

Interviews and the Historiographical Issues of Oral Sources

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Abstract

This chapter discusses the similarities and differences in the plurality of practices regarding the use of interviews by historians of economics – i.e., either the use of someone else’s interviews as sources or the use of interviews conducted by the historian for her or his work. It draws on methodological and historiographical contributions from other disciplines where the use of interviews is more systematic to characterize the practices in our discipline and to sometimes suggest further or new developments. The characterizations of the use of interviews by historians of economics focus on three interrelated factors that impact the relation between the historian (potentially as an interviewer) and her or his sources (i.e., the interviewee): (1) the goals of the research project for which interviews are used, (2) the potentially perceived threats to scientific credit and legitimacy that history of contemporary science can trigger for some scientists and (3) the specificities of oral sources compared to more traditional written sources.

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Introduction

The interview, as an exchange between an interviewer and an interviewee, is a type of interaction that takes many forms and pervades our contemporary society. We have all been interviewed for a job and some readers might have also interviewed job applicants. In the media, we more than often come across a journalist, a columnist, a television or radio host interviewing a politician, an artist, an athlete, a scientist, an ordinary citizen and so on. Some sociologists even argue that we live in the “interview society”, where the interview is seen as an accepted mean of getting information and as a privileged way of disclosing authentic subjectivity (see Atkinson and Silverman, 1997; Holstein and Gubrium, 2004; Gubrium and Holstein, 2012). Medicine, psychology, the social sciences and the humanities, they argue, are active participants to the interview society. Indeed, the interview – again, under many forms – is a research tool that is widely used in these domains, where methodological and historiographical writings about that tool are incredibly profuse and diverse (for a representative sample of this diversity, see Fielding, 2009).

How does the history of contemporary economics fit in this picture? Restricted to the most common form of interview encountered in other disciplines, i.e., the oral (usually face-to-face) interview, a quick search reveals more than a hundred papers and a dozen books over the last forty years where historians of economics have used interviews. That involves either the use of someone else’s interviews as sources (which roughly represents about sixty percent of these contributions) or the use of interviews conducted by the historian for her or his work (which roughly represents the remaining forty percent). Nearly all of these contributions do not offer any comments on the specificity of using interviews and on the conditions of production of the interviews. Less than ten contributions (not necessarily using interviews themselves) propose methodological and historiographical reflections informed by other disciplines about using interviews in the history of contemporary economics (Mata, 2005, Appendix; Weintraub, 2007; Mata and Lee, 2007; Emmett, 2007; Freedman, 2010; Cherrier, 2011; Svorenčík, 2015, Appendix). These reflections are precious but they are either quite brief or focused on a narrow object (usually a set of interviews conducted by the author). The goal of this chapter is to broaden the scope of these reflections in two directions. Firstly, the chapter covers a larger territory of contributions in the history of economics to show the similarities and differences in the existing plurality of practices regarding the use of interviews in our discipline. Secondly, it draws on a larger diversity of methodological and historiographical contributions from other disciplines using interviews to characterize the practices in our discipline, sometimes suggesting further or new developments.

The chapter focuses on interviews as face-to-face *oral* exchanges between an interviewer (e.g., a historian of economics) and an interviewee (e.g., an economist), possibly recorded and used in written transcripts. When the historian is the interviewer, the “historical vicinity” discussed by Düppe and Weintraub (this volume) as specific to the history of contemporary economics translates in a *concrete human interaction* between the historian and her or his sources¹. Most methodological and historiographical reflections across disciplines stress how this interaction, i.e., during the interview, is part of a broader relation between the interviewer and the interviewee, i.e., what they both do regarding the interview before and after it. The chapter proposes to characterize the use of interviews by historians of economics in terms of three interrelated factors that impact, and are impacted by, this relation: (1) the research project for which interviews are used, (2) one specificity of science as a topic of interview or of scientists as interviewees and (3) the specificities of oral sources compared to more traditional written sources. The first point seeks to capture a basic concern expressed in methodological and historiographical writings on interviews (and other research methods for that matter): that, usually, “research aim should dictate research method” (Weiss, 1994, p.17). In other words, one objective of the chapter is to characterize how different research goals influence different uses of interviews in the history of contemporary economics. The other two points seek to capture less basic concerns in the sense that they are only discussed in fairly restricted sets of methodological and historiographical writings.

The second point is 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 discipline. This comes from the broader issue that the history of contemporary economics or science can produce historical narratives that challenge scientists’ self-produced historical narratives, and history is one source for the establishment of scientific credit and legitimacy (see, e.g., Klamer, 1983, p.250; Söderqvist, 1997; Weintraub, 2007; Düppe and Weintraub, 2014, preface). Indeed, pointing out the importance of institutional, social or technological factors for a given result can be taken as reducing the scientific credit scientists deserve for that result. Similarly, pointing out the cultural or ideological ladenness of the meaning of scientific results can be taken as

¹Scholars in other disciplines characterize more precisely the concreteness of the human interaction in face-to-face interviews as a synchronous communication in both time and place – by contrast with the synchronous communication in time but asynchronous communication in place characteristic of telephone interviews and on-line instant messengers, and the asynchronous communication in both time and place characteristic of e-mail interviews (see, e.g., Opdenakker, 2006). Written interviews, especially on-line through e-mails, are increasingly discussed as a proper research tool by scholars in other disciplines (see, e.g., Meho, 2006). A historian of economics sending a couple of questions by e-mail to an economist and then using (or just acknowledging) the answers in a contribution can qualify as using (written) interviews. This chapter does not focus on, though many of the reflections apply to, this set of practices.

reducing the scientific legitimacy of a discipline. Historians of economics and of science who have reflected on the implications of this issue for the conduct of interviews usually recommend a rather high level of transparency about the goals behind the interview request and engaging the interview in a collaborative spirit while remaining tenacious when conflicts arise (see, e.g., DeVorkin, 1990; de Chadarevian, 1997; Hoddeson, 2006). One objective of the chapter is to characterize how different practices of historians of economics regarding the use of interviews involve the potential perception of a threat to scientific credit and legitimacy – and how that has been (or can be) managed.

The characterizations of historians of economics' practices of using interviews will also draw on contributions from the discipline of oral history, a discipline where historians conduct interviews and use them according to a number of more or less consensual rules (concerning the conduct, critical use and archiving of interviews; see Oral History Association, 2009). Oral historians have produced a considerable amount of reflections on the specificities and historiographical issues of oral sources compared to more traditional written ones (see Perks and Thomson, 2016). One of the main specificity is the role of the relation between the historian and the interviewee in the very existence of the source. As a prominent oral historian puts it:

“The content of the written source is independent of the researcher’s need and hypotheses; it is a stable text, which we can only interpret. The content of oral sources, on the other hand, depends largely on what the interviewer puts into it in terms of questions, dialogue, and personal relationship.” (Portelli 2016 [1979], p.55)

Furthermore, this content expresses meaningful elements that are proper to orality in the sense that they can only be indicated in a written transcript but not reproduced as some words can: “silences, sighing and respiration, laughs, [...] changes of tonality, [...] hesitations”, all of which constitute the rhythm of the speech (Descamps 2005, Part III, Chap. 1, §24)². Oral historians also argue that, when it comes to personal memories about one’s life and experiences, a proper relation between the historian and the interviewee confers to orality the virtue of expressing certain themes better than it would (if at all) have been done in written: “the invisible, the collective unconscious, the imperishable, the secret, the desire, the anxious” (ibid, Part I, Chap. 4, §57). A central preoccupation of oral historians has been the role of various issues with memory such as false or wishful memories, forgetting and the like in the content of oral discourses. Beside pointing that written documents have similar issues,

²Florence Descamps’s book on oral sources is the main resource in oral history from which this chapter draws. It is written in French and all translations are mine. The numbered paragraphs correspond to the ones that are explicitly indicated in the online edition.

oral historians recommend careful scholarly critic of oral sources, especially when they are used to reconstruct the past. Another way of dealing with these issues has been to take them as objects of study, not to reconstruct the past but to explore “the relationship between the present and the past; what the past means now”, i.e., the “history of memory” instead of the “memory of history” (Portelli interviewed by Penner, 2012; for a similar position on the status of memory in history of economics, see Weintraub, 2005). One objective of this chapter is to characterize how these specificities of oral sources are involved in the practices of using interviews by historians of economics.

The chapter is structured in three sections, organized so as to roughly depict an evolution in historians of economics’ practices, whereby three broad types of interview topics emerged as relevant for historians of economics in a certain chronological order: economists and policy-making (section 1), economists and the content of their academic work (section 2) and the careers and lives of economists (section 3). In each section, a few representative examples of practices of using interviews are first described and then characterized regarding the factors discussed above. A conclusion sums up the main points made throughout the sections and propose further or new developments inspired from other disciplines using interviews.

1 Economists and policy-making: the influence of Coats

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, p.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; p.341; 1981c, p.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, p.381). Coats used the documents – which do not seem to be confidential – explicitly (p.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, p.391).

This paper was part of a special issue of the journal *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 Advisor*] 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, fn18p.523). In sum, interviews are a mean for Coats and the others to get access to places where elites make decisions which impact economics or involve economists. What they are after is information about that decision making process³.

A first observation that helps to characterize this set of practices is that Coats’s initial motif for using interviews, i.e., to bypass legal confidentiality restrictions on written material, is a classical justification in political history (see Descamps, 2005, Part IV, Chap.1, sect. 1) and history of contemporary science (de Chadarevian 1997). The main reason is, Descamps (ibid) explains, that the law is not as clear on the oral communicability of confidential information as it is on written communicability. The reader is however left in ignorance as to what exactly was supposed to be confidential. Indeed, the very status of the information as confidential makes it hard to do otherwise and might further inhibit the historians of economics to explicitly comment on the contexts and contents of the interviews. Here the underlying conception of the oral source is the traditional one in oral history, as a “palliative source” (ibid, §2) for written documents to which the historian lacks accessibility or for impressions and information that simply do not exist in any written document (e.g., the impressions from the field or Murphy’s anecdote about Nourse). Indeed, that the interview process tend to lead to other

³I thank Tiago Mata for suggesting this way of summing up what these practices are about. An earlier paper that somewhat shares some of these practices of using interviews is Allen (1977), who profusely cites a number of non-anonymous interviewees, including famous economists (Tobin, Shultz, Kindleberger), about their experiences in policy-making institutions. The historical dimension of his paper is however very limited. Later papers on the same object (economists in policy-making institutions) and using interviews in the same ways as discussed in this section include Yohe (1990), Montecinos (1996), Klein (2001), Maes (2011), McGregor and Young (2013), Teixeira (2017).

non-confidential documents (in Coats's case) and to information not present in any written sources are two classical justifications for the use of interviews by historians of science and of economics in addition to getting around confidentiality restrictions (see Söderqvist 1997, p.8; Hoddeson 2006, p.187; Weintraub 2007, p.5).

The capacity of orality to express that which falls under the theme of *the secret* seems to play a main role in these practices. Interviews allow historians to “break in the cultures of secrecy that surrounds the exercise of power and sovereignty” (Descamps, *ibid*, §4). Hence it is not surprising that the uses of the interviews are in a large part invisible in the texts: no comments on their contexts and contents (only brief acknowledgments of their existence), anonymity of the interviewees and few verbatim quotes giving voices to them. The few cases when we do ‘hear’ the interviewees are restricted to an “illustrative” (or “ornamental”) type of use of oral sources (*ibid*, Part III, Chap.2, sect.1). This means that the content of the quote brings some life and gives some flesh to an argument in the text but does not contribute much to the force of that argument (most of which comes from other evidence and reasoning that are independent of the quote). In sum, even though the interviews are crucial in the research process and in shaping the historical narrative, they are nearly invisible in that narrative.

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 (resulting in the distribution of scientific credit and establishment of scientific legitimacy), is a research theme that can easily generate a perceived threat to scientific credit and legitimacy from the scientist in her or his relation with the historian (see, e.g., Gaudillère, 1997, pp.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 the potentially perceived threat is most reduce 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. Furthermore, these conflicts, as they are described in the papers, threaten the legitimacy of politicians much more than the legitimacy of economics – because economists are described as doing the best they can while remaining unheard by politicians. Finally, the self-produced historical narratives of economists are typically silent on their role in policy-making institutions, which

greatly limits the possibility of a clash with the historians' historical narrative.

Note that the only case of a non-anonymous interviewee, Murphy, does not involve the historian, Barber, as an interviewer. Barber uses instead 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. Although that interviewees should not as far as possible be anonymous is one of these rules, it can be argued that the status of the interviewees with respect to the object of study here 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. The reason is that without such meta-data it is difficult, if not impossible, to proceed to a scholarly critic of oral sources (before their use or about another historian's use). Here, not only historians of economics do not provide access to recordings or transcripts, but the meta-data are nonexistent to a point where the reader does not even know whether the interviews were audio recorded or if references to the interviews come from historians' memories or written notes taken during or after the interview. Breaking these rules is nevertheless not proper to historians of economics because they are also largely broken by most scholars in disciplines other than oral history. Descamps (2005, Part III, Chap. 5, §33) actually suggest that oral historians might be interested to archive interviews from these scholars (if possible with some meta-data)⁴.

2 Economists and their academic work: the influence of Klammer

Compared to the contributions of historians of economics on economists and policy-making institutions, a radically different set of practices regarding the use of interviews was introduced in a book by Klammer (1983). His interviewees were not anonymous: they were famous macroeconomists such as Lucas, Sargent, and Tobin. Their voices were not silenced: they constituted the core of the book as every chapter but the first and last ones were transcripts of the interviews. Furthermore, Klammer interviewed economists *qua* academic economists. As he later admitted, "[t]he initial reason" behind these interviews "was to check my interpretation of their [i.e., the interviewee's] work" (1989). Klammer's

⁴Most Presidential Libraries contain oral history interviews with some economists who were involved in policy recommendations, usually in the *Council of Economic Advisers*. These interviews represent a good part of historians of economics' uses of interviews conducted by someone else. For more information on the Presidential Libraries regarding the history of economics or oral history, see (respectively) Cochrane (1976) and Greenwell (1997).

explicit goal for the use of interviews, at least as stated in the book (i.e., not necessarily as presented to the interviewees), was to study the rhetoric of economists in controversies.

More precisely, Klammer was defending that economics is a rhetorical activity where theoretical and empirical arguments are not the most important elements of persuasion among economists. He presented the interview as a methodological tool suited for making explicit the other, more persuasive, types of arguments and rhetorical processes that are mostly implicit in published contributions: “the conversations convey a vivid personal sense of what is happening in the world of economists” (1983, p.xii). Indeed, 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 controversies⁵. One implication which is fairly rare compared to other uses of interviews (in history of economics and beyond) is the incisive and improvised ways by which Klammer introduced his own opinions about, or counter examples to, the interviewee’s narrative. Sometimes he did so by suggesting his own interpretation – as when he let Lucas know that he (Klammer) found some terms used in a paper with Sargent to be “*very strong*” or “*quite strong*” (p.34). At some other times he did it through others’ interpretations – as in this exchange: “[Lucas:] Everybody likes the idea of rational expectations. It’s hardly controversial. [Klammer:] *But if you talk with Post-Keynesian economists they think it’s a lot of nonsense*” (p.35).

One outcome of his research project is the identification (in the last chapter where he interpreted the interviews) of five sets of elements of persuasion that are either potentially “nonrational” (p.238) and more important than theoretical and empirical arguments or that are influencing such arguments (but not the other way around). The main one is that the clearest contention in macroeconomics is not theoretical or empirical but normative, mostly related to political beliefs about the virtues or evils of policy interventions on the economy (pp.238-240). The other ones are five types of arguments (which he labels “epistemological”, “philosophical”, “commonsense” and “meta”; see pp.245-251 for the details) and one category regrouping “personal and social factors” (pp.254-251) – which he admitted are on the overall relatively scant because he did not put them enough in the foreground of the interviews. Klammer’s interviews also had a historical dimension in two senses: (1) they were carefully put into historical context by both the introduction and the first questions in each of the interviews and, more trivially, (2) they were historically situated (i.e., in the early 1980s). As such, they can be used by historians of macroeconomics (e.g., Sent 1998; Rancan 2017, p.169).

⁵Here again I thank Tiago Mata for suggesting me this way of putting Klammer’s work.

A number of historians of economics have, explicitly or unknowingly, followed various subsets of Klammer's practices of using interviews. The remainder of this section focuses primarily on characterizing the methodological and historiographical issues raised by Klammer's practices while the practices of his followers are characterized indirectly, by means of comparisons. It can be argued that the main goal behind Klammer's interviews is akin to the main goal behind the use of interviews by most anthropologists (e.g., Spradley, 1979, Chap. 1) and some historians of science (see Doel, 2003, p.358): making explicit what is tacit, i.e., taken-for-granted, in a given culture or subculture. Most of the elements of persuasion identified by Klammer are not explicit in the written texts of his interviewees. These elements are nonetheless necessary for a non-member of the subculture of academic macroeconomics to understand the dynamics of intellectual exchanges through written texts in that subculture. Here the underlying conception of oral sources can be thought of as a specific variant of the palliative one (as characterized by Descamps and presented in the previous section) for the scientific domain. Interviews are used to uncover information which are implicitly there in the written sources but not directly accessible to the reader (especially when he or she is not part of the subculture).

Compared with Klammer, other historians of economics who have used interviews for a similar purpose tend to let economists tell their narratives without intervening too much into it. Hence the set of tacit presuppositions potentially revealed by the discourse is narrower, usually restricted to the theoretical, empirical and policy domains (e.g., Snowdon and Vane, 1999; Colander et al., 2004). Furthermore, these contributions do not provide an equivalent to Klammer's concluding essay where the historian interprets the interviews and argues what can be made explicit from them. That is left to the reader – or to another historian, e.g., to Blaug in Snowdon and Vane, (1999, pp. 314-333). There are however some contributions that are in Klammer's spirit and that even bring some innovations to his practices. One example is in a book by Sent (1998) on the role of Sargent in the history of rational expectations. Her last chapter before the conclusion is an interview with Sargent where she confronts him with the methodological and historical points made in the preceding chapters. She however does not subsequently interpret or analyze that interview (e.g., in the book's conclusion). Another example is Halsmayer (2014), who 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” (p.231) opened the door for further methodological and historical characterization by Halsmayer.

It can be argued that the capacity of orality to express that which falls under the theme of *the*

invisible plays a main role in these practices. However, the existence of reflexive essays on scientific contributions by scientists and historians alike suggests that written expression can make the implicit explicit just as well. A nontrivial difference is that what is made explicit in an interview is not the result of either the scientist or the historian alone, but of their conversation, i.e., of a concrete human interaction that goes beyond the reading of one another. Indeed, some anthropology-minded sociologists argue that the intonations, body language, facial expressions and the like of even the most careful interviewer will always leak interpretations influencing the interviewee's responses (Holstein and Gubrium, 2004; Gubrium and Holstein, 2012). One implication, they argue, is that interviewers should embrace more "active" styles of interviewing, which roughly correspond to Klammer's. Most oral historians (e.g., Portelli in the introduction) and historians of science using interviews (e.g., de Charadevian, 1997) share the starting point of this reasoning. However, only historians of science, as some like to point out (esp. Hoddeson, 2006), tend to follow its implication in terms of interviewing style.

This issue of more or less intervention from the interviewer carries over to the type of use of oral sources made by the historian. Unlike in the previous section, how we 'hear' the interviewee's voice is here not merely "illustrative" (or "ornamental") in Descamps' sense. It is rather one of the two other types she identifies. An "expressive" (or "restitutive") use of oral sources consists mostly in long quotes with very little contextualization and very little critical perspective on the content of these quotes (Descamps, 2005, part III, Chap. 2, sect. 2). Historians who present interview transcripts as book chapters, which is akin to a very long quote, do usually contextualize for the content of the interview but not for its context: a few elements about locations, time and date are usually offered but nothing is provided on the relation between the interviewer and interviewee, e.g., about the presentation of the goals behind the historian's research project. And as already mentioned in the previous section, that kind of meta-data are to perform a scholarly critic of the content of the interviews. Descamps (ibid, sect. 3) favors another type of use of oral sources that she labels the "in-depth use", which is very demanding in terms of the analysis and critic of content to end up in a balanced interplay between quotes that are relevant according to the critical analysis and comments from the historians on these quotes. Klammer's concluding essay partly fit this description because of the balance between the quotes and his analysis.

Because the historical narrative produced by Klammer suggests that political beliefs and other potentially "nonrational" (1983, p.238) elements of persuasion are more important than – or influence

– empirical and theoretical arguments in macroeconomics, it can obviously generate a perceived to the scientific credit of macroeconomists or to the scientific legitimacy of macroeconomics. For the ideal of a value-free science is usually part of the historical narrative self-produced by most economists (see, e.g., Blaug, 1998; Putnam and Walsh, 2011; Hands, 2012; Castro Caldas and Neves, 2012; Reiss, 2017). Indeed, when questioned directly about this theme, Klamer’s interviewee usually deny any political motivation in their contributions to economics (e.g., Sargent, p.80) or express the belief that a requirement for the scientific legitimacy of economics is that normative considerations about the economy ought not to influence theoretical and empirical propositions (though the other way around is acceptable) (e.g., Lucas, p.52). How does Klamer manage the potential perception of a threat to scientific credit or legitimacy that his historical narrative might create? First, the explicit purpose of the interview is probably not presented to the interviewees as being the production of such a narrative⁶. Second, it can be argued that the very potentiality of a perceived threat to scientific credit or legitimacy is the key element in Klamer’s practices. Indeed, setting economists into the position of arguing is well achieved in Klamer’s case by tackling controversial topics head on after a having established a good rapport through some biographical questions. In this practice, what seems to loom large is Klamer’s conversational skills. Finally, the other historians of economics mentioned in this section do not have to worry so much about the perception of a threat to scientific legitimacy, either because what they want is a personified account of the historical narrative self-produced by economists or because they engage in an explicitly more collaborative way with their interviewee as in the case of Sent and Halsmayer. Such collaboration takes a very scholarly form in Halsmayer’s case by Solow’s providing comments on an earlier draft of her paper (2014, p.229).

One can interpret the publications of interview transcripts as chapters to be a step toward oral history of economics, as Klamer himself suggested (1991, p.131). Although this cannot be considered as proper archiving in oral historians’ sense, it can at least constitute sources for other historians to check or to work on. Proper archiving in oral history, as already mentioned in the previous section, also involves the conservation of the audio or video recordings along with meta-data about the relation between the interviewer and the interviewee. These practices can be relevant if the goal of the interview is to make explicit what is implicit in discourses because specific oral characteristics such as tonality, silence, laughs (and eventually body language in video recordings), are helpful for interpretation. Notice here the complicated relation between the written and the oral. The use of interview is first

⁶Note for the present version: I plan to get extra information about this by contacting Klamer and by looking at Lucas’ archives where I am told that there is the exchange between Klamer and Lucas pertaining to their interviews.

motivated by an issue with written contributions, which contain too much implicit elements to fully understand an intellectual dynamics from an outsider yet informed perspective. Whether or not the oral conversation makes the implicit explicit, the end result is again a written text (interview transcripts and their interpretations), on which both the interviewer and more problematically (Weintraub 2007, p.3) the interviewee have intervened *ex post* by means of written expression, i.e., transcripts are usually edited by interviewees before being archived or published.

3 The careers and lives of economists: the emergence of oral history of economics?

Another set of practices of using interviews in history of economics emerged at the end of the 1980s, with historians of economics focusing more on the historical context in which economists produced their academic work than on their academic work *per se*. More precisely, they focused on the lives and careers of economists – and other scholars related to economics – as contexts in which the meanings and histories of their academic work can be enriched. By comparison with Coats or Klammer in the previous sections, there is no central figure here who motivated subsequent followers to use interviews in a certain way.

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 emigration. 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 example, Leonard's (1992) account of the development of game theory was partly based on interviews he conducted (not with economists but) with mathematicians who worked at RAND in the late 1940s. Although the voices of his interviewees are heard in his later 2010 book, they do not surface in the 1992 paper. Instead, the interviews are noted as sources to support claims about matters such as mathematicians first learning about game theory well after von Neumann and Morgenstern's book (1992, p.59) or about RAND's internal organization (p.67).

In yet 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, pp.115-117). The interview, which is fully transcribed (ibid, pp.125-154), 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. Düppe (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. Düppe 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 in this chapter has written reflections on her or his own practice, i.e., on the specificity of constructing or using oral sources⁷. 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. For instance, he explains that his goals were to explore how economists from a common group would share the same (present) sense of the past and to get an understanding of this sense that would guide him into the written literature. He justifies how these goals lead him to choose, among the different methods of interviews available across disciplines, the life story interview from oral history. In that kind of interviews, the interviewee recounts his or her life under minor guidance from the interviewer and following a more or less chronological order. He also explains as well 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 further reflections)⁸.

Emmett (2007) also furnishes a reflection on the process of his oral history project on Chicago economics. One of his goals behind his interview project is worth noting: giving voices to the people that do not leave much written traces and are absent from histories focused on eminent economists. The closest he considers to have gotten to achieve this goal is by interviewing Marianne Ferber, one of the rare woman to obtain a PhD in economics at Chicago in the beginning of the postwar period (1954). Furthermore, Emmett explains how he was interested in the fact she did not self-identify with Chicago economics, which allowed to provide multiple perspectives on the Chicago department of economics.

⁷This remark is not intended as a criticism. If anything it is meant to highlight how natural interviewing seems when doing history of *contemporary* economics. Freedman (2010) discusses how unprepared he was when engaging, in the late 1990s, on a oral history project about Stigler. Reading some methodological literature from oral history afterward, he judges that it would not have changed his interviews much if he had done so beforehand.

⁸Among the other details present in the methodological and historiographical discussion are how the social encounter of the interview afforded him access to some archives; or how he prepared himself for the interview, notably by reading other interviews, especially Klammer's.

Among the other details worth noting is how he sent questions in advance to the interviewees.

Finally, Svorenčík (2015, Appendix) provides a discussion of the methodological and historiographical issues raised by the more than fifty interviews he conducted with experimental economists and used to reconstruct a history of experimental economics. He explains that, unlike Mata, his interviews were “not traditional oral interviews in the sense of deeply personal accounts” (Svorenčík, 2015, p. 246). Yet his “mixed focus [...] on the social history of experimental economics, the interviewee’s perspective and participation in it, and their intellectual trajectories” (ibid) greatly overlaps with the focuses of the other historians of economics discussed in this section. He also used the opportunity of the interviews to convince experimental economists to deposit their papers in archives and (explicitly) to gather materials in view of a subsequent witness seminar (see Svorenčík and Maas 2016; Maas, this volume). The main historiographical issue with which he is preoccupied is to avoid the potential biases he might have created as a historian interviewer in his interviews to carry over to the historical narrative he constructed. That is why he explains the critical stance he had on the content of his oral sources in at least two respects. Firstly, he tried to cross-check information with archives and across interviews. Secondly, he gave priority to written archival sources when available to establish a point in his narrative. Finally, he emphasizes how establishing and maintaining trust with his interviewees in even the tiniest social interactions was constantly a crucial issue.

Is there a common goal behind all these apparently quite diverse practices of using interviews discussed in this section? One can be identified by contrast with the previous section: to get a better sense of the “personal and social factors” which Klamer (1983, pp.254-251) admitted to have left too much in the background of his interviews. That goal is indeed shared with most historians of science inspired by the field of science and technology studies who have used interviews or commented on their potential (Doel, 2003, pp.357-363). This is quite broad a goal, which is declined in a variety of nuances on at least two dimensions. The first dimension is whether the historians’ primary object of study is a group of economists (Craver, Mata, Svorenčík), an individual economist (Weintraub, Dütte) or an institution in economics (Leonard, Emmett). The second dimension is how the historian’s focus is balanced between the personal and the social factors. The underlying conception of oral sources here goes beyond the traditional palliative one in Descamps’ sense when the information uncovered or produced by the interviews are not supposed to be part of the published scientific record (or of any written record about the historian’s object of study). Hence it is better characterized by either one or both of the other two conceptions she distinguishes: oral sources as a basis for enriched biographies

and socio-biographies (2005, Part III, Chap. 2) and oral sources as a basis for histories of organizations (ibid, Chap. 3). Indeed, interviews can be a mean of saving time for the collection of either biographical or organizational information that could be found in the archives – or at least as a guide for what to look for in the archives. But most importantly it can be the only mean to gather personal impressions and psychological feelings that are harder to find in the archives – hence enriching a biography or socio-biography based on written sources only.

The specificity of orality that plays a role in these practices is its capacity to express a range of themes, from *the collective unconscious* to *the intimate* (Descamps, 2005, Part I, Chap. 1, §29), depending on one’s goals behind the interviews. In a sense, oral discourse here compensates for the lack of expressions in written about social relations, institutional contexts and personal matters, which the interviewee cannot – or does not naturally – put in print. The main type of use of these oral sources here is in-depth: either in Descamps’ sense of a balanced use of quotes and the historians’ comments on it (sometimes even with explicit critical scrutiny of the content, e.g., Svorenčik) or in another sense of the historian impregnating herself or himself ‘in-depth’ with the interviews in order to influence the historical narrative produced – e.g., Mata performing “repeated listening [of] the recordings” (2005, p.289). Note that Mata and Emmett also display two variants to the palliative use, which are common in different areas of history. By using interviews as guides to explore the published record, Mata (2005) is in a sense employing an anti-palliative use of interviews, i.e., not because there is too little written documents but too much. This is a classical justification from historians of science to deal with the “documentary overload” (Söderqvist, 1997, p. 4) characteristic of the contemporary period (see de Charadevian, 1997; Hoddeson, 2006). And by using interviews to hear the voices of non-eminent economists, Emmett is employing the traditional palliative use but from social history instead of political history, i.e., elites’ voices completing the written records. However, intellectuals such as Ferber, a prominent feminist economists who left plenty of written sources could still be considered as an elite from oral historians in social history. Relevant for historians of economics and potentially considered as non-elites interviewees could be the secretaries, engineers, technicians and the like (Doel, 2003, p.359 hints in that direction, though he seems to want to get information about their roles not necessarily by interviewing them).

The potentially perceived threats to scientific legitimacy and scientific credit involved in the relation between the historian and the interviewee are here minimal because the focuses of the interviews are not primarily on the content of scientific contributions. 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. If any threat is perceived in the final narrative of the historian that is more likely to be due to interpretations of written sources (especially when archives are involved). However, 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). Descamps (2005, Part II, Chap. 3, §23) argues that the very format of life story or career story interviews, in which interviewees tell a more or less chronological narrative about himself or herself, provides a natural way of managing the potentially perceived threat to integrity or reputation. Her argument is that, by contrast to the more thematic format of interviewing (akin to those used in the previous section), the interviewee is here considered primarily as a person and secondarily as a source. She argues that, in the beginning of the interview, apparently trivial questions about childhood or education create a dynamics of reciprocity whereby the interviewee progressively opens up by giving away elements of his or her life and the interviewer gives back some help for the reconstruction of a life or a career. Once that dynamics impulsed, it is easier to explore in depth potentially more delicate themes with minimal potentially perceived threat.

Finally, it should be noted that some of the historians of economics discussed in this section have archived the transcripts and sometimes even the recordings of their interviews. For instance, most of Craver's interviews were conducted by her and/or her husband Axel Leijonhufvud as part of an oral history project at UCLA's *Center for Oral History*. The transcripts are still available there, some can even be found on-line⁹. Weintraub also archived in his papers at the Rubenstein Library of Duke University both the tape and the transcript of his interview with Debreu. The main historiographical issue raised by this common practice in oral history (and it is raised by oral historians themselves) is that if the goal of the interview is to capture the intimate then the fact that the record of the conversation might be public or semi-public will evidently constrain the interviewee to open up (Düppe, this volume; Maas, this volume; Descamps, Part II, Chap. 2, §55). One traditional way of dealing with this is to emphasize that the recording can be stopped at any moment if anything want to be said off record or that any sequence can be deleted *ex post* from the archived record. In that case these information can still impregnate the historian's narrative but they cannot (legally) figure explicitly within it¹⁰.

⁹see for instance Hayek's transcript: <https://archive.org/details/nobelprizewinnin00haye> last consulted on 06/12/17)

¹⁰See 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> (last consulted on 10/06/17).

Conclusion

It seems that there has been an evolution over the last forty years in the history of economics regarding the use of interviews whereby 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. The metaphor of quantum indeterminacy is sometimes used to characterize the interview as a research tool (DeVorkin, 1990; Hoddeson, 2006): the very act of observing the material (here through interviews) influence the information one can get from the interview in unpredictable ways. Indeed, the complexity of interactions among all the parameters that define the relation between the interviewer and the interviewee is so great that the outcome of any interview is indeterminate, at least from a theoretical perspective – only practice and experience can reduce the indeterminacy.

Indeed, the diversity of parameters defining the relation includes, for instance, the preparation of the interviewer, the goals of the research project, the personalities of both the interviewer and the interviewee, the location of the interview, the way the interviewer dresses up, speaks, uses body language and so on. Obviously, 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.

One key issue is worth mentioning by way of conclusion, namely an eventual 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, p.39), i.e., reduces the potentially perceived threat to scientific credit and legitimacy. One can hope for an equivalent initiative sponsored by the *American Economic Association* or another similar institution (maybe in Europe) to launch such a project (Weintraub, 2007, has already expressed something similar to this hope). If that were to happen, one can further hope that it would take full advantages of the combination of digital technologies with the internet, which has led to the last paradigm shift in oral history according to Thomson (2007) and is well discussed by Boyd (2016 [2014])¹¹.

¹¹On the *American Institute of Physics* oral history project, see <https://www.aip.org/history-programs/niels-bohr-library/oral-histories> (last consulted on 06/13/17). Regarding economics, it is worth noting the existence of the *Economist Life Stories Project* at the University of Berkley: <http://www.lib.berkeley.edu/libraries/bancroft-library/oral-history-center/economist-life-stories-project> (last consulted on 10/06/17). It should also be noted that Boyd is involved in the *Oral History in the Digital Age* project, the website of which is an invaluable resource to see the potential of digital technologies for oral history and get some training to take advantage of it: <http://ohda.matrix.msu.edu/> (last consulted on 06/10/17).

Appendix of Personal Frustrations with this First Draft

Here are a couple of frustrations about things I wanted to do or include but did not mostly due to time or space constraints. I would be more than happy to have the reader's opinion regarding whether I should try to include or do some of these things (and if possible, what to cut elsewhere in order to make space for that).

1. I have contacted several historians of economics to get some insights regarding the “behind the scenes” of their interviews but I have not used them in the current version yet and I wanted do so more systematically by ‘interviewing the interviewers’ but I did not really started that. Maybe keeping it to personal communication by e-mail will be sufficient...
2. Include an appendix of the few *concrete* ‘tips and tricks’ for interviewing one can find in the methodological and historiographical writings?
3. Include (as another appendix?) what concretely that means to make a critic of the content of one’s oral sources by reproducing Descamps’s (2005) and Yow’s (2005) sets of questions one should ask himself or herself about his or her interviews to be critical of it?
4. Include footnotes in sections 2 and 3 with a number of other references corresponding to the use of interviews discussed in these sections, as with footnote number 3 in section 1? (but that is going to make for a very long bibliography).
5. In section 2, discuss the issue of “informed” vs “naive” interview as raised by some scholars in STS (how much shall the interviewer knows about the science?), who furthermore argue that interviews are not good to explicit the tacit (observations of practices are better)? (here it seems the historians of economics and of science have find ways of partly proving them wrong).
6. Say something about Klammer on *conversation* as a better metaphor for the economy than *market*.
7. Say a word on Colander’s and Klammer’s interviews with (anonymous) young economists, which they did to interpret quantitative data from surveys they conducted about the making of an economist (is this still history of economics?)? (this at least allow to raise more broadly the point that interviews can be used to complement quantitative data, and hence cross referencing Claveau and Herfeld from the volume).
8. Say more about the traditional oral historian’s focus on working on ‘what the past means now’ (as opposed to the past *per se*), especially for Craver’s and Tiago’s work?
9. Discuss non-historians of economics who have used interviews to make contribution to the history of economics (e.g., Marion Fourcade, Donald McKenzie)?
10. Say more about legal issue because they do raise historiographical issues? (another appendix?).
11. Discuss the potential of Skype as a natural (non intimidating way) of recording video interviews?

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