

Method and Perspective¹

This paper belongs to the “Theory” section. It makes me feel a bit embarrassed, since I think that there is no such thing as the “Theory of Microhistory”. There are, of course, several theoretical issues to discuss in connection with microhistory, but none of them are distinctively or exclusively microhistory-issues. Maybe we can get the “Theory of Microhistory” if we add up all the theoretical characteristics once were ascribed to microhistory: we have to pursue a name through the archives; it must be an “exceptional normal”; we have to take care for both the lived experience and the invisible structures in which this experience is articulated; we have to invite the reader to take part in the process of research and interpretation; we have to confine ourselves to some Cartesian operations, and so on. The result, I suppose, would be microhistory, but I am not sure that we could not call it otherwise, such as “new history” in the sense that Peter Burke gives to the term, history from below, or (with a curse upon our head) even historical anthropology, and the like. Sometimes –most of the times – we all do call it otherwise, actually.²

I do not mind this conceptual confusion. It can be disturbing only for those who would like to define microhistory. As for me, I have no bold aspirations like that. The only thing I would like to do is to pay attention to the tension between the practice of microhistory and the theoretical reflection to it.

In an essay published in *History and Theory*, Matti Peltonen speaks about two waves of microhistories, one that took place around the middle of the 1970s and one in the 1980s. He notices that “since then the flow of microhistories has been constant, though more variable in quality and without any really popular success. The 1990s saw a number of important theoretical articles explaining the thinking behind the idea of microhistory” (Peltonen 2001, pp. 348). There is nothing striking in these words, Peltonen just tells here a short historiographical story about microhistory. However, I would like to take this summary as a point of departure, because of the expectations suggested by it.

As I see it, microhistory has already become something what Thomas Kuhn would describe as “normal science”. Its success was simply due to its historiographical significance. This means that after the pioneer work has been done and after the historiographical story has been told we have no reasons to expect further success. The “important theoretical articles” contributed to the well deserved historiographical success of microhistory, but, at the same time, they are responsible for the lack of further success. As practitioners tried to come to terms with their own practice, they transformed microhistory into methodology, or more precisely, into methodologies. Since taking practices as methodologies means favouring applications and risking the possibility of further innovations and experiments concerning those practices, it was a regrettable mistake.³

I find it quite sad, even if I don't think that methodology-talk is completely unnecessary. There's a tiny room for it, perhaps, if someone takes microhistory as a historical formation we have already left behind. This is how the Hungarian András Szekeres likes to speak about it, and he is right in saying that in this respect there's no microhistory today (Szekeres 2005). This is microhistory like a fleeting trend in pop music, or like a spring collection of a fashion designer. This is microhistory as a completed mission. So if we are interested in explaining our practices as completed missions, we may think that (for this occasion) methodology-talk will do. But if we are still interested in doing microhistory, and I think this is what practitioners desire, methodology-talk is a rather shady business. Just as shady as sounding like ABBA at Glastonbury 2009, or as drinking Jelzin vodka in the reign of Medvedev. But this is what we do actually, if we take microhistory as a method. In this case we take other practitioners completed missions as if theirs also were ours, and as if the circumstances, under which they worked, were the same as ours. And what we sacrifice in this case is being

experimental. When microhistory looked so promising a generation ago, the main source of high hopes was its experimental character. This experimental character was ascribed to microhistory as a practice, long before methodological articles started to appear. Today, as I see it, we waste too much energy to picture those past practices as if they were applicable methods to us, and we don't invest enough in experimental work.

So while Peltonen says that "to me the most interesting aspect of the new microhistory is methodological" (Peltonen 2001, pp. 348), to me the most unfortunate aspect of theorizing about microhistory is speaking methodologically. This should be realized, I think, by all those microhistorians who speak about the fluidity and openness of normative systems (Levi 1992, pp. 107), or about the malleability of social structures (Magnússon 2003, pp. 720). If microhistorians would not take their own practices as methodologies, they could still be innovative and experimental. In this case, they would behave in accordance with their own a priori views of the relationship of (historical) actors and social (and other) structures. Not only the examined macrostructures would be "what men and women involved in actions make out of them in every moment" (Lepetit 1997, pp. 16), but microhistory would be what microhistorians – men and women – make out of them in every moment. It would lack the rigour of methodology, but practice would be malleable, and open to further transformations.

Escaping mere application and restrictive methodology-talk, the "looking back" type of theorizing should be exchanged for a "looking forward" type. At least partly. I wish I could do this also in the following pages of this essay, not only in this introduction. Since I am not sure that a general, normative statement concerning the disadvantages of methodology-talk is convincing enough, I think that I have to come up with some "looking back" type of theorizing in the interest of the "looking forward" type. In doing so, I shall speak about three methodological traits commonly ascribed to microhistory and intend to show that they are not parts of a sort of methodological armour. These traits are sometimes explicit in theoretical discussions, but sometimes they are just underlying assumptions. The first is that microhistory has a distinct method of writing, described as "the involvement of the readers"; the second is that doing microhistory is the way to catch hold of the "lived experience" of past people; and the third is that this way their "otherness" can (or should) be presented.

The involvement of the readers

Giovanni Levi names as a common characteristic to microhistory, that "the reader is involved in a sort of dialogue and participates in the whole process of constructing the historical argument" (Levi 1992, pp. 106). In spite of the fuzziness of these words several scholars take natural that microhistory is equal with the involvement of the readers, without discussing what it means. To Levi it is

incorporating into the main body of narrative the procedures of research itself, the documentary limitations, techniques of persuasion and interpretive constructions. This method clearly breaks with the traditional assertive, authoritarian form of discourse adopted by historians who present reality as objective. In microhistory, in contrast, the researcher's point of view becomes an intrinsic part of the account.

(Levi 1992, pp. 106)

Microhistories, of course, do incorporate into the narrative at least two of the features Levi mentions above. Even though the presentation of the techniques of persuasion would be an almost impossible (but highly amusing) enterprise and doing history is always writing interpretive constructions, microhistory – just as other kinds of the so-called "new history" – often offers a second story to follow. This is the story of the historian from which the reader could get informations about how historians work. To Carlo Ginzburg this means that since historians cannot take too many things for granted in their journey through the archives, "the hypotheses, the doubts, the uncertainties became part of the narration" (Ginzburg 1993, pp. 24). Other aspects can be incorporated into the second story as well. As for myself, my personal favourite is the story which Arthur Imhof tells in the thick of his *Lost Worlds* about how he wrote letters to the obstetricians and midwives in Bavaria. In the winter of 1982/83 he conducted a poll to assess whether a certain belief he

ascribed to past Bavarians is still living among present inhabitants (Imhof 1996, pp. 137-139).

Ordinarily, the easiest way is starting right with the second story, as Ann Wroe does it in *A Fool & His Money*. The book is about the everyday life of a medieval town, Rodez, located in France now, but the first chapter, entitled "The place, the sources and the story", is about the everyday life of Rodez in the 1970's, when Wroe spent two years there. It is about her efforts to become a part of this life. She tells stories about how she tried and how she gave up learning the occitan language, or stories about the people she met day after day in the archives, and so on (Wroe 1996, pp. 7-21). Another historian, Pietro Redondi starts his *Galileo: Heretic* in a somewhat similar, but more scholarly fashion. The opening scene of his book is a room in the Palace of the Holy Office in Rome in the summer of 1982; the moment when Redondi sees a painting on a wall, a portrait of Cardinal Bellarmino, the inquisitor of Galileo. He observes that the painting is a twentieth-century copy of an older portrait, and laments over a change that has been made in comparison with the original: the gaze of an inquisitor has been replaced with the gaze of a saint. For Bellarmino was beatified in 1923, Redondi notices that it was necessary to change Bellarmino's gaze according to his new image. The incident raises the question to Redondi whether the same could have happened to Galileo (Redondi 1987, pp. 3-8), and the main story is ready to begin.

What matters here is obviously not the look in Bellarmino's, but the look in Redondi's eyes, which is the gaze of an expert. Both Wroe's sometimes clumsy interjection into the Rodez life and Redondi's expertise create an atmosphere of authenticity around them. They appear as the right persons to tell stories about past people. Just like anthropologists, who are also great storytellers in this sense, appear as the only authentic persons in their own tales (Atkinson 1990, pp. 104-129). This reinforcement of scholarly positions can hardly be seen as an involvement of the reader into the making of the main story. Fortunately or unfortunately, historical accounts are not yet Wikipedia-articles, not even metaphorically. These second stories rather mark that historical writing has become a self-reflective, but still strictly scholarly endeavour. Thus the fact that microhistories let their readers know how historical accounts are made, does not mean more than microhistories let their readers know that what they read is a certain kind of book labelled as an historical account.

Another aspect of the involvement of the readers calls for a closer look. If all the efforts discussed above are officially for the sake of readers, it would be good to know something about the audience. Most of us are (or should be) fully aware of the fact, that historical accounts are addressed primarily to other historians (de Certeau 1988, pp. 63-64; Carrard 1992, pp.138-141). From competent persons to competent persons, one might say in an elitist language. This does not mean, of course, that there is no intention to reach other readers as well, outside the academy. But even if the intention is given in the case of microhistory, the one single examination I know of the expectations of the so-called "general audience" can only be a wake-up call, rather than an enforcement of those intentions. According to Carrard, who compared the different editorial strategies in reaching different audiences, the "general reader" of historical accounts

is someone who prefers short books to long ones, at least when it comes to nonfiction; who is unconcerned with evidence, particularly if the documentation consists of graphs and statistics; and who practices a strictly linear reading, being reluctant to have his or her rhythm broken by tables, and ill-equipped to employ the up-and-down, text-to-notes kind of reading which scholarly texts call for.

(Carrard 1992, pp. 145)

If this simplified profile has anything to do with men of flesh and bone, it is doubtful that the efforts of microhistorians could hit the target. Conducting a second story for the sake of the "general audience" looks just as futile an effort as doing it for the sake of other scholars. While incompetent persons (general readers) do not seem to be curious of a story about how historians are doing their job (that is to say, about what makes a historian competent), competent historians know the tacitly accepted tricks, devices and techniques well.

Moreover, the role of the reader, either of the colleague or of the “general reader”, is simply to follow the two stories and their interaction. Even if, as Levi says, “the researcher’s point of view becomes an intrinsic part of the account”, no one forces readers to construct their own points of view. The second story stands for being more persuasive concerning the first story, and if it invites the reader for something, this is just to accept the researcher’s point of view, which is, of course, the right one. It means that Levi vainly denies that microhistorians intend to present “reality as objective”,⁴ the second story, in presenting the techniques of the historian, reveals how to attain objective knowledge of an either objective or non-objective past reality. Thus the gap between the historian and the “general reader”, the competent and the incompetent, is growing instead of narrowing (or disappearing), and the democratic connotations of “the involvement of the readers” are turning here into another kind of “authoritarian form of discourse”.

In sum, the participation of the reader in “the whole process of the construction of historical argument” is literally impossible and metaphorically misleading. The supposed writing method of microhistories should be rather seen as a part of historians’ self-examination, as a part of a growing tendency among historians to do their job more and more self-consciously. Its place is among the home affairs, even if sometimes it makes microhistory entertaining for outsiders as well as for academics, such as in Jill Lepore’s case (Lepore 2001, pp. 141). But this entertainment is not what readers could influence by getting involved. They just get it as an academic product.

Lived experience

Theorizing is often about distinctions, exchanging old ones for more useful ones. Here I would like to introduce two of those. The first comes from the field of aesthetics. In his *Pragmatist Aesthetics* Richard Shusterman revives and recasts John Dewey’s distinction between “the art product” and “the actual work of art”, because he shares Dewey’s conviction that “the essence and value of art are not in the mere artifacts we typically regard as art, but in the dynamic and developing experimental activity through which they are created and perceived”. It means that works of art can exist only “in lived dynamic experience” (Shusterman 1992, pp. 25-26).

The second distinction is from Carlo Ginzburg and Carlo Poni:

Microhistorical analysis therefore has two fronts. On one side, by moving on a reduced scale, it permits in many cases a reconstruction of “real life” unthinkable in other kinds of historiography. On the other side, it proposes to investigate the invisible structures within which that lived experience is articulated.

(Ginzburg – Poni 1991, pp. 8)

Since dealing with structures and other invisible entities has dominated historical writing from the beginning of its institutionalisation, one might even agree with the statement, that “placing lived experience at the centre is probably the most important feature of the approach of the new social history” (Szijártó 2002, pp. 212).

As I see it, despite all the undeniable differences, Shusterman and microhistorians make a similar distinction. As for the differences, while Shusterman places the objectified “art product” in the context of “lived experience”, Ginzburg and Poni do the opposite. They place “lived experience” in the context of the objectified structures. As for the most important similarity, the emphasis is on “lived experience” in both cases, because of the common assumption that it has the potential to open the way towards inquiries into either “the art product” or the structures.

There is, however, something wrong with this assumption. I am not sure that there is any relationship between “lived experience” and structures. I feel tempted here to explain my doubts by twisting Sigurdur Gylfi Magnússon’s words about “The Singularization of History” (Magnússon 2003, pp. 716-724). I warmly welcome his refusal concerning the connections between micro and macro. His Singularization-project, in this respect, could support my refusal concerning the relationship of “lived experience” (as micro) and structures

(as macro). So far so good, but I am not sure that our refusal has common roots. This is why I wish to emphasize also my disagreements with him. I think we both perceive a methodological failure, but we draw different conclusions from it. Magnússon offers a new and better method in the shape of his project, since he thinks that former methods are failed because they weren't good enough. I think, however, that those methods are failed not because they were worse methods than Magnússon's fresh method, but because they were methods; because certain practices were introduced, managed and treated as methods. In other words: Magnússon, in favour of experimental practice, wants to add a new chapter to the Big Book of Historical Methods, while I think that, for the same reasons, it would be better to cast that book aside.

Since Magnússon, even in his best moments, remains a methodologist, I rather make use of a third distinction between "emic" and "etic", the one that Simona Cerutti used in depicting her activity as a microhistorian (Cerutti 2004). Cerutti treats the two categories as "methods of analysis", she thinks that Ginzburg mistreats them as "contexts of behaviour", and I treat them in a more modest manner, as approaches. An "emic" approach means that someone, like Cerutti, is curious of the historical actors' points of view; it is being interested in what they themselves were aware of. An "etic" approach means that someone is interested in describing historical actors by employing her or his own categories, from her or his own point of view; it is being curious of what historical actors were not aware of. Methods are only ways of satisfying these curiosities. They are in play (when they are in play at all) only after a (sometimes occasional) decision is made about what precisely is that something one would like to know.

In favour of "lived experience" an "emic" approach seems to be promising, since experience must be lived by historical actors, while an "etic" approach seems to fit to the examination of structures. Cerutti favours the first, while in *Inheriting Power* Giovanni Levi (1988) opts for the second. Levi deals with "family strategies" and creates models about the relation between exorcism and medical science, or about a finely structured "land market", which are "art products" in Shusterman's language. Levi's actors could not have known anything about the "products" they made up. Even if these "products" can be considered as results of the actors' interaction, the "product" is "visible" only to the eyes of the historian. The same can be said about the book of Paul Boyer and Stephen Nissenbaum, *Salem Possessed* (2001). Examining the Salem Witchcraft as an economic struggle between the accusers and the accused has nothing to do with what Salem people had in mind during the accusations and the trials. "Emic" is not to ignore actors' "illusions", as Cerutti puts it referring to Luc Boltanski (Cerutti 2004, pp. 29). Saying that the trials were results of an unconscious psychological game, Boyer and Nissenbaum make clear that their approach is purely "etic".

I do not think that "emic" and "etic", the examination of "lived experience" and structures, could be weld together. Nevertheless, one may decide that it is worth trying to connect them somehow. Perhaps an inquiry into "the structures of lived experience" or into the "lived experience of social structures" could be a solution, but microhistorians are talking about something else in their theoretical essays. For them the connection is more than a grammatical genitive case. In practice, it seems to be a doubtful causal relationship. In *The Cheese and the Worms* Ginzburg (1980) introduces a miller from the sixteenth century, Menocchio, who has noticeably strange ideas about the Creation. The presentation of those strange ideas is "emic", since Ginzburg, unlike the inquisitors to whom Menocchio talks, does not ignore Menocchio's "illusions". But Ginzburg introduces another protagonist beyond Menocchio. This is the oral popular culture, the "etic" component of Ginzburg's approach. The supposed connection between Menocchio's "lived experience" and the structural component is rather simple: he has his ideas thanks to the fact that he belongs to the oral popular culture. As Dominik LaCapra notes it, this culture provides the only single key to Menocchio's cosmos (LaCapra 1985).

Reading Ginzburg on Menocchio is just as exciting as reading him on popular culture, but reading him on the supposed relation of the two makes me feel that it would have been better to write two books, one about this and one about that. In depicting the millar's personal cosmos (the historical actor's point of view), I am not sure that there is anything that Ginzburg has derived straight from the popular culture (the historian's category). Regarding this culture, I am not sure that there is anything that he has derived straight from Menocchio's personal cosmos. In other words, what Ginzburg knows about Menocchio's particular "lived

experience”, he knows from somewhere else, not from his knowledge about popular culture in general. And the same in reverse.

Nevertheless, whatever I have said up to this point about the impossibility of welding together “emic” and “etic” or “lived experience” and structures, there are backdoors, as almost always. One of those is Imhof’s *Lost Worlds*, which I regard as an “etic” approach of “lived experience”, even if the examination of it with the help of categories invented by the historian seems to be a self-contradiction. Needless to say, this difficulty must be among the main reasons why historical writing, being interested in structures and “invisible” entities, has kept distance from “lived experience”. For historians are not eager to explain why they have done so, Shusterman’s help is needed again. The reasons why the notion of “aesthetic experience” was evaded by analytic philosophy of art might be very close to the reasons of many historians’ refusal concerning “lived experience”.⁵ The three main reasons, according to Shusterman, are (1) the fear from the private character of experience, (2) its resistance to analytic definitions and classifications, and (3) the conviction that experience is tied to emotions (Shusterman 1992, pp. 27-29). The complaints of sociologist, who also became interested in “lived experience”, are the same (Ellis – Flaherty 1992, pp. 2-3).

Imhof is just as clever in dissipating these fears as Shusterman or the sociologists. Dealing with what Shusterman would call “shared emotional experiences”,⁶ he evades the fear from both the private character of experience and its resistance to analytic definitions. Writing about the intensity of traumatization of past people – caused by wars, plagues and famines – Imhof imposes his own well defined or circumscribed categories of emotions on those people:

By “fear” I mean real and concrete fears, such as the feeling of panic when the plague broke out in one’s own village, house, or family; the dread of famine that came when the crops failed and the sparse grain was already starting to disappear at the beginning of the winter; the terror spread by soldiers who were plundering, burning, and murdering, who had already completed their vile work in the next community and who were now on the march toward one’s own; and the fear of robbers rumored to be in the forest that one had to pass by in order to bring cattle to the market. By “anxiety,” on the other hand, I mean to signify worries that were aroused by vaguely perceived and lurking dangers, such as the anxiousness about the recurrence of plague on the other side of the territory’s border or its rumored recurrence among pagans; the worry about the Turks who might yet overrun Europe after one of their countless attempts to do so, bringing war and gruesome carnage as they rolled over it; and the apprehensiveness about the grain not ripening the summer after days of continuous rain and cool weather, leaving hardly anything for humans or animals to eat at harvest time.

(Imhof 1996, pp. 71-72)

No doubt that categories like “fear” or “anxiety” are words about which existentialist philosophers could write complete libraries. But it is exactly the plainness of Imhof’s categories that makes “lived experience” available for a quantitative research. Comparing it with Levi’s book – another quantitative analysis – one thing seems to be obvious. Though both Imhof and Levi like to speak about the uncertainties among which past people had to live and against which they had to organize their lives, Levi deals only with what they did in order to attain security, while Imhof, in addition to that, tries also to explore what were these uncertainties and how people “lived” them. In doing so, Imhof has nothing to say about structures or models, but this is how choices and decisions work.

Imhof’s way to satisfy a certain curiosity is only one among many. Creating categories in connection with “lived experience” will do as well as letting the actors speak through direct quotations, as Ginzburg’s Menocchio often does. But these techniques, even if they are equally suitable, are not the same. Furthermore, letting those speak who spoke to inquisitors like Menocchio and letting those speak who spoke to the members of their families in letters as in the *Three Behaim Boys* of Steven Ozment (1990), is not the same either. There can be important differences also in how historians let their actors speak. While Ginzburg quotes Menocchio’s words inside a story, Ozment compiles a story by publishing parts of the correspondence

of his actors and comments them. All this raises questions whether Imhof, Ginzburg, or Ozment deal with the same thing under the term “lived experience”, and – in general – whether the ways of satisfying curiosities have backlashes on those curiosities. I cannot give here an adequate answer to these questions, only intend to note that if they do so, the identification of a distinct microhistorical method (or of a certain number of appropriate methods) would be much more harder. Not to say, we are in real big trouble even without these questions.

Otherness

Just another non-exclusive microhistory-issue. According to the well-known story, the question of the “other” arrived to microhistory through anthropological mediation. Dealing with otherness, however, does not make a historical account microhistorical. We are told in several ways that what became interested in otherness is historical writing itself, in the broadest sense. The past seems to be now, to use the famous expression of David Lowenthal (1985), a “foreign country”.⁷ Taking a trip to these “foreign countries” means facing otherness. We are facing otherness in situations when we react like this: “Oh, I can’t believe it!”; when our stories begin like this: “I was astonished to see that...”; or when we suddenly and often automatically conclude like this: “Damn, it makes no sense!” But these situations are only our starting points, they force us to make efforts to understand (without any philosophical connotations of the word) this otherness and even our own reactions to it. So our sudden and automatic conclusion is followed by a question like this: “How is it possible that it makes sense to them?”; our stories continue like this: “...then it became clear that...”; and after our first reaction we ask ourselves in a way like this: “Why is it so hard to believe it?”

The most famous example of the process described above is probably *The Great Cat Massacre* of Robert Darnton (1984). He just could not believe that some eighteenth-century people found so much fun in slaughtering cats. Reading his source about the massacre he could have asked “What’s got into those guys?” and then he just could have passed over it. In this case, however, he would not have behaved like a historian. More precisely, he would not have done anything what is usually expected from a historian, that is, the paradox operation what the anthropologist Vincent Crapanzano ascribes to his colleagues, that they “must render the foreign familiar and preserve its very distinctiveness at one and the same time” (Crapanzano 1986, pp. 52). Not the easiest task, if possible at all. But discussing the possibility of the task is a matter of epistemology, a different (but not distinguished or elevated) level of talking, and epistemology is something I do not want to deal with here. According to the objectives of this paper, it would be enough to assess whether microhistories are successful in fulfilling the operation or not, independent from its epistemological feasibility. But this one seems to be a tough nut to crack, too. The only way I can manage to wriggle out of this challenge is by claiming that historians must not try to fulfil an operation like that. Therefore I shall speak about exploring and presenting otherness as I spoke about “lived experience”, as a curiosity one may choose (or even invent) to satisfy.

Some microhistories are simply not interested in otherness. They suppose that past people were the same as we are now. Levi’s people are behaving throughout his book according to Herbert Alexander Simon’s “bounded rationality” theory. Though Simon describes only the behaviour of his contemporaries (Simon 1983), Levi projects Simon’s results back to the past, as his point of departure. This does not come as a surprise, since – according to Levi – the work of microhistorians “has always centered on the search for a more realistic description of human behaviour” (Levi 1992, pp. 94). Here the emphasis is on the word “human” which does not intend to show any sign of otherness.

As for the Salem Witchcraft case, Boyer and Nissenbaum are close to Levi in assuming that Salem people were motivated by economic interests, just like most of us in the present. Since both *Inheriting Power* and *Salem Possessed* are “etic” approaches, one may think that “emic” ones fit better to the presentation of otherness. Some say that, indeed, what makes past people different from us is what they felt, perceived, thought, believed, and the way they did all of this (Wickberg 2007). So to say, Menocchio’s strange ideas are certainly foreign to us. But, on the one hand, Imhof’s “etic” approach in dealing with worlds we have lost is clearly stands for presenting otherness. And on the other hand, there is Ann Wroe’s “emic” approach, which

intends to show how familiar past people are:

The advantage of approaching so close to fourteenth-century Rodez is that, when one does so, the medieval crowd splits up into its individual elements; each man or woman can be seen as the neighbour in our street, the passenger on our train, the worker in our office, a human soul, despite the mists of the past.

(Wroe 1996, pp. 5-6)

So here we stand at the crossroads again. In choosing a road to walk on we have to give up all the others. We then later walk back to the crossroads and choose another one to walk on, of course, anytime we just want, but everything we have seen on the previous road, remains there. Decisions are sacrifices as well. Failing to consider these necessary sacrifices shows a craving for an old-fashioned notion of a possible historical syntheses which reveals every aspect of human history.

Method and Perspective

Methods are, as I have emphasized a few times above, ways of satisfying our curiosities. Applying methods require making choices beforehand. First we have to have some ideas about what we want to know, that is, about what we are curious about. But our curiosities waiting to be satisfied are complex and manifold. We get into trouble even if this complexity means only two choices. We can be curious of some structures and their similarities, just as we could choose some structures and their differences compared to our own time. We can decide in favour of “lived experience” and its otherness, just as in favour of “lived experience” and its sameness. I cannot imagine what microhistorical method could satisfy these twofold curiosities, if there are several ways in satisfying only one single curiosity, such as “lived experience”.⁸

Nevertheless, we have no reasons to be desperate. For lack of any identifiable method there is, however, a common perspective to talk about in connection with different microhistories. A perspective, unlike a method, does not require application. It is, just like our curiosities, something what we carry with us in our heads into the archives. I do not want to pretend that there is an agreement concerning what this microhistorical perspective means and what it implies. Definitely, it is about action, behaviour, and their preconditions. To Levi it is about “the continuous modification of individual behaviour within a network of contradictory norms”, to Cerutti it is the “creative character of action”, that “people’s actions are not so much relevatory of objective determinations, but are rather claims, intentions, proposals” (Cerutti 2004, pp. 27). What controversial is, is the quality and the limits of that creativity and behaviour. While Cerutti thinks that Levi’s actors, in acting merely within norms instead of contributing to the formation of those norms, are not acting creatively enough, to Michael Seidman the actors in microhistorical accounts in general – so both Cerutti’s and Levi’s actors – are still shackled by different sorts of determinations and by a too coherent and stable social order (Seidman 2007; Seidman 2002, pp. 3-13).⁹

Anyway, perhaps Levi is the one who stresses most the importance of taking microhistory as a perspective. However, it seems to me that he is not doing it sharply enough. He says that “in defining microhistory, one should emphasize the methodological and practical aspects as well as the perspectival ones” (Levi 1998, pp. 55). If we rather speak about microhistory in terms of a certain perspective which is still being shaped, I think that there is no need to define microhistory at all, or to tie it with methods waiting to be applied. In addition, methodology-talk concerning microhistories is in contradiction with the perspective itself. The “creative character of action” should be ascribed to microhistorians as well as to past people. To me it is always surprising to see how different microhistorians (and their fellow “new historians”) are talking about themselves and their own practices. While they speak about the past in terms like “situated cultures” (Cerutti 2004, p. 36-38), or in terms of “occasionalism” (Burke 2005), and while they say that “changes occur by means of the minute and endless strategies and choices operating within the interstices of contradictory normative systems” (Levi 1992, pp. 107), they tend to speak about themselves in terms of methodologies. Unfortunately, this methodology-talk involves that they do not see themselves as actors operating within any interstices, acting situatedly or behaving differently in different occasions. When they think of themselves,

they forget about microhistorical perspective and they rather see themselves as method-oriented experts, as chefs cooking from standard recipes and cook-books.

This doesn't mean, of course, that in historical writing there are no methods at all. There are. But, if we see ourselves just as we see past people, the importance of our methods, our reliance upon them and our expectations concerning them cannot be that far-reaching. The perspective makes us see that satisfying our curiosities is the best methods can do. And it makes us see that methods themselves won't tell us anything about how to satisfy our curiosities. Overcoming the exaggerated commitment to methodology-talk suggest almost the same what Hayden White was talking about in a conversation with Ewa Domaska. It is worth quoting him:

No, I historicize historical learning itself and this is what most historians do not do. They think that they can historicize by putting an event into its context. They do not realize that 'history' is not only *about* change but it is itself – whether understood as a process or as accounts of a process – constantly changing; they do not historicize their own operations. For example, in the field of history of science, they historicize science, but when they take their own field as an object of historical study, they neglect to historicize history.

(Domanska 2008, pp. 16; emphasis in the original)

I read White's words as an appeal which says: "Let's do the same with our own operations what we do with the past..." It means that microhistorians should consider themselves as persons who belong to institutions what Sande Cohen calls "strongholds" (Cohen 2008), and as persons who act within the contradictory norms of academic pressures (which they shape as well). They should take seriously the incoherence of the archives and the contingent aspects of archival work (Starn 2002), the role that accident and luck plays in their researches (McClellan III 2005), the fact that their statements about past people are fashioned by other historians' contesting statements about those people as well as they are fashioned by their own researches, and so on. Seeing microhistories and the behaviour of microhistorians from a microhistorical perspective like this leaves no place to strict application of any microhistorical method. This is what I have tried to show in the earlier pages. Despite methodology-talk, microhistorians often do not, sometimes even cannot, and certainly they should not act methodologically in practice. In their practice they do not necessarily come up neither with a distinctive microhistorical method on every occasion, nor with any appropriate method on certain occasions. They just do their best in coping with documents, questions, problems, narrations and so on, without any methodological consistency, but they do it from their perspective that can be called microhistorical.

One may say that in holding these views I am guilty of reduction. I rather see it as a broadening of possibilities. If we theorize about microhistories as methodologies, we tend to forget about the occasionalism of doing history, and we want only followers. But if we theorize about microhistories as occasional attitudes of practitioners who do not have more common than a certain perspective, we force other practitioners to find their own ways. The latter is, as I see it, the way to keep practice open and flexible. And this is the pragmatic end for which it is worth to stop methodology-talk.

A microhistory of microhistory

Up to this point I was quite cautious in referring to philosophers. Even if sometimes I have turned to them, I did it not because of their deepest insights, but because of their technical devices or useful advices. (Deep philosophical insights have nothing to with the practice of historical writing, they have something to do only with other deep philosophical insights.) So I would like to keep myself to this treatment and finish this essay with a brief autobiographical story from Arthur Danto. I hope that it makes the problems with methodology-talk more apparent. In "The Decline and Fall of the Analytical Philosophy of History" Danto (1995) notices that good theories are those that can explain themselves in their own terms. He is an old Hempelian, so it would not be daring to expect that right after this claim comes a defence of Carl Hempel. But no. Even if Danto still believes that Hempel was right, he has to concede, that Thomas Kuhn's theory about paradigm shifts, the theory that superseded Hempel's "covering law" model, explains itself. Kuhn's book about paradigm shifts

demonstrated a paradigm shift itself.

Transforming Danto's words into a useful advice, the result is the following: the best way in which microhistory (as a practice) could explain itself is, I think, writing a microhistory about doing microhistory. Even if this isn't exactly what I was doing here, I see a strong chance that it would reinforce my position, namely, that talking about microhistory in terms of methodology is anything but not microhistorical. If keeping practice experimental is the pragmatic end, writing a microhistory of microhistory could serve as the technical end for which it is worth to stop methodology-talk. I really hope that someone is going to do that job soon.

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Notes

[1](#) Revised and extended version of the paper presented at the Workshop „Theory and Practice of Microhistory” at Collegium Budapest on 19 June 2009.

[2](#) Beyond these unifying tendencies there are dividing ones too, which multiply the number of microhistories. One can speak about microhistory in a broader and a narrower sense (Szijártó 2003, pp. 499). The latter category contains the work of some distinguished Italian scholars, while the former includes everything that has grown out of a discontent with macroscopic models. There is a social and a cultural microhistory, even if “now seems to be a new convergence of interests” that connects them (Cerutti 2004, pp. 19.), and even if it is much more fortunate to call them “rather social” and “rather cultural” (Szekeres 1999, pp.

3). Another distinction can be made between an episode-like and a systematic (Szekeres 1999, pp. 3), or between a holistic and a reductionist microhistory (Lugosi 2001, pp. 26), and so on.

[3](#) I have the sad impression that there is no better scene to illustrate the domination of the will of methodological application than the Hungarian. See, for example, Erdélyi 2005, or Szilágyi 2007.

[4](#) This is a rather dubious claim anyway, since microhistorians tend to waste hundreds of printed papers to argue against a philosophical position they call relativism. There can be many positions between objectivity and relativism, of course, or one can even say, as some philosophers say, that this distinction is not a really useful one, but microhistorians in their debates with the relativist position seem to be on the side of objectivity. Epistemological issues in detail are out of the scope of this paper, so, unfortunately, I have to make statements here without arguments.

[5](#) For an account on historians who have not tried to evade different features of what is called here “lived experience”, see Wickberg 2007.

[6](#) Following Dewey, Shusterman claims that the fear from the private character of experience “stems from identifying experience with but one narrow philosophical conception of it: as essentially subjective, atomistic sensation of feeling. This conception whose roots he [Dewey] traces to empiricism and the romanticist advocacy of the inner life is not only historically parochial and philosophically narrow but empirically false”. I do not think that Shusterman doubts here the possibility of private experiences, he only notices that our experiences for the most part are shared. Later, speaking about memorable episodes of living, he continues as follows: “For no one but the philosophical sceptic in his professional capacity doubts for a moment that we can ever share such memorable experiences, even if they are always somewhat differently inflected through our different personalities and histories. Indeed, such heightened experiences are frequently remembered not only *as* shared but *because* they are shared. It is only philosophy’s rigid quest for certainty which, seeing the troubling recurrence and perpetual possibility of personal misunderstandings, constructs this into a prison of privacy which bars shared emotional experience” (Shusterman 1992, pp. 27-28; emphasis in the original).

[7](#) In the light of some present commitment to establish close connections with the past, Lowenthal’s claim does not seem to be convincing. Here I think of the enchantment by “memory” as a replacement of history (Klein 2000), or of the rise of postcolonial and subaltern studies. The latter, with the stress on what Dipesh Chakrabarty calls “historical wounds” (Chakrabarty 2007), surely considers the past as a familiar country.

[8](#) Moreover, I cannot say anything about those methods one may apply. I doubt that microhistorians have a lot to say about them. They speak about what they are doing in the archives: they speak about clues (Ginzburg 1989), or about how important is to get lost in coping with their sources (Walker 2006). They speak about techniques of research, but none of them tells anything about what is happening between coping with sources and making statements about the past, about what is the relationship between the process of research and the result of the research. This is, of course, a philosophical notion of method. I am not sure that so much improvement has happened concerning historical methodology in this sense. (Except that philosophical enterprises concerning methodology declared themselves bankrupt.)

[9](#) Calling this perspective “methodological individualism” would be a hit below the belt. What this perspective offers, is rather an anthropology in making.