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Practices of Using Interviews in History of Contemporary Economics: A Brief Survey

Dorian Jullien*

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The oral face-to-face interview is commonly used and methodologically discussed across the social sciences and the humanities. A quick search for the use of such interviews by historians of economics – as a research tool (when the historian is the interviewer) or as only a source (when the historian uses someone else’s interview) – reveals over a hundred papers and a dozen books over the last forty years. By contrast, there is only a few existing historiographical and methodological discussions of the use of interviews in the history of economics, which are precious but either quite brief or focused on a narrow object (usually a set of interviews conducted by the author). This short paper proposes a broader perspective to highlight the diversity of practices regarding the use of interviews by historians of economics. I will briefly discuss the evolution of these practices under one historiographical theme, which has been highlighted by previous historians of contemporary science and economics: that the research project for which the historian is conducting interviews might be perceived by some scientists as involving a *potential* threat to their scientific credit or even to the scientific legitimacy of their discipline¹.

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¹See especially Söderqvist (1997), de Chadarevian (1997; 2016), Gaudillère (2007) and Hoddeson (2006) for the historians of science and Weintraub (2007), Cherrier (2011) and DÜppe and Weintraub (2014) for the historians of economics. For a broad overview of the use of interview in other disciplines, see the papers in Perks and Thomson (2016) for the field of oral history and the papers in Fielding (2009) for many other disciplines. The companion chapter to this short paper devotes more space to these other disciplines and the differences across them regarding the use of interviews. The quick search mentioned in the paragraph consisted in searching for the keywords “interview”, “conversation” and “personal communication” and their French equivalents in several history of economics journals and then reading the paper to figure out whether the keywords were indeed used in the sense of an oral face-to-face exchange. The companion chapter briefly touches on the practices of purely written interviews that are much more common among historians of contemporary economics (e.g., e-mail exchanges).

Economists and policy-making

In 1978, Coats encouraged historians of economics to study the role of practicing economists in policy-making institutions. He argued that “special attention must be given to the opportunity, at least for recent periods, to interview economists about their working experiences” (Coats, 1978, 313). His main justification for the use of interviews was that they allow one to get information that could otherwise not be obtained from written documents due to confidentiality restrictions (ibid; 1981a; 341; 1981c, 690). In a yet earlier paper, Coats acknowledged “many government officials who have traced documents” and “a number of valuable interviews with senior agricultural economists in Whitehall and in the Universities” (1976, 381). Coats used the documents – which do not seem to be confidential – explicitly (383) to argue that the U.K’s Ministry of Agriculture shaped British agricultural economics as a professional sub-field. However, the interviews were neither cited nor mentioned anywhere in the paper (besides the acknowledgment footnote). Coats briefly gave voice to an interviewee in another paper on the role of economists in the British Government to illustrate an argument: “a lone economist has been imported into a department because a minister or senior official had the vague notion that it might be helpful “to have a tame pundit around the place.” [fn57: “This is an actual quotation from an interview”]” (1981b, 391). This paper was part of a special issue of *History Of Political Economy* responding to Coats’s 1978 call, in which issue five papers (including Coats’s) out of ten used interviews. The interviewees in Ambirajan (1981), Haddad (1981) and Petridis (1981) were anonymous as in Coats’s papers and are barely given voices. Instead, their interviews were mentioned either to give general impressions from the field, to better interpret statistical data or to fill some gaps in these data. By contrast, Barber (1981) used an interview from the oral history project on President Truman to provide an anecdotal illustration of politicians’ tactics to make economists’ reports sounds less neutral and more in agreement with the Administration’s line: “Murphy [Legal Counsel to Truman] has observed that “we found out along about midnight that Dr. Nourse [first president of the *Council of Economic Advisers*] would begin to agree to anything. So we’d do most of the work after midnight.” Oral History Interview with Charles S. Murphy, Harry S. Truman Library, p. 122.” (1981, 523). In sum, interviews are a mean for Coats and the others to get information about deliberations in places where elites make decisions which impact economics or involve economists.²

The interaction between the political and the scientific domains, i.e., between a country’s political state, its science policies and the activities of scientists, is a research theme that can easily generate

²I thank Tiago Mata for suggesting this way of summing up these practices.

a perceived threat to scientific credit and legitimacy (see, e.g., Gaudillère, 1997, 122-124). Here both the guarantee of anonymity – reinforced by the scarce use of verbatim accounts – and the use of interviews conducted by other scholars could be seen as ways for the historian to manage such potentially perceived threat. Yet it can be argued that the way by which such potentially perceived threat is most reduced is by the framing of the research goals behind the use of interviews. With the exception of Coats (1977) who sought to understand the influence of policy-making activities on the organization of economics in academia, the other papers were concerned with conflicts between economists in policy-making institutions and other politicians. Hence economists are not interviewed *qua* academic economists, which blocks potential threats to their scientific credibility. Note also that behind the explicit justification of getting around confidentiality restriction to gather information, which is a classical justification for the use of interviews by historians of contemporary sciences (de Chadarevian 1997), the interview process tend to lead to other non-classified documents and to information not present in any written sources (two other classical justifications, see Söderqvist 1997, 8; Hoddson 2006, 187; Weintraub 2007, 5)³.

Economists and their academic work

A radically different practice of interviewing economists was introduced in a book by Klammer (1983). His interviewees were not anonymous (they are famous macroeconomists such as Lucas, Sargent, and Tobin), and their voices constitute the core of the book as every chapter but the first and last ones are transcripts of the interviews. They were interviewed as economists *qua* academic economists. Klammer used interviews as a mean to push economists into an argument, into justifying their approaches to economics, i.e., to observe how economists verbally behave in controversies⁴. More precisely, as he later admitted, “[t]he initial reason” behind these interviews “was to check my interpretation of their work” (1989). His working hypothesis was that economists’ political beliefs partly influenced their academic work. This can obviously generate a perceived threat to the scientific credit of macroeconomists or to the scientific legitimacy of macroeconomics because the ideal of a value-free science is usually part of

³The only case of a non-anonymous interviewee is Barber using an interview done by professional oral historians who tend to follow some rules (see Oral History Association, 2009) that historians of economics often implicitly break. For instance, interviewees should not as far as possible be anonymous. But here it can be argued that the status of the interviewees with respect to the object of study is likely to warrant anonymity for oral historians as well. A rule that is more clearly broken here is that oral sources need to be institutionally archived to get the status of sources, i.e., available to other historians for eventual verification or further developments. Another one is that so-called ‘meta-data’ should be provided (at least in the oral archives) about the context of the interview and the relation between the interviewer and the interviewee.

⁴I thank Tiago Mata for suggesting me this way of putting Klammer’s work.

the historical narrative self-produced by most economists (see, e.g., Putnam and Walsh, 2011) including most of Klammer's interviewees (see, e.g., Lucas, 52 or Sargent, 80). Klammer managed that potentially perceived threat most of the time by introducing counter arguments as expressed by other economists and by directly asking political questions as soon as the state of the conversation warranted it (usually at the end).

A number of historians have also edited books that more or less follow Klammer's template (e.g., Snowden and Vane 1999; Colander et al. 2004; Herfeld 2017). They deal with the potentially perceived threat to scientific credit or legitimacy either by focusing more on historical matters and/or on consensus rather than controversies. Sent (1998) and Halsmayer (2014) illustrate two variants to Klammer's use of interviews, which are in a sense more collaborative with the interviewees. Sent (1998, chap.6) introduces the purpose of the interviews to her interviewee (Sargent) as an opportunity to respond to the methodological and historical points she made in the preceding chapters. The potentially perceived threat to scientific credit or legitimacy is here tempered by the historian being upfront about it and open to dialogue. Halsmayer (2014) conducted an interview with Solow while she was working on methodological and historical dimensions of his modeling practice. Without publishing the transcript, Solow's retrospective description of his practice as "engineer in the design sense" (231) opened the door for further methodological and historical characterization by Halsmayer. The collaboration takes a very scholarly form here as Solow provided comments on an earlier draft of her paper (2014, 229).

One can interpret the publications of interview transcripts in Klammer's template as a step toward oral history of economics, as he himself suggested (Klammer 1991, 131). Oral historians usually do not consider interviews conducted for historical purposes to be proper oral history until the audio recordings and transcripts are indexed, if possible with meta-data about the context of the interview, so that they become sources that others can check or even use (Doel 2003, 351). This has implications for the tension between historians and scientists because the scientist's speech in the interview is partially public in the case of archiving with possible access restrictions or totally public in the case of publications in books such as Klammer's. This of course diminishes the chances that the historian will get information that the scientist would be reluctant to put in print. The problem is reinforced by the fact that original transcripts are usually edited by interviewees before being archived or published (Weintraub 2007, 3). This makes it hard for the historian to take full advantage of "themes dear to orality (secrecy, intimacy, interiority, gestures)" (Descamps 2005, chap. 1 §29, my translation). Ensuring anonymity may meliorate part of the problem, but some historians of economics have argued

against the value of this approach (Tribe 2011, 621).

The careers and lives of economists

Another motif in interviewing economists emerged at the end of the 1980s with historians focusing more on the historical context in which economists lived their lives and produced their academic work than on their academic work *per se*. An early instance of this practice was Craver's (1986) historical account of the intellectual milieu of economists in Vienna from the 1920s to their emigration in the 1930s. Her account was based on interviews with twelve economists (e.g., Hayek, Morgenstern, Machlup) and two mathematicians (Alt, Menger) who took part in this emigration⁵. Craver used the interviewees' voices to give vivid illustrations of how scholars judged each other on personal and intellectual dimensions, of the institutional locations of various communities, and of experiences of anti-Semitism. In another fashion, Weintraub conducted an interview with Debreu in 1992 which he used in subsequent publications to illustrate a part of the origins of the change in mathematical economics around the 1950s. In these uses, the voice of Debreu recounting moments of his education is given equal footing to the one of Weintraub commenting on it (2002, 115-117). The interview, which is fully transcribed (ibid, 125-154)⁶, focused on the role of mathematics through Debreu's education and professional career. But it still delivered information about other people and institutions because Weintraub probed in that direction when possible. D  ppe (2012) complemented this approach with information about Debreu's personal life and subjectivity, notably obtained by interviewing acquaintances of Debreu, including his widow and his daughter. D  ppe and Weintraub (2014) show how such information can further our understanding of an episode of the history of general equilibrium theory.

None of the historians of economics discussed so far has reflected on her or his own practice, i.e., on the specificity of constructing and using oral sources. By contrast, Mata (2005, Appendix) provided methodological and historiographical reasons for the sixteen interviews he conducted with radical economists about whom he wrote a history of the social context that shaped their work. He explained, for instance, why he chose an interviewing method from oral history, how he prepared himself (notably by reading other interviews such as Klammer's), how he had to tailor a semi-structured guide for the single two hour sessions and how he made summary transcripts (instead of full ones) that helped him get a better understanding of his historical object (see also Mata and Lee 2007 for

⁵Most of Craver's interviews were conducted by her and/or her husband Axel Leijonhufvud as part of an oral history project at the UCLA. Some transcripts are available on-line (see for instance Hayek's transcript: <https://archive.org/details/nobelprizewinnin00haye> last consulted on 06/12/17).

⁶The tape is also archived in Weintraub's paper at the Rubenstein Library of Duke University.

further reflections)⁷. Emmett (2007) gives details on the process of his oral history project on Chicago economics. He explained, for instance, how he sent questions in advance to the interviewees and how he selected unknown interviewees to get multiple perspectives on Chicago economic department. Finally, Svorenčik (2015, Appendix) discussed how he managed his relations with more than fifty experimental economists (i.e., his interviewees) in order to avoid his own historical biases to influence the interviews and the history of experimental economics he wrote partly based on them. He emphasizes how the issue of trust was crucial in even the tiniest interactions he had with his interviewees before, during and after the interviews. This was of specific importance in his case because the other goals of these interactions were to convince experimental economists to deposit their papers in archives and (explicitly) to gather materials in view of a subsequent witness seminar (see Svorenčik and Maas 2016; Maas, this volume).

The potentially perceived threat to scientific credit and legitimacy is generally managed in at least two ways here. On the one hand, historians put biographical questions in the center of the interviews without interference with questions about scientific legitimacy. By contrast with the interviews in the previous section, there is here a greater potentially perceived threat to personal integrity and individual reputation that the historian has to subtly manage (Düppe, this volume). On the other hand, historians use interviews in their work as sources in conjunction with other sources, especially archives. Hence the voices of the interviewees are not used as means to deconstruct scientific achievements, but rather to place scientific achievements in a broader context, thereby enriching their meaning.

Conclusion

The new generation of historians of economics tend to engage in a collaborative spirit with their interviewees and to reflect on their own practices regarding the construction and use of oral sources. Only a very limited subset of the underlying methodological and historiographical issues have been illustrated here and they have not been discussed in depth. There are many more issues but there are also a large number of reflections from other disciplines on how to deal with them. A key one is the coordination on standards of sharing recordings and transcripts through the establishment of a bank of interviews. This is done by the American Institute of Physics, which has a large and institutionalized oral history project that helps historians “gain the confidence of physicists” (Weart 1990, 39)⁸. Perhaps a similar initiative between historians of economics and economists would help

⁷See also the video oral history on Craufurd Goodwin by Mata and Maas about the birth of history of economics as a sub-field: <https://www.youtube.com/watch?v=uLjmq3BFWV4>.

⁸See <https://www.aip.org/history-programs/niels-bohr-library/oral-histories> last consulted on 06/13/17

better manage the potentially perceived threat to scientific credit and legitimacy involved in the process of an interview.

Contributor's note

Dorian Jullien is a post-doctoral fellow at the Center for the History of Political Economy, Duke University. His Ph.D. dissertation, entitled *A Methodological Perspective on Behavioral Economics and the Role of Language in Economic Rationality*, was defended on June 2016 at the University of Nice-Sophia Antipolis.

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