, Cantril and 'the complete record and data of the study'.

Lazarsfeld, as you know, has been doing Marketing Research in Newark for the past year or two, and has a very complete knowledge of Newark and its environment. It occurred to us that for the first few months or the first year, it might be well to use Newark and vicinity as a testing center, where Lazarsfeld has so much information regarding educational, vocational and economic status. Would the Foundation have any objections if Lazarsfeld physically spends the majority of his time, and had an office, at the University of Newark?⁶

A few weeks later, Stanton – who apparently had not taken part in the summer's negotiations – and PFL paid their first official visit to the RF, where PFL managed to live up to his life-long reputation for being spectacularly late.

At the time, there was no consensus on the duties bound up with being a project director. Cantril obviously felt that the task, being as it was a regular job, excluded any other activities, while Marshall took the line that would later be the rule, namely that the receiving party had to meet the objectives agreed upon with the sponsors, no matter how. Given that there was no day-by-day contact between the RF and its grant recipients, this contractual interpretation of 'management by objectives' obviously met its expectations for control (and the practice soon became the binding norm).

In terms of content, however, the PRRP was still as vague two months after its official start as it had been in Cantril's first letter to PFL. One of its objectives

was to develop methods for determining the 'actual rôle of radio in the lives of listeners'. PFL declared that, for the time being, rather than proceed with staff recruitment he would engage in more elaborate studies whenever the opportunity presented itself. His concern was with the motivation of radio listeners, since the main question was, 'why people listen. When he [PFL] has laid out the groundwork for a methodological approach to this question, he intends proceeding to such other questions as Who listens, When, How, and What is listen[ed] to.'

To this end, Marshall goes on to say in his memorandum that PFL had organized a conference with other social psychologists, among them Robert Lynd, and general psychologists, such as John Dollard,⁷ with a view to benefiting from their approaches. PFL himself had been interested in radio research for a couple of years and, together with his wife, had worked and published on the issue in Austria.

PFL used his first months as the new project director to proceed with his ongoing studies at the Newark Research Center and to pass them off as contributions to the elaboration of a working programme for the PRRP. In this, he acted on a maxim that would become the trademark of Lazarsfeldian institutions: receipts from new contracts were used to cover the debts run up by previous projects.⁸ PFL did not act on Cantril's initial proposition to hire four assistants or let Herta Herzog be one of them. The latter decision may also be due to his wish to avoid being accused of nepotism⁹ and to the fact that his lavish salary seemed more than sufficient to serve as a family income.

PFL used his contact with Stanton to familiarize himself with the market research that was being done at CBS (Columbia Broadcasting System) or was otherwise accessible via Stanton, from time to time suggesting improvements as well. From this, and rather by coincidence, a new research strategy seems to have emerged: the collection and reanalysis, from a new perspective, of the data of past studies. Thus, 'secondary analysis' was born,¹⁰ a solution that was in more than one way ideal given PFL's needs and dispositions at the time. He could rapidly gain access to a wealth of data, and he could be lavish in collecting them by relying on the numerous members of his staff. As an individual worker, given the costs of mechanical (and absence of electronic) computing devices at the time, not even PFL would have hit on the idea of collecting the data of others. But as the director of a large-scale project, he had collaborators whom he could train in the use of statistical procedures and methods of data analysis with which he was either already familiar or developed. His collaborators-cum-apprentices, in turn, were likely to find work on real problems more rewarding than exercising with dull textbook examples. Finally, the whole procedure was bound to be relatively cost-effective since it can be assumed that the Newark Research Center did not need to buy the data but got them for free, as they were of little value to their owners once their analyses were done.

As a sideline, PFL organized the above-mentioned conferences with psychologists and sociologists with the aim of profiting by their expertise.

For this, he recruited psychoanalysts such as Erich Fromm and Fritz Redl¹¹ in New York and went to Chicago to meet the local band of social scientists, among them Ernest W. Burgess, Louis Wirth, Herbert Blumer and his own former collaborators Arthur Kornhauser, Samuel Stouffer and L.L. Thurstone.¹² PFL also invited John Marshall; his attendance at both of them demonstrated the RF Humanities Division's strong interest in the progress of the PRRP.

The very fact that such meetings took place is remarkable and substantially differs from habitual European practices then and later. At the time, social research conceived of as a cooperative undertaking was already an established form of communication in the United States, where colleagues were not generally seen as rivals from whom one seeks to hide as much as possible for as long as possible. Rivalry among social scientists was limited to priority rights for publications but did not extend to discussions about research projects. It can be assumed that even in Vienna, PFL, well aware that transforming bright ideas into serious results usually requires a good deal of hard work, would not have been haunted by the fear of somebody stealing his ideas. But in Vienna, and even disposing of the same means, he would not have dreamed of offering Sigmund Freud (or anybody) \$20 in remuneration for participating in a discussion on the design of a planned study.¹³

A NEW TESTAMENT

On New Year of 1938, PFL drew up a memorandum of fifteen pages containing a detailed research plan for the PRRP.¹⁴ During the first radio years, researchers studying it in the United States had primarily focused on technical issues of frequency allocation, operating stations, and the relation of private and public broadcasting:

As broadcasting and listening rapidly increased, it became evident that it was not the technical aspect alone which needed systematic organization and planning based on actual knowledge. Who should broadcast, when, and how, has been the main concern in the past. To whom one should broadcast, what, and why, has now come to the foreground of general interest.¹⁵

The PRRP did not need to start out again from scratch, but could draw on the wealth of practical experience and systematic research findings accumulated by commercial radio stations in the previous years. Non-commercial operators in particular needed detailed knowledge about their public, since their programmes were targeting specific audiences. While the commercial stations could be satisfied when their isolated message was effectively broadcasted, educational programmes needed to know how their programme compared to other activities in terms of relevance. Therefore, the first thing to find out was if there were any patterns of interest in radio listening: 'Do the people who listen to the spoken word neglect music over the radio? Does the interest in modern dance music compete with that in the classics?' The point was to find out what part radio listening played for each individual, whether

it brought about changes in their behaviour and attitudes and if so, which kind of changes. The insights of psychological personality research – an allusion to Cantril's teacher, Gordon Allport, albeit without naming him – could be made fruitful for radio research.

In order to fully appreciate this research perspective it should be noted that the new medium had to break up conventional behaviours and make listeners modify their time budgets. Wanting to know which other activities might be superseded by radio listening was therefore a legitimate concern. In later decades, television and, most recently, internet use would give rise to the same question. Concerns about the new medium's potential to eliminate other well-established and well-regarded activities were as widespread then as in later years. PFL shared neither this cultural pessimism nor the fears that arose from it, pointing out that the new medium could act both ways, as likely to balance existing social disparities as to be a source of new forms of experiencing insecurity:

Here is the farm agent in a southern area who feels that the sharecroppers will take a more independent stand toward their landlords when they can learn the price of cotton directly over the radio. Here is a journalist on a local newspaper who fears that he will lose his job because the radio news services and the news commentators will do away with the local newspaper all over the country. Here is a musician who hopes that the radio will contribute much to the advancement of modern music because the classical forms will be more quickly worn out due to the increasing frequency of their presentation over the air.

Besides investigating the personality (types) of the radio listener, another aim was to identify successful programmes (i.e. those which appealed to a great variety of listeners). For this, the procedures used in experimental psychology could be drawn upon. Stimulus variations could be implemented with the aim of systematizing the technique of programme analysis. Due to their limited financial scope, however, non-commercial radio stations could not run tests on every single programme. But if the experiments planned in the PRRP turned out to be successful, this might result in:

an abbreviated panel method by which non-commercial broadcasters can stay in contact with a representative group of the listeners and get leads as to the extent of and reasons for acceptance of programs ... We shall try to develop a body of directions as to how listener panels should be selected and contacted, what questions they can and should be asked, and how far that information can be used for the guidance in program making.¹⁶

This is the first time in PFL's *œuvre* that the term 'panel' is used to describe a specific method. It is still far from what it would later be understood to mean, but the PRRP was to become the site where this method, so closely bound up with the name of PFL, made its first steps into the cache of social research techniques.

While according to PFL, the commercial radio stations were only interested in the 'sales effect', the educational programmes were committed to more long-term interests and more sustained effects. Therefore, one of the core objectives of the PRRP would be to investigate the effects of the radio in three ways. A first would be to use selected programmes, such as the round-table discussions broadcasted by the University of Chicago, to study the immediate effects of this kind of stimuli, using control groups to consolidate results. But since it was impossible to know beforehand which programmes might or might not have an impact, a second approach would be to collect data on 'incidental situations' that had demonstrably had an impact on the life of the listeners.

In a third step, since one could not reasonably assume that any impact the radio might have would be an isolated incident, 'radio biographies' would be collected to establish the chain of influences that had acted on the individual, while allowing for the possibility that these effects might well be different at different times. In this respect, comparisons between adults and younger people were the obvious method, since only the former could look back on periods in their life where there was no radio effect. For those who had grown up with radio, changes in their relationship with the medium that occurred over the course of their lives could be observed in detail.

The impact of the radio on social life cannot, however, be studied solely on the level of individuals, but must take into account the larger social trends that either alone or in a covariance yet to be established determined social development. Expectations concerning the manifestations of such trends in the population were powerful determinants of individual action. It had occasionally been argued that the radio contributed to the urbanization of smaller and rural communities: 'In a special situation, how should we test the extent to which this urbanization has taken place? How can we separate the influence of radio from other factors working in the same direction, such as the car and the movies? Does this urbanization come about with different speeds in different areas? What accounts for the differences?'

However, if the PRRP were to limit the range of its studies to the topics described, the results might quickly become obsolete because of the rapid evolution of the whole field. The only remedy for this was to seek results that were universally valid and, thus, applicable to changing conditions:

The kind of questions pertaining here are usually given such forms as: What are the basic motivations of people, why do they act or feel the way they do? We shall try to attack this most difficult sector of any social research proposition; but so little common understanding exists here that a theoretical background must first be gained. One of our publications will consist of a scrutiny of all the theories and techniques available for studying motivation in social research, paying particular attention to radio.

Certain technical aspects of radio programmes should not be ignored either. In the movies, 'personal glamour and rich settings' played a major role because

they could be shown on the screen, while on the radio these elements were irrelevant:

Could it, therefore, be that the radio play will become more sober and more adapted to the habitual middle-class setting of the listeners than is the movie? The microphone permits acoustical effects which a concert hall does not have. Will that affect the future trends in music? The political speech over the radio commands attention in a different way than does the printed page. Will that finally lead to a division between the content that is conveyed over the radio and that which remains preserved for newspapers and magazines?

Since all these questions were highly dependent on the respective fields, the investigation had to be narrowed down to just a few of them. The following four had been provisionally selected: music, book-reading, news, and politics.

The research plan elaborated by PFL after a few months as a project director does not strike the reader as overly precise with respect to what was to be done. It is, rather, a loose collection of potential sub-studies concerned with the role of the radio in one way or another, tied together by a vague conceptual framework. PFL sought to adapt the studies initiated in Newark before the start of PRRP to the new task, and said so quite openly when talking to Marshall, as the latter noted:

At the same time, a number of tentative surveys are being undertaken to test the feasibility of certain mediums of inquiry; e.g. the recent New Jersey gubernatorial campaign provided an opportunity for gauging the extent to which radio is influencing political opinion. A random sampling was made in a number of New Jersey cities which has produced rather interesting results.¹⁷

Cantril's original exposé, 'The Essential Value of Radio to All Types of Listeners', (disparagingly dubbed the 'old testament' at Newark in contrast to PFL's 'new testament' memorandum; see Rogers 1994: 270), contained a list of questions that needed to be cleared up before starting. It resonates with Harold Lasswell's classical formula for the objectives of content analysis, even though Lasswell focused more explicitly on the nexus of radio station, medium and listeners.¹⁸

The orientation to educational radio programmes that looms large in the 'new testament' of the new year of 1937/8 is another feature that PFL seems to have inherited from the original exponent of the Princeton Radio Research Project. PFL permitted himself to introduce some of his own hobby horses into the programme. Listener motivation certainly was one of them, as well as the consistent emphasis on the role of musical programmes which, given PFL's endowment with cultural capital as a Viennese and lifelong violinist, is not surprising. The listener survey conducted for the Austrian RAVAG had already brought to light the high importance of musical broadcasts (Mark 1996). What is more surprising is that some of PFL's more recent experiences, which he had

found important enough to mention in his preliminary talks with Marshall, were not mentioned in the research programme. Herta Herzog's Vienna doctoral thesis on the patterns of attribution of social traits to given voices and other psychological experiments seem to have been of less interest to PFL by this point.

If PFL was not too busy giving lectures to meetings of potential clients, who meanwhile included all those who were interested in the PRRP or might eventually become important for its success, his primary concern was with the improvement of the techniques used for analysing surveys done by questionnaires. In the context of local elections in New Jersey, he had in the autumn of 1937 started his first experiments with early forms of the panel technique, which he described in an enthusiastic report not only to Marshall, as mentioned above, but also to his two associate directors in regular updates.

As to the political survey, we are still very busy developing the questionnaire. The interviewers are making a number of interviews every day and then I change the questionnaire on the grounds of the returns ... our main interest is to develop the right kind of questionnaire which will next time be used immediately after the election in a pre-organized way. We will now get a lot of important material which will permit us to make an inventory of all the very different ways in which radio speech might influence people with different kinds of political interest.¹⁹

Cooperation with Cantril and Stanton developed along rather different lines. Stanton, whom PFL hardly knew, soon turned out to be a loyal partner who provided PFL with everything that could be helpful for the PRRP and the Newark Research Center. A friendship soon developed that far outlasted the project. Dealing with Cantril was more complicated, since he was not only suspicious of the repositioning of the content of his project but also seemed unable to meet deadlines for the tasks he was supposed to supervise or carry out himself. Moreover, he thought of himself as the real boss – intentionally or not, his August 1937 declaration of not wanting 'to boss' the PRRP soon turned out to be inaccurate. To a certain extent, however, Cantril had no choice: since the financial part of the project was handled via Princeton, he acted as the local contact and was probably given a piece of the administration's mind concerning PFL's chaotic bookkeeping more often than he had bargained for. The mutual friendship of former years and the first months of the project cooled and later broke up completely.

PFL had decided early on to give considerable room in the project to the study of music on the radio. At the time, the radio was the only medium that offered musical performances to a broad range of listeners. The record had already been invented but played a very minor role, leaving those who wanted to listen to music to go to a concert hall or another more or less exclusive place dedicated to musical performances, or to perform themselves. More than half of the broadcasting time of all American radio stations was dedicated to musical programmes. This almost inevitably raised a number of questions. Would

the radio contribute to the elimination of the distinction between elite serious music and popular music? What kind of music people would choose to listen to on the radio, and why? Did listeners give their undivided attention to the music they heard or was it rather a constant background stream? For these and other questions, PFL was searching for adequate collaborators.

RECRUITING A MUSIC EXPERT

Although PFL had met Theodor Wieselgrund in the summer of 1937 in Paris, shortly after the launch of the PRRP he chose to contact him via Horkheimer. At the time Adorno was struggling to complete his Ph.D. in England. PFL was familiar with his article 'Zur gesellschaftlichen Lage der Musik' (On the social position of music) published in the *Zeitschrift für Sozialforschung*, but perhaps did not know that Hektor Rottweiler's 'Über Jazz' (On Jazz) actually came from the same author. What he could not know by any means was that Adorno and Horkheimer had repeatedly discussed him in their letters, reaching a consensus that was clearly unfavourable.

In October 1936 Horkheimer had informed Adorno that an acquaintance of the latter, Paul Oppenheim, would be 'delighted' to contribute to the forthcoming issue of *Zeitschrift für Sozialforschung*:

Lazarsfeld, who has long been called upon to write an English article on how to employ a number of American empirical sociologists who are interested in the Institute, has now availed himself of the opportunity to take on Oppenheim and speedily write a little article on the concept of type. Since I wrote a note making it quite clear that Lazarsfeld's criticism of O. as expressed in this study is not based on dialectical logic but remains inscribed in the field of statistical logic, our theory-minded readers will at once see that his contribution is by no means about philosophical issues as such but about special problems of purely empirical social research. On the other hand, Paul [Lazarsfeld] and a number of others who are interested in this kind of discussions will see a scientific finesse in this study.²⁰

On 20 October 1937, Adorno received a telegram from Horkheimer offering to relocate him to the United States: 'Part-time job with new Radio Project Princeton University. Two-year income to be provided \$400 per month guaranteed.'²¹ In a letter written on the same day, Horkheimer explained the arrangement in more detail. After his arrival, Adorno would have to take in hand the relationship to PFL. On no account should he work as PFL's assistant, 'but participate in the implementation of the research project for which considerable funds have been allotted to Lazarsfeld by Princeton University'. For both correspondents, the European model of the assistant quite naturally came to mind.

Leaving aside the question of how Adorno's transfer to New York would impact the balance among the planetoids orbiting around Horkheimer (cf. Wiggershaus 1986: 293–364), Horkheimer made fairly clear in his letters what he was offering Adorno with respect to life outside the universe of the

institute: an opportunity to establish himself in the New York world of social science or music. To a certain degree, Adorno seems to have not only understood this but subscribed to it. Thus, in December 1937 PFL reports to his two co-directors in terms that are otherwise rare in his memoranda:

In the meantime I have had more correspondence with Dr. Wiesengrund whom I suggested for the musical study and came to some tentative arrangements in case he can be here by the end of January. His record is really marvelous and splendidly suited to our purpose ... I had a letter from him yesterday telling me that during the time he was in charge of music in the Frankfurt radio he got good contacts with American people which he has maintained since he is working in Oxford. That will make work with him easier. I think he is another case where a foreigner can be gotten at half the price we would have to pay an American of equal competence.²²

In this context, it should be noted that unlike Horkheimer, who used to spend hours each day on his large correspondence, calculating it tactically to the last detail,²³ PFL was a lousy letter writer who wrote only when something really important was at stake. PFL's letters to Adorno clearly show that he did not just want to do Horkheimer a favour.²⁴ He obviously expected a major contribution to radio research from Adorno as a collaborator. In the first letter of 29 November 1937, he asks Adorno to discuss ideas concerning his future work by letter even before coming to New York: 'I intend to make the music section, so to speak, the hunting-ground for the "European approach." By that I mean two things: A more theoretical attitude toward the research problem, and a more pessimistic attitude toward an instrument of technical progress.'²⁵

It is the first point that is primarily brought to Adorno's attention. PFL argues that even though research in the PRRP was to be done from an empirical perspective, it could only benefit from previous theoretical reflections. Thus, Adorno's article in the *Zeitschrift für Sozialforschung* was exactly what he and his associates expected Adorno to contribute. However, the theoretical reflections would need to be taken two steps further: (1) toward an empirical research problem; (2) toward an actual execution of the field work.

For inspiration, PFL adjoined a copy of the letter that he had addressed to the New York psychoanalysts explaining the research questions he had sent them, and asked Adorno to provide a list of the problems he considered to be of special importance. In all this, PFL was fully aware of the difficulties involved with coming to a fruitful cooperation with Adorno: 'I understand completely that it will take much writing and talking back and forth before we would really understand one another.'

Adorno took his time answering, going to Italy on a vacation in the meantime, and did not send an answer to 'dear Mr. Lazarsfeld' until the end of January 1938. This seven-page letter is arguably the first document in which Adorno comments on questions of empirical research, and for this alone merits

further attention. He starts out by saying that his theoretical stance does not at all imply an aversion to empirical research:

On the contrary: the concept of 'experience,' in a very specific sense, is more and more in the core of my thought. However, it is my conviction – and on this, I believe, we fully agreed in the talks we had in Paris: that 'theory-free' research is a fiction and that there is interplay between theory and empirical research: the very interplay we usually describe by the term of dialectical method. I am particularly delighted to see that you, too, share this point of view in your letter, and I think that I can in return promise that my part of the work will not be confined to the realm of free reflection either.²⁶

This passage makes use of some highly familiar techniques of persuasion, such as claiming that anyway there is complete agreement, which had been recently stated by both parties, and that the other had said so himself in his previous statement. Adorno at least pretends to believe that there was no problem in coming to an understanding. He ignores PFL's allusion to the need for 'much writing and talking back and forth before we would really understand one another', implicitly shrugging it off as exaggerated misgivings on the part of the other. The 'very specific sense' of the concept of experience is not further elaborated on, and the truism that there is no research without theory is used to postulate a consensus even before the positions from which such a consensus might be reached have been specified.

For Adorno, the conclusion to be drawn from the 'theory-based approach' is that it seems unreasonable to set in motion the whole clumsy research apparatus in order 'to come to results that are clear as daylight even before one started'. Those who have never dealt with any such 'apparatus' tend to cultivate prejudices against the unknown, which always boil down to saying that one might as well do without it. This argument has been raised against procedures of participation as well as against instruction sheets and, in this case, empirical research. Adorno's favourite truism is: 'to employ the mental and economic power of extensive research works ... where it is really *worth* employing, i.e. where findings are obtained that would be impossible to obtain without this effort' [*italics in the original*]. Whether or not the findings obtained as a result of an empirical study are worth the effort is something one never knows beforehand – otherwise one could indeed do without the whole effort. In the PRRP, despite assertions to the contrary (Wiggershaus 1986: 276), there was no client to define specific targets, and sponsors and researchers in those days were quite undaunted in their belief that empirical research just made sense.²⁷

For Adorno, the 'clumsy research apparatus' he repeatedly alludes to is reducible, or reduced, to the mechanism of questioning. At any rate, he feels that it should primarily be brought into play, 'where the theory has proposed theses of an exposed or problematic nature, thus using the research apparatus as an agency of control,' and ironically adds: 'I believe that this is not the

opinion of a hopeless Hegelian highbrow but also the wisdom of the provident paterfamilias.' The example he then gives illustrates his level of familiarity with research issues:

Thus, in order to find out that the acoustic type, in terms of experimental psychology, will respond more strongly to the radio than the optical type, one need not set in motion a large research apparatus; not only can the result be prophesized, but the discrimination between acoustic and optical individuals is such a neutral one, even on the social level, that even the most reliable statistical data cannot be expected to contribute to a social theory of the radio.

It is something of a mystery where Adorno unearthed this typology, which is to be found neither in PFL's memorandum nor in the psychological literature of the time on radio listeners. Allport and Cantril considered a broad range of aspects to understand radio listeners – age, income, sex, intelligence, cultural level, attitudes, occupation, etc. – but it never occurred to them to use an approach based on a distinction of optical versus acoustic types (Cantril and Allport 1935). Actually, what Adorno wanted in the first place was not to propose a concrete psychological hypothesis but to reject psychological studies in general as superfluous or at least of secondary importance:

Generally, I would like to advise against a 'psychological' method that conceives of individual consumers as test subjects and hopes to obtain valid results by testing them. This kind of procedure presupposes nothing less than a liberal view of society where people act according to their psychological constitution and where the societal side of their action, their social character, becomes visible only as a variable in the framework of psychological constants.

Adorno rejects the idea of humanity that underlies this kind of research, but it remains unclear whether he believed that the presupposition he describes could be replaced by a different view of the societal determination of individual action. His remark concerning the presupposed liberal views of society is ambiguous since it can be read as saying both that the psychologists thus criticized were presupposing one of several views and that they took the existing American society as their unquestioned starting point. Referring to social character as a variable in contrast to psychological constants results in a mix-up of the concepts of social research with those of philosophical anthropology where the search for constants (i.e. for universals of social action) is a longstanding favourite.

In his general rejection of psychological methods, Adorno completely failed to see that PFL's understanding of psychology was, at the time, pitted against two rival views. PFL tried to democratize, as it were, the older European tradition of introspection by postulating that trained psychologists were not the only ones who could produce valid data on individuals' mental processes. Rather, he felt that by refining questioning techniques he could enable ordinary people to give valid information on why they had acted as they had.

In this, he was opposed to the American behaviourists, then entering their golden age, for whom this was just mumbo-jumbo.

The only thing to be gathered from Adorno's remarks on issues of questionnaire design is that he had no real knowledge of the field but thought himself wise enough to lecture others. He was generous enough, however, to concede that it would be 'in everyone's best interest ... to put [my] theses on verification to the test of adequate survey methods'. As to who had the power to define what an adequate survey method was, and under which conditions it was adequate, nothing is said.

For the rest, most of Adorno's remarks are dedicated to an exposé of his ideas on music on the radio. He apodictically states that, 'for the radio, as for political economics as a whole, the decisive information is to be gained from the relations of production on which consumption depends'. His understanding of this analogy between the relations of capitalists and proletarians (i.e. the relations of production in a Marxist perspective) and the relations of radio production soon becomes clear when he writes that 'the analysis of the musical radio needs to start out from its technique, as its mode of production'. However, the analogy with the criticism of political economy, arbitrarily introduced by Adorno, suddenly gets muddled. Just as the mode of production it is not exclusively determined by technology in the realm of material production, it seems similarly unreasonable to assume that the 'societal relevance' of the radio could be exclusively explained by an analysis of its 'technical method of production'. This, however, was his key thesis – underlined for emphasis – which 'needed to be verified and the verification of which I see as the essential task of our cooperation'.

Adorno later proposed a rather consistent explanation of what he had in mind, relying on the ideas published by Ernst Krenek in an article entitled 'Bemerkungen zur Rundfunkmusik' (Remarks on radio music) in the *Zeitschrift für Sozialforschung*: presentation of music on the radio fundamentally differed from presentation in a concert hall; radio listeners were not only confronted with the lesser quality of the broadcast but were induced, by factors that are not described in detail, to attribute something in the nature of a picture to what they heard. Moreover, while in the concert hall, the real apperception of time resulted in an '*Aufhebung*' (suspension) of chronological time, radio listeners were continuously made to feel this real time as a result of the '*Hörstreifen*' ('hear stripes'; e.g. the rolling background or 'white noise' sound of radio, a constant reminder of its reproduction). But rather than elaborate in detail on the possible inter-connections this suggested and confront the experts of social research with the demand to think up, or at least try out, an adequate survey technique, Adorno felt sure from the start that this could not be done, couching his judgement in the rhetorical question: 'But how could this mode of apperception be captured by surveying the listeners?'²⁸

However, not carrying his obvious aversion to empirical research to extremes, Adorno conceded that after a detailed analysis of radio production,

‘it might be possible for us to develop methods for analysing their “correlates” in listeners’. Thus, once his exploratory work for a theory of the production of radio music was accomplished, he wanted ‘to conduct interviews ... at key points of radio production, that is, with operators, technicians and artists’.

Adorno then proposed – ‘since I am already making propositions in empirical terms’ – to dedicate one of the listener studies to an analysis of listeners’ letters, since he was familiar with this kind of material from his time with the radio in Frankfurt.²⁹

As a rule, interpreting [these letters] should be done with a good deal of caution since the authors, just like those who write letters to newspaper editors, generally belong to the type of the know-it-all, the grumbler, the stuffed shirt while those who are positively impressed are much more likely to be quiet. On the other hand, these letters are more informative than the responses given to questionnaires since they are written spontaneously and without the inhibitions produced by questionnaires. Most notably, the grumblers here are much less likely to pretend to a higher intellectual level than what is actually theirs, and will much more often give vent to their hatred of superior intellectual demands, which I assume, once more drawing on my Frankfurt experience, to be rather wide-spread among today’s passive radio listeners.

Thus, where Adorno can draw on primary experience in the field, he brims over with suggestions but at the same time already knows all about the grumblers and their motives.

PFL’s hopes that he had scored a hit by inviting Adorno were indeed justified. He could reasonably expect that they would be able to talk the latter out of his prejudices against empirical survey techniques, and any lack of knowledge just spurred on the pedagogue in PFL. Even before Adorno left for New York, he got an answer. PFL wrote that he had been much impressed by Adorno’s remarks on the changes imposed on music through broadcasting, as well as by the paper by Krenek that he had read in the meantime. He also agreed that ‘such an approach needs a theoretical analysis first’. On the other hand, he never tired of stressing what Adorno would have to do: ‘We shall have to understand that you have to end up finally with actual research among listeners, although in many cases we might have to stop with the formulation of the theoretical problem and discussion of techniques to answer them, simply for reasons of time.’³⁰ All of Adorno’s most valuable remarks needed to be tested to see whether they were true:

Of course, there is no reason to expect that the answer could be found just by a questionnaire method, and I certainly do not think that empirical research and interviews are identical. It might be that experiments are necessary, or a very detailed introspective analysis with a few subjects especially trained for introspection, as the Wurzburg school used them. At any rate, I feel very strongly that the experimental beginning taken in the German psychology of thought has never been properly exploited for more realistic problems. Opportunities for experimental broadcasts will be easily given here.

PFL also agreed with Adorno that the 'dulling character of entertainment music is of much greater social importance than the direct propaganda which might be exercised over the radio'. This thesis was at odds with the views of 'liberal educators ... in this country' and would have to be further elaborated. However, the PRRP was subjected to two restrictions. First, it was limited to the 'American scene', which precluded comparisons with Europe; still, the European experience could be drawn upon for analysing the American situation. Second, the working conditions of musicians and similar issues were not part of the research project: 'I am enclosing the original statement of the project on the basis of which the money was granted. Although we have, as you know, much more precise ideas as to what we should do, the basic trends of this draft are still binding for us.'

In mid-February, Adorno crossed the Atlantic, and on 26 February 1938, he first set foot on the premises of the Research Center at Newark (Wiggershaus 1986: 268). What was going on there must have made a lasting impression on him, since even three decades later his memories were still so vivid that he used them as an introduction to his report on his activities in the PRRP:

At the time, the center of the Princeton Radio Project was neither at Princeton nor in New York but in Newark, New Jersey, and was set up in a rather improvised way in a disused brewery. When I went there by the tunnel beneath the Hudson, it felt a bit like Kafka's Nature Theatre of Oklahoma. I admittedly was rather attracted by this absence of prejudice in the choice of locality, which is hardly imaginable by European academic standards. My first impression of the ongoing studies, however, was not marked by much comprehension. On Lazarsfeld's suggestion, I went from room to room, talking to the staff, listening to words like 'Likes and Dislikes Study', 'Success or Failure of a Programme' and the like, which at the time meant next to nothing to me. Still, what I did gather is that this was about collecting data that were supposed to be of benefit for the planning agencies in the field of mass media, either directly in the industry or by way of cultural advisory boards and similar bodies. (Adorno 1981: 303–4)

What Adorno believed he understood was not quite what the PRRP was doing, since their goal was precisely not to let anyone directly benefit from their data. Nevertheless, on a half-day basis, Adorno started to direct the 'so-called Music Study' (Adorno 1981: 302) which in the first half-year, however, meant that he was at the same time the conductor and the orchestra. In a letter to Walter Benjamin written a few days after his first workday at the PRRP, Adorno even went one step further and claimed to have taken over the overall theoretical direction of the project (Adorno 1994: 313). There is nothing to suggest that Adorno was joking.

A PROJECT SEEKS ITS OBJECT AND FACES ADVERSITY

In the spring of 1938, two of the four fields that the PRRP wanted to cover in its studies – 'music, book-reading, news, and politics' – had actually started

operating. Besides Adorno's work, there was a study directed by Cantril, described as the Commentator Study in the memorandum, which had been given priority over the Success and Failure Study by PFL. For this study, James Rorty had already been recruited as a collaborator at the end of 1937. Not unlike Adorno, he had been assigned the task of a month's collection of material followed by a presentation of his ideas on the subjects he had singled out for further research.³¹ In May, 1938, PFL complained to his two co-directors that Rorty was still conducting 'interviews with commentators and other people on the production side' and failed to realize that 'we are running a listener project and that [h]is material is valuable as background material and as a lead to listener research but not as an independent unit.' He goes on to say: 'The Commentator Study is definitely a strategic unit. It really could show the interlocking of general information as Rorty collects it and of special experiments as they are now summarized in Had's [Cantril's] final outline. I did not feel that the two Rortys [Rorty's wife was also working for the project] are aware of the importance of those twelve special experiments.'³²

In November 1938, PFL and Cantril agreed that a showdown with the Rorty couple was inevitable. PFL once more wrote a memorandum of several pages summing up his experience with both of them and comparing it to the Adorno case. For understanding the relationship between PFL and Adorno, the following lines are significant insofar as they were written after their first 'showdown':³³

The best way for me to describe my feelings is to put them on a comparative basis between the Commentator and the Music Study ...

1) W[iesengrund] contributed a great number of original ideas and in many discussions with the Rockefeller people and with broadcasting specialists, I realized how stimulating some of his observations and theories are once they are translated into understandable language. R[orty], as far as I know, has hardly contributed any ideas to the whole study. The original plan was worked out from an incidental remark by Gallup and I had all the details formulated and explained to R at my apartment when I laid the whole plan before all of you – pretty much the same as it is now. It is true that I still have some difficulty in getting W down to earth but there can be no doubt of his originality and the fruitfulness of his approach. With R, I do not even know whether he has produced a new aspect although Had might correct me on this point.

2) W. took his assignment very earnestly from the beginning and he is certainly spending the greater part of his day on the project. R. might do some work now but he certainly was not honest about his time when I had the opportunity to observe him.

3) W. considers himself definitely a part of the project. He attends the seminar, keeps himself informed of the other studies, and is helpful wherever he can be. R. might have to be a lone wolf insofar as his intellectual production is concerned, but there is certainly not the slightest justification for his keeping himself absolutely isolated from the project in spite of my efforts to draw him in. I just do not believe that someone could work with us who does not participate, even to some small degree, in our own development and who does not catch some of the atmosphere in which we all work.³⁴

On the same day on which this memorandum was sent to his co-directors, PFL also sent a letter to Marshall presenting the PRRP publications plan. He wrote that they were planning 'a series of monographs' and gave a detailed account of the first four titles: Stanton was to write a volume on 'Measurement Techniques', PFL himself would produce an article on 'The Art of Asking Why', James Rorty and Hadley Cantril would contribute a study on 'Radio Commentators', and the fourth volume would be 'Music on the Air', written by Adorno.³⁵ None of these monographs ever saw the light of day in the world of publications. The first project PFL had to write off was the Commentator Study because cooperation with Rorty was past saving even by Cantril (though he was not ultimately given notice until autumn 1939).³⁶ As a consequence, PFL must have all the more felt the need for the Music Study to be a success even if Adorno had not been the always helpful and interested collaborator PFL had made him out to be to his co-directors.

Rorty and Adorno were not the only ones to become a problem. After only half a year, a heated argument arose between Cantril and PFL about the latter's easy-going way with the budget. At the end of March 1938, faced with PFL's statement of expenses for the PRRP's second quarter and having by chance heard that a union had been formed by the project collaborators, Cantril's patience gave out. The result was an ultimatum to PFL, summoning him to present the following information at the next directors' meeting: 'If you could bring down not only your latest budget, but an outline of the persons working in Newark, the salaries they are receiving, their assignments, and the tenures they have been promised.'³⁷ Once this information was produced for all to see, he said, they could decide on a budget that was strictly binding for all of them and to which all three directors would commit themselves by signing a statement set up by Cantril. The argument went on for a certain time, but in the end Cantril failed to impose himself. Within just six months, the balance of power had clearly shifted in favour of PFL, who as a good tactician knew how to rally Stanton for his cause.

However, the differences did not only concern PFL's notorious easy-going way in budgetary matters as well as with promises made to collaborators, but also the choice of projects, the selection of collaborators and relations with the International Institute of Social Research where PFL was still due to deliver the study on the impact of unemployment on American families. Auxiliary work for this study was partly paid for by PRRP funds, and Cantril insisted that the money be claimed back from Horkheimer's institute. Given his delicate position with the institute – the study that he, that is, Mirra Komarowsky, was conducting for it was already two years overdue and would take another two years to be terminated – this was the last thing PFL wanted to do.

Another difference that kept turning up in the three directors' internal correspondence concerned the conductor of the Music Study. A few months after his arrival in the United States, Adorno had submitted a 160-page typescript which, it is true, drove PFL to despair but – contrary to the accounts given in Jay (Jay 1976: 227–8 and 265), Morrison (Morrison 1998; Morrison 1978a),

Wiggershaus (Wiggershaus 1986: 272–3) and Dahms (Dahms 1994: 247–53) – did not prompt him to terminate their cooperation. In a long letter PFL accused Adorno of using an unnecessarily difficult language, blithely ignoring empirical reality in favour of unproven ideas, indulging in polemics against an allegedly American method that actually was PFL's preference for introspection (and was thus strictly opposed to American psychology) and of ignoring the most elementary rules of argumentation at the height of his polemics. At the same time, he assured Adorno of his high esteem and desperately tried to make him see that unless he was bent on jeopardizing the continuation of the PRRP, his pamphlet could not be made public.³⁸

Adorno remained a collaborator of the PRRP for two-and-a-half years, and the RF's refusal to further employ him only came when negotiations about transferring the project to Columbia University had started. This refusal was preceded by talks between PFL and Friedrich Pollock as well as Max Horkheimer, who both spoke in favour of Adorno. In the letters accompanying these talks, all parties sought to find a way of keeping Adorno in the music project.³⁹ PFL's later attempt to obtain separate RF support for Adorno failed due to the reservations expressed about him by various people inside and outside the RF as well as Adorno's own failure to come up with a final report after the two-and-a-half years working on the project. The reason ultimately given for the refusal was that Adorno's working plans did not fit into the programme structure of RF sponsoring measures. Even then, PFL tried to keep Adorno in the project but had to give up in the end.

Since both the Commentator Study and the Music Study were more or less stuck, PFL was reduced to hoping for success in the other sub-studies. One of these was the Success and Failure Study mentioned above, concerned with the relation between the intensity of radio listening and the reasons for discontinuation, on the one hand, and specific traits of radio listeners, on the other. Conducting this study was easier thanks to the comprehensive material that was made available for secondary analysis by the commercial radio stations. The procedures used, as described by PFL, sharply contrasted with conventional textbook wisdom:

I think that we should go out quite naively first to get any available information. In this way, we will see what all the factors are which enter into the situation. Then we can work out a sort of list containing all the accounting relationships which should be considered in all cases so that our results may become somewhat comparable. There is little to say about that at this moment, however, because all the methodological problems will come up only as we proceed.⁴⁰

As a matter of fact, these reanalyses and supplementary surveys were the part of the PRRP that advanced most rapidly. They served as a basis for the first articles that can be attributed to the PRRP. PFL's enthusiastic comment, which suggests that some results of the primary surveys 'are about as important to social psychology as the first learning curves were in experimental psychology',⁴¹

must, however, be attributed to the near-desperate situation of the PRRP at the end of its first phase. To compensate, the director emphatically took to whistling in the dark.

The first major publication of PRRP findings was an omnibus volume of short articles in the February 1939 issue of the *Journal of Applied Psychology*, which was devoted wholly to radio research. Besides the introductory remarks signed by PFL as a guest editor, there were so many articles he had written himself that he fell back on a trick to cover up for it: he published two under the pseudonym Elias Smith. Topics that should have been dealt with in the monographs he had so often promised were presented in a highly condensed fashion in the articles of this issue, for instance an eight-page article by Stanton modestly entitled 'Notes on the Validity of Mail Questionnaire Returns', or one of PFL's own entitled 'The Change of Opinion During a Political Discussion' (Lazarsfeld 1939b). Actually, however, this first issue devoted to radio research – there would be another one in the following year – mainly presented reflections on the problem of indices. As a topic, this had been announced in neither the old nor the new 'testaments', but it clearly reflects what PFL had frankly admitted in his very first letter to Cantril after having been offered the position as director: 'Radio is a topic around which actually any kind of research methods can be tried out and can be applied to satisfactorily.'⁴² One-and-a-half years later, PFL might have further extended this sentence by adding that the radio also was a topic around which new survey techniques and, above all, new techniques for analysis could be elaborated. Shorter still, radio research allowed for serendipity – in this case, the finding of new methods one had not looked for or even been charged to look for.

One event that comes close to the serendipity pattern (Merton and Barber 2004) opened up an unanticipated opportunity for the PRRP to conduct a special study. On Halloween of 1938, several radio stations broadcasted Orson Welles's adaptation of H.G. Wells's *War of the Worlds* from New York's Mercury Theater. Thanks to Welles's impressive stage technique and the use of fictitious radio news, about a million listeners responded to the broadcast of Wells's Martian invasion with panic. On the very evening this happened, PFL and Herta Herzog decided to do an in-depth investigation of this phenomenon and wrote a detailed project proposal. PFL contacted Stanton the next morning to ask him for money from the CBS research fund, and then both of them approached the RF General Education Board for additional funding. Three decades later, PFL observed with resignation that today this kind of 'improvising is made so much more difficult by the bureaucratization to which the granting of funds for research is subjected' (Lazarsfeld 1969: 313). In 1938, this blitz project came to be realized.

Herta Herzog rapidly conducted a large number of exploratory interviews with listeners. Many of the initial ideas could not be realized, but in the end, the project team had gathered a total of 135 in-depth interviews. They also drew on survey data from Gallup's American Institute of Public Opinion (a representative national survey conducted six weeks after the broadcast),

presented an analysis of more than 10,000 newspaper reports, and conducted further surveys (with school principals, radio stations, telephone companies, etc.) and statistical analyses to assess the scope and intensity of the panic.

Cantril then took on the task of distilling a book from this material, presumably moved, among other things, by the prospect of being able to pass it off as a substitute for the failed Commentator Study. The published version essentially consists of a qualitative analysis of the in-depth interviews. While Cantril was still busy compiling the book, a heated argument arose between him and PFL, the latter accusing him of monopolizing the authorship at Herta Herzog's expense.⁴³ In the preceding weeks and months PFL seems to have harboured an ever-growing grudge against Cantril that now exploded on the first occasion that presented itself. Cantril retaliated with all the harshness of the upper-class American: he accused PFL of being infantile and proposed that director's uniforms with badges of rank be introduced in the PRRP to make sure that each and everyone was aware of PFL's outstanding role. Cantril once again stressed that he would rather do his own work instead of having a part defined for him by others in the radio project.⁴⁴ Even though relations between both men were beyond patching, Cantril did not abandon his unloved role as a co-director who was outshone by the others until he could finally withdraw without doing harm to anyone.

The second issue of the *Journal of Applied Psychology* devoted to radio research did not appear until December 1940 because the editors were unwilling to dedicate two consecutive issues to the same topic (Lazarsfeld 1969: 326, n. 73). The number of articles dealing with the radio and its audience had actually increased, but the articles on technical and methodological issues still outnumbered those dealing with other aspects of the new medium. One of the mediating articles presented the reader with PFL's first invention: 'The "Program Analyzer": A New Technique in Studying Liked and Disliked Items in Radio Programs.' The Stanton-Lazarsfeld Program Analyzer, as it became known in the history of communications research, provided a device for an experimental design where up to ten subjects listening to music could push a button each time they did not like what they heard. The device recorded each subject's 'dislikes' on a moving paper tape. Both of its creators were inordinately proud of their device, and this pride was shared by all those who were socialized in the atmosphere of the PRRP and its successor organizations, the Office of Radio Research and the Bureau of Applied Social Research. Those less involved would probably consider it a rather curious kind of toy and tend to agree with Adorno: 'A little machine, the so-called program analyzer, which allowed one to signal what one liked or disliked etc. by pushing a button while a piece of music was presented appeared to me, in spite of the apparent objectivity of the data it provided, to be sadly inadequate given the complexity of what was to be explored' (Adorno 1981: 306).

'Little Annie', as insiders called the machine (Rogers 1994: 275–7), was later primarily employed as a source for clues to be used in the in-depth interviews

that were conducted after the presentation. In this form, it helped to elaborate the technique of the focused interview (Merton 1987b: Merton *et al.* 1990); in its original form, it was long used in the advertising business.

EVALUATION AND ITS CONSEQUENCES

In the spring of 1939, after eighteen months of work, extensive negotiations began for the continuation of the Princeton Radio Research Project beyond the initial two-year period. By that time, the project was no longer located in Newark since that local university had gone bankrupt in the autumn of 1938. The PRRP had moved, without having to change its name, to a new location on Union Square in Manhattan, where during the next months it operated under the name of 'Princeton University, Office of Radio Research, Field Headquarters, 22 East 17 Street, New York'.

The RF had set up a reviewer committee of nine members charged with evaluating the PRRP's output. Members of the committee included representatives of the two big radio broadcasters, the National Broadcasting Company (NBC) and Columbia Broadcasting Company (CBS), social scientists such as Robert Lynd and Harold D. Lasswell, and other representatives of universities and educational institutions. Three decades later, PFL described the atmosphere of these months as follows: 'The outside impression one got of the Research Center [left] something to be desired. One could not identify a central topic, and rumors were beginning to reach us that key people started to ask themselves if we really knew what we were up to' (Lazarsfeld 1969: 317).

All in all, evaluation by the reviewer committee was positive, and the RF decided on a continuation of another six months and a further funding of \$17,500 (\$267,000 in 2010).⁴⁵ These six months, as seen by Marshall, were to be dedicated to the elaboration of a programme for a subsequent three-year project phase. There were, however, two conditions bound up with the additional half-year and the promise of further continuation. The first was that PFL had to present an overview of the results obtained so far; the second was that continuation would, then, only be approved if no new projects were initiated. Instead, the data collected thus far were to be analysed in more detail. Stressing the urgency for PRRP activities to get more focused, Marshall informed PFL by telegram of the demand for an interim report:

Discussions in office indicate reluctance to invest in new research pending formulation of present findings Stop feeling here that need is for breathing spell to save project from being victim of its own success Stop result decision to make no recommendation to trustees now Stop ready to review situation in June if formulation is sufficiently advanced by then to provide basis Stop Marshall⁴⁶

By their criticism, the members of the reviewer committee and the RF Humanities Division, who were indeed favourably inclined towards PFL, had hit on the PRRP's sore spot. Overjoyed at having so much money at his disposal, PFL – besides using part of the new funds to complete other projects

that were overdue – had initiated so many sub-projects and started so many trial balloons that the coherence of the whole enterprise was somewhat lost to view. This constellation – foundation officers interested in the issue, favourable external reviewers and a project director brimming with ideas – is also interesting for evaluating the development of empirical social research from a sociology of knowledge perspective, since it illustrates the felicitous interaction of forces that prevent free academic research from dissociating itself from reality, which is in turn likely to result in a loss of purpose and a failure to reach its goals. In the case of PFL, it brings to mind the influence exercised by Otto Bauer on the young social psychologist in Vienna in the early 1930s, which prevented him from engaging in a study on leisure time behaviour and directed his attention to the problem of unemployment. Free research, conceived of as an opportunity for scholars to pursue their ideas, fancies and inclinations to the last ramifications of an issue, is only too likely to end up in idiosyncratic constellations of incessant activity while even the most discerning outsider is at a loss to identify its output.

In the spring of 1939, PFL was facing two problems: he had to find some future institutional affiliation for the PRRP, and he had to put himself to the task of bringing its previous work into a presentable form. With respect to the former, support by Lynd enabled him to manage a smooth transfer of the Office of Radio Research to Columbia University after two-and-a-half years of merely formal affiliation with Princeton. The field headquarters on Union Square were closed down and transferred to a Columbia building on Amsterdam Avenue. Yet this would not have been possible if PFL, in the spring of 1939, had not focused all his efforts on the requested report on the work done so far for the radio project.

The typescript submitted by PFL at the beginning of June 1939 was in the following year published by a commercial publisher;⁴⁷ the title then was *Radio and the Printed Page: An Introduction to the Study of Radio and Its Role in the Communication of Ideas*. PFL had worked virtually night and day for weeks on end to complete it. The only other authors to contribute reports of their own were Herta Herzog and Samuel Stouffer, whom PFL had roped in for support from Chicago.

The book largely consists of a presentation of the results of reanalyses that had been done for all the studies that the PRRP had collected during its first months of operation.⁴⁸ For these reanalyses, the socio-cultural level of the various study participants was consistently used as the independent variable. In order to differentiate between cultural levels, PFL relied on a discovery he had published in the first issue of the *Journal of Applied Psychology* devoted to radio research that he called ‘the rule of the interchangeability of indices’ (Lazarsfeld 1939a). Differences in literacy, he argued, were strongly correlated with socio-cultural position. Since socio-cultural position was a multidimensional phenomenon, different cultural strata could be delimited by drawing on data that were more easily accessible, such as income, housing quality and housing district.⁴⁹

It is not hard to see the problems bound up with this procedure: as long as no clearly defined indicators are used, the validity of such intuitive classifications is impaired by prejudices, stereotyped and erroneous perceptions, etc., in short, by the whole bunch of what has meanwhile become known as the bias effects of social classifications. In PFL's case, this bias arguably was rather small since he was content, or had to be content, with a very rough division of strata. Occasionally, listeners were just dichotomized into those with a (somewhat) higher and those with a (somewhat) lower educational level.⁵⁰ The four-level classification of social strata was adopted from market research where the first four letters of the alphabet were used to identify four strata, from A for the highest to D for the lowest level. In PFL's analyses, the latter level was rare because the surveys had been conducted by telephone – there were not many telephones in this stratum so it could only be incompletely covered (cf. Beville 1985). These 'ratings' from information on what subjects listened to and when (or, more precisely, on when their radio was turned on) were consistently used as the dependent variable in PFL's analyses. Age and sex were introduced as the intervening variable whenever this was feasible. No factor analysis, as announced in one of PFL's memos, had been realized, nor had any of the ideas that had filled the PRRP papers in the first months: listener biographies, psychoanalytically informed in-depth interviews, experiments under hypnosis, etc.

Still, the results are not completely trivial. Of course, it is difficult to say, after more than sixty years, which of the findings reported is most likely to have impressed the readers at the time or which of them had at least been felt to be new, but some results and interpretations are still rather interesting in a historical perspective, at least when they are considered in terms of social history and the history of mentalities. The result most likely to interest prospective readers was that the members of the higher strata listened to the radio less frequently but preferred the more serious programmes. In contrast, members of the lower social strata were more interested in light entertainment and spent more overall hours close to a turned-on radio. The programme type that was most popular with audiences of a lower cultural stratum was the quiz show, in which ordinary people had to answer questions.

The most impressive part of the book is thus Herta Herzog's report on a case study conducted with a small number of listeners to a programme called 'Professor Quiz'. This programme was less popular among members of the top socio-economic group but ranked significantly higher among those of the B stratum than among those of the next lower stratum. The programme offered listeners various gratifications: competition, instruction, self-assessment and sportsmanship. (One may remember that at the very beginning of the PRRP, PFL had wanted to consult some psychoanalysts about the gratifications that people were likely to get from radio listening.) As a listener, one could compete with the contestants in the quiz; by trying to answer the questions oneself, one learned something about the knowledge one actually possessed; and even if one was more or less passive, one could get a lot of casual fun from just

listening to a contestant trying to find the correct answer. Herzog brings to light a number of instructive details concerning the 'love triangle' (Lazarsfeld 1940: 66) of listeners, candidates and questions. The secret opponent of all listeners was the group of those with a higher education level to whom they could demonstrate, in this competitive setting, that they were not as dumb as the latter tended to make them believe. If the contestant's education level was too high, virtual competition with them ceased to be fun. Sitting at home, however, one could just skip some 'irrelevant' questions, which was an easy way to improve one's hit ratio, as Herzog dryly notes. If, on the contrary, one could find the correct answers, one could be proud of oneself and of one's knowledge. Finally, listening was a means of gaining knowledge that could come in useful next time one met someone who was talking about things learned in the quiz. Herzog's small and simple study plainly shows that measuring programme preferences was not the only way to gain insight into the effects of radio listening; in-depth interviews with listeners on how they actually got involved with what they heard resulted in interpretations that were much more instructive as well as more generalizable.⁵¹

Stouffer's contribution, filling the void left by the failed Commentator Study, provides a very detailed analysis of a large representative survey that had been co-designed by the PRRP and conducted by Gallup's American Institute of Public Opinion. This survey was about the reading of and listening to political news or commentaries. While listening ratios to radio news were approximately the same in all strata, only the more well-to-do also regularly read newspapers. Stouffer produced a finely honed statistical analysis, though the results are hardly interesting today (in 1940, some readers may have been impressed by the statistical competence of its author, and others by the wealth of individual findings).

PFL concluded with some reflections on future developments. The impact of the radio would be visible in its influence on people's attitudes and behaviours. These, however, must not be seen independently of the more general trends in social development:

Radio can, for better or for worse, facilitate many tendencies toward centralization, standardization, and mass formation which seem prevalent now in our society. But of the many alternative developments that can be visualized now, very few will occur 'by a tip of the balance'. They will be the result of powerful social forces which during coming decades will affect radio broadcasting much more than they will be influenced by it ... We can be fairly sure only that radio will not itself mold the future. (Lazarsfeld 1940: 331–3)

In a different perspective, the prospect thus outlined by PFL was to become, as is well known, the trademark of the Lazarsfeld School, i.e. the so-called 'Columbia tradition' of communications research: the social 'effects of radio' were not as important as the conservatives and the critical theorists feared. In this, Lazarsfeld was anticipating findings that would only later emerge from

further research – and one may well ask to which degree these later findings were influenced by the researcher's prior assumptions.

The research design of most of the sub-studies was of the early positivist type: there was no systematic elaboration of the problem, let alone any prior formulation of the hypotheses to be tested. The frame of reference for the presentation of results was provided by a largely uncritical adoption of the perspective of the group of readers that was primarily targeted. In some cases, the findings were at odds with the prior assumptions of the 'educator', which arguably was decisive for the resonance of the book, but the key idea of the well-meaning representatives of educational institutions was only rudimentarily challenged in the concluding remarks quoted above. In this respect, however, *Radio and the Printed Page* is not an example of 'administrative research' discussed below (a derogatory label created by Adorno and later adopted by Lazarsfeld in order to turn it, with little success, against its author). The most adequate response to the book is no doubt to regard it as a report on the first explorations in the domain of radio use, disproving some of the more popular assumptions on the impact of radio listening. As for the alleged cumulation of the variant of mass communications research that would later be associated with PFL's name, in which critics refused to see more than an effort at measuring short-term 'effects', while there are certainly some signs of it, more prevalent here is a pre-exploration done by simple surveys and rudimentary statistical analyses.

ADORNO'S TEMPORARY CONVERSION TO CRITICAL EMPIRICISM

Given that the interim report had been submitted in due time and had been favourably evaluated by an external reviewer charged by John Marshall, the latter could have started to approach the RF boards on the issue of how to continue the radio project in a different institutional setting. However, world politics did not play along as the outbreak of war in Europe brought the well-practised RF routines to a temporary standstill.

This did not keep PFL from maintaining his relationships with the foundation officers. Additional guests were invited to attend one of the regular PRRP 'staff seminars' on 26 October 1939. The referent of the day was Adorno, who was scheduled to talk about the 'principles by which his research has been guided'.⁵² There is no report on who exactly attended this seminar, but John Marshall certainly was there, and other visitors no doubt included a good number of those regularly attending the monthly meetings initiated by Marshall in September 1939 to discuss the direction of mass communications research.⁵³ Inviting them to Adorno's presentation was thus a kind of counter-initiative with the aim of giving the established radio researchers some idea of the divergent position of a European theorist who had made himself conspicuous in previous months by repeatedly demonstrating his social incompetence.

Adorno's presentation at the seminary is available as a manuscript, 'On a Social Critique of Radio Music'.⁵⁴ It was written with the assistance of George Simpson, who had been assigned to Adorno when an attempt at cooperation with another staff member had ended in disaster. Adorno's junior by five years, Simpson had translated Émile Durkheim (*La division du travail* and *Le Suicide*) and, having graduated from Columbia University, had worked as an instructor at Queens College since 1940. Notwithstanding this rather obvious status difference, Adorno, a beginner in sociology, felt the need to belittle his co-author as an 'adjoined assistant' in his memoir (Adorno 1981: 313), generously crediting him with 'rather a theoretical frame of mind'.

In his presentation, Adorno pursued a double purpose: he wanted to explain what social critique of the radio with respect to music might be, and he wanted to show the methodological implications of this perspective. The conventional approach, as seen by Adorno, was to study the radio by registering its impact on the audience, exposing certain groups of subjects to a specific stimulus, observing their reaction to it and subsequently selecting the settings that had produced the desired effect. The whole procedure was based on an 'administrative technique': how can a more or less 'central agency' successfully influence given groups with the aim of making them behave as this central agency wanted them to behave? 'The logical form of such investigations is moulded [...] according to the ideal of a skilled manipulation of masses.' In this, the patterns followed were those of market research, even though nothing was sold. This 'administrative research', as Adorno called it, came in two variants: an 'exploitive' and a 'benevolent' one.

Adorno describes his own approach as the exact opposite of the exploitive variant of 'administrative research' and as a complement to the benevolent variant. The question of how good music could be made more accessible to a larger audience could be studied using both forms of 'administrative research'. In contrast, Adorno goes on to write, his own concern was with the question of what good music was, in the first place. Even the best music, such as for instance music by Beethoven, had degenerated, through constant reiteration, from a vital force to a museum piece. Even if Beethoven might not yet be worn down in a young country, musically speaking, such as the United States, it still was far from certain whether or not broadcasting this music had a detrimental effect on its quality.

Adorno's concern was not with the individual effects of the radio on its audience but 'to discover the social position and the social function of radio and particularly of radio music'. To this end, one had to start out from four 'axioms': first, people lived in a society of commodities; second, in this society of commodities, there was a general trend towards a concentration of capital, constraining the free market and leading to an ever increasing standardization of goods; third, the more strongly contemporary society struggled to continue in its present form, the more imminent the prospect of ideological levelling

would become; fourth, contemporary society was increasingly characterized by antagonisms.

In the second part of his presentation, Adorno illustrates the method underlying the critical approach in music research by describing a study he had conducted himself: a very detailed study of listeners' mail sent to a rural radio station dedicated to the promotion of 'serious music'.⁵⁵ This mail had consistently taken on the form of 'standardized enthusiasm', and the reason for this was that, 'the listeners were strongly under the spell of the announcer as the personified voice of radio as a social institution and his call to prove one's cultural level and education by appreciating this good music'.

Adorno goes on to argue that while he was well aware of the dangers of arbitrariness and prejudice, he believed that they could be avoided by what he called 'musico-technical control of sociological interpretation'. Music was a rigorous discipline that did not allow for arbitrary turns but made clear statements on what was right or wrong, true or false. Thus, if correctly employed, a musico-technical interpretation could easily show that distinct light popular music varieties of the time, such as 'sweet' and 'swing', were essentially the same.

Adorno concluded his presentation by pointing out the relationship between a social critique of music and empirical research. In an almost conciliatory vein, he argued that while his approach and that of 'administrative research' were indeed antagonistic, they proved to be mutually supplementary whenever they could directly deal with the material. His own contribution to listener research could consist in the elaboration of a provisional typology of music listening (a continuum extending from the 'expert listener – the musically trained and fully conscious listener' to the 'musically ignorant' and the 'music hater'). The interviews that had so far been conducted showed that the extreme types were rarely found. Both the theoretical and the empirical typology were compatible with 'administrative research'. Types in terms of a psychology of music were sociologically not invariant. This had nothing to do with the fact that the more affluent preferred more serious music or that rural listeners were more inclined towards 'old-fashioned popular music'.

With respect to mass audiences, it had been shown that there were mainly two types: the emotional listener who could also be characterized, in musical concepts, as the 'harmonic-melodic' listener, and the rhythmic type. The former was similar to the 'moving-picture spectator'. Music was experienced as 'release', and the happiness music induced was due only to its power to allow the listener to be momentarily *unhappy*: 'Music for them, is a remedy against the "keep smiling" attitude.' The rhythmic type was also adapted, in relation to the monopolistic mode of production, to given social conditions: examples were Nazi stormtroopers on the march, or people who admired authoritarian discipline in sports.

Adorno's presentation of 'social critique' reveals that he was willing to modify his position in light of the criticism he faced. At the same time, the text highlights the cornerstones he was by no means willing to give up. One of

these was his conception of a theory of music, which is nothing short of scientific. What is disconcerting about it is not so much his use of the term of 'musico-technique', although given the Frankfurt theorists' relentless insistence on the importance of word usage as part of their criticism of ideologies and their more or less endemic phobia of technology, one might well ask why 'musico-technique', of all things, should be an exception. But it is the content of Adorno's remarks that strikes one as quite antagonistic to the conception of science he subscribes to in his other writings. Since he seems to conceive of 'musico-technique' as a set of general rules for the correct way of producing musical effects, one may well ask why such general regularities should exist in the field of artistic skills while they are definitely ruled out for other domains, particularly for the social universe. Second, one might ask how people should come to know these production rules: why is the production of music, which presupposes highly sophisticated intellectual competencies, something that can apparently be learned, which in turn presupposes that knowledge of the underlying rules is possible, while knowledge of any such rules in the field of social regularities is not?

This musical scientism is all too obviously part and parcel of the normative stance of the well-educated bourgeois who knows exactly what is good and what is bad in cultural matters. Undaunted by any relativism whatsoever, the admirer of the modern conception of music delivers judgements on those who incurred his displeasure, such as Toscanini, Tchaikovsky or Sibelius, among others, and passes off these judgements of value as objective. However, since the whole PRRP was driven by an unquestioned preference for serious programmes serving an educational purpose, it would be unfair to accuse only Adorno of normativism. Rather, it could be argued that Adorno was subject to a normative fallacy, while what PFL stood for could be described as an empirical fallacy. The former is sure of the real facts prior to any empirical research, the latter tends to believe that only what can be empirically found actually exists. PFL was at least conscious of this, as when, with a maximum of goodwill, he summed up Adorno's position as follows:

Omitting a number of details and specifications, the 'operation' basic in this approach consists of four steps:

- (a) A theory about the prevailing trends toward a 'promotional culture' is introduced on the basis of general observations ...
- (b) A special study of any phenomenon consists in determining how it expresses these prevailing trends (introduced in (a)) and in turn contributes to reinforcing them.
- (c) The consequences of (b) in stamping human personalities in a modern, industrial society are brought to the foreground and scrutinized from the viewpoint of more or less explicit ideas of what endangers and what preserves the dignity, freedom and cultural values of human beings.
- (d) Remedial possibilities, if any, are considered. (Lazarsfeld 1941: 12–13)

As for defending the opposite view, for which the label of 'administrative' is accepted without protest, PFL confines himself to saying that 'fact-finding'

is indispensable, and to refuting its alleged 'futility' by a figure of thought that he would use again a few years later in an extended review of the *American Soldier* (cf. Lazarsfeld 1949):

One might ... tell such an opponent [e.g. of empirical social research] that according to studies which have been done people who make up their minds during a political campaign as to how to vote are influenced by very different factors than those who have more permanent political affiliations. The opponent will find that immediately understandable and will say that he could have come to this conclusion by using good common sense. It so happens that the opposite is true and that it is possible to predict to a high degree the vote of originally undecided people by means of the same characteristics which describe people with actual party affiliations. There are many other examples by which common sense first can be led to conclusions which then are proved by actual data to be incorrect. (Lazarsfeld 1941: 8, n. 1)

Although PFL does not explicitly say so in his text, it is clear that for him the common-sense point of view of the opponents of empirical social research is the one subsumed under point (a) of his summary of the critical approach. Nevertheless, he insists that critical analysis must be an integral part of the everyday workings of social research. Yet one cannot help feeling that he chose the wrong place for his message, since the task here would rather seem to be to convince the readers – not to speak of the makers – of the *Studies in Philosophy and Social Science* (as the *Zeitschrift für Sozialforschung* was now called) that empirical social research, vilified as 'administrative research', was quite justified in laying claim to a place in the universe of critical theory:

It might very well be, however, that we are all so busy finding our place in society according to established standards of success that nothing is more important at this moment than to remind ourselves of basic cultural values which are violated, just as it was of decisive historic importance a hundred years ago to remind the English middle classes that they were overlooking the sacrifices which the new strata of industrial laborers underwent when the modern industrial world was built.

There is a touch of heroic futility in this attempt to submit this utterly social democratic credo to the Frankfurt parlour Bolsheviks.

But let us get back to Adorno's 'social critique'. Four aspects should be noted. First, Adorno was willing, at least in principle, to submit his 'theory' to the empirical test. Second, he tried to advance empirical work by proposing a draft typology, even though it is unclear how he constructed his types or where he got the material he relied upon. Adorno seems to have taken no account of PFL's text on the issue that was published in the critical theorists' house magazine, since in a letter to Walter Benjamin he described it as something one could safely ignore.⁵⁶ Third, Adorno's typescript contains remarks on the social function of the radio, which are described, in a somewhat unclear use of the concept, as axioms.⁵⁷ Adorno's axioms of commodification,

standardization, ideologicalization and social antagonism were indeed reconcilable with the terminology of social functions.

The most momentous aspect of Adorno's 'social critique', however, was the fourth – his creation of the term 'administrative research'. While in his presentation, he still differentiated between its exploitative and its benevolent aspects in order to prevent his opposition to what PFL stood for from becoming too obvious, he – and, with a vengeance, his followers – later dispensed with this differentiation by disposing of all that did not conform to the commandments of critical theory in the garden dump of 'administrative research':

Lazarsfeld later explained ... the difference between administrative research and critical social research as pursued by our Institute. In the context of the Princeton Project, however, there was little room for critical social research. Its charter, laid down by the Rockefeller Foundation, explicitly stipulated that the studies be conducted within the context of the established US commercial radio system. This included that this system itself must not be studied, but only its consequences in terms of a sociology of education. I cannot say that I strictly abided by this charter. (Adorno 1981: 304)

To represent so many things in so distorted a way in less than ten lines is a rare feat indeed. PFL more than once made an attempt to reconcile critical and empirical social research, but to no avail. Citing all the cases in which Adorno's polemically distorted description became an arcanum would be nothing but tiresome (one that is representative of all the others is Bonß 1982: 194–201).

WORTH READING, NOT WORTH SPONSORING

Adorno's presentation in October 1938 resulted in a series of intensive efforts to maintain the Music Study as part of the continuation of the PRRP. A few days after the presentation, PFL sent copies of Adorno's typescript to the RF where it was circulated as usual. The head of the Humanities Division, David H. Stevens, jotted 'well worth reading' on its title page. In December, the following note is found in PFL's 'Proposal for Continuation of Radio Research Project 2', as part of the 'Summary of Project 2': 'Completion and preparation of a book on the Music Project: By Dr. Adorno, assisted by Dr. George Simpson'. The duration of this project is given as 'March 1, 1940 to October 1, 1940'.⁵⁸

In order to secure continuation for Adorno's work, PFL sent a three-page, single-spaced letter to Marshall on 27 December 1939, attaching an exposé of a 'Study of Likes and Dislikes in Light Popular Music' (apparently the study was published in 1941 as 'On Popular Music', see Table 5.1) and another two essays by Adorno: a revised version of 'social critique', from which the above quotations were taken, and an empirical study of the 'Damrosch Music Appreciation Hour'. This radio programme, named after its creator Walter Damrosch and produced and broadcast from Manhattan, sought to give listeners an understanding of serious contemporary music, and Adorno had analysed the printed 'booklets' that were used to advertise the programme. In the

second part, Adorno had interpreted the social implications of the programme. The programme was, PFL goes on to write, 'an example of what Dr. Adorno calls a social critique, and although it will probably be of great interest to radio educators, its position in his whole work is to exemplify principles and concepts. I suppose you will agree with me that such an analysis of an actual broadcast is a good bridge between mere theory and empirical research.'⁵⁹

In order to fully convince Marshall and all those in the RF or among its advisors who might harbour doubts about the importance of continuing Adorno's work in the Radio Research Project, PFL informed him that two further studies were nearing completion, and gave a detailed description of their content. A manuscript entitled 'The Radio Voice' corresponded to that part of the presentation where Adorno discussed the question of whether good music broadcasted on the radio would remain what it was before the broadcast. To answer this question as precisely as possible, Adorno had chosen music of quality beyond doubt: Beethoven's symphonies. In the following, PFL wholly adopts Adorno's description, including those parts that he could hardly have been convinced were valid: 'The result is that a symphony transmitted over the air is in danger of becoming a piece of entertainment. Some basic categories of the social critique – particularly those of atomistic listening, of musical fetishism (quotation listening) and of the objective conditions for a general retrogression of listening – are *deduced* from this technical analysis.'⁶⁰

The third study, PFL wrote, which Adorno had written on PFL's request, showed that the previous 'communication studies' had failed to sufficiently conceptualize the problem. All in all, using his manuscript on 'social critique' as a basis, Adorno was planning six essays where empirical material would be used.

A typology of music listening existed as a draft, and a certain number of experiments dealing with the 'likes and dislikes in popular music, and atomistic listening' had already been conducted by the PRRP. A study on the structure of the material of light popular music had been launched. According to Adorno, the 'vested interests' of the four big music producers and the three radio networks was conducive to standardization. Their gigantic advertising efforts resulted in the eviction of all those rivals who were not integrated in this network, and in the factual elimination of the market. The obvious hypothesis was that listener reactions, too, were confined to this frozen pattern, but tendencies to the contrary should be studied in more detail. Since a large part of the popular music that was broadcasted consisted of jazz arrangements, a detailed socio-psychological theory of jazz was indispensable. In a second part, the manuscript was going to study the automatism of 'plugging'.⁶¹ In a step-by-step investigation, the trajectory of a hit would be reconstructed from the moment it was being formed in the composer's mind until it reached fame and, finally, became 'obsolete and dead music'. The theoretical thesis concerning the monopolistic and manipulative character of today's 'mass fads' would be grounded in detailed empirical material. The third part would be dedicated to the psychology of listener behaviour. Listeners' 'likes and dislikes' were not only socially preconditioned but, rather, 'socially "substituted"' – free choice

of what one liked to hear had been superseded by institutionalized and monopolistic 'tunes'. A theory of 'substitutional listening' would be elaborated, addressing the silent substitution of divergences in musical taste by schematic patterns. It would be shown that what had been demonstrated for the narrow field of music also applied to all the other branches of the entertainment industry. There were a number of 'analytic studies of current American literature on music' that would be dealt with in a separate study, including essays by 'some practical musicians'. Also, a comprehensive report would provide a résumé of all the existing material on musical programmes, and finally one of Adorno's essays would 'contain detailed, positive suggestions for program-policy in the field of radio music'.

PFL was perfectly aware, he wrote, that the approach proposed by Adorno was unorthodox and daring, but the more the latter's work was advancing, the more optimistic PFL felt. And, apparently feeling that all this was still not enough, PFL concluded by referring to 'moral responsibility' as another reason for the Music Study 'not to be dropped now'. He felt that a device of such major importance as the radio should not be studied without situating it in the frame of reference of our entire culture. In doing so, one was sure to obtain rather controversial results, which was the main reason for PFL to recommend grounding them in a field that was least subject to public distrust. Music, luckily, was such a field.

The whole letter echoes the concern of a project director seeking to convince the sponsor and his advisors of the palatability of a sub-project that was dear to him: 'Besides the great intellectual expectation I attach to Dr. Adorno's work, I should feel that the Project had failed its major task if nowhere in its work were a social critique attempted.'

PFL's letter was not the only statement on Adorno and the Music Study that John Marshall would dismiss. Anthropologist Geoffrey Gorer and Charles Siepmann (who became a leading proponent of responsibility in broadcasting), notwithstanding a number of critical points, pronounced themselves in favour of the continuation of the Music Study, whereas the representatives of the radio stations and the world of music more or less clearly opposed granting further support to Adorno.⁶² Ultimately, Marshall decided against the Music Study and further financial support for Adorno in the context of the Radio Research Project, soon to be accommodated at Columbia. In a long memo, he noted the reasons for his decision. He did not doubt Adorno's originality but felt that unless he was made to cooperate with a competent co-author he could not be trusted to commit his ideas to paper. He doubted that Adorno's exposition was of any use since he 'seems psychologically engaged ... by his ability to recognize deficiencies in the broadcasting of music to an extent that makes questionable his own drive to find ways of remedying them'. Marshall informed PFL orally of his decision and the reasons for it and asked him to inform Adorno in the way he thought best. PFL pronounced himself against the decision to discontinue the project and suggested giving a personal 'grant-in-aid' to Adorno to enable him to complete his work outside the Radio Research Project. Marshall

refused to comply with this as well, pointing out that by now Adorno had a full-time job at the International Institute of Social Research anyway, and that a break in which he could think things over could only do him good.

Adorno's manuscripts were committed to the RF archives to keep them 'available for possible future development'.⁶³ Marshall did not even unearth them when, in March 1940, he got PFL's jubilant message:

You will be interested, I am sure, in hearing that on the basis of Dr. Adorno's analysis of the Damrosch Hour, WNYC has offered him the opportunity to give a Music Appreciation Hour of his own every Sunday from 1:00 to 1:30. He will start in April. I suppose that this is the kind of direct application of our work you are looking for.⁶⁴

Adorno's papers indeed include two texts from 1940 entitled 'Zum Rundfunkkonzert' ('On Radio Concerts'), which remain unpublished according to the current collections of his works. Adorno seems to have overcome his aversion to musical announcers and commended, as was to be expected, Schönberg, Zemlinsky, Eisler, Krenek, Berg and Mahler to his audience. The *Hörstreifen* (white noise) problems played no part in the musical announcement, nor did any other of his musico-technological subtleties (Adorno 1984). Further indications of Adorno's activities as a radio announcer are found in his collected letters to his parents (Adorno 2003b), in which he repeatedly informed them about his study of the Damrosch Hour as well as of his own appearances. In December 1939, he still confined himself to telling his parents that he had, 'completed a major critical study on the musico-pedagogical hour presented by Walter Damrosch for NBC, in English, for the Radio Project, complementing each point of my criticism of this execrable enterprise by positive propositions that show how one should go about acquainting children and laypersons with music' (Adorno 2003b: 49). On 20 February 1940, he wrote: 'The concert, then, will be on Thursday, 1 p.m., on WNYC, wave length 801 kilocycles. But be sure to start listening 10 minutes earlier, we may start before that' (Adorno 2003b: 68). A few days later, he proudly wrote: 'New York City Radio has charged me with their musical education program ... Every Sunday noon, with half an hour entirely at my disposal, the best time there is, with an audience of literally hundreds of thousands of young people.'⁶⁵ The letters to his parents prove beyond doubt that Adorno was very much interested in being a musical educator on the radio, and was proud to report it. The sour grapes suddenly, and temporarily, had turned sweet.

On reading the RF documents one gets the impression that something else, besides what was committed to paper, must have weighed in on Marshall's decision. While none of the other RF documents I studied impressed me with anything like 'vested interests', these seem quite palpable here. The mystery was solved some time ago by David Morrison who, in the course of the research he did for his doctoral thesis on PFL, had conducted an interview with John Marshall himself in 1973. Here, Marshall conceded that he had suffered

a personal insult from Adorno. Adorno had remarked, in the presence of third parties, that the RF Radio Research Project was 'at the whim of a young ignoramus' (Morrison 1978a: 342). Although Marshall felt neither young nor an ignoramus he abstained from retaliating with an outright rejection, which is evident in his offer to intervene at the Carnegie Foundation in favour of Adorno.⁶⁶

There is another point of view from which the whole procedure seems rather strange, since Marshall himself had repeatedly assured PFL, when the project had started, that he had no intention of intervening in its internal affairs – something both sides agreed included the recruitment of collaborators. In contrast, one might argue that deciding which sub-projects of a proposal submitted to the Foundation should be approved – and, thus, made possible in the first place – was well within the RF officers' responsibilities. The peculiar position of the Radio Research Project in the world of sponsored research at the time is again highlighted by Marshall's decision against support for Adorno. No other RF-supported projects I know of show such an amalgamation of researchers and foundation officers, on the one hand, and third parties belonging to the radio, advertising and, arguably, music industries, on the other. The decision to discontinue financial support for Adorno had been reached – as Marshall recalled in his interview with Morrison – after talks with representatives of the music business.

It would be erroneous, however, to attribute Marshall's attitude towards Adorno to the latter's non-conformism or to suggest that the RF had objected to his critical theory, let alone its hidden Marxism. The RF documents abound in cases where support, often essential for somebody's survival, was granted to scientific outsiders provided the latter could at least rely on the backing of some people in the scientific community in question. Thus, W.I. Thomas, having been sacked by the University of Chicago, had only been able to pursue his sociological studies because he was granted RF project funding. More specifically, the case of Hanns Eisler shows that the RF was indeed quite willing to support the activities of outsiders among the refugees.⁶⁷ Thus, there is every reason to believe that Adorno lost his RF-funded salary because of his arrogant attitude towards the foundation officers and other New York locals. Allowances were surely made for neo-immigrants, but arrogance will have been the very last among them.

Three decades later, two of the people involved in these events were offered the opportunity to have the memories of their past encounters published in one and the same book. On the invitation of the two Harvard historians Donald Fleming and Bernard Bailyn, Adorno and PFL both contributed exclusive essays to *The Intellectual Migration: Europe to America 1930–1960*. Given that PFL's daughter was married to Bernard Bailyn, Adorno would surely not have been invited to contribute to the book if PFL had objected to it. Thus, PFL once again let Adorno enter the American scene where, in contrast to PFL himself in the late 1960s, he was a completely unknown entity. Adorno abundantly profited by the opportunity and without delay republished his contribution – which

he had written in German and sent to Harvard – in his new old homeland where the worldview he propagated was taken up much earlier and much more effectively. Thus, the approach to memory that prevailed in German-language sociology (and beyond) was determined by Adorno rather than by former Vienna resident PFL, who was much less known in Germany. Actually, PFL's text was translated into German one year before his death, but failed to attain a similar standing in the collective memory of German-speaking sociologists, let alone in the spheres of the semi-educated with respect to the history of science.⁶⁸

Besides the differences in the prominence and specific appeal of the two authors, there may have been another reason for their different reception, which may have to do with the texts themselves. PFL was bent on writing a thoroughly sociological text. When he started working on it, he wrote a letter to the co-editor of the planned book, his son-in-law Bernard Bailyn, telling him that he planned to write a '*biografie structurale*'. This choice of words not only superficially reflects the fact that the letter was sent from Paris, where PFL was a guest professor at the Sorbonne at the time, but probably also his proximity to Fernand Braudel with whom he had, with support from the Ford Foundation, set up the Maison des Sciences de l'Homme and whose writings he was well familiar with:

How to treat the personality of the biographee has a parallel in literary criticism. The Romantic School centred on his person. The 'new criticism' centers only on the work. But, for a sociologist, the biographee is a transmission channel, the characteristics of which have to enter in a characteristic way. (I think Adorno's piece is so dull because he just makes propaganda for his philosophical ideas, without relating them to anything.)

The second point is one to which I want specially to draw your attention. What difference does it make if a structural biography is written by the biographee himself? Take your work on Jefferson and the Enlightenment. Can you speculate on what you would have wanted to interview Jefferson about, in order to add his introspection? I will try to think about this problem further.⁶⁹

PFL's solution was to see himself as an 'expert witness'. This term has an unequivocal meaning in US law. An expert witness reports to a court of justice or a House investigations committee on his scientific findings rather than as a subjective witness. Thus, the text is organized along systematic rather than chronological lines and, according to its author, aims to describe how his European background had affected the way he had structured the two activities in the United States that he considered to be the most important ones: the founding of research institutions and the elaboration of a research style (Lazarsfeld 1969: 270). In doing so, PFL much more strictly abided by the editors' guidelines than his Frankfurt colleague did, given that Bailyn and Fleming's concern was 'to isolate the impact of this group [of émigrés] on American intellectual life' (Fleming and Bailyn 1969: 4). A few years later, on the occasion of the (first) reprint of his autobiographical text, PFL once more explained its underlying structure. As often as not, the social researcher was

facing the problem of how to come to a meaningful integration of a large number of individual data in a 'conceptual "formula"' that would allow him to connect with a 'more general imagery'. This was also true for one's own memories. In his case, these were organized around two such 'integrating formulas': 'anticipatory clues' and 'latent strategies' (Lazarsfeld 1972: xii–xiii).

Adorno's text, 'Scientific Experiences of a European Scholar in America', differs in more than one way from this. Adorno starts out by saying that he had been called upon to write this contribution, and that he considered this to be a licence for him to engage in an autobiographical narration since he had entered the scene not on his own initiative but because he was asked to do so. The text sticks to a chronological order and fails to name any sources that would allow the reader to proceed to his or her own verification of what is said. Bibliographical references are omitted even for Adorno's own writings. Reporting anecdotes is a key structural element of the text, with the author seeking to stress his role as a subjective witness by introducing the occasional English term or even sentence into the German text to prove that he had actually been there.⁷⁰ On the other hand, he is more than reluctant to provide any names, writing about more than one of his interlocutors, colleagues and contacts without naming them, and fishes for compliments from his readers by telling them that 'only the so-called secretarial assistants at once strongly responded to my suggestions. Even today, I gratefully remember the ladies Rose Kohn and Eunice Cooper.'⁷¹

Behavioural demands inherent to the new surroundings – before February 1938, Adorno had been to the United States only once, for a short time – are reported in the form of impersonal imperatives: 'Thanks to my affiliation with the Institute of Social Research I was not as strongly exposed to direct competition and external pressure as this would otherwise have been the case' (Adorno 1981: 302). The framework for Adorno's text is provided by the pressure to adapt, which he mentions at the beginning of the text as something he was immediately confronted with – "'adjustment" still was the magic word' – but felt himself 'not capable of ... by my nature as well as my background' (Adorno 1981: 299). At the end of the text, the reader is told that the testifying hero succeeded in resisting coercion and temptation and, in doing so, had even learned something.

In the late 1960s, PFL probably thought that his status in the history of social science was proof against any damage coming from Adorno, who had been back living in Frankfurt since 1950. Moreover, in the previous years, he had repeatedly shown his interest in sociological historiography and had contributed to it himself. Finally, as for cultivating his own reputation, he tended to a mild form of self-destruction which is manifest in the fact that he repeatedly granted his most violent critics more social space than it would seem advisable for someone interested in maintaining and enhancing his own fame. The 'atrophy of his political instincts' (Stehr and Lazarsfeld 1976: 788; Stehr 1982: 151) may well have affected his memory policies as well. He not only did refrain from vetoing Adorno's invitation to contribute to *Intellectual Migration* but,

soon after, included three texts by 'expert witnesses', as he once more called them, in a volume of selected articles by himself. Two of these 'witnesses' were loyal followers, but the third was the one person who during and after his stay in the United States had managed to cause near-worldwide sustained damage to PFL's reputation by creating another defamatory label. This was C. Wright Mills who, having been a collaborator of the Bureau of Applied Social Research in the 1940s, had caricatured PFL's line of research as 'abstracted empiricism' in *The Sociological Imagination* in 1959. PFL published this text in 1972 in *Qualitative Analysis*, declaring it to be 'a good antidote in a book where I might have overplayed the humanistic strain in my work' (Lazarsfeld 1972: xvi).⁷²

While Mills's accusation of 'abstracted empiricism' would have gained notoriety even without PFL's support, the same cannot be said for the label of 'administrative research', which had only been reanimated by Adorno's contribution to Bailyn and Fleming's anthology.⁷³ In this 1968 contribution to *Intellectual Migration*, Adorno's verdict on PFL was given the form it would have from then on – and it should be noted that in the meantime, Adorno had apparently forgotten the differentiation that was so important to him in October 1939, namely, the distinction between an exploitative and a benevolent version of 'administrative research'.

In the 1960s, selective memory, which always plays so great a part in commemorative writing, played tricks on both Adorno and PFL, since they both attributed the discontinuation of Adorno's work with the Radio Research Project to their methodological differences. These differences of course existed, but it is no less true that in 1940 Adorno would have been quite willing to dispense with his reservations had he been invited to continue his work there.

By way of corroboration, one could cite all the articles, manuscripts and reports that Adorno had either written himself, or had been a co-author of, or had overseen as a 'conductor' (Table 5.1). Significantly, those of Adorno's writings that were largely in line with the demands of the methodological paradigm of the PRRP have not been integrated in Adorno's *Gesammelte Schriften* (Collected Writings).

After Adorno's collaboration in the Radio Research Project had ended, two of his publications resulted in a surprising success for the project outcast. Winthrop Sargeant, who was working as an editor for *Time* and had obviously been sent an offprint of one of his reviews by Adorno himself, responded with a friendly letter and invited Adorno to further talks. The composer and musical critic Virgil Thomson used the publication of the essay 'On Popular Music' as an opportunity to publish excerpts from it in his column in the *New York Herald Tribune* and to recommend reading it to whoever was interested in music.⁷⁴

PFL, whom Adorno kept informed of these successes, profited by them to once more approach Marshall on the subject of granting Adorno individual support. In his letters to Marshall, he enclosed copies of the letters of approval that Adorno had received as well as a copy of the article from the

Table 5.1 Theodor W. Adorno's Participation in the Princeton Radio Research Project

Title (Year)	Collaboration, Context	Original Publication	Pages	Reprint
Plugging study (1939)	By T.W. Adorno and M. MacDougald	60 pages, unpublished research report B-0070 of PRRP and BASR respectively	60	
Cuthbert Daniel, three types of like reactions in judging popular songs (1940)	The experimental work reported in this study was done by Mr. Gerhart Wiebe with the assistance and supervision of Dr. T.W. Adorno	Journal of Applied Psychology: 746-8	2	
On popular music (1941)	With the assistance of George Simpson'	Studies in Philosophy and Social Science, 9: 17-48	31	
The radio symphony. An experiment in theory (1941)	The author wishes to express his indebtedness for editorial assistance to Josef Maier and George Simpson (note to title)	Lazarsfeld, Paul F. and Stanton, Frank (eds) (1941), <i>Radio Research 1941</i> , 110-39	29	
Analytic study of the NBC Music Appreciation Hour (1938-41)			52	<i>Musical Quarterly</i> (1994), 78 (2): 325-77
Review of Wilder Hobson, American Jazz Music and Winthrop Sargeant, Jazz Hot and Hybrid (1941)	With the assistance of Eunice Cooper	Studies in Philosophy and Social Science, 9: 167-78	11	<i>Gesammelte Schriften</i> (2003c), vol. 19, 375-99 (no remark on Eunice Cooper)
A social critique of radio music (1941)	With the assistance of George Simpson	<i>Kenyon Review</i> (1945) 11 (2): 208-17	9	
Review of W van de Wall, The Music of the People (1941)	Probably written for <i>Studies in Philosophy and Social Science</i> , not published because of closing of the periodical		2	<i>Gesammelte Schriften</i> (2003c), vol. 19, 373f.
Zum Rundfunkkonzert vom 22. Februar 1940			3	<i>Gesammelte Schriften</i> (1984), vol. 18, 577-80.
Zum Rundfunkkonzert vom 11. Juni 1940			2	<i>Gesammelte Schriften</i> (1984), vol. 18, 581-3.

New York Herald Tribune and a copy of another letter from the *Dictionary of the Arts* inviting Adorno to collaborate.⁷⁵ Thereupon, at the end of June 1941, Marshall and Adorno had a talk. Marshall repeated the reasons that, he felt, made it impossible for the RF to award Adorno a 'grant-in-aid'. Since PFL, however, had informed Marshall that what Adorno really needed was 'recognition and hearing of his work', he suggested that Adorno should try to publish his studies with one of the big publishing houses. In order to help him, Marshall called several music experts and collaborators of publishing houses he knew, all of whom however were unable to discuss the matter with Adorno before autumn. One-and-a-half years after his brusque refusal to give further aid to Adorno, Marshall seems to have softened a bit in his judgement of him. After his talk with Adorno, he wrote a memorandum specifying his current position:

The grounds given Adorno undoubtedly justify declining to consider further aid, but this talk left JM rather unhappy about that decision. He is convinced now that Adorno's work has substantial value ... Adorno clearly has something to say about the social position of music in this country ... He can earn his living if he undertakes other research which is within the program of the International Institute of Social Research at Columbia, to which he is attached. But these studies which he began with Lazarsfeld and which he is at present most keenly interested in do not come within the Institute's program. Hence he cannot complete them unless he finds some further support.

JM has suggested informally to Lazarsfeld that he discuss Adorno's work with Dollard of the Carnegie Corporation. If DHS [David H. Stevens, director of the RF Humanities Division] agrees, JM would like to leave the possibility open of considering a smaller grant-in-aid than Lazarsfeld asked (\$1,000 to \$1,500) toward Adorno's further work with Lazarsfeld, if funds for it cannot be found elsewhere.⁷⁶

This last attempt to secure an income for Adorno as a researcher in the field of radio music also failed. One of the reasons arguably was that it seems Adorno's interest in this kind of work had diminished considerably since he had seen the opportunity to boost his position in Horkheimer's circle. In April 1941, Horkheimer had relocated to California, at first allowing only his friend Friedrich Pollock and Herbert Marcuse to accompany him. Adorno was left to stay in the New York branch of the institute, a task easily imposed on him since at the time not all seemed lost for his further work with the Radio Research Project. But when Horkheimer's scheming resulted in Marcuse's capitulation and transfer to a job in the Office of War Information, the way was finally open for Adorno. The perspective of being able to collaborate on a book on dialectical logic that Horkheimer had long been planning obviously had a stronger appeal for Adorno than staying in New York to grapple with people he clearly felt to be intellectually second rate. In November 1941, Adorno moved to California, thus putting an end to his collaboration at the Radio Research Project. There, his next encounter with empirical social research was only a few years away (see Chapter Six).

TRANSFER TO COLUMBIA UNIVERSITY

In early 1940, it became evident that the RF would go on funding the Radio Research Project. Negotiations between Marshall and the president of Princeton had resulted in the project being set free, so to speak, by Princeton. Robert Lynd once more acted in the role of a mentor in favour of PFL. He succeeded in convincing the authorities at his university that Columbia would profit from taking in the PRRP, now called the Office of Radio Research (hereafter ORR for short), and giving it the former premises of the Medical School. On top of it, he obtained a lectureship for PFL in the Department of Sociology.

Knowing for sure that he would be the director of a well-endowed research project for another three years, PFL considered reorganizing his research agenda. But this reorientation was also motivated and made possible by the clear downfall of the Music Study. To put it bluntly, with Adorno gone, the way was open for pursuing the development and sophistication of the panel method. As for matters of content, recalibration was facilitated by the fact that a new supervisory board had been set up where Frank Stanton was the only member to represent the radio stations while all the others were professors from Columbia University, with no psychologist among them. Thus, the row with Cantril had also had an impact on the content of future work. There was no need any more to consider using experimental designs, never a favourite with PFL. On the PRRP's last day in February 1940, PFL wrote a detailed letter to Marshall, using the research plans formulated in November as an occasion for self-evaluation.⁷⁷

The promise he made in this letter, that all the studies that had not been terminated in the Princeton phase of the Radio Research Project would be completed at Columbia, was, however, never fulfilled. This was due to three developments: a new research assignment that turned out to be ideal for linking up several of the research interests pursued by PFL during the previous years; his appointment to the Department of Sociology at Columbia, which enabled him to shake off his dependency on RF financial support; and his acquaintance with Robert K. Merton, which helped him to embed his own work in a more concise theoretical paradigm.

The research project that was to give a new turn to PFL's work originated in the very part of the Radio Research Project that PFL, in the above-mentioned letter to Marshall, had described as being terminated. PFL had succeeded in finding other sponsors to supplement the funds he could dispose of from the RF sub-project. The new sponsors, however, had demands of their own on the researchers. The first of these new sources was the United States Department of Agriculture, which wanted them to explore the possibilities of using the radio for propagating its agricultural policies. As a consequence, the field selected for this research was a rural area: Erie County in Ohio. In later years, PFL himself was unable to recall why he had transformed a study of the use of the radio into a study of voters' choice (Lazarsfeld 1969: 330). The presidential elections scheduled for the autumn of 1940 probably motivated him to take up again his early Newark attempts of 1937 to use election campaigns to study

how voters made up their minds and what influence the radio might have on this. Anyway, he started to look for sponsors. He found one of them by selling the rights to the first publication of the planned study to *Life* magazine. This also implied that he had to come up with results before the autumn of 1940. Whatever he might say about voting preferences before or after the elections would also be a taking of sides in the inveterate struggle over election forecasts. During the election of 1936, George Gallup had conducted a study on a small but well-drawn random sample and succeeded in routing the *Literary Digest* (the previous champion in the field of election forecasts) and its so-called 'straw-polls' because the readers of the latter journal, who had been called upon to send back their ballot papers, had not been representative of the electorate (Keller 2001).

But the funds PFL could rake in were far from sufficient for the panel studies he planned, and so he transformed a practice that had proven to be successful during the first months of the PRRP into a new means of funding empirical research. At the time, he and his collaborators had managed to persuade a number of commercial market researchers to include one or another question that was of interest to the academic radio researchers in their questionnaires. So why not resort to this practice to aid his own projects? PFL found a producer of radio sets, and another enterprise that wanted to sell refrigerators, and proposed to do market research for them.⁷⁸ This was the invention of the omnibus survey which since has become the common technique, racking the nerves of even the most willing respondents with its reiteration of the phrase 'and now for something completely different', and which arguably worked much better in the beginning than it did later. Thanks to this invention, PFL was able to raise the handsome sum of \$100,000 (\$1.5 million in 2010), much more than the PRRP had disposed of for the entire first two-and-a-half years of its existence.

Erie County had a population of about 40,000, half of them living in urban and rural agglomerations, respectively. The industry consisted of various firms, and the county was interesting mainly because, since 1900, elections there had always rather faithfully mirrored overall results. A random sample of 3,300 inhabitants was questioned on socio-demographic traits, radio listening and reading habits, and political attitudes. From this sample, ORR collaborators selected sub-samples of 600 respondents each. Between May and November 1940 these sub-samples were canvassed (by Elmo Roper's firm) in seven waves of interviews addressing their opinion about the coming elections. In addition, three same-size control groups were included in the study. Its results were published in 1944 in a book entitled *The People's Choice: How the Voter Makes up His Mind in a Presidential Campaign*, which attained the status of a classic of election research as soon as it was published. The belated publication date was, at least in part, deliberately agreed upon in the hope that a book on voters' choice would draw more public attention if published on the eve of the next election.⁷⁹

While previous surveys had sought to detect the roots of decisions that had been reached in the past, and surveys conducted with radio listeners had asked for preferences more or less at the time of the survey, this kind of survey investigated decisions pertaining to an event that was sure to come in the near future and obliged voters to come to a choice – or a non-choice in case they decided to abstain from the vote. On their way to this choice, opinion building could be studied, as it were, as an ongoing process. Moreover, the date up to which this process had to come to a conclusion was sure to set a limit to the activities of the social researchers, as well, since they, too, had to be content to collect data during the election campaign. At any rate, this type of research precluded extending data collection into an open future (or going into ever more details in reconstructing the past history of a decision). After the day of the election, all the researchers could do was to analyse the data since conducting additional surveys would no longer make sense.

In May 1941, PFL was made an associate professor at Columbia University.⁸⁰ The appointment was preceded by a heated controversy between Robert MacIver and Robert Lynd, accounts of which have not been entirely mutually corroborative.⁸¹ There is agreement on the fact that there had long been a vacancy at the Department of Sociology: Robert E. Chaddock had voluntarily ended his own life and the committee that had to decide on his successor could not come to an agreement. Thereupon, the university's patriarchal president, Nicholas M. Butler, appointed another committee that did not include any Columbia professors. These external advisors, while equally unable to come to a unanimous recommendation, at least suggested that Chaddock's salary of \$8,000 (\$115,000 in 2015) be split up between two younger candidates. This suggestion enabled the opposing sides in the Sociology Department to each propose the candidate that suited them, but also required them to agree to the other's choice. Agreement was quickly reached on MacIver's candidate, Robert K. Merton, but not so for the other half. Williard Waller, who taught at Barnard College, the part of Columbia still at the time reserved for women, was the favourite for a certain time. Then Lynd brought up Louis Wirth and Samuel Stouffer, although nobody expected that either of them would accept the offer. Finally, PFL's name came up as a potential candidate. Theodore Abel, who was an associate professor in the department, took it upon himself to present PFL to MacIver by arranging evening games of bridge during which the Scotsman could closely examine the Central European (Abel 2001: 305–6). PFL passed the test, and MacIver agreed to his being appointed.⁸² Since it had previously been agreed that the older of the two candidates would be given the position of associate professor and the younger that of assistant professor, the remarkable situation presented itself in which PFL, who as everybody knew had held no regular university position in either Vienna or Newark, skipped the first rung of the academic career ladder. His initial salary as a professor was \$5,500 (\$79,000 in 2010), plus \$2,500 (\$36,000 in 2010) from ORR earnings.⁸³ Still, PFL's skipping of a career step was less unusual than the formal downgrading

of Merton, who abandoned his position as a full professor as well as his function as a chairman at Tulane University to become an 'assistant' in New York.

It is part of the myth about the appointments of Merton and PFL that both opposing camps of the department hoped that a personal conflict, passed off as a rivalry between theorist and empiricist, would continue. But things turned out differently. When the two newcomers, who had previously neither met nor known each other's work, were informed that they would be future colleagues in the same department, they exchanged polite letters and recent publications (Merton 1998: 166–7). PFL, who was nine years older, felt called upon to invite the younger one and his wife for dinner soon after they had taken up their duties. On their arrival at the Lazarsfelds' house, their host gave them an unusual welcome: 'What a pleasure to have you here. But keep on your coat, dear Merton. I have got a sociological surprise for you. We will have to let the ladies dine alone here and will try to be back as fast as possible' (Lazarsfeld 1981: 338).

PFL had received a telephone call that very afternoon that required him to conduct a listeners' survey on a new radio programme. This programme was broadcast by the Office of Facts and Figures, the predecessor of the Office of War Information, as part of its moral rearmament effort directed at the population in view of the impending American involvement in the Second World War. The two men took a taxi to the radio studio where a group of listeners was assembled who, after the programme 'This is War' had been broadcast, were interviewed on their 'likes and dislikes' as recorded by the Program Analyzer. Merton, taken by surprise, had initially been sitting nearby, but after a while had started to slip little notes with suggestions for improving the interview to PFL, who was conducting the first group interview. Whether Merton's rhapsodic experience as an interviewer (Merton 1998: 202, n. 13) was sufficient for him to suggest improvements or whether it was his self-assurance that prompted him to do so cannot be decided here. At any rate, when the second round of listeners was invited to come in, PFL asked Merton if he wanted to conduct the interview himself. Merton did so to the satisfaction of the director of the ORR, and when they were finished, PFL decided that they should further discuss the matter. He phoned the waiting wives at home and asked for further leave. The two men went to the Russian Tea Room where, in the following hours over caviar and champagne, they came to know each other better. That very night, PFL invited Merton to join him in completing the report.

With the report due in a week or so, the weekend and the days following found us hard at work on our first, and impromptu, collaboration. The man who had come to dinner stayed on for almost a third of a century, first at the ORR (Office of Radio Research) and then, at its successor, the BASR (Bureau of Applied Social Research). (Merton 1998: 168)

This episode was the birth of the focused interview, which was improved by Merton during the following years and later said to be the model for the

technique of using focus groups. In contrast to the way participants' preferences were measured by the Program Analyzer, the most momentous innovation of the focused interview was that a random, or even natural, group of participants could be confronted with a specific stimulus of the so-called 'particular situation'. Subsequently, participants were questioned in individual interviews that built on the prior analysis of the material – which could be a film, a radio broadcast, a text to be read, an experiment, or participation in 'a political rally, a ritual or a riot' (Merton *et al.* 1990: 3). In the course of this 'content or situational analysis', hypotheses concerning the conditions and the consequences of the stimulus for the participants would have to be formulated and, then, used as a guideline for the subsequent interview. The interview itself was centred on the subjective experience of the initial situation so that the researches could work out, based on the Thomas theorem,⁸⁴ how the situation was being defined. At first, the new method primarily served to find valid interpretations of statistical correlations that had already been established. Later, the emphasis was on the non-directive nature of the interview because this was the only way to obtain 'unanticipated responses'⁸⁵ from the participants. The compendium later written by Merton, with Marjorie Fiske and Patricia Kendall (Merton *et al.* 1990), is a model piece of evidence for the successful mutual stimulation between theorist Merton and empiricist PFL.

Within less than two years, the situation for PFL and the Radio Research Project had changed as fundamentally as possible, and for the best. With the panel method, PFL had found a new way of investigating decision-making that allowed him to overcome the limitations of the Program Analyzer, where only isolated 'likes and dislikes' could be recorded. Social change could now be investigated with empirical accompaniment, so to speak. Given the security of his position as a professor at Columbia, PFL could now leave behind the well-founded feelings of insecurity that had marked his initial years as an immigrant – even though it remained one of his inveterate habits to recall his status insecurity to others⁸⁶ – and with cooperation soon turning into friendship with Merton he had found someone who knew how to reign in his intellectual flights and could put up with his propensity to improvisation in organizational matters.

But for PFL, entering the new world of a university professor was not only bound up with the security he had longed for but also with the challenge to live up to different normative expectations. His old mentor-cum-friend Lynd, while acting as the chairman of the ORR advisors' board, refused to get involved with daily routines and, unlike Cantril, was much less given to treating PFL as his inferior. What he did want, however, was to make sure that his protégé became acclimatized to the different mores of academic research – an undertaking in which he was supported by John Marshall who, as before, was in charge of the Radio Research Project at the RF Humanities Division.⁸⁷ They both felt that the ORR, with PFL in a regular university position, could be expected to operate with a reduced global budget and that PFL himself, given his secure position, should be content with a reduced salary. Primarily though,

they agreed that the activities of the ORR needed to be reoriented in a more general way. PFL's consulting activities for private enterprise, which had lately seemed to get a bit out of hand, were to be reduced and restricted to such tasks as had a topical link to the research programme. PFL needed to be confronted with a clear alternative:

In short, it [is] necessary for Lazarsfeld to decide which of the two alternatives confronting him he is to take. If he chooses the academic course, he must do so with a clear recognition of the fact that he is giving up the other alternative where, with the present interest in polling and marketing research, he could undoubtedly earn a considerably larger salary. Lazarsfeld has always said he preferred academic research, but he must now decide.⁸⁸

Knowing what we do about PFL so far, it comes as no surprise that he did not comply with the recommended restrictions on his entrepreneurial ambitions. In doing so, however, his aim was not to maximize his personal income but to keep his research institute set on an expansionist course since he was always full of ideas he thought worth investigating. Support came, as it were, from world politics. After the start of the war, the United States was in a hurry to establish a large network of organizations that engaged in intelligence activities and collected data on the morale of the enemy as well as their own fellow citizens, and soon also began to plan for postwar times. Social scientists were ranking high in this, and soon universities and research-only institutions began to suffer from a lack of personnel since they were unable to keep up with government salaries. Thus, ORR collaborators, if they did not go to Washington, DC in the first place, also multiplied their consulting activities. The ORR became a provider of social science human capital for the war effort.

In the summer of 1943, PFL asked the RF for permission to suspend the operations of the Radio Research Project until the end of the war, since he lacked the staff and time to continue radio research. He informed Marshall that if the RF chose to freeze support until after the end of the war, when redundancies were to be expected among those who now worked for the government but would then no longer be needed, and to resume it when those who returned would need to be paid, this would be all right by him.

ATTEMPTING AN EVALUATION

PFL's trajectory and eventual establishment between 1937 and 1941, as described on the preceding pages, was accompanied by the emergence of a number of products that now need to be reviewed in a more systematic fashion. When speaking of 'products' in the sciences, publications are, of course, what first come to mind. Other achievements that could legitimately be seen as products might be patents and the like in the case of technicians, and, in the case of social scientists, methodological and other research innovations that could be evaluated on the same level as things written. A third dimension that is well worth considering is the successful formation of new generations of

academics. The difficulty in evaluating all these products is that there is little agreement on the measurement tools, indices and benchmarking procedures to be employed. Furthermore, there are hardly any such evaluations of past social science achievements so far, which means that there is no way to even begin with a comparative evaluation. It is obvious that the only competitor in a one-man race cannot lose, but judgement on the success or failure of a single actor can be formed if his initial goals are known. Where these have been handed down to us – and for the PRRP this was indeed the case, given PFL's self-evaluation for the RF – the problem is probably only transferred to another level, i.e. having to judge whether the goals in question were realistic. Where programmes never existed or failed to be preserved, one has to rely on a more or less intuitive evaluation of what was actually achieved. This should be kept in mind in the following. Wherever it is possible, and necessary, the question of evaluation standards will be reconsidered.

In the case of the PRRP and its successor organizations, the existing data are indeed sufficient to allow for a rough analysis and evaluation. The following account is based on three sources: the reports and, more particularly, the financial accounts that were submitted to the RF for the first phase of the Radio Research Project, allowing for a number of simple quantitative analyses of these two-and-a-half years; an internal RF evaluation made after four years of project operations, providing material for qualitative considerations; and Judith S. Barton's bibliographical Bureau of Applied Social Research (BASR) documentation (Barton 1977). In this bibliography – as well as in all the other reports on the BASR – the founding date is given as 1937 even though the PRRP/ORR did not take on this name until 1944. Whatever it was that motivated this backward extension of the BASR lifespan – Converse (Converse 1987: 149–50) contrasts PFL's approach with that of Cantril, whose autobiography is centred on his foundation years of the later Princeton Office of Public Opinion Research and ignores the years 1937 to 1940 – it is thanks to this bibliography, with its record of both published and unpublished (hectographed) works, that an analysis of the written output of the PRRP is possible at all. As a supplement to these sources, the analysis will also include the three journal issues devoted to the work of the Radio Research Project and the three monographs and two omnibus volumes published between 1937 and 1945, which suggest a number of additions and corrections to the official bibliography. The period covered is somewhat longer than the duration of the projects that were financed by the RF, because some works were only completed or published later. For an analysis of the staff structure I will limit myself to the first phase of the project, i.e. the years from 1937 to the end of 1941, the period that ended with the Office of Radio Research clearly transferred to Columbia University.

Marshall's final report⁸⁹ of September 1941, which was submitted to the RF officers and the members of the 'board of trustees', lists the 'substantial accomplishments' of the Radio Research Project: its research, based on the idea that the radio was a public service, had met with approval. In its initial stage,

the radio listener had only been seen as a future buyer of the products that were advertised on the radio. At that time, the idea that the radio listener might turn on his set in order to be informed or to seek cultural satisfaction was little if at all understood. This new conception of the listener was due to the PRRP and manifested itself also in the fact that the radio and advertising industry, initially rather reserved towards the project, was now consulting it for advice on their own initiative. Here, of course, the Program Analyzer had to be referred to since, according to Marshall, it gave the industry an idea of how listeners would respond to a new programme even before it was broadcast.

Marshall's report states that during the first two years 'some ninety studies' had been conducted. Given the unexpectedly large number of single findings which would not have been possible without secondary analysis, it had seemed plausible to analyse the large data set in more detail and to use the half-year extension and first months after the transfer to Columbia to do so. Marshall roughly classified the studies of the Princeton phase as follows:

- 1) study of the characteristics of radio stations and programs on which listener interest seems to depend; 2) studies of the characteristics of listeners attracted by programs of different types; and more specific studies of; 3) news broadcasting, including its relation to news and print; 4) of broadcast music; 5) of the varied stimuli radio offers listeners, for example, in rural areas, or during political campaign.

Half of the studies had been of a methodological nature. In general, the studies had shown that techniques of social psychology were meaningful and reliable in view of obtaining information on the value attributed to the radio by listeners. One of the most remarkable results was the development of the panel method. A series of individual interviews conducted with the same group of persons over time had allowed the social scientists to study change in listeners' attitudes, conduct and way of taking in information.

Besides the methodological studies there had been studies on the following four substantial fields: '1) Educational radio programs; 2) Reading and listening; 3) Radio news and commentators; and 4) Music and radio.' Thus the four fields initially designated by PFL as promising in terms of substantial results – 'music, book-reading, news, and politics' – resurface in the final evaluation, but in an inverted order. The study on music on the radio, the initial number one, was now relegated to the last place, and politics had mutated into educational programmes, to which Marshall had always given more importance and therefore were now promoted.

In the areas of educational radio programs ... the project made a study of listener's reactions to educational broadcasts. The following are general conclusions formulated from the data collected by this study: 1) educational broadcasting is more effective when done on a regional, than a national basis; 2) educational radio succeeds best when embedded in an institutional or psychological set-up; and 3) people will come to like good programs if they are exposed to them long enough.

Reports on the studies of reading behaviour can be found in the book *Radio and the Printed Page*. A summary of its findings was published in the *Trustees' Bulletin* of the RF. Marshall's report concludes by giving a list of PRRP publications that had appeared so far.

Reading Marshall's final report, one must of course keep in mind that these evaluations invariably were also self-evaluations, since the foundation officer had to convince the members of the board who decided how to invest the Foundation's money that he had successfully fulfilled his own role as an intermediary. Later analyses, which can dispense with the consideration that a person in Marshall's position had to stand in his own best interest, might well come to different conclusions. Given that the publication of research findings may have been delayed due to factors over which the researchers had no control, it stands to reason that the period chosen for our evaluation should exceed the formal termination of project support. Therefore, the following account will include all PRRP, ORR and BASR publications that appeared between 1937 and 1945, with the caution that there is no way of telling which of these publications were actually produced in the context of the Radio Research Project and which of them were either funded from other sources or conducted for other clients.⁹⁰

It can be reasonably assumed that at least in the second half of the period covered, the number of ORR, then BASR, collaborators who did not work for the Radio Research Project increased considerably. At any rate, in September 1942, PFL is obviously proud to report the achievements of the first year to his new supervisory board, to the secretary of Columbia University, and to John Marshall:

September 4, 1942

To Members of the Advisory Council (Copies to John Marshall and Philip Hayden)

From: PFL

RE: Financial Operation of the Office of Radio Research during the past financial year

...

First year of our present three-year grant

RF grant \$ 25,000 and the income from the Consulting Division was \$ 25,581.09

We thus were able to double our grant this year. It should be perfectly clear, however, that this rather high consulting income is really contingent on the existence of the Rockefeller grant. It enables us to develop procedures and ideas which then become 'marketable.'⁹¹

The names of the clients of the Consulting Division are not mentioned in the bibliography. It stands to reason that a certain number of publications were produced for these third parties.

According to Barton's bibliography, 112 articles were published in these 9 years, with a total of more than 100 different author's names (and about 100 hectographed reports). For half of these articles, of course including the three

Table 5.2 Authors and No. of Articles, Mimeographed Manuscripts and Chapters in Books, including Co-authorship and Pages Written between 1937 and 1945

Author	No. of articles	Pages
Lazarsfeld, Paul F.	55	1247
Herzog, Herta	19	652
Fiske, Marjorie	17	455
Merton, Robert K.	10	318
Berelson, Bernard	9	271
Suchman, Edward	9	168
Green, Janet (Jeanette)	7	187
Gaudet, Hazel	7	183
Curtis, Alberta	6	157
Bayne, Martha Collins	5	122
Watson, Goodwin	4	268
Robinson, William	4	94
Franzen, Ray(mond)	4	47
Kendall, Patricia	4	44
Daniel, Cuthbert	4	36
Sayre (Smith), Jeanette	3	126
Dichter, Ernest	3	102
Adorno, Theodor W.	3	90
Peterman, Jack	3	65
Wiebe, Gustav D.	3	45
Stanton, Frank	3	32

‘Elias Smith’ articles (Lazarsfeld’s pseudonym, as previously explained), PFL was either the author or co-author. All in all, article output totalled 1,169 printed pages, 416 (or 36 per cent) of them being authored or co-authored by PFL. In addition, there were the three monographs published during this period: *Radio and the Printed Page* (Lazarsfeld 1940), *The Invasion from Mars* (Cantril *et al.* 1940), and *The People’s Choice* (Lazarsfeld *et al.* 1944). These comprise 1,427 pages, 357 (or 25 per cent) of them by PFL, if one discounts the contributions signed Stouffer and Herzog in *Radio and the Printed Page* and attributes one-third of the authorship of *The People’s Choice* to PFL (but none for *The Invasion from Mars*).

Among the other 70 authors who produced articles or contributions to omnibus volumes are Herzog (with 19 contributions totalling 652 pages), Fiske (17 contributions for 455 pages), Merton (10 contributions for 318 pages) and Bernard Berelson (9 contributions totalling 271 pages) (see Table 5.2 for further details). The 35 collaborators whose non-published manuscripts are listed in the BASR bibliography include, again in order following PFL (with 472 pages in 14 texts), Herzog (457 pages in 13 texts), Fiske (423 pages in 11 texts),

Table 5.3 Ranking of Authors, Total Pages and Periods (1937–41 and 1942–5)

1937–41	Pages 1937–41	1942–45	Pages 1942–45	1937–45	Pages 1937–45
Lazarsfeld, Paul F.	750,0	Lazarsfeld, Paul F.	496,5	Lazarsfeld, Paul F.	1247
Herzog, Herta	252,5	Fiske, Marjorie	424,5	Herzog, Herta	652
Suchman, Edward	133,0	Herzog, Herta	399,5	Fiske, Marjorie	455
Gaudet, Hazel	124,0	Merton, Robert K.	317,5	Merton, Robert K.	318
Bayne, Martha	121,5	Berelson, Bernard	271,0	Berelson, Bernard	271
Collins					
Robinson, William	93,5	Watson, Goodwin	268,0	Watson, Goodwin	268
Adorno, Theodor W.	90,0	Green, Janet	161,0	Green, Janet	187
		(Jeanette)		(Jeanette)	
Cantril, Hadley	80,5	Schneider, Helen	113,5	Gaudet, Hazel	183
Curtis, Alberta	75,0	Sayre (Smith), Jeanette	110,0	Suchman, Edward	168
Arnheim, Rudolf	71,5	Curtis, Alberta	81,5	Curtis, Alberta	157

Goodwin Watson (268 pages in 4 texts), Merton (223 pages in 3 texts) and Bernard Berelson (192 pages in 6 texts) (see Table 5.3 for further details).

Thus, there can be no doubt about PFL's outstanding role for the volume as well as the intellectual content of the publications of the Radio Research Project and of the other studies that were terminated under his guidance in those years. He was not only the director, but also the author who outshined all the others in terms of scientific findings. If one is to believe the account he gives of himself, his role went even farther. His internal correspondence with his co-directors in the early days of the project, as well as his memoir, suggest that he also had to rewrite the texts of others.⁹²

While in the first years, the bulk of the publishing contributions after PFL's were provided by his wife Herta Herzog and a number of junior collaborators, the second half was marked by Merton in fourth place while Adorno and other collaborators contributed distinctly less. It therefore does not seem unfounded to say that PFL's expectations concerning Adorno and Merton were basically the same, namely to find a partner who was productive and full of ideas and who could bear part of the burden of producing an immense number of research reports and articles. While Adorno was perfectly capable of fulfilling the writer's role expected of him, he failed to establish himself as PFL's partner in theoretical matters due to his social incompetence which, in retrospective, he glorified by framing himself as an outsider. The success of the Lazarsfeld-Merton duo, however, was something that others soon tried to emulate and in later years the two Columbia professors were themselves actively involved in an attempt to clone their model of cooperation: when Stouffer, PFL's long-term partner in statistical matters, was appointed to a chair at Harvard, Merton took to commuting between New York and Cambridge

for one semester with the aim of launching cooperation, based on the New York model, between Stouffer and Merton's teacher and friend, Talcott Parsons. (The attempt failed even faster than PFL's effort to find a basis for cooperation with Adorno.⁹³ Intellectual cooperation obviously is a delicate matter.)

In the present chapter, reference was repeatedly made to the methodological innovations achieved in the years of the Radio Research Project that were financed by the RF. Some of these, which were adopted by others – 'inventions' that saw successful diffusion in the social sciences – can truly be described as outstanding. This is true beyond doubt for the panel design and for the focused interview, even if the former turned out to be a technique that attained textbook honours but actually was less and less often used (let alone in the excessive manner that characterized its early use) because the funds it required ceased to flow as abundantly in later years. In contrast, by the end of the twentieth century, the focused interview (in its vulgarized form as the 'focus group interview') had become a serious rival of the personal interview, long assumed as the ideal way to do social science research. The 'focused interview' is singled out with a name of its own due to Merton's predilection for semantics, but one also must not overlook the various improvements of interview techniques, among them notably the detailed interview, all further milestones of the Radio Research Project.

While these are the 'mega-hits', as one is tempted to say, the other innovations that date back to the Radio Research Project should not be underestimated, such as secondary analysis and improvements in the formation of indices as well as some minor achievements in methodology. Among these, the analysis of divergent cases is worth mentioning even though it has, quite unjustly, been rarely emulated. Insiders and BASR worshippers are more likely to give priority to the Program Analyzer.

The third aspect that remains to be examined is the output of the Radio Research Project in terms of training for social science research. As one may recall, PFL's primary concern in his initial negotiations with Cantril was to make sure that he could maintain his Newark Center as a training facility. During all the phases of the work supported by the RF, this concern remained very important to PFL. He succeeded in convincing the RF officers to award grants to American students and graduates to have them participate in the PRRP. The project's appeal for junior scientists was such that, for instance, Bernard Berelson saw to it that the grant he had been awarded, in view of his cooperation with Ernst Kris and Hans Speier on studies on propaganda at the New School, was transferred to Columbia. About half of the almost fifty collaborators whose names appear in the PRRP accounts (because they received individual payments of more than \$100) have no publications to their name for the period covered. Some of these paid collaborators had evidently not been hired for this kind of work – they were charged with computations, copy-editing or typing assignments. Those who were hired for doing research work that included the production of pages obtained very different incomes from the activity. A calculation of the 'fee' per page for the initial years of the PRRP, however, shows that wage levels were not correlated with either status

Table 5.4 Salaries and Productivity

Collaborator	\$ received from PRRP	\$ received per page
Wagner, Isabella	1,200	120.00
Sayre (Smith), Jeanette	1,679	104.94
Stanton, Frank	3,154	98.56
Fiske, Marjorie	2,333	77.77
Wiebe, Gus D.	580	58.00
Ollry, Francis	221	44.20
Cantril, Hadley	3,400	42.24
Adorno, Theodor W.	3,800	42.22
Stouffer, Samuel	899	33.92
Gaudet, Hazel	3,555	28.67
Lazarsfeld, Paul F.	18,750	25.00
Fleiss, Majorie	493	21.43
Suchman, Edward	2,817	21.18
Curtis, Alberta	1,082	14.43
Peterman, Jack	722	11.11
Daniel, Cuthbert	375	10.56
McCandless, Boyd	50	9.09
Robinson, William	750	8.02
Beville Jr, H.M.	50	2.94
Green, Janet (Jeanette)	65	2.50
Herzog, Herta	350	1.39

or personal proximity to the project supervisor; on the contrary, PFL's wife was plainly exploited while Adorno's salary more or less equalled that of Cantril (Table 5.4). But even those who had been hired for administrative tasks fell in love with social science research. PFL's long-time secretary, Rose Kohn, decided to give up her job to major in sociology and was so brilliant a graduate that she was immediately – and as the first woman ever – appointed to a position in the Department of Sociology at Cornell University where, under her married name Rose Golden, she was active for many years.

PFL had initially succeeded in placing former collaborators with radio stations, but after the outbreak of war, this also changed. More and more collaborators left the PRRP to work in one of the new government agencies. As an unwelcome side-effect of this migration, PFL found it ever more difficult to recruit collaborators at all. Government salaries were much higher, and more than once a potential collaborator chose the more remunerative government position.

On examining the list of collaborators of the Radio Research Project, two things stand out: the uncommonly high proportion of female collaborators, and the fact that while most of the collaborators produced no, or very few, publications during their time with the project or immediately after it, a considerable

proportion of them made a name for themselves with publications in later years.

There certainly is something coquettish about the way PFL, in an interview shortly before his death, claimed that his achievements in the social sciences consisted primarily in the foundation and establishment of the BASR (Stehr and Lazarsfeld 1976), but certainly there is no doubt that he was very successful in this field.

After the end of the war, PFL did not resume his collaboration with the RF Humanities Division. The last document to be found in the Rockefeller Foundation files on radio research is a paper entitled 'Columbia University's Bureau of Applied Social Research. Its Objectives and Purposes', one half of it a condensed statement of accounts, the other half a plan for future activities. What ranks first in the memorandum is the necessity of providing training in research techniques for students. In contrast to statistics classes and book-based research, the teaching of social research competences should be about coming to know the procedures used for collecting and analysing data: 'If the training of the student is to be fully rounded, he must receive parallel schooling in sociological findings and in the specific techniques for the methodological analysis and appraisal of researches.'⁹⁴ In the following two decades the BASR, co-directed by PFL and Merton, played a major part in the realization of this principle, with dozens of scholars who received their primary formation in this setting.

The detailed reconstruction of the workings of the Princeton Radio Research Project and its successors shows that European social scientists who emigrated to the United States due to the handing-over of power to the Nazis in Germany and Austria encountered an institutional environment that fundamentally differed from its Central European counterpart. Both the funding modalities and the work style were very different from European ways. In the United States, relatively flat hierarchies combined with the welfare-state work-creation schemes of the New Deal to offer promising openings for those who were capable of taking the opportunity, and willing to adapt to the new environment. In addition, there was a readiness among the larger public to give support to the new empirical social sciences, and to take their results seriously. Whoever seized the chance that was offered him, as PFL had done, could succeed rather quickly and could provide younger and more marginal new immigrants with jobs. And due to the openness of the social science field, new procedures for collecting and analysing sociological data could be created.

The following chapter will be dedicated to another case of European migrants trying to obtain funding for, and make advances in, their research in the United States.