

**ARTICLE**

Else Frenkel-Brunswik and Contemporary Sociologists

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Abstract

The article presents and discusses an unpublished critical remark written by Robert K Merton which addressed Else Frenkel Brunswik's contribution to *The Authoritarian Personality*. The author contextualizes both Merton's remarks and the book's reception by other contemporary sociologists.

Keywords

Else Frenkel-Brunswik, Robert K. Merton, *The Authoritarian Personality*

INTRODUCTION

Else Frenkel-Brunswik's name (in the following abbreviated to EFB) is seldom mentioned in sociological texts, partly because her name is hidden in the "et al." behind T. W. Adorno, whose *The Authoritarian Personality* (TAP) (Adorno et al. 1950) has found some resonance in sociology, past and present. EFB's non-existence in sociology is rooted in the closed-minded attitude of mainstream sociology towards social psychology, which was abandoned after a short interlude in the 1950s when attempts to establish an interdisciplinary field between psychology and sociology finally collapsed. However, it is worth noting that EFB's education and academic career took place in departments of psychology. Given this circumstance, it comes as a surprise to find some sociologists showing up in EFB's life as discussion partners, peers, and advisors. The following paper proceeds as follows: I start by looking at EFB's years in Vienna and her relationships and networks there; then I switch to the Studies in Prejudice project and its advisory board; finally, I describe and discuss a memo written by Robert K. Merton (abbreviated to RKM) at the time when EFB was working on her part of TAP. In his comments, RKM put forward some suggestions and criticism vis-à-vis EFB's draft. His major concern was related to the differences between psychology and sociology with regard to the standards of explanation.

EFB'S VIENNA YEARS

Else Frenkel was born in 1908 in Lemberg, Galicia (today: Lviv, Ukraine), and lived in Vienna from 1914 until 1938. She attended high school at the private reform school of Eugenie Schwarzwald—and began her further education in the autumn of 1926 when she entered the University of Vienna. Her

social background could be described as upper middle-class and Jewish, where her Jewishness was not only through descent, but also through religious observance, at least as far as her parents were concerned. After a short period of orientation, during which she attended courses in mathematics and physics, she found her calling in psychology. Since at that time psychology was taught by professors holding professorships in philosophy, EFB learned much more philosophy in comparison to the present-day psychology curricula. Karl Bühler and his wife Charlotte directed the Institute of Psychology as a separate entity within the Department of Philosophy, where Karl Bühler was officially professor of philosophy and Charlotte a lecturer, or *Privatdozent*, with only the title but not the position of professor. However, Charlotte was the recipient of a grant from the Rockefeller Foundation, which enabled her to hire research assistants for her work in developmental psychology. EFB's dissertation topic was closer to Karl Bühler and his only paid assistant, Egon (von) Brunswik, who specialized in the psychology of perception; EFB's PhD thesis belonged to this specialty. Under the direction of Charlotte Bühler, EFB switched to life-cycle and biographical psychology. Whereas only part of her PhD thesis had been published as *Atomismus und Mechanismus in der Assoziationspsychologie* [Atomism and Mechanism in Association Psychology] (Frenkel 1931), in the eight years following her graduation in 1930 EFB published two conference presentations, one handbook entry about the psychology of fairy tales, one article in an English language journal (*Character and Personality*) and, in 1937, the first volume in a new series *Psychologische Forschungen über den Lebenslauf* [Psychological Research on the Life-Cycle] written together with Edith Weisskopf and edited together with Charlotte Bühler (see list of her writings in Frenkel-Brunswik 1996: 314-6).

During her Viennese years, EFB was influenced by three thought collectives: besides the academic psychology of the Böhlers, she received psychoanalytic training from Ernst Kris and intellectual stimuli from the circle of philosophers around Moritz Schlick, Rudolf Carnap, and Otto Neurath. In contrast to her peers, EFB seems not to have developed any political interests or activities. One of her fellow students remembered that if Else did not appear at the institute every day, everyone was concerned about her health (Marie Jahoda, quoted in Paier 1996: 289, n. 84).

EFB IN BERKELEY

After the takeover of power by the Nazis in Austria in March 1938, EFB managed to escape to the United States. Egon Brunswik had been living in Berkeley since 1935 thanks to an invitation from Edward C. Tolman, who had visited the Böhlers' institute in 1934. Frenkel and Brunswik married after her arrival in New York.¹ Because of what was known as the anti-nepotism rule, EFB was unable

¹ Some claim that the ceremony took place on board the ship to ease EFB's entry into the United States. The official documents of her arrival do not contain any hints of this story. EFB traveled on the SS Britannica and the ship had departed from Le Havre on 27 May 1938 and arrived at the harbor of New York on 5 June 1938. "Psychologist" Else Frenkel (no title added) gives her "nationality" as "Austria" and someone has corrected it by hand to "Germany", her "place of birth" is reported as "Poland, Lemberg" and in the column "race or people", the answer given is "Hebrew." (The form lists about 50 categories to determine "the stock from which aliens sprang and the language they speak. The original stock or blood shall be the basis of the classification, the mother tongue to be used only to assist in determining the original stock.") EFB further indicated that she will travel to California to join her fiancé, Dr. Egon Brunswik. <https://heritage.statueofliberty.org/passenger-details/czoxMjoiOTAxNzYxODM2OTMwIjs=/czo5OiJwYXNzZW5nZXIiOw==> (accessed December 22, 2022).

to obtain a position at the University of California Berkeley and remained a research associate for the rest of her career.

In 1943 EFB was invited to join a small research group, which later on became known as the Berkeley Public Opinion Study Group. The head of the group, R. Nevitt Sanford (1909-95), had studied in Harvard with Gordon Allport and Henry Murray and came to Berkeley as a professor in 1940. In 1943 the provost of his new university offered him \$500 (about \$8000 in 2021) given by an anonymous donor for an investigation into antisemitism. Sanford accepted and hired his PhD student Daniel J. Levinson (1920-94). EFB became a member of this group when Max Horkheimer approached Sanford and invited him to collaborate on a larger research project on the same topic.² EFB had met Sanford and probably also Levinson over the preceding years at Berkeley's Institute of Child Welfare. The fourth co-author of TAP, Theodor W. Adorno (1903-69),³ made only irregular visits to see these three because he lived in Southern California in the same neighborhood as Horkheimer, who had retreated from muggy Manhattan to the airy Pacific Palisades for alleged health reasons. From 1944 Horkheimer was officially the research director of the huge Study in Prejudice project.

Starting in around 1941, Horkheimer and his associates had been forced to raise money following the loss of a large proportion of the Institute of Social Research funding foundation's assets in speculative transactions (see for details Fleck 2022). One of their most promising future partners became the American Jewish Committee (AJC), the advocacy organization founded in 1906 on behalf of Jews in the US and abroad. The negotiations, conferences, and meetings brought together German refugee scholars, American academics, philanthropists and administrators. In October 1942, the AJC awarded a grant of \$10,000 (about \$170,000 in 2021) to Horkheimer's Institute of Social Research. Somehow, the Horkheimer circle also finally persuaded the representatives of the AJC to expand the funding by establishing the Studies in Prejudice project (for a more detailed heterodox analysis see: Fleck 2011, chapter 6).

Later on, in early 1945, a special advisory board was established to help the Horkheimer circle improve their research agenda. Members of this committee were individuals with weaker and stronger ties to EFB and Sanford. Paul F. Lazarsfeld did know EFB from their shared years at the Bühlert's institute in Vienna, and Ernst Kris had been her psychoanalytic therapist in Vienna. Gardner Murphy and Solomon Asch represented psychology and social psychology on the committee. Gordon Allport, Sanford's advisor at Harvard, did not participate in this advisory committee but had occasionally acted as an expert for the AJC. Besides these personal networks, Sanford had also published several papers which might have come to the attention of the Horkheimer Circle. These papers were certainly known to fellow psychologists:

- "Some personality correlates of morale" (Sanford and Conrad 1943),
- "Scales for the measurement of war-optimism: I. Military optimism; II. Optimism on consequences of the war" (Conrad and Sanford 1943),
- "A Scale for the Measurement of Anti-Semitism" (Levinson and Sanford 1944)

² When Horkheimer started looking around for American collaborators, Ernst Simmel, a German psychoanalyst who had been living in California since 1934, most probably suggested Sanford as a candidate. See Horkheimer (1995): 585-7 (letter from Horkheimer to Simmel, 21 April 1939) and 602-3 (letter from Simmel to Horkheimer, 8 May 1939); Adorno and Horkheimer (2004): 206-7 (letter from Horkheimer to Adorno, 28 August 1941).

³ In the book, Adorno's name is given ungendered as "T.W. Adorno", most probably not because of gender troubles but to avoid having to choose between the German and English versions of his first name Theodor/e.

Levinson completed his studies in psychology in 1947 with a thesis on the measurement of ethnocentrism.

Sanford had also had a psychoanalytic education. While EFB had visited Kris back in Vienna, Sanford saw Hanns Sachs, an early collaborator of Sigmund Freud, who was now based in the Boston area. Sachs had migrated to the US in 1932 from Berlin, where he had been living since 1920. Whether Levinson undertook psychoanalysis is unknown to this author; Adorno did not need a therapist, learning whatever he needed to know from his reading and imagination.

In contrast to EFB, Sanford's background in psychological testing stemmed from his collaboration with Murray, who invented the Thematic Apperception Test (TAT) in 1935. Sanford also seems to have possessed more expertise with scaling, while it is likely that EFB's interest in and study of mathematics opened her mind to this style of analysis. Adorno, on the other hand, lacked any competency in testing or statistical analysis and instead favored a version of building typologies, which were already outdated at the time the four began their collaboration. An eye-witness account of the situation in Berkeley has been offered by Marie Jahoda, who visited the team at that time while working as an assistant to Horkheimer at the AJC. Her impression of Adorno, whom she did not know from earlier days, was not very favorable, to say the least (Dahms 1996). From the published correspondence of Adorno and Horkheimer, the reader definitely has an impression of a working climate that left much to be desired. Sanford and Frenkel-Brunswik complained more than once about their exclusion from decision processes and about unfair remuneration—the final dispute about the order of the authors' names did not come out of the blue (Horkheimer 1995, 1996a, 1996b; Adorno and Horkheimer 2004, 2005; Fleck 2011, chapter 6).

The Berkeley group's links to sociology were weaker and less remarkable. Of course, Adorno would have claimed to be at the forefront of the theoretical conception of advanced modern societies, but his short encounter with empirical social research in Lazarsfeld's Office of Radio Research had strengthened his disapproval of team research (for a detailed analysis of this encounter see Fleck 2011, chapter 5). No CV of any of the three professional psychologists indicated any familiarity with empirical social research or sociological theorizing.

Given this distance from sociology, it is even more surprising that EFB approached a rising star of contemporary sociology when her core chapter for the collaborative research report was being prepared for printing.

EFB ASKS ROBERT K. MERTON FOR COMMENTS

Merton (1910-2003) had participated in several of the meetings of the advisory board set up by the AJC to support Horkheimer and his collaborators. There are no hints as to whether EFB and Merton met each other in person, but among the Robert K. Merton Papers I found one letter from EFB and a reply from RKM. The undated handwritten letter (on stationery from the Department of Psychology, University of California, Berkeley) from EFB reads as follows:

Dear Dr. Merton,

This manuscript is my contribution to a book, for which Adorno, Sanford and others have also written some contributions.



Skipp section A and C entirely. Section D. is most important. The term “high-scorer” refers to the prejudiced subject, low scorer to the unprejudiced. In section D. psycho-analytical hypotheses alternative with sociological (see page 114/115). If you have time to dig into the data and to discuss possible alt hypotheses, I would be most grateful.

There is much more material for discussion. But this manuscript might provide a good basis for departure.

Thanks again

Else Brunswik

Office Child Welfare Inst.

Campus

Home: Thornwall 3 – 9417.⁴

The informal style of writing might indicate that the two must have met each other on some previous occasion. Unfortunately, the Merton Papers contain only four typed pages “Running Notes and Comments on the Else Brunswik ms.” No covering letter and no dates survive.⁵

We therefore do not know which parts of EFB’s drafts Merton received for commentary. Her single-authored part of TAP is titled “Personality as revealed through clinical interviews” and runs in the printed version from page 291 through page 486. Three of the five chapters are subdivided by capital letters and run down to D or further into the alphabet. The most likely scenario is that EFB’s first three chapters were sent to Merton (IX. The interview as an approach to the prejudiced personality, X. Parents and childhood as seen through the interviews, XI. Sex, people, and self as seen through the interviews).

As indicated, the annotated manuscript itself has not been preserved, but we get a reasonably good idea of Merton’s criticisms from his four pages of single-spaced typed comments. His remarks start with reference to “D 67” and cover the next 50 pages of EFB’s manuscript (the last comment refers to “114-115”).

One general, but for Merton only “minor terminological point,” are the “rather awkward terms” of “high-scorer” and “low-scorer.”

I assume you want to use terms which remind the reader of the actual basis of classification, i.e. their scores, thus avoiding connotations. Well and good. Once you have impressed upon the reader the criteria of classification, why not use terms which take up less space and lead to less awkward grammatical constructions? Almost any neologism or conventional term will do.

A rather broadly argued concern on Merton’s part relates to the calculation of percentages and it seems that this point at least has been taken on board by EFB. In the manuscript, the “neutrals”—those with neither high nor low scores—were divided equally between the highs and the lows. Merton

⁴ Robert K. Merton Papers, Box 5 Folder 5-7, Columbia University Libraries, Archival Collections, Rare Book & Manuscript Library Collections.

⁵ For readers unfamiliar with the Merton Papers, the sketchiness of the “Brunswik file” is surprising because Merton was a very careful correspondent and self-archivist. However, that her letter and his memo were found in the collection of correspondence labeled “BR, 1945-2002” indicates that Merton did not exchange more letters with her because it had been his custom to start individually labeled folders when correspondence became regular.

explains at some length the consequences of such an arbitrary data strategy with regard to the significance of the results.

One point of disagreement Merton repeats is the lack of an elaborated theoretical model. He quotes from the manuscript: “it was expected on theoretical grounds that ...,” before asking where it is that these theoretical grounds are explained. After the final repetition of this concern he adds in parenthesis: “I shan’t repeat this general point hereafter for fear of laboring ad nauseum – the nausea being yours, of course.”

Several remarks are directed towards EFB’s attempts to develop an explanation of the family dynamics of prejudiced and non-prejudiced respondents. Where EFB writes about the “conventional idealization of parents,” Merton wants to know the “nature of such idealization. Do you treat it as a ‘sub-culture pattern’ (found typically in certain subgroups and strata) or as an ‘individual’ (i.e. idiosyncratic) byproduct of particular sets of interpersonal relations in the subject’s family?”

Merton continues his questioning of EFB’s argumentation when he asks whether “idealization” and “objective appraisals of parents” are correlated with formal education “or any other index of articulateness: Otherwise the reader may wonder whether the difference is not essentially one of ability to articulate, to verbalize fine distinctions, to ‘observe’ less obvious characteristics of parents, rather than a genuine difference in parent-appraisals.”

Another serious criticism, which again indicates the differences between psychologists’ and sociologists’ frames of reference, Merton labels as the “‘culture-pattern’ interpretation.” EFB writes “from this material we may infer that repressed resentment leads to glorification of the parents” and Merton remarks:

This is an example of your moving promptly to a ‘psychological’ interpretation of a finding before considering the possibility that it can be derived sociologically. You’re probably right, in this instance; your interpretation has a certain plausibility. But should you not consider the alternative that the correlation between ‘glorification’ and resentment of parents may not only represent this dynamic of psychology but also a culture-pattern: e.g. in certain groups, it is laid down as cultural axiom that ‘parents can do no wrong’, thus accounting for ‘glorification’ among the conformists to this axiom, but in due course this leads to resentment as the individual experiences discrepancies between the parents’ alleged infallibility and his actual behavior.

To make his point, it would have been sufficient for Merton to direct EFB to consult his paper on anomie from 1938 (Merton 1938), which was still famous at that time. However, Merton continues to elaborate alternative explanations when he writes:

Pursuing the ‘culture-pattern’ interpretation further: IF some of your subjects have been exposed to a pattern which asserts that ‘not only are one’s parents above criticism, but they have deep and abiding obligations to their children which they will carry out’, is it not likely that these subjects will more often feel themselves ‘victimized’ by parents, since their expectations of continuing care, having all dependencies recognized and succored by parents, are too exaggerated to be fulfilled. Thus the ‘dynamic’ relation bet[ween]. glorification, resentment and victimization may be derived from the pre-existing culture pattern. The problem is, of course, where does one locate this pattern in terms of social structure – in which groups is it typically

to be found? Italians, for example, typically have this authoritarian family pattern, with rigorous discipline of children, particularly girls in their adolescence, plus a requirement of family solidarity.

The Italians Merton references here are obviously American-Italians, and it seems plausible that he extracted this finding from William Whyte's recently published study on the Italian district in Boston (Whyte 1943).

Merton elaborates the same culture-pattern perspective when he comments on the conceptualization of "father-dominant vs. mother-center[ed] or egalitarian." He calls it the "key-item discriminating the two culture patterns in American society. 'Henpecking-dominance of mother' is merely a variant of the authoritarian pattern." And in parenthesis he adds suggestions for further reading: "Have you seen some of Franklin Frazier's accounts of the matricentric family, the egalitarian family, etc.? I believe Burgess has something on this too. Neither are particularly penetrating accounts, but they will lend credence to these being established and identifiable culture patterns."⁶

A further culture pattern discovered by Merton in the manuscript is the distinction between "principled independence" and "rebellion against authority." Parents who "subscribe" to the pattern of "principled independence" will not view the behavior of their children as hostile or aggressive; "they will accept it as 'proper' without having it sabotage the relation with their children." But if "the culture pattern prescribes submission to parents," a deviation by their children will force the elders to react with outrage. "The intensity of feeling, therefore, may be derived from the mores or culture pattern" and thus may not stem from character or personality, upbringing, or unconscious drives. He then further elaborates a sociological explanation of measured differences:

The authoritarian family pattern includes a basic orientation of parents toward conformity with the pattern; they are concerned, above all, with whether children abide by the 'rules', i.e. the pattern. Affection may occur, but it is not prescribed; what is prescribed is discipline. Thus, the parent becomes for the child largely an object which must be taken into account if the child wants to satisfy his impulses: the parent becomes an object, an obstacle, a source of (or a denial of) things, not an independent source of affection. The parent is, so to say, as much a part of the social environment as a police officer; he is not so much a person as a relevant and possibly dangerous object – one to be manipulated, if possible, or utilized. Hence, the utilitarian orientation toward the parent derives from the parent's role in playing out the instructions of the culture pattern.

And he continues by elaborating a role theory as an alternative explanation:

"The quid pro quo sub-pattern has much the same source, I suspect. The child is given things, not because the parents love him, but because the parents are obliged to take care of him; correlatively, the children have their rigorously defined role; they must reciprocate, not so much in term of love (which they have not received) but in terms of an implicit 'agreement' of reciprocity involved in the authoritarian culture pattern. Reciprocity of love and reciprocity

⁶ Merton might have had in mind these books by these two sociologists of the family, who were influential at that time: E. Franklin Frazier, *The Negro Family in the United States*, Chicago: University of Chicago 1939; Ernest W. Burgess and Leonard S. Cottrell, Jr., *Predicting Success or Failure in Marriage*, New York: Prentice Hall 1939; Ernest W. Burgess and Harvey J. Locke, *The Family: From Institution to Companionship*, New York: American Book 1945. None of these authors are cited in TAP, however.

of things are very different; the first does not imply a careful weighing of equivalences, the latter does.

Since you say that the ‘utilitarian outlook’ will play a central part in your later discussion, it would seem all the more desirable to attempt to derive this outlook, rather than take it as a datum.

It is not without some irony that Merton has to remind EFB to avoid stereotypes with regard to the sexes (gender as a category was not yet born). He asks in criticism:

why do you leave ‘unexplained’ [i.e. underived] the hypothesis that H[igh scoring] men have a greater ‘submission ‘problem’ than H women? Is it derived from the different sex-roles in relation to authority? From what? If you don’t ‘derive’ the hypothesis, it becomes merely an empirical generalization, which some readers may even ascribe to biologically grounded differences between the sexes.

EFB seems to have accepted this point but her solution in TAP is not very convincing when she writes:

related to glorification of parents is an attitude to be characterized as submission to parental authority and values out of respect based on fear. ... It is therefore interesting to note that this category shows marked differences between prejudiced and unprejudiced interviewees. The percentage of high-scoring men who manifest this attitude Is greater than that of the high-scoring women. (TAP: 350)

Deleting the remark admitting the necessity of further study of the hypothesis of a difference between men and women with regard to authoritarian submission does not prevent mis-ascriptions by readers.

Whereas most of Merton’s concrete criticisms resulted in rewriting or deletion, the changes EFB made with regard to the sex differences discussed above were minimal. Instead of leaving the hypothesis “unexplained,” it found entry in the printed version as a given—but without any further discussion or elaboration: “This gives some support to the hypothesis that high-scoring men are faced with a more serious submission problem than high-scoring women.” (TAP: 350)

Finally, Merton explicitly stressed the disciplinary rift between psychology and sociology:

What you as a psychologist take as a datum – ‘they received more love’ – I as a sociologist take as problematical – ‘why did these people and not the others receive more love?’ If you reply that it was simply (i.e. exclusively) a matter of individual differences among parents, then you are legislating sociology out of existence. If you reply that you don’t know and don’t care, then you assume the role of a psychologist who admits the existence of sociology but disclaims any concern with it. If you reply, let’s find out, why, i.e. let’s try to identify the groups which carry ‘love toward children’ as an integral part of their family culture pattern, then you are a sociologically oriented psychologist. Are you ready to vote?

To round out the picture, it should be noted that while, overall, the reprimands outweighed the praise, Merton did find some formulations appropriate. “Status-concern,” “status-relaxation,” and “types of dependence” got the approval of the sociological theorist, who was enjoying his first peak of academic acclaim in around 1950.

“Status concern” is the title of part 4 of chapter X (TAP: 382-384) but in the two pages, there is not a single reference to Merton’s culture-pattern argument. And EFB uses the same concept elsewhere but, again, without further discussion: “In the same record there are signs of the subject’s relaxation about the status of his own family and that of his father.” (TAP: 364) The contrast between the dependence on things versus that on love is discussed twice in TAP but in both instances, there is no echo of the culture-pattern approach outlined by Merton in his remarks.

At the very end of the typed comments, Merton suggests that “we use these few comments as a basis for our discussion.”

We do not know when and where this meeting might have taken place, but what we can recognize is that in the printed version of TAP, the authors persist in their devotion to a pure individualistic view on the problem of prejudices.

MERTON PROBLEMATIZES PSYCHOLOGICAL APPROACHES

This private correspondence and presumed in-person debate were not the only instances where Merton elaborated his mode of research. In several instances of his work from the 1930s and 1940s he presented his own explanatory models. Already his PhD thesis had been concerned with an analysis of the religious and, therefore, cultural background variables of the members of the Royal Society in 17th-century England. What later on became known as the “Merton thesis” was outlined in the revised published version (Merton 1938). At the heart of his study was the question of how to explain the shifts in focus in the research of early English scientists. Merton argued that particular religious orientations could be identified as the causes for it. Removed from all historical specificities, Merton attempted to explain overt behavior (i.e. shifting foci of scientists) by pointing to particular patterns of beliefs, sentiments, attitudes, and mental forces as causes (Merton 1938; on its reception see: Cohen 1990).

The aforementioned elaboration of the anomie theory, published in 1938, the very same year as his thesis appeared, expanded this approach by differentiating between patterns of cultural orientation distributed unevenly in a society. Less well known are two more attempts by Merton to develop what, at that time, he termed paradigms of social analysis.⁷ Earlier on he had proposed such paradigms for the sociology of knowledge, functional analysis, and anomie. In 1948, so early enough for the Berkeley group to have seen it,⁸ he published a paper on the triad of creed—attitude—action. In this paper, he discussed the question of why some people discriminate and others do not. The historical background was the ongoing debate about discrimination against ethnic minorities in the United States. Merton challenged Gunnar Myrdal’s thesis, expounded in his voluminous study (Myrdal [1944] 1962), according to which a gulf between creed and conduct created the “dilemma.” Merton objected. In a similar vein to the anomie paper from 1938, in 1948 he starts with what he calls the “American Creed”: “a set of values and precepts embedded in American culture to which Americans are expected to conform” (Merton 1976: 190). This creed, as with any other faith, is neither historically fixed nor

⁷ In the later re-publication of Merton’s 1948 work, he added some sentences on his attempt to make “paradigm” a “logical design for analysis” (Merton 1976: 211) and admits that Thomas S. Kuhn has been more successful with his version of paradigm.

⁸ The Else Frenkel-Brunswik Papers at the Archiv für die Geschichte der Soziologie in Österreich (AGSÖ) in Graz contain only of a small part of her records. We therefore do not know whether Merton handed copies of his recent publications to Frenkel-Brunswik.

shared unanimously by all Americans. In some corners of the country, in some strata of society, binding to the creed will have been stronger, in others weaker. Since not all individuals are tied in to the same extent, evasion can happen. “The evasions themselves become institutionalized, giving rise to what I have described as the ‘institutionalized evasion of institutional norms’.” (Merton 1976: 190) To get a handle on this, it is necessary to go beyond the simple opposition of “high cultural principles and low social conduct” (Merton 1976: 191). What is needed is to enlarge the picture from a two-variable model to one which encompasses three variables. Differing from the cultural creed are the beliefs and attitudes of individuals; and the actual practices of individuals are not always in accord with the creed or their attitudes.

Merton cross tabulated these insights in a way that strongly resembles the older essay on social structure and anomie. Conformity to the creed or deviation from it can be found both under “Attitude” and under “Behavior,” where prejudice or non-prejudice refers to the first, discrimination or non-discrimination to the second. Merton names the resulting four types in the following way:

Unprejudiced non-discriminator or All-Wheater Liberal,
unprejudiced discriminator or Fair-Wheater Liberal,
prejudiced non-discriminator or Fair-Wheater Illiberal and
prejudiced discriminator or All-Wheater Illiberal. (Merton 1976: 193-198)

In an astonishingly outspoken ‘activist’s’ style, Merton then discusses at some length the conditions under which the two types exhibit or hide their attitudes under cross-pressure.⁹ Central to his presentation is the concept of expediency, which in one situation demands one kind of conformity but supports the opposite behavior in other circumstances. There is no need to elaborate further on Merton’s arguments and insights on prejudice and discrimination. What should be highlighted, however, is that his paper displays exactly those thoughts on successfully combatting prejudice which the American Jewish Committee expected from the social sciences when its officers hired Horkheimer and his collaborators for the Studies in Prejudice project.

It is surprising to note the complete lack of references to this paper by Merton in TAP. But given this, it comes as less of a surprise to see that another relevant contribution from the very same sociologist had been ignored by the authors of TAP. In 1940, Merton published a paper on the problems of measuring attitudes. Besides technical criticism directed towards L. L. Thurstone, the inventor of this kind of research, Merton did not side with those who belittled opinions and favored analyses directed towards overt behavior.

A further mooted point [...] is the relation of opinion to overt behavior. [...] In some situations, it may be discovered that overt behavior is a more reliable basis for drawing inferences about future behavior (overt or verbal). In other situations, it may be found that verbal responses are a tolerably accurate guide to future behavior (overt or verbal). It should not be forgotten that overt actions may deceive; that they [...] may be deliberately designed to disguise or to conceal private attitudes. The question of the relative ‘significance’ of verbal and overt responses must as yet be solved anew for each class of problems. The apriori assumption that verbal responses are simply epiphenomenal is to be accorded no greater weight than the assumption that words do not deceive nor actions lie. (Merton 1976, 260)

⁹ Merton did not make use of this Lazarsfeldian concept explicitly but followed the line of argumentation first presented in Lazarsfeld et al. ([1944] 2021).

Merton's argument might have been merely a theorist's deliberations back in the 1940s; it has since become cutting edge in psychological attitude research (Guyer & Fabrigar 2015), although it has yet to conquer all corners of sociology. Back in the late 1940s, neither the psychologists from the Berkeley Public Opinion Research Group nor the social theorists assembled around Horkheimer and Adorno approached the level of sophisticated thought exhibited by Merton in his two contributions.

SOCIOLOGISTS' REACTIONS TO TAP

Let me finish by taking a brief glance at the reception of TAP by contemporary sociologists. Merton himself paid tribute to this study only once. In the second edition of his seminal *Social Theory and Social Structure* (Merton 1968), Merton inserted a chapter on "Continuities in the Theory of Social Structure and Anomie" that refers to EFB's concept of "intolerance of ambiguity," which she had elaborated in separate publications (Frenkel-Brunswik 1949, 1954). His praise for her insights is highly qualified:

What these studies lack by way of systematic incorporation of variables and dynamics of social structure is largely compensated by their detailed characterization of the components which presumably enter into ritualist responses to patterned situations and not only into the structure of the rigid personality. [...] The concept of intolerance of ambiguity refers to 'an excess' of designated kinds of perception, attitudes and behavior (as indicated by such terms as 'undue preference', 'over-simplified', 'unqualified', 'over-emphasis', and the like). The norms in terms of which these are judged to be 'excessive', however, need not be confined to the statistical norms observed in an aggregate of personalities under observation or to norms of 'functional appropriateness' established by considering individuals *seriatim* in abstraction from their social environments. The norms can also be derived from the standardized normative expectations which obtain in various groups so that behavior which, by the first set of standards, may be regarded as 'psychological over-rigidity' can, on occasion, be regarded by the second set of standards, as adaptive social conformity. This is only to say that although there is probably a linkage between the concept of overly-rigid personalities and the concept of socially induced ritualistic behavior, the two are far from being identical. (Merton 1968: 241)

This detailed rejection of the psychology approach was the last comment Merton elaborated on TAP. He was, however, not alone in his reservations towards this study and its rigidly individualistic approach. TAP was reviewed immediately after it appeared in print in 1950 but the early reviews published in sociological journals raised several reservations:

- "the concept of the nonethnocentric personality is utopian" (Jurczak 1950)
- "Professor Horkheimer unfortunately uses the same dichotomous approach here against which his students and collaborators warn" (Bunzel 1950)
- "Critical readers will wonder at the absence of any reference to the scaling approaches of Guttman and Lazarsfeld during the entire discussion of scale construction" (Bredemeier 1950)
- "the reader may wish that middle scorers (probably more representative of the population as a whole) received as much intensive study as the high and low scorers" (Schermerhorn 1951)
- "The authoritarian type is the one most closely approximating the 'perfect' politician in the sense of one whose primary goal value is power, whose preferred base value is also

power (threat or use of extreme deprivations in any sphere), and whose basic expectations are that the most important human relations are matters of power. By contrast, the democratic character is not centered on power but upon multiple values.” (Lasswell 1951);

- “Thus, personality, as selective sensitivity and orientations toward categories, is important. But attitudes and action are not simply the unfolding of character structure. The demands of other participants in a collective transaction, especially the shared expectations of what each person is to do, are also of crucial importance.” (Shibutani 1952)

The more or less critical reception of TAP by sociologists in the early 1950s then culminated in a book-length evaluation of the study by a group of experts. Under the editorship of Marie Jahoda and Richard Christie, both at that time affiliated with the Bureau of Applied Social Research at Columbia University, the collection of essays appeared in a series titled *Continuities in Social Research*. EFB was the only author of the study under investigation who contributed a chapter of her own, in which she developed some of her perspectives further but did not react to several of what were, overall, very critical contributions by others. Horkheimer and his circle never responded in public to any of the severe criticisms raised by the contributors to the edited volume of evaluation. Instead they invested considerable effort in sidelining Adorno’s co-authors and presenting him as the mastermind behind the study.

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