

Losing the Phenomenon: Time and Indeterminacy in the Practice of Anthrohistory

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Paper prepared for the edited volume, Unsettling Knowledge, Questioning Discipline, David William Cohen (ed.) Ann Arbor: University of Michigan Press.

During the night of 23rd July 1950, around forty peasants from the village of Bârsești (Vrancea County, Romania), armed with firearms and other weapons took control of the local police office, the village council, and the post office and detained several key communist officials as well as the most well-known communist party members in the village, whom they picked up from a dancing party.¹ Towards morning, they made phone calls or sent messengers to several neighboring villages, where similar gatherings were supposed to take place simultaneously. They found out that the other groups had dispersed at dawn, without taking any kind of action. Further south, in the village of Nereju, hundreds of people (some say 300, others 400), armed with pitchforks, axes, clubs, automatic rifles, and pistols gathered in a mountain meadow. They waited for a sign - the noise of a plane, the voice of a messenger, or the light of a fire – in order to begin fighting. The signal never came and so they left, planning to meet again.

The next day, *Securitate* (former Romanian Secret Police) units arrived in the region and there began several months of hunting and terror until all the participants in these events were found, to be arrested or, in case of riposte, executed on the spot. Most of the villagers involved had run away to hide in the mountains, but they were either discovered by the *Securitate* patrols or they turned in willingly, after hearing that their relatives at home were relentlessly questioned and tortured. By late November 1950, 308 people were apprehended, and put on trial in July 1951, the majority of them being sentenced to severe prison terms (from 10 to 25 years).² By 1964 they had been all released, except those who had died in prison. Their families suffered house arrest, humiliation, abuses, and the confiscation of property.

Locally, these events are ambivalently referred to as *chermeza* (fr. *Kermesse*), a revel, or merry celebration held outdoors with music and dancing.³ Subsequent accounts,

¹ My dissertation research in Vrancea (Romania) in 2003, 2004, and 2005-2007 was funded by the University of Michigan's International Institute, the Wenner-Gren Foundation and the Social Science Research Council (IDRF). I am indebted to Dorin Dobrinu for invaluable archival help regarding the 1950 events in Vrancea, to David William Cohen, Josh Reno, Katherine Verdery and Joseph Viscomi for useful suggestions, and to Daniel Lățeș for never letting me err on the side of infallibility.

² There were also five death sentences (Dobrinu 2006). Most of those sentenced to prison were men; there were several women and they received more lenient sentences (1-2 years).

³ Former participants in the events are generally called *chermezani* and they also refer to the events by the moniker *chermeza*. Some of my interlocutors from Nereju suggest that the name *chermeza* was derisively applied by "the communists" so as to mock the failed insurgency, while others connect it to the events in the village of Bârsești where the insurgency coincided with a public ball organized by communist youth union. In the rest of the paper, I follow the use of *chermeza* as it is the appellative with the widest currency in the region. I also follow my interlocutors' usage of "communism" to refer to the political regime in Romania since 1947 to 1989.

those of *Securitate* files and of historians after 1989, speak of “counter-revolution” or “terrorist and subversive activities,” respectively “anti-communist resistance.” *Securitate* investigators, researchers as well as former participants agree that the events had been planned for months beforehand to result in a synchronized insurgency in all the highland villages of Vrancea, with the idea of triggering revolt in other counties and finally, if possible, overthrowing communism in Romania. Furthermore, these accounts also agree that the immediate cause of the July events should be traced to the nationalization of forest properties in 1948 and the confiscation of locally owned lumber saws.⁴ Life in the highland villages of Vrancea was based on the communal ownership of forests, lumber being the main commodity traded in the lowland towns in exchange for cereals and other products.⁵

One name, Victor Lupșa, crops up again and again in all these accounts. He was the organizer, the leader, the Colonel, the liar, the spy and the traitor. Lupșa appeared in Vrancea County in late 1948, living as a fugitive in the mountains and proceeded to organize and coordinate the activities of groups of people from different villages, offering promise, advice, and instruction. After the July events, he disappeared. The archives of the former *Securitate* suggest that he managed to hide in the mountains until 1955, despite the assiduous searches going on for him. Apparently, he surrendered or was captured in 1955 and was executed for his participation in the *chermeza* in 1956 (Brișcă 1998; Dobrinicu 2006). But as the former *Securitate* was never in the business of producing historical truth, it is difficult to take this as a purely factual, disinterested, and complete account.⁶

*Nobody can say whether he was a Securitate agent or an honest man. Almost all who knew him were full of doubts.*⁷

*Whatever he was, I can't say. He pretended to be a colonel. He said he had been in Yugoslavia, since '47, I don't remember exactly, and that he returned to organize people against communists. That's what he told us, but we can't know the truth. He didn't have any relatives in these parts.*⁸

⁴ Soldiers who had fought on the eastern front also brought distressing stories from the Soviet Union, shaping people's expectations of the new communist regime. Hunger constituted a leitmotif of these stories, and in this context, the confiscation of forests and lumber saws was widely interpreted as the policy of a regime bent on condemning its citizens to a slow death.

⁵ Since the Vrancea region was a privileged object of inquiry for the Romanian interwar sociologists and historians, there are numerous works discussing the history and characteristics of local communal ownership over forests. See especially Henri Stahl (1939).

⁶ It is important to note that the archives of the former service have only partially been opened for research. Furthermore, *Securitate* files are records of activities that often involved the recruitment of informers, deals and covert operations. In this sense, some of them could have been deliberately designed to ensure obscurity, especially in view of the fact that the *Securitate* was not a monolithic organization and that its various branches could (and sometimes did) pursue conflicting goals. See Lynch and Bogen (1996) for an excellent analysis of the vicissitudes of evidence in the investigation and historicization of a famous covert operation, the Iran-Contra affair.

⁷ Interview with former participant Simion V. Cojocaru (Bârsești) in Anghel and Alupei (1992: 9). I use real names when referring to interviews already published by journalists and other investigators. For my own research, I refer to interlocutors by pseudonyms.

⁸ Interview with former participant Nicolae Burlui (Bârsești) in Mihăilă (1994: 35).

In different circumstances and to different people, Victor Lupşa was or might have been many things: he was bald, slim and had a lame leg, he had a wife and kin in his native village (Zagon, Covasna county), he was deeply religious, he liked to keep written records, he was involved in money fraud, he lacked training and organizational skills, he was a good organizer, he was trained in Yugoslavia in guerilla tactics, he was in contact with Americans, he was a lumber merchant, a colonel, an honest man, a pathological liar, a leader of anti-communist resistance, a *Securitate* agent, and a coward. Victor Lupşa remains an unknown.⁹

Doing anthrohistory

My goal here is not to set the historical record straight, but rather to examine the kind of work that goes into making history. A good historian might uncover a less ambiguous story by sticking closely to the trail of files from the archives of the former *Securitate* (for instance, Dobrinu 2006). But seeing that I can lay claim to the position of an anthrohistorian (at least in virtue of my training and institutional affiliation), I could perhaps get away with exploring just how ambiguity and indeterminacy are produced and made into resources constitutive of a history.¹⁰

In the course of my dissertation research on the reconstitution of communal forests in the highland region of Vrancea, I heard numerous stories about, and came across various documentary traces of, the failed insurgency in 1950. Contemporary accounts of these events are marked by ambivalence, continually oscillating between potentially contradictory versions of failure. This indeterminacy is actively generated by people whose knowledge practices are fully entangled with the ways in which events unfolded and were investigated at the time. In this essay, I use the situated epistemologies of the “practical historians” (Garfinkel 2002; see also Livingston 1987) of these events as well as some ethnomethodological insights, to discuss the prospective and retrospective features of anthrohistorical ways of knowing.

Participants and witnesses of the events in July 1950 are concerned with making what they see as an instance of failure into a still storyable event. Recounting revolves around the question of betrayal, which is a useful device for making sense of what happened as well as for keeping at bay the retrospective settling of meanings and accounts. Was Victor Lupşa a genuine revolutionary acting in good faith to help orchestrate an insurrection that constituted an inevitable course of action for the villagers from Vrancea, threatened as they were in their livelihood by the nationalization of forests? Was he an agent of the *Securitate* whose specific mission was to set up a loyalty test, that is, to ascertain the extent of villagers’ allegiance to the new communist regime

⁹ Unlike the randomly chosen character of Alain Corbin’s re-creation of a 19th century clog-maker’s probable life and perspective on the world (Corbin 2001), Victor Lupşa is not unknown due to the absence of any kind of traces. I argue that Lupşa is made unknowable in virtue of the kinds of evidence available, the stories possible, and the implications of narrative choices.

¹⁰ I am inspired here by Kenneth Burke’s work on the ambiguity of motives as a resource for (Burke 1969), and his use of “perspective by incongruity” as a method of, analysis (Burke 1984 [1937]). My understanding of the indeterminacies of situated historical production owes much to David William Cohen and A. S. Atieno Odhiambo (2004) and Michael Lynch and David Bogen (1996).

by inciting them to rebel, and thereby deceiving them into an action of improbable success that would ultimately prove destructive?¹¹

Situated knowledge and the production of indeterminacy

These questions only indicate the high points of a whole spectrum of doubts and uncertainties surrounding the events of 1950, the role of Lupşa and implicitly of those who trusted and followed him. They are also the questions that become undecidable, just as they come into the focus of local historical knowledge. In the process of knowledge production, former participants and witnesses relied on a whole array of procedures for the generation, evaluation, and contestation of evidence. Many of these procedures were hopelessly intertwined with the investigative work carried by *Securitate* agents in the aftermath of the revolt as well as with the specific features of the various sites in which evidence could be obtained and interpreted.

Knowledge practices were part and parcel of people's involvement in the organization of the July events: the selection of trustworthy "recruits," the management of secrecy and avoidance, the surreptitious collection of firearms, the setting up of secure methods of communication and the formulation of instructions and goals were all achieved in an uncertain environment where the exchange of words, and even glances, had to be closely monitored. Doubts loomed large while they were on the run, hiding in the mountains and depending on the goodwill of other villagers for food, protection as well as information on the relatives and families sequestered in the village.

During interrogation or in the courtroom, they had to deal with deliberately misleading questions, pre-historicized accounts prepared by *Securitate* agents in the form of "forced declarations," slips of the tongue and, last but not least, fear of death under torture. The interrogatory setting was especially permeated by uncertainty: one could not know if and how much the others had confessed or if the *Securitate* interrogators simply used alleged confessions as bait. Rumors and accounts seeped through the walls of prison cells as well, but given that virtually anyone could have been a planted informer, the questions and answers of other prisoners were necessarily suspicious. For those who made it back to their villages after serving their terms, there followed monthly sessions with the *Securitate* officers assigned to supervise them, as well as intense conversations and disputes with fellow insurgents in addition to other villagers who had developed their own theories in the intervening years. There was also pain, confusion, humiliation and ridicule. And finally, after 1989, it is the time of testimonies at the *Association of the Former Political Prisoners* (AFPP), public debates at the village council on the reconstitution of the communal forests, and interviews with journalists, historians and ethnographers whose questions reflect the timeliness of a grand anti-communist narrative.

I will give a brief illustration of such situated knowledge practices, as they are woven through the texture of the events they are meant to illuminate. A former participant from Nereju, whom I will call Radu, reports his discussion of the question of Lupşa's innocence at a meeting of the AFPP. He reviews his association with Lupşa, the circumstances of their initial meetings, and the facts he knew from Lupşa himself as

¹¹ This series of questions and potential inferences doesn't represent interpretive processes in the minds of individual actors; it is merely shorthand for the different and potentially contradictory versions of the events that would occur during the same conversation, as my interlocutors would actually trace the history of how they came to know that Lupşa was or not a traitor.

background to “the decisive proof.” Radu’s proof is built as follows: Lupşa was insistent on making written records after each meeting of the organization, which he placed in glass bottles, sealed with wax, and buried at an unknown location. Had he been a covert agent of the Securitate, those records should have surfaced during the interrogation and at the trial as incriminating evidence. This is not to say that Radu’s interrogators lacked evidence: apparently they knew he had provided Lupşa with weapons and clothing on a specific occasion, that he had carried an automated pistol on the night of July 23rd, and that he and several others had been charged with a particular mission, to detain the chief of the Forestry office and to take over the local agricultural cooperative. However, “nobody found his writings. Nobody knows what Lupşa and I did together. They [the *Securitate* interrogators] didn’t know what Lupşa and I had talked about.”¹²

Radu is not perturbed by the agents’ knowledge of his actions - after all, they might have obtained that information from other prisoners who were “too stupid” or who simply caved in under torture and confessed - as long as knowledge that was particular to his relationship with Lupşa was missing. For instance, Radu believes he had enjoyed Lupşa’s special trust because of his experience in the war (WWII) as a paratrooper trained for undercover missions behind the front, which made him uniquely qualified for the secret operations required for the organization of the insurgency. Such details of his shared relationship with Lupşa never made it into “the file” (the prosecution’s statement at the trial) and their absence exonerates Lupşa in Radu’s view. However, Radu’s “interactional method of proof” (Miyazaki 2004: 82) discounts the fact that neither his *Securitate* interrogators, nor the prosecution were interested to obtain evidence of what made his relation to Lupşa meaningful, but simply the facts that indicated the criminal nature of his actions. The knowledge he gained then, as well as its retrospective account now, was produced in response to specific questions designed to enable the successful organization of the interrogatory or courtroom setting.

For many other participants, the *Securitate*’s swift intervention the next day after the July events indicated prior information about their activities. While everyone involved was arrested and convicted, Lupşa’s disappearance after July 23rd and his absence at the trial seemed to point him out as “the informer” or “the traitor.”¹³ At the very least, his absence made him suddenly suspect and unbalanced people’s previous understanding of the events. “Almost all who knew him were full of doubts.” These doubts persisted for decades and they remain vivid now, despite the expert assurances of journalists or historians after 1989.

Questions about Lupşa are disconcerting; they make people shift, shrug, sigh and fidget. This is not necessarily because of forgetting or a reluctance to verbalize certain kinds of knowledge. It is rather because instead of answers, they have too many questions of their own. What is perhaps left unspoken is that many people would prefer to leave the

¹² Interview at Nereju (Vrancea), July 20th, 2006. *Securitate* files report that in September 1950 a wooden box containing Lupşa’s writings was unburied at a shepherd’s cote in the mountains near Nereju (Dobrincu 2006). Another set of documents was apparently retrieved upon his capture in 1955, long after the trial of 1951 (Brişcă 1998). However, I do not think these facts would affect Radu’s reasoning in this case.

¹³ Interviews with two men and three women who participated in the 1950 events, Nereju, 2003-2006. *Securitate* files claim that there was indeed an informer infiltrated in the group of insurgents at Nereju, apparently a local teacher. The alleged informer provided very sketchy information about local unrest that is only retrospectively connected by *Securitate* agents with the events in 1950. The details provided clearly indicate that it could not have been Lupşa in this case.

issue undecided, insofar as to settle the Lupşa issue is another step towards settling the meaning of the *chermeza*.

If they were indeed the dupes of a well-trained Securitate agent, their misplaced trust brought immense suffering for themselves, their kin and fellow villagers. Moreover, this was a costly mistake that effectively canceled the chances of a successful insurgency. Not only did they fail to recover the forests confiscated by the communist regime, but they spent long years in prison and returned home to a different world where they were seen as “enemies of the people” or, at best, as misguided fools who prepared for insurrection as they would for an outdoors party (*chermeza*). If Lupşa was not a traitor, then how does one explain the dimensions of failure? How to account for the fact that they did imagine they could intervene successfully against a regime backed by Soviet troops, when they had only pitchforks and guns left over in the mud of the forest by retreating Germans in World War II? How to explain that they had wanted to believe Lupşa’s promises of American intervention, “atomic bullets” and aircraft defense?¹⁴

As a *locally* occasioned history, the *chermeza* persists in virtue of the questions made undecidable by the situated generation and contestation of knowledge. In this sense, indeterminacy is the practical achievement of people whose methods for evidencing claims concerning historical knowledge are constitutive features of the events they describe. By such methods, they try, as best as they can, to be true to the events they experienced.¹⁵ The construction of either version of failure entails not simply an ethical stance, but also a deeply visceral process of knowledge making. Coming to know that Lupşa was or not a traitor is a way of knowing oneself and others as the bearers of historical potentialities, as the vectors of actions that could, and sometimes did, introduce a swerve in the flow of the past.

Time and accountability

It would be simple to argue that the device of Lupşa’s betrayal stands for a refusal to confront the past and to accept responsibility for failure. It is possible to talk of the *chermeza* without making any mention of Lupşa and to talk in a way that explicitly suggests that failure was perhaps the best outcome, insofar as a local success would have prompted a much more aggressive military intervention by the Soviet troops stationed in

¹⁴ “I don’t know about Lupşa, but I know they were fools who believed in Americans and other lies” said Sanda, the wife of a former participant, in a tone that suggested both pity and resentment. “Of course, the Americans never came and things stayed the same or they got worse for some of us.” Sanda was newly married in 1950 and had a one-year old baby. Her husband was imprisoned and returned home after fourteen years. They separated sometime in the late 1960’s and even now are not on speaking terms. Interview with Sanda, Nereju (Vrancea), 10th August, 2004.

¹⁵ It is often the case that specific kinds of repressive states – including the communist one – help to produce indeterminacy and uncertainty in the social body, and that this reinforces their power. However, my concern is with how people themselves work toward creating indeterminacy so as to indefinitely postpone the local ascription of failure. Victor Lupşa – and his potential betrayal – gives a particular figuration of failure, making up a manageable storytelling device that prevents any lapse into generic categories and keeps the story firmly rooted in a local context of accountability. Implicit here is the notion that failure is hard to recount, explain and live with; ironically, the narrative production of indeterminacy requires just as much hard work (it is not a given state, but something that needs to be made and re-made with every telling of the story of the *chermeza*). I thank the editors for helping me to clarify this point.

Romania at the time.¹⁶ It is also possible, in specific situations that require potent arguments, such as public debates on the restitution of communal forests or official encounters with government representatives, to fully uphold an unambiguous narrative of the *chermeza* as heroic anti-communist resistance. But the possibility of such accounts does not necessarily indicate that the meaning of the 1950 events has been settled. It does, however, offer clues about the extent to which narratives are embedded in temporally specific contexts of accountability.

The locally emergent history of the *chermeza* assembles both the temporality of the 1950 events as well as the sequence of accounts of the events produced after the fact in the interrogatory setting, the courtroom, the prison, village conversations and disputes or interviews after 1989. As the events in July 1950 unfolded, actors were, so to speak, caught up in the production of history. They had to anticipate and respond in real time to unforeseen circumstances and to accomplish actions that revealed themselves only partially as they developed. They did not fully know they were involved in “counter-revolution” or “anti-communist resistance.” As a former participant confessed “I went to the meeting [on 23rd July] because my older brother told me to. I didn’t know exactly what we were doing, but something had to be done.”¹⁷ In the temporality of the original events, actors did not have the benefit of hindsight, relying instead on the “in-hand intelligibility of a world ‘not yet’ reflected upon” (Garfinkel 2002: 153), a world they were busy producing on the spot with just the knowledge available at the time.

The sense of those actions as a “prospective achievement” fraught with uncertainty, misunderstanding and surprise, but nevertheless realizable, can scarcely be recovered from retrospective accounts, be they produced immediately after the event or fifty years later. For instance, the assembly of 300 or 400 people on July 23rd appears retrospectively as unproblematic, as the following of instructions presumably given by Lupșa. But Lupșa himself was in direct contact with only a handful of people and so the work of making the assembly possible – how such an action was made intelligible at the time, through what kinds of motives and methods – is largely lost.¹⁸ The unfolding coherence of events at the time is different from the already produced coherence of the retrospective telling.¹⁹

¹⁶ In response to one of my tendentious questions, asking him to choose among possible reasons for his involvement in the 1950 events, Radu gave the following account: “We heard this rumor in ’48 that they [communists] had begun the nationalization of forests. They wanted to take our forests and people were in despair because they didn’t have other means of living. And then we rose in rebellion. For the forest. And maybe our rebellion wasn’t well thought out, but something good could have come out of it. Because, maybe if we succeeded here, other people would have rebelled in the lowlands and everywhere... But this was the time of the Russians and their program was that if they couldn’t suppress rebellion, they came to bombard the villages and turn them into steppes. To destroy! Nobody wanted to have their villages destroyed. And so, in the end, I was content that they caught me. If I ran in the mountains, I’d have been shot.” Interview at Nereju (Vrancea), July 20th, 2006.

¹⁷ Interview with “Toma,” Nereju (Vrancea), July 1st, 2006.

¹⁸ In the way of more or less grounded speculation, I can suggest that some of the villagers went to the July 23rd meeting not because Lupșa instructed them to do so, not to fight communism or to recover their forests, but because their brothers, neighbors or people whom they respected told them to do so. Even if no one told them anything, how could they be missing when several others to whom they oriented their everyday actions went there and so it seemed the normal, ordinary and sensible thing to do?

¹⁹ For a slightly different take on this problem of narrative temporality, see Bauman and Briggs (1990) on the distinction between narrated events and narrative events.

Retrospectively, the practical historians of the *chermeza* cannot but silence many of the endless minute contingencies which had to be met in organized anticipation at the time of the events. However, to the extent that their accounts are grounded in and keep track of the variously situated knowledge practices that made reflection possible, they remain accountable to the temporality of their own processes of memorialization and historical production. This specific accountability makes it impossible for them to find and accept invariable answers. The indeterminacy produced by the device of Lupşa's betrayal, among others, constantly undercuts the formation of a locally definitive account.²⁰ In doing so, it stands as a local reminder of historical fallibility, informing prospectively the repertoire and qualities of achievable actions.

Losing the phenomenon

This discrepancy between the prospective and retrospective features of social practice is by no means a new issue for the social sciences (Schutz 1967 [1932]; Mills 1940), but some of its implications bear repeating. Let me approach the problem via Harold Garfinkel's penetrating critique of theorized accounts, which find in the world precisely the same phenomena they had already postulated (2002: 263-285). He illustrated the pitfalls of this process by reconstructing Galileo's inclined plane demonstration of the real motion of free falling bodies. Garfinkel did not attempt the exact replication of Galileo's experiment as an already made event; on the contrary, he deliberately introduced small "inaccuracies" in the experiment so as to understand what would have been the problems faced by Galileo in his real-time endeavor to design what eventually became a successful demonstration. As Rawls (2002: 47) puts it, Garfinkel "wanted to understand what about the experiment *could go wrong*" (author's italics). By doing so, he took most seriously the issues involved in the recovery of a prospective achievement rather than a retrospective account (Rawls 2002: 34). His key insight is that, in contrast to theorized enterprises that work backward from an already refined product, the process of discovery is always liable to "lose the phenomenon."²¹

Retrospectively, Galileo had to design an experiment that would prove something about gravity, being accountable to the existing field of scientific knowledge. This is what Garfinkel (2002: 173-5) calls "classical accountability." Prospectively though, Galileo lost his phenomenon many times until he designed an experiment that would actually work, being "naturally accountable" to a wealth of concrete contingencies: the minute problems of measurement, timing, the resistance of various materials that made up "the phenomenal field of detail" (Garfinkel 2002: 278). Garfinkel's pedagogical re-enactment preserves the indeterminacy of the original experiment, revealing the ways in which the phenomenon could have been lost as well as the sort of organized anticipation of contingency that prevented that.

²⁰ Dwelling on the temporal incongruity between retrospective anthropological analysis and the prospective orientation of Fijian knowledge practices, Hirokazu Miyazaki (2004) understands locally produced indeterminacy as the necessary impetus for action, the precondition of a practical philosophy of hope.

²¹ Losing the phenomenon is a constant preoccupation in Garfinkel's studies. In particular, his "tutorials" dealing with recordings of everyday actions such as rhythmic clapping in time with a metronome or the act of listening for a ringing phone try to recapture the work of anticipation that is lost in retrospective accounts, be they recordings or narratives (Garfinkel 2002: 145-168).

Being able to lose the phenomenon, or dealing with indeterminacy, is generally essential to all kinds of practices, including everyday, ordinary doings and academic inquiries. I also think it is specifically important for the transdisciplinary and as yet unfolding project of anthrohistory.²² Is anthrohistory liable to work like a theorized account rendered virtually infallible by retrospection? Is it able to lose phenomena?

By way of critical illustration, I point to the problems of a “historical anthropology” (Shaw 2002) that is motivated precisely by a puzzle of the unexpected: the seeming forgetting of slave trade and colonialism in postcolonial Sierra Leone. This kind of functionalist false surprise about forgetting is first of all an effective and common rhetorical strategy, because, as expected, it only paves the way for a thorough excavation of memories (see also Cole 2001). It is also a device by means of which silences and things not spoken of are rather summarily treated as kinds of forgetting (Sider and Smith 1997). Last, the very use of forgetting in a transitive form – the forgetting of the slave trade – makes retrospective knowledge claims, implying that there once existed an experiential object such as the slave trade.²³ Furthermore, it would follow that such an object of forgetting can indeed be neatly mapped out on “the slave trade” as the product of scholarly periodization and synthesis.

Shaw’s answer to this puzzle takes the form of an equation of practice to memory. Memory is practice insofar as it is also non-discursive, “implicit” or “embodied”, while practice is memory insofar as it has the capacity to condense and perpetuate historically patterned actions and meanings.²⁴ Thus, Temne ritual practices – such as techniques of divination, diviners’ visionary experiences, rumors of cannibalism, and practices of witchfinding – are memories of the Atlantic slave trade and the colonial “legitimate” trade, spanning four centuries of forgotten historical processes.

Shaw can scarcely claim a total non-discursivity for the ritual practices she investigates; it is their mnemonic qualities which lack verbalization and need to be properly excavated by the anthrohistorian. In this context, only the observer equipped with the necessary historical knowledge can probe the temporal depth of practices and thereby identify them as “memories.” Shaw does so by tracing continuities or disjunctures between the observable practices of current diviners and the accounts of past practices offered by colonial officials, missionaries or academic historians. For instance, a current Temne practice of divination based on contracts between diviners and tutelary spirits “recapitulates” or “crystallizes” images of landlord-stranger contracts from the Atlantic slave trade, practices of intermarriage between Temne and Mande invaders as

²² Ian Hacking (1995: 234-257) would say that indeterminacy is as important for history and in general for the memory sciences, to the extent that newly available descriptions of actions have retroactive effects on the definition of past actions, so much so that the past itself is rendered indeterminate.

²³ Jeff Coulter (1985) notes that transitive forgetting is predicated on a positive ontology, in contrast to a failure of recall or a simple “I don’t remember” that remain equivocal with respect to the existence of the event or action in question. In a critique of the inherently positive valuation of memories, Johannes Fabian (2007: 77) articulates a similar insight: “Is it not inherently contradictory to use forgetting as a transitive verb, designating an action that has an object? The contradiction lies between the *negation* expressed in a forgetting whose meaning is constituted in opposition to remembering and the *affirmation* of some content that is being forgotten.”

²⁴ Both analytic gestures are more or less indebted to Pierre Bourdieu’s notion of habitus as “forgotten history” bodily remembered as “second nature” (Bourdieu 1977). See Throop and Murphy (2002) for a pertinent critique.

well as colonial practices of seizure for indebtedness (Shaw 2002: 106-115). Whatever the often invoked “recapitulation” means, it is difficult to understand how a present *practice* is constituted as the memory of variously situated and occasioned *accounts* of past observers, unless one ignores even the possibility of a “fallacy of misplaced concreteness.”²⁵

Non-discursive memory is predicated on a process of embodiment that does not necessarily require the input, awareness or even understanding of Temne actors.²⁶ Rather, it occurs by recursive recapitulation: memories of the Atlantic slave trade mediated experiences of colonialism, and together these mediate the experience of postcolonialism, acting as a critique of the contemporary injustices brought by neoliberalism. This alleged hidden and silent underside of practice, which has so exasperated social theorists (Turner 1994) is recovered here as an eminently passive analytic material.

It is important to note that Shaw’s fieldwork in the late 1970’s dealt with questions of power, knowledge and gender in divination rituals. It was only in the early 1990’s, while residing at Harvard, when she read historical sources from the 15th to the 19th centuries and acquired “the conceptual framework of historical anthropology” that she came to see rituals as memories (Shaw 2002: 43-45). It is then unfortunate that anthrohistory, retroactively applied, yields the isomorphism between theory and ethnography. Shaw seems to have found exactly what her theorized account promised: ritual practices not only embody memories, but the memories are implicit condemnations of the postcolonial present. In a refreshingly straightforward essay, Sanders (2008) articulates this very problem, by taking up the literature on African ritual and occult practices as critiques of neoliberalism (Sanders 2008: 108-9). In this context, Sanders discusses what I think is a more pervasive problem: a seductive but tautological analytic strategy by means of which anthropologists strive to produce ethnographic evidence for their own politicized sensibilities and theorized expectations. Such analytic strategy works like clockwork, being unable to lose any phenomena.

Against retroactive anthrohistory

The encounter between anthropology and history unfolds as a highly theorized enterprise that constantly questions and reshapes its domain. As a work of situated retrospection, the project of anthrohistory cannot but keep track of all the dialogues, exchanges, critiques and clashes that, in fact, make up its history. But to the extent that it is (and should be) a work in progress, sometimes precariously situated in-between disciplinary boundaries, anthrohistory is also a form of anticipation, prospectively orienting the attention of its

²⁵ My point is that the status of these historical sources requires minimal problematization: in what sense can they be taken as descriptions of the past, and especially of a past that persists in present practices? Would they be recognizable to the Temne as accounts of their past? Birth (2008) develops this as a critique of homochronism, that is, the equation of “history, a representation of the past, with historicity, a representation of a connection to the past.” But see Trouillot (1995) and Price (1998) for a sensitive treatment of multiple historical accounts. On the fallacy of misplaced concreteness, see Whitehead (1997).

²⁶ Shaw gives little consideration to how memory practices are “always socially occasioned,” as Jennifer Cole (2001: 106) puts it. Engaged in a similar project in Madagascar, Cole treats the memories of ritual practices as potentialities that require social occasioning, individual purposes and projects, and social recognition, in order to be variously actualized. The practical nature of remembering is the substantive achievement of variously situated actors, and not an inherent feature of formally designed processes of recapitulation. See also Lambek (1996).

practitioners to a world where pasts and presents are still being made. Actually using anthrohistorical ways of knowing helps one tune into the uncertainties of knowledge production be they problems of transdisciplinary reflection, or the resilient questions of people practically historicizing their experiences and their methods for making sense of the world.

It is a commonplace that knowledge is most often actualized retroactively. As William James (1997: 102-20; see also Latour 2008) put it, “the retroactive validating power” courses through all the knowledge acquisition pathways in a “continuous scheme” of sequential experiences. Yet the very continuity of this uninterrupted chain of experiences that makes up the practice of knowledge implies a constant real time engagement, a process that is, and must be, liable to failure in order to preserve and fully account for the countless indeterminacies of the world. A familiar trope approximates this trajectory by the moving back and forth between theory and ethnography. My attempt here has been to tamper a while with the latter distinction in order to emphasize the usefulness of anthrohistory as a real time practice, a method of historicizing knowledge, rather than simply a (potentially infallible) theorized account. In other words, I suggest that anthrohistorians should take delight in their liability to err and fail. Or, if this proves too disheartening, to at least keep in mind the many things that could and perhaps did go wrong until the events and practices they study finally took (theoretically) recognizable shapes. To apply anthrohistory retroactively as a self-sufficient, closed, conceptual framework is possible, but it *would* lose the phenomenon.

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In-text fragments

1. [If possible on p. 9, current pagination, right before paragraph that begins with “If they were indeed the dupes...”]

“My memory is full of holes,” people say casually, nonchalantly, not wishing to dwell on the matter... If it isn’t indispensable, what’s the use of tiring oneself, driving oneself crazy in the effort to fill that hole, why waste one’s time? But what it has left behind it here, this opening, this disjointed, dislocated breach, makes everything reel, the hole must absolutely be filled in, it must at all costs come back, embed itself here once again, take its full place... (Sarraute 1997: 18).

2. [If possible on p. 17, right before the section “Against retroactive...”]

Analyses begin with a simple and anthropologically appealing proposition, the idea that modernity, neoliberalism or globalization does bad things to good people. Because this popular notion fits neatly with our anthropological sensibilities, and because there is plenty of empirical evidence to support it, it often provides the taken-for-granted and hence untheorized foundation upon which such analyses are erected. The analytic strategy then becomes one of producing ethnographic evidence to support this point. The question for readers is not what the answer will be – we already know that – but rather how authors will muster their ethnographic data and cleverly craft their argument to get there. Such analyses then conclude by restating their starting point that good people have been hard done by bad things (Sanders 2008: 108-9).