Embracing the Personal

And Tackling the Contemporary in the History of Economics

Till Düppe[1]

August 2017

Abstract: This essay reflects on historiographical questions that are related to the fact that we write history of that which is still remembered by some among us. At a theoretical level, I propose a framework that is necessary for fully embracing the presence of the personal in science: the scientific self, a concept at the center of what I discuss as *lived epistemology*. Trying to write histories of the economist's self, in practical terms, poses questions of harmony and conflict of interests. I argue that the question of bias that overshadows the historiography of the contemporary is a *Scheinproblem* that appears if one avoids the underlying ethical questions when assuming the personal.

Key-Words: scientific self, *Zeitgeschichte*, lived epistemology, interest conflicts, existential meaning of science, oral history.

Word count: ca. 8000

[1] Assistant Professor, Department of economics, Université du Québec à Montréal, Pavillon des Sciences de la gestion, 315, Rue Sainte-Catherine Est, Montréal (Québec), H2X 3X2, Canada, e-mail: duppe.till@uqam.ca.

Embracing the Personal

And Tackling the Contemporary in the History of Economics

Introduction

Contemporary history, as a field, emerged in the post-War period in response to the experience of the 20th century as one of 'extremes' (Rothfels 1953, Catterall 1997). It is that branch of history that deals with the past that is still remembered by living persons. Contemporary history, *Zeitgeschichte* in German, is history that is still time - that is, lived memory that is not yet and may never be settled in clean facts and hard documents. It is marked by open wounds, distorted by manipulations, haunted by unsettled conflicts, anxieties to fall into oblivion, oppressed memories, disappointment, melancholy, grief, and other feelings attached to the personal past of our parents' and grandparents' generation. It is history that concerns us. While historiographical issues of preceding periods mainly involve how and in what terms we can bring back a past that is foreign to us today, contemporary history has to deal with these sentiments and negotiate them between generations.

And so does the history of contemporary science. As difficult as the relationship might be between modern masters of knowledge and the ephemeral stream of time, scientists must also rely on being remembered. And as elsewhere, emotions run high when negotiating what will be forgotten and what will endure. In fact, the very struggle over the validity of truth, in practice, comes down to a struggle over what is remembered in science. It is in the midst of this struggle that the historian of contemporary science adds his contribution to the production of collective

memory. The presence of those relevant to contemporary science can be as virtual as an imagined reader – such as Robert Lucas, who will never read historians' accounts of the representative agent – or as intimate as E. Roy Weintraub writing about his father (2002). In any event, it is unavoidable. All practical questions at each point of one's research – the choice of sources, the way texts are read, the style of writing, the kinds of arguments, the choice of contexts, but also the way to respond to referees and to communicate results – are not only scholarly, but also personal and, furthermore, moral choices. The history of economics becomes a matter of you and me, of them and us. Whose interests will my history serve? How do I relate to eyewitnesses, to those directly involved, to those who still have an axe to grind? Shall I ignore them, confirm them, or shall I blame or even provoke? While historical distance leads historians often to simply translate ideas for today's readers, the presence of those who remember force us to think about our *ethos* and self-consciously demarcate our voice. In the following, I will reflect on the historiographical questions related to the fact that we write the history of that which is still remembered by persons living among us.

Contemporary history is historically specific. In classical historiography, say that of the 19th century historical school of the Rankean kind, historical vicinity was perceived as threatening to the professional codes of proper documents, reinforced by the doubt that eyewitnesses are not reliable historical sources as they are as tainted by interests (Vierhaus 1957). Historiographical debates since this period have been overshadowed by the question of whether the presence of particular interests in the writing of history causes a *bias*, and if the historian becomes an accomplice or in any sense instrumental to current power struggles. The experience that forced western historians to nevertheless enter contemporary history was the 20th century as a time of extremes – referring initially to the disaster of the two World Wars that

resulted in a renewed sense of historical responsibility when thinking about the present. The field thus emerged from a *sense of urgency*, an immediate "need of self-articulation" (Rothfels 1953: 5) caused by the lack of understanding between cultures, people, and generations that are separated by their extreme experiences. And the sciences, economics in particular, having gone fairly blindly through major transformations in the 20th century, have added their share to the lack of comprehension of how we ended up in the world we live today. The sciences are a *symptom* of the 20th century.

Responding to this sense of urgency, the following essay provides a historiography of the contemporary that fully embraces the presence of the personal in science. I begin by presenting a theoretical framework for writing histories of the scientific self - a concept at the center of what I call *lived epistemology* (1).[2] Writing such histories, in practical terms, raises questions of harmony and conflicts of interest in the history of science (2). I argue that the question of bias that overshadows the historiography of the contemporary is a *Scheinproblem* that appears if one avoids underlying ethical questions when assuming the personal.

(1) Lived epistemology

[2] History and theory being exclusive of each other in 19th century discussions, the meaning of the term "historical theory" is not obvious. The notion of "historiography" might indeed be considered a placeholder for topics regarding the conceptual framework (theory) and the methodology (practical tools) in historical research. Among historians of economics, only certain highlights in historical theory are known (Popper, Kuhn or Foucault). In the following, 'theory' in the history of science refers to the question of what is the historical nature of knowledge, what are the historical *sujets* that follow from it, and what is its intended result. Together with the discussion of the practical questions related to applying this theory, one might speak of a historiographical approach, if not a research program.

If contemporary science requires a discussion different from ancient science, knowledge seems to have a historical nature. This in itself is a contentious claim. In the history of philosophy, reflections on the historical nature of knowledge grew out of dissatisfaction with the conceived, specifically Kantian, contrast between that which is known and that which is ephemeral. For Kant, time is merely a form of apperception (*Anschauung*) but not one of cognition (*Verstand*). He also placed time awkwardly as the "scheme" or pattern of imagination (*Einbildungskraft*): that which renders intuitive categories of cognition (such as causality, unity, or necessity) when being applied to what is presented to our senses. It was only at the beginning of the 20th century that our sense of before and after became a central concern in the context of neo-Kantian epistemology when scholars like Wilhelm Dilthey, Ernst Cassirer, and Edmund Husserl tried to spell out how cognition is enacted in concrete and how knowledge, and science in particular, results from historical life (see Rheinberger 2010). Time and history, for them, were central to what it means to be an understanding being. This same post-Kantian desire is at work when, for example, Lorraine Daston describes her work as a "history that would pose transcendental questions in a highly particularistic mode" (2000: ix).

Consider, for example, Edmund Husserl. The starting point of his historicized epistemology was the "subjectivity that accomplishes science" (1970: 295, see also Hyder and Rheinberger 2010). To speak of the subjective accomplishment of science is to focus on the necessity of someone to carry out knowledge: Someone needs to keep track of, has to be with, and has to go through the reasoning, the evidence, the judgment, the research material, etc. Even the most general or abstract theory is the result of a unique and concrete course of *efforts* required to accomplish it. This is not only a matter of "practices" that can be described in a behavioral fashion (Stapleford 2017), but requires a distinct attitude (*wissenschaftliche*

Einstellung) that brings about a different way of experiencing oneself. Knowledge is not only a set of justified beliefs representing something, but, first of all, an experience for the subject carrying out this knowledge. Science can be viewed as a way of feeling, viewing, and relating to oneself, others, and our surroundings. In this way, science can be described as any other experience through which we come to understand the seat of science in life, as Husserl said. Or, to quote a more recent historian of science: "Just as we have social histories of eating, dying, breeding, and getting and spending, so too we can have a social history of truth making" (Shapin 1994: xxiii).

Husserl spoke of such analysis as a 'regression to the life-world', as the 'digging out of buried sense-accomplishments' (*Ausgraben verschütteter Sinnesleitungen*). What must have already been accomplished in our life for an epistemic interest to settle within us? What is the 'act of meaning formative for the experience of scientific thinking'? (Dodd 2004: 7) The history of these buried accomplishments is what he calls, in contrast to the history of facts, the history of sense, the writing of which is the "tremendous task of a true and genuine philosophy of science" (Hua VI: 398*). In the context of the mathematization of the natural sciences, he asks:

> Where is that huge piece of method (…) that leads from the intuitively given surrounding world to the idealization of mathematics and to the interpretation of these idealizations as objective being? (...) How formulae in general, how mathematical objectification in general, receive meaning on the foundation of life and the intuitively given surrounding world — of this we learn nothing. (Husserl 1970: 296)

This huge piece of method is particularly daunting when considering 20th century economics. What remains as its 'residual', its body of knowledge, are some vague intuitions – 'doing the best', 'scarcity', 'waste', 'tastes', 'markets' – treated with formally defined theoretical concepts – equilibrium, aggregation, mechanisms, sunspots – that are treated by a set of techniques – graphs, functional analysis, axioms, regressions, calibrations, simulations – which are some of the ingredients of what are called models. Now think of the 20th century: two wars that exceeded what the world had seen before, atomic destruction, genocide, growing inequality, the space race, the rapid development of communication technologies, and what not. How did it come that *this* 20th century created the conditions of *that* kind of knowledge to prevail over other forms of economic knowledge? The last century was a time of historical ruptures that made many silence, paper over, and oppress memories that us with incomprehension between people, cultures, and generations. 20th-century science has contributed to this great incomprehension, insofar as it tells us little about its origins. 20th-century man is a struggled being, and so are 20th-century economists; the halls of economics offered one place in this harassed world, but nobody tells us what place that is. This is how I came to understand Husserl's notion that science 'lacks the knowledge of what gives meaning to it'.

Heidegger also had something similar in mind when he mentioned, without elaborating, an existential in contrast to a logical concept of science (1962 [1927]: §69b). According to the logical concept, science is viewed in terms of its results; that is, "something established on the interconnection of true propositions." According to the existential concept of science, instead, he asked what were the "existentially necessary [conditions] for the possibility of Dasein's existing in the way of scientific research" (Ibid: 408). Science is, in his words, a "mode of Being-in-the-world," a "way of existence". The distinction between the personal and the scientific is, in this

case, a “privative mode”, as Heidegger would have said, of a more intimate connection between the two. Modes of reasoning, modeling techniques, can be understood as attitudes we adopt toward our experiences and concerns, thus a kind of “self”. The scientific self tells us about the tone and posture with which one speaks to others and allows to raise our voice with weight and thus to demand others to listen. Scientific claims not only result in anonymous truths that may or may not be the case; the question of truth, rather, refers back to the state of being able to make a truth claim, which is, first of all, a “claim on me.” Science in this sense, as Söderqvist boldly claimed about the case of Niels Jern, is like writing a diary (2003). In the words of one of the commentators of Husserl:

> Thus to reflect on the possibility of making the claim myself, in my own voice, not only brings the truth of a proposition into question, but it also brings myself into question as well — for the question here takes the form: what would it mean, to be the one who would make such a claim. (Dodd 2004: 9)

With this in view, the questions to be answered are thus: What kind of person do I need to be to lend my voice to this or that scientific truth? What moral identity is induced by doing so? From which existential project is economic knowledge the result of? How is it to speak as an economist? Which attitude must one to adopt to form an interest in economic science? How does one get to see oneself as an economist, and how do others come to support this self-perception? One might call this theory of knowledge, in resemblance with the notion of lived experience, *lived epistemology*. In lived epistemology, we view knowledge not as a cognitive activity but as an experience that happens to someone.

***

Clearly, this understanding of science is *critical* to the extent that science itself tells us little about its experience. Science is a representation of what is, such that speaking about oneself is limited to being subject to the rules of conduct of methods. Science is 'selfless' insofar as its truth is independent of whether someone is interested in it. It requires, in Galison's words, the 'right way of self-abnegation' (2015). It is as if the scientist, watching a mirror, believes they are behind the mirror. Or, in Husserl's words: "Merely fact-minded sciences make merely fact-minded people" (1970: 6). Instead, in lived epistemology, science is not considered a representation of the world, but a *response* to it; it tells us from attempts and failures to find one's place in a historical situation; from an existential struggle of which it is a symptom or a therapeutic means. It is this critical element that renders lived epistemology a non-trivial task. The impersonality of scientific expression, reinforced by the degree of technicality, creates a *complex*, or better, a historically contingent relationship between 'life' and 'work'. It is not obvious from Debreu's work that he preferred mathematical rigor to economic chit-chat because he inherited a basic *Angst* from his broken family (Düppe 2012); it is not obvious from Krelle's publications that he wished to modernize German economics out of feelings of guilt that he, rather than his comrades in the trenches next to him, survived the war (Düppe 2018b).

There are several *sujets* that can be tackled when writing histories of the scientific self. One of them is what has been called the *scientific personae*; that is, the character the scientific community or society associates with the scientist and which an individual aspires to (Daston and Sibum 2003). In contrast to a mere activity, science implies a social role, a calling, an ethos,

or in any event an aspiration to identify with. Scientific personae are historically specific, as one might sketch with a big brush: the ancient scholar, the medieval learned, the Renaissance instrument maker, the early modern traveler, the natural philosopher of the enlightenment, the 20th century intellectual, today's experts, etc. (for an overview, see Part in Lightman 2016). In all these cases, being a scientist requires committing to specific epistemic virtues - such as precision, patience, moderation, but also radicalism, purism, and perseverance - that not only discipline thought but offer a moral identity – such as seriousness, honesty, dedication, selflessness, or responsibility. As Steven Shapin took as a precept of his *Social History of Truth*:

> What we know of comets, icebergs, and neutrons irreducibly contains what we know of those people who speak for and about these things, just as what we know about the virtues of people is informed by their speech about things. (1994: xxvi)

Note that we do not require that these epistemic virtues ever be fully realized. They do, however, provide the scientific experience with a teleological frame. The scientific personae is, as Daston argued, located between the individual biography and the social institutions of science. It is a 'mask' providing a social identity. While social epistemology can categorize them, and historical epistemology can describe their transformation, in lived epistemology we spell out the life path that is subject to these virtues. Why is it that a certain scientific personae is attractive as a choice of life for this or that person in this or that time? Which historical situations bring about the conditions under which these virtues become attractive models of life?

Aspiration is one source of the scientific self; another is the *psychological need* these aspirations respond to. What are the psychological conditions of the possibility of feeling and

acting like a scientist? Due to an age-old *intrinsic value o*f knowledge as if a basic instinct of mankind, stylized by epistemic virtues such as selflessness, we have a very poor understanding of this need. However, considering that a large part of mankind lives without scientific ambitions, and considering the costs of a scientific career – long periods of education, higher mobility, lower social bonds, small markets due to high specialization – it is not at all obvious why someone develops such a degree of commitment. We might look in vain for a general theory of these needs as they are, like the scientific personae, historically contingent.[3] But we can observe individual cases: insofar as it allows for solitude, it might attract those who react against attachment (as Shapin (1990) has shown for 17th-century scholars); insofar as emotions are left out of discourse, it might attract those with emotional disorders (John Nash being a well-known example); insofar as scientific principles promise to anchor what is otherwise felt as being uncontrollable, it might attract those needing to deal with anxiety (as Leonard (1998) has shown for Carl Menger in the tumults of Vienna of the interwar period); insofar as science is only hypothetically related to reality, it can provide consolidation for suffering as for the Russian mathematician Sofja W. Kowalewskaja after witnessing the death of her sister: "At such moments mathematics are a relief. It is such a comfort to feel that there is another world outside one's self" (in Koblitz 1993: 202); or, to go yet further, insofar as scientific objects promise immutability, its aesthetics might attract a death instinct as the daughter of the mathematician Claude Chevalley said about her father:

[3] There are surprisingly few attempts that ventured a psychology of science. A small field journal in the *Psychology of Science and Technology* has been launched by Gregory Feist. It is limited to statistical surveys of pre-defined measures, such as personality tests (Feist 2008). Another, epistemologically more profound attempt, is still the classic study of Gaston Bachelard published in 1938, *The Formation of the Scientific Mind* (2002), which, in a modernist spirit, tries to explain scientific progress.

> The way my father worked - it seems that this was what counted most – was the production of an object which then became inert; dead, really. It was no longer to be altered or transformed. Not that there was any negative connotation to this. (…) [My father] thought of mathematics as a way to put objects to death for esthetic reasons (in Senechal 1998: 26).

In all these cases, it is the experience of knowledge and not its representational content that explains its meaning for the scientist.

Epistemic virtues as well as epistemic needs usually do not come in isolation but are manifold and can be in conflict or in harmony with other virtues. Selves, in contrast to academic compartments, are not neatly limited. How is science integrated in the rest of one's life? The precision, dedication, and patience that is needed for building up a large-scale macro-econometric model, for example, can be in surprising harmony with nationalist sentiments and Protestant virtues of hard labor (Düppe 2018b); the virtue of communitarianism, to mention one of Merton's classic norms of science, can come into conflict with other virtues of the reward system of science (Düppe and Weintraub 2013); the very desire for individual happiness can be compromised by the devotion and "monomania" needed for achieving a certain degree of intellectual depth (Daston 2008); and the need for feeling relevant is consistently frustrated by various limits of scientificity, a repeatedly lamented source of scientific pessimism and cynicism in the heterodox critique of economics (Colander and Klamer 1987). The same question can be posed on a more global level: Do the values of science match those of the rest of society? Such was the big question of Mertonian norms in the face of WWII, a question that was formative of

the very field of science and technology studies (1942). While in social epistemology one can observe and state these conflicts, it is in lived epistemology that their experience is described.

To be sure, the scientific self can be a topic of individual life writing, but it can also be the subject of a larger, *cultural history of science*. What kind of science is brought about by witnessing the French Revolution, the October Revolution, the Holocaust, the atomic bomb? Two examples: The general mistrust in economic affairs, during mercantilism, for example, can explain why visions of social structures at which this mistrust is neutralized, are attractive to those who are subject to this mistrust, merchants (Düppe 2011); the general feeling of anxiety during the first years of the Cold War can explain the rise of the protocol-based notion of scientific rationality (Erickson et al. 2013). Such more speculative histories allow us to understand the culture of science in a twofold sense of our culture being determined by science *and* by science constituting its own culture – two facts that surprisingly often do *not* form a contradiction. As hard as 20th-century scientists tried to express themselves differently from other forms of cultural expression (literature, art, and even religion), as little they understood how much they were caught by them.

If we cannot expect a general theory resulting from lived epistemology, then what is its purpose? While it presumes a shared sensibility for life stories, it is not a form of voyeurism. Scientists, though very reflective when it comes to justifying their practices, have little means to think about themselves in other than the terms provided by their methodologies. If one has not independently acquired a language from art or literature, one has no means for thinking about the meaning of one's profession. Lived epistemology can provide such language through examples. Just as art can intensify the visual experience of forms and objects, so can lived epistemology intensify the intellectual experience of knowledge. Lived epistemology aims at intriguing and

intimidating the scientist to evoke a sense of self and thus of responsibility. This might be best compared with what *parables* do - describing moral dilemmas, questionable decisions, and the suffering of the consequences of these decisions. Παραβολη means to "walk aside"; histories of scientists' selves can be apologues that transmit a moral question in an indirect, but nevertheless concrete way. In this sense, life-writing in science can then indeed be called an "edifying genre", as Söderqvist has called it (1996).

Coming back to the contemporary, these *sujets* of the history of the scientific self are, in principle, not limited to a specific period. Lived epistemology can rely on sources that are not limited to the contemporary such as diaries, letters, and personal notes – all what helps in getting to know someone. But for two reasons the contemporary leans itself to lived epistemology. First, historical vicinity makes it easier to get to know someone as we can actually meet him or her. In other words, there are more "sources" of personal memories. Second, today's specific regime of knowledge of economics favors abstraction over description, which increases the urgency of drawing back this form of knowledge into the life-world of economists' selves.

(2) Interest conflicts

When applied to contemporary history, this research program pushes the limits of that which is considered 'the private sphere' – that has its own history and cultures. If archives impose a 30-year limit for legal reasons – a criminal record would otherwise not be a historical but a legal document – and the encounter of someone in person gets around this limit, contemporary history runs the risk of violating privacy laws. When the lines between the personal and the

representational in science, questions of interests thus impose themselves. In this section, I reflect on these questions.

The root of the problem is that historical work is only one minor source of the production of collective memory. Economists themselves fondly remember, and express their feelings through review articles, science prizes, honorary doctors, theorem tagging, journalism, and blogging, but also in person when one meets and talks to them. History is a strategic realm in which several interests are negotiated among those who have access to it. And if one does not want to be a popularizer of their ideas, a caricature of their self-display, or imprisoned in one's reactionary feelings against them, questions of conflict and harmony of interests must be negotiated, as well as reflected in historical work. Before publication, one is often asked to assure that there are no potential conflicts of interests, and one might push doubts aside because of the pressure of publication. But the lack of self-interrogation in this respect is a serious problem in the field of contemporary history of economics. So what are the sources of harmony and conflicts of interests?

Consider the following correspondence I received in response to a commissioned work on the history of the economics department in Bonn at the occasion of a university anniversary. The circumstances are telling: while all other faculties had internal authors, some retired professor who is considered as having sufficient knowledge of the department's past, there was no past economist who was deemed to have had enough expertise about their own history - one example of the cultural ruptures typical of the 20th century, in this case the rupture between generations of literary economists such as Arthur Spiethoff, Beckerath, and Schumpeter, and a younger generation of technical economists such as Wilhelm Krelle, Werner Hildenbrand, and

Reinhard Selten. After the work was done and the internal refereeing process finalized, the higher faculty positions changed, and I received the following message:

> "As a (high faculty member) of the Faculty of Law and Economics at the University of Bonn, I would like to thank you very much for your beautiful draft of the history of our department. (...) May I make a cordial request? There are two passages that I beg you to consider critically. (...) (One) concerns Mister Krelle: The description of his family situation seems to me ... not that which readers expect from a department history published by the university ... I would be very pleased if you could consider my concerns when preparing the final version" (March 30, 2017).

What happened? And how to respond?

There is harmony of interest insofar as the field of history is considered a source of scientific *credit*. As the gatekeeper of the archive, at its best, the history of economics is perceived as a selection process of that which is worth preserving. Whether one is supposed to add to already given credit, or, like a lender of last resort, grant credit that had been refused, the hope is that we *feed their ego* (or that of their deceased mentor and their community). Often incapable of distinguishing personal admiration from gratitude for career opportunities, economists think of history as a form of worship. Clearly, as the main source of cooperation, this power to grant credit makes contemporary history also vulnerable to *censorship* (Cantor 2006). Economists cooperate because they wish to *influence* the first sediments of history. Writing about those who have been already credited, one is not supposed to deviate from the official terms; writing about those who have *not* been credited, one is equally supposed to undo past

injustice. The role of the historian as a *scholar* in explaining scientific performance that is nevertheless *instrumental* for the reward system constitutive of this performance is thus deeply precarious. You might have received similar messages as this:

> "You proclaim the message that the development of the department was essentially the result of a game of power of different networks, in which nationalism, if not Nazism, and the desire to find the connection to the USA were the driving forces. You are free to hold this opinion, but I consider it to be wrong and not well-founded. (...) You do not see that the very great success of the Bonn Department was essentially due to the scientific quality brought about here" (famous German economist to author, January 28, 2016).

One way out is to appeal to the shared scholarly ethos by claiming the right to 'set the historical record right', thus hiding behind facts. I used this rhetorical move when responding to the attempt at censorship cited above.

> "The background of the family was well-known in the department, and is therefore a historical fact of the faculty. (…) To ignore this fact would mean to be caught by history of the department instead of reporting about it" (author to high ranked faculty, April 3, 2017).

Clearly, the issue was not the fact of there being a fact, but to undermine his feeling that family information is inappropriate as presentism. Reference to facts in contemporary history easily begs the moral issues looming behind the noble motive of setting the record right. It is to play

down conflicts without facing them – as if there was only one way of writing history, which is the very denial of the historicity of human life. Note also that it is only once one reduces the writing of history to the reporting of facts that the question of bias can be posed. It is thus a derivative of an attitude that tries to avoid questions on how to relate, as a person, to those who are concerned by the stories one tells.[4]

Another source of cooperation, next to the power to grant credit, is the anonymity that the halls of modern knowledge provide. Whatever the historian writes, a model or a proof stands. Because nobody would see the value of a great theorem diminished by what kind of person the discoverer is, technical economists speak more easily about their lives, about the existential dimensions of their work, the sacrifices their career required, and the disappointments their career evoked. Mathematicians often lead a light life which is reflected in a rather liberal attitude, flirting with the scientific personae of an eccentric non-conformist. Having asked Monique Florenzano if she wondered why Gérard Debreu stuck out as a person, she replied: "No I did not. In this profession, people are crazy anyway, and he was not weirder than others" (personal conversation).

However, the person matters in economics more than in other sciences insofar as the discipline always travels under a cloud of ad hominem arguments and ideological suspicion. This was a specific challenge to my work with *socialist economists* in East Germany (2017). Being trained to utmost professional dedication, and to personally represent their work, they had a strong work ethos to defend and to communicate. However, their professional dedication was a

[4] The issue of bias is put forward in many forms. One of them, a cognitive bias, is related to memory hubs and memory loss put forth in cognitive theories of biographical memory. Adopting the perspective of the encounter of the historian and historical actors does not lend itself to this discussion. One does not need a commitment to historical positivism in order to balance out what someone says and who says it – we do this all the time in our daily lives.

political duty, controlled ultimately by the secret police, such that they learned to hide or control the display of personal matters. Also, they clearly suspected that I, born in West Germany, embrace the winner's version of history and treat them as mere dogmatists. General respect for historical truthfulness was largely burdened under the tenets of dialectical materialism, where historical memory is but a symptom of power relations. Cooperation stood and fell ultimately with their own coordination. They acted as a "collective", though the interviews were conducted individually. They might have coordinated what to reveal and what to hide, which made their memories no less interesting. Even if their commitment to the party-line was of varying degrees of dogmatism, the order of the interviews was of increasing dogmatism, according to their own judgment. However, it turned out that all of them were similarly critical about the political limits imposed on them, and that they shared very similar biographical memories. After all, the solution was to write on a "generation", which allowed me to distance from claims about individuals.

Another group that can be an important source for contemporary historians are those who suffered from the intellectual obsession of economists and from their elevated ego brought about by the academic reward system: family members and friends. Bringing in their voices meets the genuine purpose of oral history to give voice to minorities otherwise unheard. Some might be protective of the self-display of their close fellow, but others are also willing to correct it and to share what happened behind the stages of representational ideas and referential truth. Talking to them is to learn about the lived struggle for ideas without knowing these ideas. Mindful of the difficulty of confronting family past, I never push, and let the participant decide how far to engage. While I try to find a way around censorship from those who wish to maintain the given credit status, it is from them that I accept censorship. So far, changes have always been minor. It is also their applause that I seek, because it is for them that our work can make a difference in

their personal lives. Regarding the same content that the high faculty member wished to omit, the daughter responded:

> Thank you for sending us the first draft, which my husband and I have read several times carefully. Many tears run. (...) I think you did very well in doing justice to the character of my father. I am very happy for him. I thank you cordially (daughter to author, April 2017).

A troubled private life, unknown to the public and difficult to be reminded of - and yet, when exposed in an academic journal, she is grateful *for him*. What more could we expect from our work than helping individuals to live through grief and sorrow.

When facing open wounds and conflicts among those concerned, *balance* is certainly an epistemic virtue of historical work. What else is historical memory good for if it does not provide inclusion, agreement, and some form of reconciliation, even if this too often means revealing conflicts that are withdrawn from the public. Debreu's daughter repeatedly referred to her father as manic depressive, though he was never diagnosed. Since this judgment hardly played a role in Debreu's life (he saw a psychologist only a few times) I did not use the word. The reader might draw such judgment from my narrative but it should not be its presupposition. One sign of having found balanced language is that those who disagree about their past, those who still live through ongoing conflicts, can agree on the way the narrative is presented. Another, equally important, sign is that, as author, one has no wish to blame or to accuse, which is, to no longer reproduce the feelings of our parents and grandparents, to no longer continue their struggle with their past. In contemporary debates, we argue about actions and beliefs. In contemporary history, we come to understand them.

Potential conflicts can be, and should be, prevented by being as transparent as is necessary and by securing prior agreement. Practices in our community differ widely, which one might take as a sign of pluralism but also of a lack of professionalism (reference Jullien). Some historians of economics of an older generation, when conversing with economists, use no form of agreement, do not record, and do not ask for approval when (indirectly) quoting the conversation, such that readers are left with good faith in the author's memory and truthfulness. The trade-off is clear: When being more formal one gains agreement but one puts potential conflicts on the table, which might be a reason for withholding information or even withdrawing from participation. Considering the precariousness of our field discussed above, however, presenting myself as one of them who, presumably, shares the same interest would be a straight lie. Full transparency is certainly impossible and also not necessary, but I learned to ask for ethics approval from my university, and use agreement forms to sign, record, and grant the possibility but avoid ex post approval of direct citations. There is no recipe for best practice, and I experienced several failures: I had interviews interrupted, statements changed ex post, and imposed self-censorship in anticipation of future approval. Fortunately, it never happened that disagreements took a legal form, the ultimate failure of having dealt badly with conflicts of interest. After all, it comes down to the person's trust, which is an entirely personal question.

Another source of conflicts of interest that needs to be mentioned is unrelated to trespassing the private sphere and the credit system in science, but is related to commercial or political interests. Historians who inquire into the strictures of economists with business and political lobby groups indeed might have no other possibility than to keep economists at a safe arm's length. When Mirowski writes about the Mont-Pellerin Society or the Nobel Prize, we would not expect him to agree with the condition that members of the Mont-Pellerin Society or

the Swedish Academy agree on his account. The nature of such works, on the contrary, is to evoke disagreement in those one writes about. But it is also for this reason that this historical work is so enmeshed in the negotiations of interests that the reader tends to learn more of Mirowski's interests than the story he tells. Getting involved with commercial and political actors – which might touch on state or business secrets – does not necessarily result in the historian becoming an accomplice of their interests. For next to balance, *subtlety* should be considered an epistemic virtue in the historiography of contemporary economics. It is the subtlety of narratives that lend to them an exposing flavour that does not threaten the professional integrity of actors.

But the most important strategy in dealing with potential conflicts of interest for my work is the appeal to the human. The contemporary history of economics can provide the inclusion of economics into human memory – which knows no taboos but understanding between generations. The instinct of the older generation to share with the younger generation is very basic. Without it, memories would be no more than melancholy of an irrevocable past. In response to the dean cited above, I wrote:

> "The interest in the person of Krelle is so great that an essay like this must react to it. There will be readers who will read the essay only to see how the military past is represented. And the family background shows that as a scientist Krelle did not only remain an officer, but also a human being. Also a reader of a department history is a human being, and therefore will not be offended by the human aspects of such a story. I do that in all my works" (author to Daniel Zimmer, dean, March 2017).

I received no response. Clearly, arguing would not help.

There is no general recipe for dealing with potential conflicts between economists and contemporary historians. If you do not reflect on them, however, you might end up being caught by them. The contemporary history of economics requires from each author to find his or her own balance between conflicting and harmonious interests. The quality of our work depends on how we deal individually with the precariousness of our field. And the more we know the personal implications of our work, the better we can deal with them.

References

Bachelard, Gaston (2002 (1938)). *The Formation of the Scientific Mind*. Clinamen Press.

Cantor, David (2006). "The politics of commissioned histories (revisited)", in Ronald E. Doel and Thomas Söderqvist (eds.), *The historiography of contemporary science, technology, and medicine: writing recent science*. New York: Routledge, 45-66.

Catterall, Peter (1997). "What (if anything) Is Distinctive about Contemporary History?" *Journal of Contemporary History*, 32 (4): 441-452.

Colander, David, and Arjo Klamer (1987). "The Making of an Economist." *The Journal of Economic Perspectives*, 2: 95-111.

Daston, L., and Sibum, H. (2003). "Introduction: Scientific Personae and Their Histories," *Science in Context*, 16 (1-2), 1-8.

Daston, Lorraine (1994). "Historical Epistemology," In J. K. Chandler, A. I. Davidson, & H. D. Harootunian (Eds.), *Questions of evidence: proof, practice, and persuasion across the disciplines* (pp. 282–289). Chicago: University of Chicago Press.

Daston, Lorraine (2008). „Monomanie in der Wissenschaft," in Heinrich Meier (ed.), *Über das Glück*. Munich: Piper: 221-252.

Daston, Lorraine (ed.) 2000. *Biographies of scientific objects*. University of Chicago Press.

Dodd, James 2004. *Crisis and reflection: An essay on Husserl's 'Crisis of the European sciences.*' Kluwer.

Düppe, Till (2011). *The Making of the Economy: A Phenomenology of Economic Science*. Lexington.

- (2012). "Gerard Debreu's Secrecy: His Life in Order and Silence," History of Political Economy, 44 (3): 413-449.

- (2017). "The Generation of the GDR: Economists at the Humboldt University of Berlin Caught between Loyalty and Relevance", *History of the Human Sciences*, 30(3) 50–85

- (2018a). "Der Bonner Wandel der deutschen Volkswirtschaftslehre"; anniversary chronicle of the University of Bonn.

- (2018b). "War after War: Wilhelm Krelle, 1916-2005", working paper.

Erickson, Paul, Judy L. Klein, Lorraine Daston, Rebecca Lemov, Thomas Sturm, and Michael D. Gordin (2013). How Reason Almost Lost Its Mind: The Strange Career of Cold War Rationality. Chicago: University of Chicago Press.

Feist, Gregory J. (2008). *The Psychology of Science and the Origins of the Scientific Mind*. Yale University Press.

Heidegger, Martin 1962 [1927]. *Being and time*. Blackwell.

Husserl, Edmund (1970). *The crisis of European science and transcendental phenomenology*. (*Die Krisis der europäischen Wissenschaften und die transzendentale Phänomenologie: Eine Einleitung in die phänomenologische Philosophie*. Den Haag: Nijhoff. Translated by David Carr) Evanston: Northwestern University Press.

Hyder, Davis and Hans-Jörg Rheinberger (eds.) 2010. *Science and the life-world: Essays on Husserl's 'Crisis of European Science'*, Stanford: Stanford University Press.

Koblitz, Ann Hibner (1993). *A convergence of lives. Sofia Kovalevskaia: Scientist, writer, revolutionary*. New Brunswick, NJ: Rutgers University Press.

Leonard, Robert J. (1998). "Ethics and tFhe Excluded Middle: Karl Menger and Social Science in Interwar Vienna". *Isis*, 89 (1): 1-26.

Lightman, Bernard, ed. (2016). *A Companion to the history of science*. Chichester: John Wiley.

Merton, Robert K. (1942). "Science and Technology in a Democratic Order," *Journal of Legal and Political Sociology* 1: 115-26 (later published as "The Normative Structure of Science").

Rheinberger, H.-J. (2010). *On Historicizing Epistemology: An Essay*. Stanford University Press.

Rothfels, Hans (1953). „Zeitgeschichte als Aufgabe", *Vierteljahreshefte für Zeitgeschichte*, l: 1–8.

Shapin, Steven (1990). "'The Mind Is Its Own Place': Science and Solitude in Seventeenth-Century England." *Science in Context*, 4 (1): 191–218.

Shapin, Steven 1994. A *social history of truth: Civility and science in seventeenth-century England*. University of Chicago Press.

Söderqvist, Thomas (1996). “Existential Projects and Existential Choice in Science: Science Biography as an Edifying Genre.” in Telling Lives: Studies of Scientific Biography, ed. Michael Shortland and Richard Yeo. Cambridge: Cambridge University Press: 45–84.

Söderqvist, Thomas (2003). *Science as Autobiography: The Troubled Life of Niels Jerne*. New Haven: Yale University Press.

Stapleford, Thomas A. (2017). “Historical Epistemology and the history of economics: Views through the lens of practice”, *Research in the History of Economic Thought and Methodology*, 35A: 113-145

Vierhaus, Rudolf (1957). Ranke und die soziale Welt. Münster: Aschendorff.

Weintraub, E. Roy (2002). *How Economics Became a Mathematical Science*. Durham and London: Duke University Press.