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Learning to get involved with people*

A conversation with Rolf Lindner about the ethnographer's fear of the field

Abstract: Rolf Lindner argues in his essay *Die Angst des Forschers vor dem Feld* from 1981, that the ethnographer's first meeting with the field might cause anxieties that, when reflected upon as part of ethnographic knowledge production, might hold important insights into the field itself, on the one hand, and into ethnography's implicit normative frameworks, on the other. In this interview, Rolf Lindner talks, 34 years on, about how the essay came about, how it connected to and intervened in dominant academic discourses at the time, and what the essay's legacy is regarding methodical and methodological teaching in contemporary cultural anthropology.

Keywords: embodiment, emotions, ethnography, fieldwork, participant observation, methodology

Methodological training in European Ethnology has undergone enormous changes throughout the development of the discipline. Participant observation, for example, has not always been an unquestioned part of methods courses in *Volkskunde* (the German term for what could be called a hybrid version of folklore studies, ethnology and cultural anthropology). That this is different today is partly due to the influence that debates about methods and methodology in American cultural anthropology had since the 1980s, but can also be understood as a result of the reorientation of *Volkskunde* as an empirical cultural science since the late 1960s, in which the researcher's experiences and their closeness to the research field play a central role. Rolf Lindner published his article *Die Angst des Forschers vor dem Feld: Überlegungen zur teilnehmenden Beobachtung als Interaktionsprozeß* ['The Researcher's Fear of the Field: Thoughts on Participant Observation as an Interactive Process'] in *Zeitschrift für Volkskunde* in 1981. Ever since, this article has had an unquestionable influence on methodological debates and training in European Ethnology. In the article, Lindner discusses the social and interactive foundations of research in *Volkskunde* and cultural anthropology. Lindner's argument, that understanding of and insight into the field are directly dependent on the researcher's personal involvement and engagement with the field and that this involvement should not be avoided as often proclaimed at the time, has certainly left its mark on the training of students and

* German version in *Zeitschrift für Volkskunde* 2017, 113 (1): 64–76. Translated by Jadon Nisly.

on research practice in European Ethnology. *Die Angst des Forschers vor dem Feld* has become part of the methodological canon in *Volkskunde* and its basic argument now often figures as a methodological starting point in discussions about knowledge production in *Volkskunde* and cultural anthropology. But how did this article actually come about? Which reflections and personal experiences as well as disciplinary discussions influenced Lindner's argument? And what relevance does *Die Angst des Forschers vor dem Feld* have in contemporary discussions about the relationship between research field and researcher? Rolf Lindner, Professor Emeritus of European Ethnology at Humboldt University in Berlin, and Sebastian Mohr, Assistant Professor of Educational Sociology at Aarhus University in Denmark, met for a conversation on November 9, 2015, to explore these questions, and the following text is an edited version of this conversation.

Mohr: Your article *Die Angst des Forschers vor dem Feld* was published 34 years ago in 1981. However, it is still relevant today, especially when it comes to the methodological training of students in European Ethnology, and that's also why I would like to talk to you about how the article came about, how it was received and what impact it had, and, of course, also about the main arguments of your article. Do you remember how it came about that you wrote this article for *Zeitschrift für Volkskunde*?

Lindner: I had originally written the text for an introductory seminar on research methods when I was still a lecturer in Sociology at the Free University of Berlin. At that time, I always contributed to my seminars with a text of my own, either at the very beginning as an introduction, or at a point where we were summarizing important themes. And that is how the text came about. It was intended for students who were taking part in that particular seminar, and it had never occurred to me that I could publish it. It was thought to be a practical teaching tool. About the same time, that would have been the winter semester of 1979/80 if I remember correctly, a colleague of mine from the journal *Ästhetik & Kommunikation* and I visited Tübingen to interview some of the cultural studies researchers there. We had done an issue about the Centre for Contemporary Cultural Studies in Birmingham four years earlier, and we wanted to continue with one about the Ludwig-Uhland Institute for Empirical Cultural Studies. In connection with this visit, I of course got to meet Hermann Bausinger. After our meeting I gave him a copy of the paper, which was just a simple typewritten text at the time, to follow up on our conversation and as an example of what I was working on in Berlin. And then something happened that I did not expect: Bausinger cited my paper in his next article (1980). And I don't remember exactly how anymore, but I contacted Bausinger, because he was the editor of *Zeitschrift für Volkskunde*, and asked him whether it would be possible to suggest the paper for publication in the journal, since he already thought it was

worthy of citation. And he wrote me that he liked the idea. Yet, the funny thing is that the paper was never written for publication and now, nevertheless, has almost become a classic text in our discipline. And that's what I really like about this story, this circumstance.

Mohr: Was there a particular reason why you wanted to discuss the problems of fieldwork with sociology students at that time?

Lindner: At the time there was almost no qualitative research being done in sociology. The custom was to conduct empirical social research with standardized questionnaires, and statistic courses with final exams lasting several hours were mandatory. Because of this focus on quantitative research, I was very interested in learning more about the kind of cultural studies that employed ethnographic research, or, in other words, 'empirical cultural studies,' since I felt somewhat unhappy among the sociologists at the time. Participant observation, fieldwork and so on were basically unheard of in sociology back then. The institute at the Free University was extremely political at the time, and you basically spent all your time on an exegesis of *Das Kapital* rather than doing empirical research on everyday life. It was, therefore, important to me to provide an impetus in that direction. I basically used the paper as a sort of midterm summary during the course.

Mohr: How did you come up with the title?

Lindner: I was inspired by Peter Handke's story "The Goalie's Fear of the Penalty Kick." (1970) And I have to admit that my own past is, of course, also reflected in the title. Before I became a student, I had worked as a copywriter for an advertising company, something which I had not shared with colleagues for a long time. My job there was to create headlines, and I think that the title should also be seen as part of that tradition. I have a tendency to capture a central theme or argument in my titles. That is probably most obvious in my book *Walks on the Wild Side* (Lindner 2004), because the core argument about the motives of early urban researchers is basically captured in the title. But I also simply enjoy coming up with titles that have more of a literary style. At the time, you probably would have used the subtitle – *Thoughts on Participant Observation as an Interactive Process* – as the actual title. I think that would have been the "normal" sociological title, but not *The Researcher's Fear of the Field*. However, that title captured precisely the main idea of the article.

Mohr: Did you rewrite the paper after it was clear that it was going to be published in *Zeitschrift für Volkskunde*?

Lindner: I kept a copy of the original version and, therefore, can retrace what I changed. The editing basically consisted of reducing the sociological touch of the manuscript, so to say, which had given me a bad conscience regarding Bausinger and *Zeitschrift für Volkskunde*. Therefore, I wrote "volkskundliche" research instead of

sociological, for example. However, I did not change anything about the argument itself, and Bausinger had also written to me, and I quote, that the predominance of ethnological and sociological examples was not actually a drawback at all, since they would signal to researchers in *Volkskunde* the fundamental importance of the argument.

Mohr: What were immediate reactions to the publication?

Lindner: The most noticeable reaction came from Ina-Maria Greverus. She sent me an encouraging letter, but most importantly, she then published an article with the title *Die Sehnsucht des Ethnologen nach dem Feld* [‘The Researcher’s Longing for the Field’] (Greverus 1982), in which she focuses on the positive emotions associated with doing fieldwork. That was, of course, the best compliment one can give: writing a second article with a title that is a clear reference to the original paper. Other than that, I don’t really know how the article was received in the discipline at the time. But, of course, you have to consider that the article also could have been looked at as an affront from people within the discipline, since I was a sociologist and, therewith, foreign to their discipline. That kind of reaction would not be so much about the article’s argument, but rather about me as a person: “Who is he, this Lindner guy from Berlin?” And Berlin (or to be exact, the Free University) was looked at suspiciously to begin with from within conservative circles of *Volkskunde* because they assumed sociology in Berlin was comprised only of Marxist-Leninists. But since I wasn’t part of the discipline (*Volkskunde*) at the time, I also wasn’t really aware of those internal voices. It is very likely that the paper itself was more or less positively received since it filled a gap in the methodological literature. Students picked up on the text rather quickly, I think.

Mohr: After the introduction, you begin your article with a clear reference to non-European ethnology. Why this sort of reference in an article for *Zeitschrift für Volkskunde*?

Lindner: I always had transdisciplinary interests. I wanted to look at specific cultural figures from different fields and how their habits and mentalities overlapped. I was, for example, interested in what the commonalities were between a detective, a reporter and a field researcher, and that is how I came across the story of Robert Park in the first place (Lindner 1990). I am not interested in a ‘banal’ disciplinary history, but I instead want to explore to what extent disciplinary history gives us insights into societal thinking and patterns that are also articulated in other cultural figures outside the discipline. And then you see parallels between the ethnographer, who is perceived as a missionary, a governmental bureau agent or a salesman by indigenous groups in South America, and the researcher, who is identified as an efficiency

specialist or a management spy by industrial workers. These sorts of perceptions in the field hold important social insights.

Mohr: What still seems so interesting about your article today is its focus – the researcher’s fear of the field – a topic which is relatively private. Aside from what could be called confessional literature in ethnography, very little is written about the topic. What personal experiences of yours formed the background for your article?

Lindner: It was actually a way of processing my own experiences in the field, where I behaved similar to what I later generalized in the article: I walked up and down the street and so on, before I rang a doorbell or even introduced myself. In other words, my own experiences contributed to it, and that is the strength of this kind of writing, that it is not just based on random abstract reflections, but rather on concrete anxieties and fears which then played a central role in the drafting of the text.

Mohr: But we read nothing about your own personal experiences in the article itself.

Lindner: I did not try to hide it on purpose; instead, I just did not want to make a big deal out of it. I did not want to compare my own research with the great works in the field. So, it was more like an act of humbleness.

Mohr: I was actually thinking more about the academic environment at the time. An article that focused on the interpersonal aspects of social relationships in the field and the personal challenges connected with that probably also challenged the so-called ideal of objectivity affective in the social sciences in 1981. You refer to this dynamic as the “methodological lie” in your article: the academic silence about how social relationships develop in the field, how they contribute to specific perspectives and experiences, and so on. An article that went beyond that by using the author’s personal experiences as a starting point might not have been publishable?!

Lindner: The signs of the times are definitely present in the text, though more in connection to an interest in scholarship on working class culture, that is actually the historic context of the article. My research in the Ruhr valley, in working class neighborhoods, makes up the experiential background that I discuss in *Die Angst des Forschers vor dem Feld*. Because to simply walk into a mining town was a rather delicate thing to do for someone who is not a smooth salesman type. Some of these neighborhoods were rather enclosed territories. And in that situation, you are as foreign as you possibly can be. These are not cities, not even villages where you would meet some openness. Instead, you are in an enclosed settlement, where literally everyone knows everyone and where people recognized you as a stranger from 100 meters away. This field experience, that is in the text. My research about punk (Lindner 1978a, 1978b) was much easier in that regard, although in that field I was also asked: “What are you doing hanging out with punks at your age?” or

was accused of being a hippie, their symbolic adversary. My experiences with these moments of initial contact in the field form the background of the article. And, of course, as a researcher, you would love to be invisible in these moments. And that kind of initial contact, those are rather delicate situations. But in those moments, you are also – and that is the important methodological argument of the article – in those moments of contact, you can gain very important insights. I still remember a specific, for me, quite embarrassing scenario rather well. I was trying to establish rapport in a pub in a working-class neighborhood, and, as a way of trying to act ‘natural,’ I simply played a slot machine. However, something happened that had never happened to me before: I hit the jackpot. And, of course, after that, I could forget about talking to anyone in the pub: here comes this guy who had never been here before, puts one Deutschmark in the machine and hits the jackpot, something which people have probably been trying to get for weeks. Looking back now, it is actually a wonderful memory, but in that particular moment, I would have loved to have been able to turn back the slot machine. And I put all the money back into the machine, so I would not leave the pub with the winnings. But that made me probably seem even more strange in their eyes, so that I truly looked suspicious.

Mohr: These kinds of dynamics of a researcher’s fear of the field can even extend beyond the initial encounter. From my own experiences, I know that anxiety about fieldwork can accompany you permanently. I have, for example, noticed some form of internal resistance before meeting people for interviews: “I don’t really feel like interviewing right now.” And that feeling doesn’t disappear until I’m in the middle of an interview.

Lindner: There are certainly individual differences in that respect. There are also people who find it less difficult. Maybe I am romanticizing too much, but some people have no problems with that at all; they can talk to anyone. And that is definitely enviable, because these people have an openness that doesn’t bother people in the sense of “Look out, here I am,” but rather an openness in the sense that they enjoy approaching people and meeting them. I think in order to do qualitative research, you have to have a certain openness and natural interest in other people. That was also my experience with students when teaching methods courses at Humboldt University. There were students who were simply better suited for the archive, they just ate it up, so to say, and then there were students for whom interviews and participant observation were a much better fit. And you can’t force that. It doesn’t make any sense to say to someone: “You have to do this now.” I am convinced that that just ends badly.

Mohr: Maybe that is part of why the paper still seems so relevant today, especially regarding teaching methods: it speaks to contemporary themes. Demands that the researcher should involve themselves in the research process are repeatedly dis-

cussed, currently especially in connection with emotional and affective experiences, and so on.

Lindner: In that sense, the article is certainly still relevant. Dialogic research, autoethnography, the role of emotions, of affects, of fears, those are all topics that are immensely popular right now.

Mohr: Right at the beginning of your article you write about the reciprocity of observation, that is, when the researcher realizes that they are also an object of observation. What is the epistemological potential of thinking through the reciprocity and sociality of fieldwork?

Lindner: It would be naïve to think that we observe, but that the people we observe do not. Of course, they are evaluating us from the first moment on. It could be that they have a completely wrong picture of us, just as we can also have a completely wrong impression of them. But nonetheless, this kind of assessment is reciprocal: "Who am I dealing with?" When a stranger enters a room, they are immediately evaluated based on their clothing, manner of speaking, their gestures and so on, allowing a social and cultural classification. And that is an age-old methodological phenomenon. For example, the first undercover researchers in factories were always recognized by the workers because of their hands (e.g. Göhre 1891). They had smooth hands and had obviously never worked hard. This kind of assessment happens, of course, all the time. If you don't know someone, naturally you want to be sure about who you are dealing with. And that's also what happened to me during my research on soccer (Lindner and Breuer 1978), where an interviewee scrutinized me in a rather likeable way. He said to me: "Oh, you know, Court Street, if you go down Court Street, you then come to... Oh, I can't think of it right now. You come to...?" And he was, of course, expecting that I would know the street that he was looking for. In that way, I was being tested to see if I really was from Bottrop and whether I had told the truth. And if you were from Bottrop then of course you know that Court Street leads to Friederich Ebert Street or something like that. If you fail in these kinds of situations, they will not rub it in, people are too polite for that, but it may make them a bit more reserved regarding what they will tell you. And these kinds of experiences of reciprocal observation, of assessment, we all live with these kinds of experiences. They are part of urbanity and life in the city, these quick assessments; who am I really dealing with here. And as an intellectual, at least at the time when I was doing my research, you were especially met with a certain amount of distrust: "What kind of guy is this? Can we trust him? Will he misuse our information?" and so on. This kind of mistrust needs to be understood as part of the encounter and should be met with the kind of openness that I was talking about. At the time, some colleagues partly criticized this standpoint and claimed that openness was not the right research strategy, that, instead, some kind of scholarly

secrecy should be preserved. But my reply to that kind of critique would be that openness is the only thing that ever helped me to learn more about my fields. Any other approach would have meant failure. For me, this has always been self-evident, and I was astonished about people's naïveté when they assumed that it is them who are in control of the situation and that they always knew how things worked and what to do. But the others, from whom they wanted to learn something, they were supposed to be stupid or dumb somehow. At the time it struck me that this kind of intellectual arrogance was the rule rather than the exception.

In that respect, times are different today. Today, it is not as unusual anymore to be approached by someone. Almost everyone knows what fieldwork is by now, and everyone also knows what an interview is. At the time though, that was different and these things were new and unknown, and a recording device caused distrustful glances: "Are you recording this?" Nowadays, people are more likely to check whether the device is the latest model, and a recording device will, thus, say something about how professional an interviewer really is. Today, in many cases, it will be more important to find out what kind of sources an interviewee referenced as part of their narrative. Here, the interesting question then is to what extent media and second-hand experiences are mixed together and presented as one's own position. My most fascinating experience in that regard was during a student research project at Humboldt University about the settlement movement in Berlin between the time of the *Kaiserreich* and the time of the Weimar Republic (Lindner 1997). Apparently one of our interviewees, somebody who had lived through the 1920s, had prepared themselves for the interview and read through some of the material from back then. And when we read through the interview transcript I realized: "Hey, I know that passage!" And we could then show which book the interviewee had read before. That too is a part of fieldwork.

Mohr: So, the question of symmetry and asymmetry that you address in your article is a completely different one today.

Lindner: The method handbooks of the time were rather positivistic, even within qualitative research. One always had the impression that the ideal form of research proclaimed in these books, which was 'sadly, sadly' not always attainable, was actually invisibility, to be able to simply observe what the research subjects are doing without involving yourself in any way, as if you were behind a one-way mirror, which of course was still normal at the time in psychological research. And even in books which reflected on this dimension, the focus seemed to be on the discomfort of being caught while making your observations. In my opinion, the handbooks were catastrophic. That only began to change during the 1970s and after American discussions about dialogic research had arrived here. But up until then, there was a tendency to pretend there was a symmetrical relationship. Yet, implicitly, the entire

field was conceptualized as an asymmetrical relation, as part of which the researcher was supposed to control everything. And this, in fact, causes the researcher's fear of the field, which, as I described at the time, was paradoxically the result of anxiety avoiding strategies.

Mohr: I also meet this kind of thinking among many students today; students who are afraid that if they speak with someone or do participant observation, they will influence the situation and that exactly that intervention would be unscientific.

Lindner: There are no encounters that do not change the situation. The encounter itself is already the first big change, and that is exactly what I wanted to get across in my article.

Mohr: In your article, you connect the question of reciprocal observation and symmetry/asymmetry with questions regarding scientific legitimacy: it is not only the individual who is constituted in and through social relations, but also sciences in general and the researcher or scholar in particular.

Lindner: That was also due to my encounters in the field. At the time, researchers would normally explain the purpose of their research by saying: I am writing a book about the topic x,y,z. But that was extremely hard to convey back then, because for most people, a book was a completely abstract thing. When I was asked about what the purpose of my work was in the context of my research on industrial workers, I used to say that it was for a graduate thesis. That kind of explanation was always met with sympathy, because people then wanted to help me pass my exam. Working-class people had a particular kind of respect for studying: "Yeah, if I can help you by having a conversation, I'd be glad to do it. If that helps you pass your exam," or something of that nature. The way they understood it was that I had to write a paper as part of getting my degree. But a book, that was too abstract.

Mohr: This kind of confrontation then also raises questions about the legitimacy of ethnographic research in general: do we have the right to explore the lifeworlds of people and ask them questions? Do ethnographic researchers have the right to enter people's private lives?

Lindner: I also have serious doubts about that sometimes, about whether we should do everything that we do. It is not for nothing that field researchers were sometimes compared to spies by people. But I think that the question of legitimacy is also connected to whether one can tell a story with an emotional depth, that it touches protagonists as well. That, of course, doesn't mean that you should approve of everything that is confided in you. Franziska Becker wrote about that once in an essay (Becker 1989): can you understand too much? And I thought that was a central question. Yes, there are limits to what we can understand, and those limits generally lie where 'understanding' would be the same as justifying protagonists'

(mis-)deeds. It would be problematic and rather dangerous to think that everything our protagonists do in the field is great, or trying to excuse yourself by saying that you cannot criticize something a person does because you are too close to them, when what they actually did should be criticized. Based on his fieldwork among members of a drug gang, Philippe Bourgois, for example, problematized one of the fundamental methodological premises of ethnographic work, that one may not pass moral judgments on one's protagonists (Bourgois 1996).

Mohr: The negative side of the researcher's fear of the field is just one aspect of fieldwork. This anxiety is accompanied by many other emotions and feelings and goes along with the researcher's hope to get along well with the people they meet in the field, to potentially even develop friendships, or it is connected to the fear of actually meeting people that you don't get along with, but with whom you, nonetheless, have to work somehow for the duration of research. One is really faced with a mix of emotions in the field.

Lindner: Well, I believe that people pick fields to which they somehow have an affinity. That doesn't necessarily happen consciously, but probably rather unconsciously; fields to which you have some sort of biographical or some other kind of affinity. Just as we pick certain research fields, the fields pick us as well in some way. We can talk about this today, but in the 1960s and 70s it was totally taboo to talk about it, particularly in the case of sexual identity. The early studies about lesbians and gay men were almost all done by gays and lesbians themselves. But no one articulated that back then, because it could have meant losing your academic standing or position. Those times are thankfully over. Today, this dynamic seems almost reversed, so that one sometimes gets the impression that only people who are affected by something are subsequently also allowed to do research in that particular field. I find that problematic as well. I would advocate doing research in teams, maybe a team of two or something like that. But, of course, this whole discussion was a completely different one in the 60s and early 70s. Back then, you were more or less condemned to silence and secrecy.

Mohr: That is, in fact, a central question: how is scholarship legitimized? On the one hand, the legitimization of scientificity/scholarliness through social relationships in the field; on the other hand, an analysis of and reflection about those relationships and thinking about how to ascribe meaning to them analytically and in the sense of gaining scholarly insight through them. Only specific empirical material, specific relationships and specific emotions are considered legitimate in that process, if emotions are considered legitimate empirical material at all. How this construction of scientificity/scholarliness functions, that is indeed an important question.

Lindner: Back then you couldn't write about choosing a specific field of research also because of personal reasons. Typically, this concerned doctoral theses or, in other words, work that qualified you for an academic degree. And, in the very worst case, that could have led to you losing your academic title if people had found out that you had gone native in that sense. That would have meant to break with the so-called ideal of objectivity.

Mohr: The American cultural anthropologist Esther Newton, who conducted fieldwork among so-called female impersonators for her doctoral research in the 1960s (Newton 1972) at the University of Chicago with David Schneider as supervisor, and who then wrote an ethnography about a gay and lesbian community on the East Coast in the 1980s and 90s (Newton 1993a), made a similar argument in an article in 1993 (Newton 1993b). She writes about the erotic dimension of fieldwork and about how an erotic relationship with one of her protagonists gave her the energy to complete her research. So, for her, the production of knowledge was closely connected to this personal erotic relationship. At the same time, Newton also talks about the fact that one was not allowed to write about those kinds of things in cultural anthropology, because there was a culture of secrecy and silence around that (Mohr 2016).

Lindner: If we apply a very broad definition of the eroticism, then the erotic of the field is an extremely important aspect, allowing yourself to become completely involved and to take things seriously, you know, the love for the field, if you will, devoting or surrendering yourself, as Kurt Wolff described it. And I mean, if you are using it like that, then I could say that I, of course, did research on soccer because I loved soccer, and my research in the Ruhr valley had to do with the fact that I was from there and that it was important to me to represent this landscape, this industrial landscape, in a way that was not about soot and filth. And it was similar with the punks. To me, punk was a new movement that had a similar rebellious character as the scenes that I was familiar with as a young person. And it was that characteristic that made me curious.

Mohr: Right at the beginning, your article contains something like a sexual metaphor: "In this way, establishing rapport has the characteristics of flirting ('to be amiable'), to reach penetration. The field researcher intrudes into the living space/environment of a group of people." This means that if we were to take the erotic of the field in this broad sense seriously, that the researcher's fear of the field is connected to this erotic play with the field.

Lindner: I did not see it quite like that at the time. Today, that kind of reasoning would make more sense to me. But at the time, my intention was more to criticize the sort of language which methodological literature made us use. "I have to penetrate the field," I understood that more as, say, a rape metaphor than as erotic play.

Mohr: From a queer-feminist perspective, this would then raise the question whether the erotic dimension of fieldwork and this whole fetish about the field are not fundamentally connected to a heteronormative logic?! But maybe we can, in closing, also talk about the use of *Die Angst des Forschers vor dem Feld* in teaching. You taught introductory courses on research methods for many years. How do you convey and communicate the researcher's fear of the field to students as part of that kind of course?

Lindner: By incorporating personal experiences into the course itself. I think that is also the strength of other researchers, known researchers such as Howard S. Becker. He is wonderful at incorporating his own personal experiences. He would not be afraid to talk about how he felt here or there and what he did here or there. So, I think it is only possible to teach something like this, at least for me, it is only possible through personal engagement and by getting personally involved, which should encourage people to allow the personal into their work from the start.

An interview experience of mine might make this point clearer. I once had the possibility to interview a soccer player for the TV channel *Arte*. They had a series called *Rencontre*, where an academic met a person whom they had always dreamed of interviewing. And I had chosen a soccer player as my conversational partner: Willi 'Ente' Lippens (Dutch-German soccer player known as 'Ente' or duck due to his peculiar way of running which reminded people of a duck), who you have probably never heard of (*Arte* 1993). In any case, he was very skeptical at first when we arrived with the TV crew, you know, something along the lines of: "Oh, one of those sociologists who don't like soccer anyway," or something like that. And in such a moment, you must engage and involve yourself to win the other person's sympathy, to win them over, so to say. It all depends on the very first question. So, you hear the word 'action' for the first take, the interview begins, and then your task is to let the other person know that you are an interested person and have no plans of making a fool of them. And my first question in that situation was: "Do you remember your first soccer ball?" And with that, I had won him over immediately and he began to talk about his life. That's how we found a way into each other, so to speak, and could also talk about more delicate topics. I used those kinds of examples as part of my teaching. I think it is very important to find something of relevance to the interview topic that wins people over, that engages them. That usually happens when you show the other person that you actually know something about the topic that you want to learn more about from them. It is completely ridiculous to play naïve in the field, to more or less say: "Oh, I have never heard of that," or something along those lines. Instead you must show that you already know something about the topic you would like to talk about with them, but that you regard your own knowledge as insufficient and, therefore, want to talk with someone who is able to qualify that knowledge, to fill in the gaps, so to say. To act naïve in those situations, that won't work. I believe

that is complete nonsense. Taking your counterpart seriously also means informing yourself about the topic ahead of time and proving yourself to be a competent, but curious conversational partner. And taking someone seriously in this sense then also means to allow yourself to become involved with people on a personal level.

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