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INTERVIEWS: SOME METHODOLOGICAL AND
HISTORIOGRAPHICAL ISSUES
OF ORAL SOURCES

BY

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Interviews: Some Methodological and 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.

Keywords: history of economics, history of economic thought, historiography, oral history, interviews

JEL: B00, B20, B29, B30, B40

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 interviews¹. 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 valuable 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, both of which aims at better characterizing the practices of using interviews in the history of contemporary economics. Firstly, the chapter covers more contributions in the history of economics to show how the similarities and differences in these practices bear on a specific historiographical issue, namely that the research project for which interviews are used might be perceived by some scientists as involving a potential threat to scientific legitimacy (section 1). Secondly, the chapter puts these practices in perspective with a larger diversity of methodological and historiographical contributions from other

¹The quick search 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 standard case is with one interviewer and one interviewees, although there can be a few cases where there are two interviewers or two interviewees. Cases with more than two interviewees are usually not considered as interviews *per se* in the literature, but as focus groups or witness seminars (on the latter, see Maas, this volume).

disciplines using interviews, sometimes suggesting further or new developments (section 2).

The goal of this chapter is not to provide a list of ‘dos and don’ts’ regarding the use of interviews for historians of economics. Indeed, there seems to be no issue on which there is a consensus among scholars who have written on the methodology and historiography of interviews – good practices are highly dependent on context (including the personalities of the interviewer and interviewees) and epistemological positions. The goal is instead to emphasize both on issues one needs to be sensitive of when using interviews as oral sources by contrast with more traditional written sources and on the interaction and interdependence between oral and written sources.

1 Practices of using interviews in the history of economics and potential threats to scientific legitimacy

A research project for which a historian uses interviews might be perceived by some scientists as involving a *potential* threat to their scientific credit, scientific reputation or even to the scientific legitimacy of their discipline. This comes from the broader issue that the history of contemporary science (including economics) 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., Klammer, 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 reducing the scientific legitimacy of a discipline. This section characterizes how different uses of interviews by historians of economics involve the potential perception of a threat to scientific reputation, credit or legitimacy – and how that has been (or can be) managed. It is organized around a number of representative examples that reflects an evolution in history of economics, whereby three broad types of interview topics emerged as relevant for historians of economics in a certain chronological order: economists and policy-making (1.1), economists and the content of their academic work (1.2) and the careers and lives of economists (1.3).

1.1 Economists and policy-making

In 1978, A. W. 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 S. Ambirajan (1981), P. R. Haddad (1981) and A. 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, William 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, fn18p.523)². In sum, interviews are a means for Coats and the others to get access

²Most 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) James Cochrane (1976) and Regina Greenwell (1997).

to places where elites make decisions which impact economics or involve economists. What they are after is information about that decision making process³.

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 the anonymity of the interview citations, their scarce use, and the use of interviews conducted by other scholars might mitigate this potentially perceived threat. Yet it can be argued that the way by which the potentially perceived threat is most reduced is by the very 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.

1.2 Economists and their academic work

Compared to these contributions on economists and policy-making, a radically different set of practices regarding the use of interviews was introduced in a book by Arjo Klammer (1983). His interviewees were not anonymous: they were famous macroeconomists such as Robert Lucas or Thomas Sargent. 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.

More precisely, Klammer was defending that economics is a rhetorical activity where theoretical

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 William Allen (1977), who profusely cites a number of non-anonymous interviewees, including famous economists (James Tobin, Charles 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 William Yohe (1990), Verónica Montecinos (1996), Judy Klein (2001), Ivo Maes (2011), Rob Roy McGregor and Warren Young (2013), Pedro Teixeira (2017).

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 controversies⁴. 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). Note that 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).

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., Snowden 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 Mark Blaug in Brian Snowden and Howard 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 Esther-Mirjam 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 Verena Halsmayer (2014), who conducted an interview

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

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.

These historians who have implicitly or explicitly followed part of Klammer's interviewing practices 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). By contrast, 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. Hence it can obviously generate a perceived threat 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., Hands, 2012). Indeed, when questioned directly about this theme, Klammer'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 Klammer manage the potential perception of a threat to scientific credit or legitimacy that his historical narrative might create? First, the explicit purpose of the interviews is probably not presented to the interviewees as being the production of such a narrative. Second, it can be argued that the very potentiality of a perceived threat to scientific credit or legitimacy is the key element in Klammer's practices. Indeed, setting economists into the position of arguing is well achieved in Klammer's case by tackling controversial topics head on after having established a good rapport through some biographical questions. In this practice, what seems to loom large is Klammer's conversational skills.

The contributions discussed here strongly highlight the role of the interviewer in the outcome of the interview. Most of the practices discussed here involve a conversational tone to the interviews and the production of narratives. That is not usually the case in other interviews of economists conducted by scholars who are not (at least primarily) historians of economics. For instance the interviews

conducted by George Feiwel (1987a; b) with or about Kenneth Arrow have a structure that is closer to a ‘Question and Answer’ (Q&A) session one can observe at the keynote speech of a conference. The answers are however more reflective than in an actual Q&A session and hence are often of some value for methodologists and philosophers of economics – as are interviews of economists by economists periodically published in, e.g., *Econometric Theory*, *Social Choice and Welfare* or *Macroeconomic Dynamics*.

1.3 The careers and lives of economists

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.

An early instance of this practice was Earlene 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., Friedrich von Hayek, Oskar Morgenstern) and two mathematicians (Franz Alt and Karl 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, Robert 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 John von Neumann and Oskar Morgenstern’s book (1992, p.59) or about RAND’s internal organization (p.67).

In yet another fashion, E. Roy Weintraub conducted an interview with Gérard 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. Till 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 in this chapter has written reflections on her or his own practice, i.e., on the specificity of constructing or using oral sources⁵. By contrast, Tiago 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 the discipline of 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. Mata also explains 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).⁶

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

⁵This remark is not intended as a criticism. If anything it is meant to highlight how natural interviewing seems when doing history of *contemporary* economics. Craig Freedman (2010) discusses how unprepared he was when engaging, in the late 1990s, on a oral history project about George 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.

⁶Among 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. See also the video oral history on Crauford Goodwin by Tiago Mata and Harro 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).

Finally, Andrej Svorenčák (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” (Svorenčák, 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 Svorenčák 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.

The potentially perceived threats to scientific reputation, credit or legitimacy in these contributions here is minimal because the focus of the interviews is 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 subsections, there is here a greater potentially perceived threat to personal integrity and individual reputation that the historian has to subtly manage (Düppe, this volume).

As oral historians (whose work are discussed in the next section) often argue, the very format of life story or career story interviews, in which interviewees tell a more or less chronological narrative about herself or himself, provides a natural way of managing the potentially perceived threat to integrity or reputation (see Descamps, 2005, Part II, Chap. 3, §23). By contrast to the more thematic format of interviewing (akin to those used in the previous subsections), the interviewee is here considered primarily as a person and secondarily as a source (ibid). 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 (ibid). Once that dynamics impulsed, it is easier to explore in depth potentially more delicate themes with minimal potentially perceived threat (ibid).

2 Methodological and Historiographical Issues of Oral Sources

This section further characterizes the three families of practices identified in the previous section by analyzing the relation between oral and written sources more closely and by drawing more substantially on insights from other disciplines using interviews. It focuses on how oral sources are used in written contributions (2.1) and how that implies different conceptions of oral sources (2.1). Special attention is given to oral history, which consists in historians conducting interviews and using them according to a number of more or less consensual rules (see Oral History Association, 2009). Illustrations of these rules and how they are (implicitly or explicitly) violated or not by historians of economics are provided throughout. It should be noted at the outset that not many historians of economics use interviews as oral historians (or practitioners in other disciplines using interviews) do. This section is not a criticism of this state of affairs because it takes practices from outside as sources of inspiration, not as constraints – which also implies that which practice comes from which discipline will not be systematically detailed.⁷

2.1 Specificities of orality and uses in written

Oral speech can express meaningful contents 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). Furthermore, 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). How are such specificities involved in the practices of using interviews by historians of economics?⁸

⁷It should also be noted that “oral history” is notoriously polysemic as it can be used to refer to an academic discipline (this is the only sense in which it is used in this chapter), to an interview conducted according to some rules formalized in that discipline, to the transcript of such interviews or to written productions from that discipline (e.g., books or academic papers by oral historians).

⁸Florence 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.

The capacity of orality to express that which falls under the theme of *the secret* seems to play a main role in the practices of those who study the relation between economists and policy-making. 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 written texts on economists and policy-making: 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 in Florence Descamps’ sense (*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.

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 the practices of historians who study the content of economists’ academic work. 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 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. 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 provide a few information to better understand the content of the interview but not much, if at all, to get a sense of 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. 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 (1983, chap.13) concluding essay partly fits this description because of the balance between the quotes and his analysis.

The very publication of interview transcripts as chapters can be interpreted as partially meeting oral historian’s rule regarding the archiving of interviews. On the one hand, such chapters undeniably constitute sources that other historians can check or work on – even though oral historians usually archive their transcripts in specific institutions often hosted in university libraries. On the other hand, however, oral historians usually insist that, in principle, proper archiving requires the conservation of the audio or video recordings along with meta-data about 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). The historians of economics interested in economists and policy-making do not provide access to recordings or transcripts and 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. Those interviewing economists about the content of their academic work, by contrast, do provide an access to the transcripts but not to the recording and not much, if at all, to any kinds of meta-data. It should be noted that, in practice, oral historians (and of course scholars from other disciplines using interviews) also break these rules, which are more akin to guiding principles than to strict procedures.⁹

Nevertheless, if the goal of an 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

⁹Svorenčík and Maas (2016, chap. 1) and Maas (this volume) offer a thorough and innovative discussion of the transcription process and the necessary choices one has to make in this process (they are discussing this regarding witness seminars but their points apply just as well to interviews).

recordings), then recordings and meta-data could be very helpful for interpretation. Notice here the complicated relation between the written and the oral. The use of interview is first 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. As this is usually done through written correspondence, imposing a demanding standard of sharing interview transcripts but not private correspondence (which is not typically demanded for publication) might discourage historians of economics to use interviews.

Finally, the specificity of orality that plays a role in the practices of the historians interested in the lives and careers of economists 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 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., in the work of Svorenčík) 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).

It should be noted that some of the historians of economics working on the lives and careers of economists have archived the transcripts and sometimes even the recordings of their interviews. For instance, most of Craver’s interviews were conducted by her 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 and some can even be found on-line¹⁰. 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 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-

¹⁰See for instance Hayek’s transcript: <https://archive.org/details/nobelprizewinnin00haye> last consulted on 06/12/17)

public can 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 archival record. In that case these information can still impregnate the historian's narrative but they cannot legally figure explicitly within it. It is usually under-appreciated that (at least in France and in the U.S) the interviewee is the legal owner of the interview and should sign a release form (besides the inform consent) to transfer his property rights to the interviewer (see, e.g., Descamps, 2005, Part II, Chap. 4, Sect. 2). Nevertheless, that procedure does not dispense the historian of the moral choices that needs to be made when information about one's privacy are turned into public discourse, which is obviously an ethical problem (see Düppe, this volume).

2.2 Different conceptions of oral sources depending on research goals

The initial motif of historians interested in economists and policy-making (e.g., Coats) 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. In the papers on the history of economists and policy-making, 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” (Descamps, 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 tends to lead to other 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).

In this set of practices, 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. That interviewees should not as far as possible be anonymous is indeed one of oral historians' rules. It can however 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. Using anonymous interviews has nevertheless raised criticisms by (at least one) historians of economics who argued that not knowing who speaks prevents the reader from assessing the reliability of the speech (Tribe 2011, 621).¹¹

The historians of economics interested in the content of economists' academic work can be thought of as displaying a variant of the palliative conception (as characterized by Descamps) for the scientific domain. Recall that these historians use interviews to uncover information which are implicit in the written sources but not directly accessible to the reader (especially when he or she is not part of the subculture). The main goal behind their 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. For instance, 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.

Is there a common goal behind all the apparently quite diverse practices of using interviews by those who study the lives and careers of economists? One can be identified by contrast with the previous two research goals: to get a better sense of the "personal and social factors" which Klammer (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 (as in the work of Craver, Mata or Svorenčík), an individual economist (as in the work of Weintraub and Dürpe) or an institution in economics (as in the work of Leonard or 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

¹¹It should be noted that Keith Tribe's target is a book by sociologist Marion Fourcade (2009), which can indeed be taken as a contribution to the history of economics but not by a self-identified historian of economics. Including books and articles by scholars such as Fourcade in this chapter would have taken too much space and would not have added substantially different practices of using interviews from the ones already discussed, hence the decision to narrow the scope on self-identified historians of economics.

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). It is better characterized by either one or both of the other two conceptions that Descamps 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 means 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. One risk looming larger on these conceptions of oral sources than on the other (palliative) conception is that the interviewer is only going to get well rehearsed myths and (self-)narratives from the interviewers. Most methodological and historiographical advice one can find about how to avoid this are usually very pragmatic, e.g., be well documented on such well-known myths or narratives and use your conversational skills to get something new. Some historians also recommend bringing written documents (e.g., archives) that contradicts the myths or standard narratives to the interview. The idea is to eventually expose these documents to the interviewees and engage in a discussion about their meanings (see Hoddeson, 2006 for an illustration in the history of science). In a sense this partly turns the individual critical work that historians are supposed to perform when using oral sources in-depth (e.g., the triangulation with archival and published written sources done by Svorenčík) into a collective endeavor with the interviewee.¹²

Note that Mata and Emmett also display respectively two variants to the palliative conception, 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). This very function of interviews make them more akin to guides for existing written sources or generators of leads for one's research rather than sources in themselves. In a sense conducting oral interviews for this purpose is not so different from more traditional written interviews whereby historians of economics correspond with economists –

¹²It seems that the use of interviews is naturally related to questions pertaining to the status and limits of biographies in the history of economics as witnessed by the frequent discussions of interviews in the 2007 annual supplement to *History of Political Economy* on Economists' Lives (Weintraub and Forget 2007).

and acknowledge or eventually quote such correspondence in the final written research output. For instance, such written exchanges were part of the initial historical work on general equilibrium by Weintraub (1983), who later used interviews for another purpose as we have seen.¹³

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., capturing non-elites' voices from those who leaves no written traces instead of capturing elites' voices to complete 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, research 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).

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

¹³Scholars 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.

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.

By way of conclusion a key issue already raised by Weintraub (2007) is worth re-emphasizing here: historians of economics could be inspired by historians of physics who are running a large scale oral history project whereby they share recordings and transcripts of interviews on-line. This institutionalized oral history project is hosted by the *American Institute of Physics*, which 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. If that were to happen, it should be pointed out that there are many ways to embed such a project more into the so-called digital humanities than is the historians of physics’ project, i.e., taking full advantages of the combination of digital technologies with the internet to build interactive video archives. On the one hand, there are good guides to do it as this corresponds to the last paradigm shift in oral history according to Alistair Thomson (2007) and is well discussed by Douglas Boyd (2016 [2014]). On the other hand, this might help the whole field to grow as there is an increasing amount of grant opportunities focused on the constitution of archives and the digital humanities.¹⁴

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¹⁴On 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 (with plenty of practical advice on every stage an interview process) 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).

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