Christian Fleck

Author's Reply to Two Very Kind Reviews

(doi: 10.2383/72708)

Sociologica (ISSN 1971-8853)
Fascicolo 3, settembre-dicembre 2012

Ente di afferenza:
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I want to express my thanks to Jennifer Platt and Johan Heilbron for their well-balanced opinions about the book under consideration. They raise only some criticism; therefore I can restrict my answer to those points where their reviews ask for further elaboration or clarification.

Platt did have trouble with the arrangement of the book and suggests that it might have even been better to divide the book into two different volumes. In connection with this she surmises that what is now the book consists of previously written papers put together between two binders. I do know that this happens sometimes but not in my case. Not even one chapter has been published in before and arranging the whole material was a heavy burden. Indeed, the original manuscript was twice as long as the book is now. (So, by Platt's account, it would consist of four books, at least.) Why did I decide to put so much into a single study? The reason is that during the research process and thinking about the topic I realized that I wanted to bring together at least five different aspects of the development of the emerging field of sociology and its institutional environment: (1) the incremental occurrence of the field we recognize today as sociology, with special emphasis on the significant role of empirical social research in it; (2) the institutional changes in academia, the higher education sector, and organized research; (3) taking into account the fact that these two processes followed different paths within different national environments, which asked for a comparative analysis on a trans-national level; (4) the role that forced migration played with regard to the composition of the personnel both in the sending and the receiving countries during what one could call the period of dictatorship in Europe; and (5) the incidence of voluntary organizations helping refugee scholars from the Third Reich to re-establish themselves abroad. All five developments occurred during the second third of the Twentieth century, some of them started earlier, some needed more time to evolve, and obviously not all of them were intrinsically tied to each other. Furthermore, one has to bear in mind the processual character, that is, the historical dimension which asks for a narrative structure of whatever I wanted to say.

Very much to my regret, I finally had to eliminate the last aspect from the present book because both the original German and the British publisher asked for shortening the manuscript (a book with the title *Die Etablierung in der Fremde* – roughly: *The process of reestablishing abroad* – dedicated to the last mentioned topic is now under consideration with a German publisher).

Concurrently I did not want to present these aspects as independent parts but show their interconnectedness and mutual modifications. Therefore, I organized the whole material along the temporal dimension – starting with a chapter on the early period with some remarks on preceding developments and ending with a chapter on what happened in Germany and Austria after the defeat of the Nazis and the reconstruction of the universities. In between, two "structural" chapters cover institutional developments in research funding and providing fellowships (mainly by the Rockefeller Foundation), while two case studies are dedicated to prominent research projects managed by refugee scholars interacting differently with "local" Americans. At the very center of the book stands what I called a prosopography of German speaking social scientists.

From a systematic point of view, any history of science has to take care of at least five dimensions: (1) Actors and the opportunity structures they envision; (2) ideas in the broadest meaning of this term, or the "cake" of scholarship; (3) instruments to establish scientific propositions and find out something new about the world around us; (4) institutions which form the structural base of scholarship and research; and finally (5) the larger environment or context in which all these happens. A sociological history of the social sciences has to put forward specified sociological concepts, social mechanisms, middle range theories, and raising research questions which should apply insights from sociology proper.

This complexity grows further if we take into consideration that every dimension can be approached at a different level of aggregation: Single authors or collectives like cohorts, generations; an isolated study or models and theories; idiosyncratic habits of doing empirical research or data analysis packages with predefined and limiting routines; arrangements in one particular research team or structural conditions as tenure track, third party funding, etc.

Frankly, nothing like this is in the book (and the short remarks here won't fill up the lacuna) but the composition of the book has tried to follow these lines of thought. I still think that the old saying that the proof of the pudding is in the eating applies for scholarship too. One does not need to elaborate always the underlying methodology (or theory). Heilbron is completely right when he laments that the study is "perhaps a bit undertheorized," and his claim that it does not become "exactly clear what (my) position is in the sociology of science" is correct too. My hesitation to join a school or demonstrate a particular affinity with regard to one of the fashionable approaches in sociology rests on the persuasion that neither of them is encompassing enough. Simply put, a theory is a shortcut or compressed version of a long story one can tell, and for personal reasons I prefer good stories over anemic theories. The whole field of the emergence of the social sciences, sociology in particular, is much too complex to be covered by a single theory. My guess is that advocates for higher levels of theorizing would immediately reply that they would not plea in favor of "one" theory, but this is the crucial point of my disagreement with them, Heilbron probably included. In accord with most other sociologists, I do not think that we do have in sociology any general theory. Unlike the majority I see a superabundance of unconnected concepts, plus a handful of theory fragments encompassing two to five variables, nowadays repossessed by those who advocated social mechanisms, and an increasing number of what their aficionados like to call theories which indeed are styles of thought, approaches, perspectives, and very often nothing else than hobbyhorses. I doubt the usefulness of the latter, but do make use of the two others, concepts and mechanisms.

What we can do and must do when we try to understand particular intellectual developments in the social sciences is to take care of as much of the above mentioned five dimensions. At each of them particular concepts or mechanisms might be helpful to get a better understanding but the potential for generalizations is very limited. To illustrate this, Galtung's intellectual styles can function as an indication: To point to differences between the Gallic, Nipponic, Teutonic, and Saxonic type his insights are helpful, but zooming on one of them, e.g. the Teutonic type, one immediately comes to recognize big differences. Whereas Galtung's quadrinominal typology is sufficient to illustrate differences at this level of aggregation, the very same concept is empty if one wants to understand the different habits of e.g. Karl Mannheim and Max Horkheimer, serving as professors at the same university, Frankfurt, at the same time but acting e.g. towards students and junior staff highly different. Whereas Mannheim tried to encourage youngsters, Horkheimer stuck to the old fashioned attitude of a chair holder jealously discouraging potential rivals from the next generation.

The same message can be detected from the collective biography data, which both reviewers praise as an innovative part of my book. What I did there was to try to find as much data for different variables for as many people possible. However, some data were hard to find and therefore the analysis could cover only a limited number of aspects. I have been happy that even this reduced universe of data was good enough to demonstrate several differences, and the technique of correspondence analysis provided the means to demonstrate them. I hope Platt's (under-specified) frustration over the "modes of visual presentation" is caused by the miniaturization of the plots and not related to any reservation against this explorative data analysis method.

Another of Platt's criticisms is directed toward the boundaries of the discipline, which I more or less ignored in creating my sample. For the time period I analyzed the objection raised by Platt does not apply. Covering the decade from the 1920 to the 1950 one has not to fear making Ibn Kaldhun (or Karl Marx) a sociologist. At this time even in German speaking countries "sociology" did exist, albeit in a mode completely different from later or present days. Back then sociology was kind of a worldview, an intellectual perspective but not a discipline. For this very reason one has to make use of several kinds of inventories to establish the members of this fleeting entity. Members joined easily and could leave the special interest group whenever they were bored with it. Nevertheless for some time or under particular perspectives all the 800+ individuals have been seen or saw themselves as sociologists, contributed articles or at least book reviews to journals considered to be sociological, even if they did not have "sociology" in their title (as was the case with the then leading German journal *Archiv für Sozialwissenschaft und Sozialpolitik)*. The 800+ are nearly the population, or a sample of the size of 90+% of the population. Therefore, the omission of individual cases won't cause any troubles for the statistical calculations, and even less for the correspondence analysis which is, as is well-known by its admirers, unsusceptible for the size of the sample. The lack of data for several of the 800+ is closely connected with the omission of less well-known scholars within a very short period of time after their drop out from the field, either because they started alternative professional careers or worked at the margins of academia.

Heilbron's hint toward supposed differences between Gurvitch and Sorokin could be complemented with other cases. In a recent paper I present a typology of uprooted scholars' adaptation towards new environments, or factors influencing a successful re-establishment abroad.[1] A dozen or so dimensions can be derived theoretically from the literature, ranging from age, via personality traits, former migration experiences, political orientation, national identity, previous occupational status, available mentors abroad, etc. For each of these dimensions a "property space" could be constructed – but the lack of data frustrates any attempt in this direction.

[1] See Fleck [2011a] and the above mentioned forthcoming monograph.

Heilbron is completely right in asking for the "experiential dimension" and I agree with him that this is kind of a desideratum. In my defense, I would only point to the fact that the standard literature by and about German speaking sociologists or sociologists of German and Austrian origins is primarily based on autobiographical information, seldom auto-ethnographies, mainly produced by authors located at the opposite ends of the success continuum. The contributions by émigrés are U-shaped, their authors have either been very successful abroad (Reinhard Bendix, Peter Blau, Lewis Coser, Albert Hirschman, Lazarsfeld, and others) or failed to get a foot in America's academic door but reestablished themselves after their return to Europe (Adorno, Günther Anders, Ernst Bloch, Jürgen Kuczynski, and others[2]). My collective biography has been partly motivated by the intention to counter these narratives.

A final criticism is put forward by Platt and other reviewers of my book.[3] They do not approve what Platt calls "robust … value judgments … without justify(ing) them." There is not enough space to go into details but some simple distinctions are appropriate. The social sciences, including sociology, tried for a while to adopt from the natural sciences a style of detached and neutral argumentation. The result was a sterile language, mimicking the observational vocabularies for uninhabited branches of investigation. In fact, sociology should try to understand humans and their judgments, assessments, etc. not only by reconstructing their judgmental utterances but also by arguing normatively. Sociologists advocating scientification do not dismiss the reporting of appraisals, but claim that they themselves are not allowed to summarize their findings using value-laden language. I do not agree with them because our scientific language should not amputate itself voluntarily as long as "normative" phrases are open to debate and refutation. The positivistic misunderstanding of avoiding any "normative" expressions results in an unnecessary reduction of our vocabulary. Positivists erroneously think that normative discourses are incomprehensible. But even value laden expressions are communicable, contestable and capable to inform us about the world around us. To give just one illustration: In chapter six, "The History of an Appropriation" (the very title summarizes the chapter's finding by using an equivocal term), I narrate at some length the collaboration between Horkheimer's Institute and the American Jewish Committee, and characterize Horkheimer's behavior as that of a "con man" [Fleck 2011a, p. 237]. Edwin E. Sutherland's *The Professional Thief* (1937) contains a lengthy analysis of this type, but his actors were ordinary criminals of lower middle class background. Pointing

[2] In a then widely read collection of interviews the sad stories of dish-washing intellectuals reverberated heavily: Greffrath [1979].

[3] See e.g. the review by Michael Pettit [2012], who charges me to be a "passionate partisan".

to the resemblance of Sutherland's con men's and Horkheimer's behavior invites the reader to think about the potential of comparative analysis and middle class members' reservation to label people of their own stock according to a vocabulary that seems to be reserved for lower classes. On the other hand, particular normative formulations are themselves informative. Calling someone ambitious or open-minded is anything but unjustified as long as the narrator embeds his verdict in a contestable argumentation.

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Abstract: A rejoinder to Johan Heilbron and Jennifer Platt's reviews of Fleck's book, *A Transatlantic History of the Social Sciences: Robber Barons, the Third Reich and the Invention of Empirical Social Research*.

Keywords: methodology of the social sciences, collective biography, émigré scholars, value judgment.

Christian Fleck, Associate Professor at the Department for Sociology, University of Graz, Austria. He was Schumpeter Fellow at Harvard University (1993-94), Fellow at the Dorothy and Lewis B. Cullman Center for Scholars and Writers, The New York Public Library (1999-2000); Fulbright Visiting Professor University of Minnesota, Minneapolis (2008); Directeur d'études invité, Ecole des Hautes Etudes en Sciences Sociales, Paris (2011).