

Questions and Explanations in Sociology: A Science Studies Field Study

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“what would be required for this [investigation of practices of social scientists and statisticians] to be worthwhile is the description of those practices within the framework of a set of key methodological concerns – focusing, for instance, on how research questions are selected and formulated; how sources of data and methods are chosen, and on the basis of what considerations; what counts as evidence, how it is produced, what is treated as sufficient evidence (...).”

Martyn Hammersley (2020:6)

Abstract

This is a study of an action research project conducted in one of the biggest university departments for sociology in a Central Eastern European capital during the first half of the 2000s. The paper shows that researchers' images of society have a strong impact on social scientific methodology, scientific explanations and narratives. I offer an example of how realist approaches to science and technology studies can be used in a field study and discuss the benefits and limitations of such an endeavor, which I define as an interpretive and explanatory social scientific work. The analysis shows ways in which latent knowledge structures influenced the wording of a questionnaire used in the research, the types of data that were gathered, and how the data were interpreted. These knowledge structures include notions concerning local policy discussions, different social policy traditions, and images of a Roma minority struggling with the effects of structural poverty and prejudices.

Keywords: Sociology, Methodology, Social Policy, prejudice, Central Eastern Europe.

Introduction

The reflexive turn in social sciences drew attention to the topic of theoretically and empirically based knowledge construction processes. Several authors (Mauthner and Doucet, 2003) highlighted the importance of reflexivity at the data analysis stage, examining the ontological and epistemological assumptions built into particular methods of data analysis. In this paper on social scientific

methodology as applied in a project conducted in Central Eastern Europe (CEE), I approach social scientific research based on two understandings. First, regarding it as an explanation tool, I study how specific research practices, in a given region and at a given time, shape scientific findings and the ways in which they are presented. Second, understanding scientific practices as things that



need to be explained, I examine how local and regional knowledge about society shapes the ways research objects are enacted and research is conducted.

The analysis is about an action research project which was conducted in cooperation of a local and a US team of social scientists in one of the biggest university departments for sociology in a Central Eastern European capital in the first half of the 2000s. My aim is to show the ways in which latent knowledge structures influenced the wording of a questionnaire used in the course of the research, the types of data that were gathered, and, finally, the interpretation of data. These knowledge structures include notions concerning local policy discussions, different social policy traditions, and images constructed by liberal sociology of a Roma minority struggling with the effects of structural poverty and prejudices. Often, evaluative studies like this one are carried out with the intention to improve one's field of study, i.e. to make sociology or other disciplines "better" (whatever "better" should mean for the authors of the work). Albeit making some critical comments, I myself do not wish to point to "more correct", "better" or "more adequate" social scientific methods, suggesting they should be employed instead of the ones used in the study under examination. Neither do I try to explain "wrong" methods, "wrong" interpretations, or invalid results of scientific practices with reference to social factors (in contrast to the "right" ones that need no explanation!), since I do not identify "wrong" or "right" methods, interpretations, or results at all. Instead, I analyze how the researchers' relation to social policy discourses of the time and the local and regional discourses and images about Roma shaped the ways in which they formulated their questions, created and then interpreted their data, and how these discourses even influenced the very understanding of researchers about the things in the world that exist and can be measured.

Though sociologists have been reflexive about their methods throughout the history of the discipline, science and technology studies research on the actual practices related to social scientific methods has been scarce, especially considering the vast quantity of research on the methods used in "hard" sciences (one of the earliest being

ethnomethodological studies such as Garfinkel, 1967). Basically, there are hardly any science and technology studies about the processes and practices employed by, and conflicts among, social scientists themselves (notwithstanding some very recent exceptions concerning ethnographic research, see the articles of a special issue about situated practices of STS ethnographic collaboration and its data practices in the 34(3) issue of this journal, Lippert and Mewes, 2021, or the special issue by Ploder and Hamann, 2020). Babones (2016) urged social scientists to extend quantitative social scientific practice involving reflexive moments concerning research methods. He rightly points out that researchers tend to convey an image of their research practices suggesting that their methods are positivistic, while in fact they are not. Often it is the practitioners of social sciences themselves who then turn to a more reflexive, empirical study of their own field (Einola et al, 2021; Hammersley, 2020: 4). There is a respectable body of (positivistic) secondary literature on statistical analyses that focuses on 'errors' which influence the research process. This literature presents better ways of doing particular statistical analyses, often pointing out that the 'subjective' judgment of the researcher plays an important role in the research process, without discussing the nature, or the origins and traditions of research practices that end up being simply called 'subjective'.

There is a significant amount of scholarly literature discussing how the same question in a questionnaire is interpreted differently by various respondents (e.g., Hardy and Ford, 2014; Galasiński and Kozłowska, 2010). Such works can hardly be characterized as belonging to the discipline of STS, since they rarely deal with explaining *how* and *why* these different interpretations come into being, or the ways in which they influence the processes and the outcomes of scientific endeavors. Rather, they simply state that social scientific research is a result of different processes of "construction", while failing to analyze the historical and social reasons that enable these 'constructions'. Mair et al (2016) is an exception in pointing out how social scientific research projects, in different locations and at different times, are shaped by the cultural contexts of the meanings from which

they originate. For instance, the completion of the same number of school grades meant a comparatively high level of education in certain periods of history, while in others it represented low educational attainment (Mair et al, 2016). There is also some recent empirical scholarly work in fields other than natural sciences about what the same number means in different contexts, and how the citing of numbers may function as a tool to achieve scientists' goals (Holtrop, 2018). In her comprehensive analysis of how social scientific research is made a subject of rigorous study, Erin Leahey (2008) distinguishes two types of studies on research practices. In some studies, research practices appear as explanatory variables, while in others as an outcome of research interests (Leahey, 2008: 36). Among the former, there are studies, for example, on how the quality of interactions between the interviewer and the interviewee is influenced by specific research practices. Among the latter belong studies on the societal impact on research practices, like which institutions use statistical testing more frequently than others, and why.

In my work, I rely partly on Lynch's (2013) approach that he characterises as an ontographical one. STS done from an ontographic perspective does not make the distinction between epistemological and ontological research questions. I believe that while investigating practices in specific cultural and historical circumstances, references to scientific, political and social contexts can be made without compartmentalizing "into domains corresponding to the traditional concerns of politics, ethics, epistemology, and ontology" (Lynch, 2013: 453-456). Focusing on both the conditions of production and the conditions of possibility, I discuss some key latent knowledge structures that define the researchers' assumptions about what entities exist in the real world and which others do not. They also inform both the process of data collection, i.e., creating questions for a survey, and the process of the interpretation of data, during the analytical phase of the research. These research practices, incorporating all such external and contingent knowledge elements or structures, shape the understanding and terminology of how certain things in society are defined, made possible and

created, and then used in other societal fields and interactions.² At the same time, I understand and analyze social scientific practices as the outcomes of social scientific traditions and the commitments to certain cognitive formulations, also pointing out that these traditions and commitments are sustained and reinforced by the very social scientific practices they engender. I pinpoint the *a priori* variables that shape the methodology of empirical social scientific research and the *a posteriori* variables that influence the interpretation of data (Bollen, 2002).

But is this a sound way of doing explanations in STS? Can I channel my knowledge about the disciplinary background of the scientists, about their experiences or their knowledge of different societies when explaining how they practice science (a realist point of view, making use of already existing notions about things in specific countries, regions, and disciplines)? Or should I rely only on the observation of the practices, discussions and workflows employed by the scientists under study? Can we explain how science is made (in this case: ask how notions regarding political and social contexts of Central Eastern European countries ought to be used as explanatory factors when discussing how sociology is practiced there, Latour, 1988) by referring to already existing "things" in the realm of history, society, politics, etc., or to the "context" of the life-worlds (Mauthner, 2015)? Can one assume the existence of "the real", even if hybrid, contingent, processual, or never completely represented (Lippert and Mewes, 2021: 2)? And is it legitimate to explain social phenomena (in our case, in the field of science) using existing concepts, notions, or terminology constructed by other scientists investigating other social phenomena? In other words: should one aim to explain some phenomena encountered when studying scientific practices with some aspects of society or politics, even though believing that the things we study and the society where they exist are co-produced? Or should STS be executed without determining the causes of the studied phenomena in the "real world"?

My answer to the last question is 'no'. The act of "explaining away" – that Pickering (2017: 135) attributes to Durkheimian social science – is in

my understanding precisely what would make science and technology studies interesting, what would produce explanations why science is being done in particular ways. Oftentimes, it seems to me, STS-studies – rather than having an explanatory aim – remain exciting, yet somewhat descriptive exercises about how human and non-human actors go about their daily businesses in the field of science. In this article, I choose to pursue a twofold approach when explaining phenomena. As also discussed by the scientists involved in that research, the specificities and differences between the local and the US workgroups I observed were striking. Since they had already been verbal about them, I often did not have to engage my knowledge of the different contexts the scientists worked in when describing how science works, how scientists act, what methods they use, or how they write questionnaires and create variables and datasets. With the purpose of shaping the research process in their intended ways, the research participants had commented on the circumstances during the discussions and negotiations accompanying the research process. However, there were knowledge structures, disciplinary or historical differences that did not come up explicitly in the discussions I witnessed: therefore it was I who included these in the analysis in a “realist” fashion, i.e. drawing on my knowledge of the researchers’ life-worlds.

And here a short notice on the topic of reliability: Talking about how scientists organize their “dances” (Pickering 2017: 136), Andrew Pickering calls a form of solution to the problem of how to produce more-or-less robust knowledge in STS “islands of stability” (Pickering 2017: 137). We as (STS) scientists might use such islands when working on our own studies; there, we have some sort of reliable regularity, while knowing very well that stability is not a once-and-for-all achievement guaranteed by knowledge (Pickering, 2017: 139-140). Or as Bloor (1999: 90) says, we can assume that observation will always enable us to uncover a reality more complicated than what we can assimilate into our current conceptual schemes and theoretical systems. Certainly my approach – just like all the others – is ready to be scrutinized by those interested in doing so, in the light of new findings and understandings.

Topic and methodology of the project under study

To understand my methodology, we first need to look briefly into the methods that were used in the project I have made the subject of my analysis. To ensure the anonymity of my research objects, I choose not to specify the exact type of the action research method, the precise location, or the exact year, since there have been only few such projects conducted in the region. It was an academic setting, at one of the biggest departments for sociology in a CEE capital in the first half of the 2000s. The local researchers (I use ‘local’ in this text to point to the researchers based in the country where the project was conducted), all of them sociologists, have long been engaged in research on attitudes of the majority society towards the Roma. The local research heads were among the most influential sociologists and survey methods experts in the country, holding important academic positions. They often consulted a colleague for social policy issues, and involved MA students to handle and analyse the data. They had been investigating prejudice against Roma for several decades, using survey as the primary methodology.

This particular research project was conducted in close cooperation with two political scientists coming from the US, who had invented the method serving as the basis of the research: an action research method, usually focusing on locally important policy topics. A project based on this method begins with a representative survey (the sample reflecting the ethnic composition of the country’s population, its age groups, socio-economic backgrounds, etc.) about the views of the population regarding the topic under investigation and about the so-called level of information on the topic. This is followed by a two-day meeting with some of the people who had been in the sample of the survey, also involving experts in different aspects of the research topic. During this meeting, the participants discuss the key questions of the research in small groups and plenary, where they are “objectively” and “scientifically” informed about the most important aspects of the topic. To achieve this, experts are invited to take part in panel discussions, and information material is handed out to the participants. Subse-

quently, the same survey is conducted again among the participants of the two-day meeting. According to the inventors of the method, the differing outcomes of the two tests indicate whether the information provided and the discussions held during the meeting successfully contributed to “more informed choices” by the respondents regarding important policy topics.

In the research project I chose to analyze in this paper, the local researchers focused both on policy issues regarding the Roma minority and on the question of whether prejudices of the majority society can be reduced by a kind of information training. Between the two surveys, during the two-day-meeting, the social factors behind the lower-than-average educational and socioeconomic status of the Roma ethnic minority were explained to the selected members of the majority society in lectures, information sheets about the Roma and in-person meetings with Roma individuals. In this paper, I give selective attention to the various elements of the research process. My main concern being how the local and the US researchers’ very different aims and backgrounds influenced the composition of the initial survey and their interpretation of the data, I pay less attention to how the two-day meeting was designed and conducted.

The topics of discrimination (Kroon et al., 2016), prejudice against (Mudde, 2005; Fekete, 2014) and the exclusion of (Kovács, 2015; Kóczé, 2020) Roma, the biggest ethnic minority in Europe, have been the subjects of intensive research throughout Europe, especially in CEE, where the ratio of this minority in the population is relatively high compared to other parts of Europe and the rest of the world. Prejudice has been one of the main subjects of attitude studies in this region, and there has also been substantial research on poverty and its causes and effects in terms of hostility against Roma (Loveland and Popescu, 2015). 2005-2015 was officially declared the decade of Roma inclusion in twelve European countries, which pledged to improve the socio-economic status of Roma and to take measures to further their social inclusion. Research over the decades has shown how and in which ways Roma face prejudice and suffer discrimination in schools and on the job market.

For many scientists, a methodological challenge when doing social scientific research on a minority is how to “explain” the unfavorable situation of this minority. It can be particularly challenging to discern the (combination of) causes behind the observed disadvantages, distinguishing between some of the main possible factors:

1. belonging to a minority and having to face discrimination and prejudices held by the majority society on that account. In CEE, such ‘visible minority groups’ primarily include the Roma, alongside some sexual minorities and certain deprived groups, such as the homeless.
2. being someone (regardless of ethnicity) who is socially underprivileged, poorly educated, and poverty-stricken.
3. having to suffer from poor infrastructure and/or policy decisions (concerning healthcare, schooling, public transportation, the job market, etc.).
4. having personality traits that hinder someone from being successful and/or efficiently raising her social status. Such traits include lack of motivation (for a myriad of reasons), not being able to handle money well, etc. (Simmel, 1908: 455-456).
5. fate, bad luck.

The attitudes of members of the majority society towards a minority, individual personality traits, the living conditions of poverty and discrimination, and infrastructural/policy circumstances all represent intersecting factors. They together influence the opportunities of people belonging to a certain minority and, thus, their chances of success. Furthermore, not only are the factors listed above used to explain the unfavorable situation of a minority, but they also serve to analyze people’s opinions about these factors (Lepianka et al, 2009). They feature more or less prominently in lay explanations regarding the causes of poverty. Explanations vary primarily according to the ways in which they combine references to the above mentioned factors. Works citing the first three factors (membership in a minority, underprivileged status, poor conditions) are usually classified under the so-called “continental” tradition, in which societal or structural explanations of poverty – external to the individual – prevail.

By contrast, the fourth factor stresses the alleged individual causes of poverty, where the responsibility for poverty lies within a person, while the fifth factor (fate) is prominent in explanations in which neither society nor individuals are blamed for poverty (Feagin, 1972; Lepianka et al, 2009: 421-423)

Methodology

The methodology applied here is mixed. Using anthropological non-participatory observation, I followed the action research project as a young researcher, with the purpose of conducting a field study on the making of sociological knowledge. I had no previous experience in minority/prejudice studies and did not know more about these things than any regular sociologists, making me relatively new to this topic. At the same time, I was specialized in the epistemology of survey sociology, so the methodology of the data collection was not novel to me. I had personal ties to the department and especially knew one professor rather well – she was a colleague of the heads of the local research group in a medium-sized department, where most researchers knew each other well and were on good terms; so the research heads agreed to my field study in their research project.

I followed through all the stages and steps in the project, which meant extensive and intensive observations, and taking part in dozens of meetings with local colleagues and others from the US throughout the duration of the project. The US colleagues – two senior researchers who introduced this action research method to the country – were in close email and telephone communication with the local research group, mainly for the purpose of writing the questionnaire together. They also came in person to the 2-day event. Besides the opportunity to witness personal conversations, I also had access to the texts of emails and conference calls between the local and the US research teams. I conducted my analysis of the different stages and versions of the text of the survey, including any comments, corrections, based on these sources. In this paper, I discuss only two steps in the whole research project: aspects of

how the survey came into being, and how some of the data were then narrated in scientific texts.

In discussing a multinational project, where the know-how and the copyrighted design comes from US colleagues who play a vital part during the whole project, also visiting to attend project meetings in person, one could easily fall into the trap of solely interpreting the situation according to a hierarchical center-periphery model: Western scientists arriving to the East, in order to colonize local research production. In this paper, I will show that this is not necessarily the case. Studies on postmodern society often urge to focus on analyses of local forms of knowledge production. This empirical study shows how different forms of knowledge interacted and came into conflict in the process of composing the survey, in discussions and disputes concerning the survey questions, pointing out how elements of certain types of social scientific knowledge emerged out of these interactions and conflicts.

Discussion

One project, different objectives?

The purpose of the US-American colleagues who participated in the project was to establish whether people's opinions change after gaining more information about certain topics (through experts' participation in the workshops and their contributions to the educational materials distributed). Their fundamental hypothesis was that the more information a person has, the more she is able to determine which policy measure suits her preferences and will contribute to her interests: people like to make rational decisions, and there is impartial information out there, which helps them do that. Hence, for the US researchers, it was essential to integrate questions in the survey concerning the respondents' knowledge of the given issues. Such questions mobilizing the respondents' knowledge on certain subjects are needed in this particular research setup for two reasons. First, those who are better informed when the two-day meeting comes to a close may change their opinions, and these questions are intended to measure this change. Second, these questions allow the people who designed the project to present it as a potentially significant contribution

to political decision making. The project “adds to the legitimacy of the process by allowing the researchers to claim that the post-deliberation opinions are also more informed opinions” (to cite an email of a US researcher sent to the researchers of the country where the project was conducted).

The local researchers had other ideas and, arguably, goals. In a conversation of ours, the head of the local research team contended that “prejudice is irrational, and when we know more, it will diminish.” For them, the priority was not so much to study policy preferences and their relationship with the level of individuals’ information, but *how to reduce prejudice*. The difference between the two groups might be defined with reference to Fleck’s (1979:39) terminology ‘thought collectives’, described as distinct communities of persons mutually exchanging ideas or maintaining intellectual interaction. The point of distinguishing thought collectives here is to show how they have emerged and which scientific practices they yield to.

Does the object exist?

In a project that focuses on social policy, differing interpretations of the purpose of the research are fundamentally entangled with notions of politics in CEE after 1990. For this reason, the very issue of which questions are suitable to measure knowledge about certain facts was contested by the participants. There were some questions that were easily passed by the US colleagues, but which were regarded as problematic by the local researchers. The local researchers tried to convince the US colleagues that there was not necessarily one correct answer to certain questions measuring the level of information/knowledge. In a conference call, the US research head asked the local one what the correct answers to the following questions were: “Who contributed the most in the last 15 years to lessen the number of poor people, the left-wing parties or the right-wing parties? And who contributed the most to ameliorate the situation of the Roma? And to lessen discrimination against the Roma? And to diminish conflicts between different ethnic groups?” One of the local research heads replied that he could not confidently give a correct answer to these questions, since “political parties are mov-

ing, are learning, and have no fixed positions”. A local senior researcher in the project touched on some of the important ways in which the local party system and policies were different from their US counterparts, and explained that questions that seem unambiguous in the US cannot be posed in their country. Questions regarding policies that have been part of the public discourse in the USA or in Western Europe are not always easy for respondents of a survey in CEE to interpret, she claimed. This is why, according to her, researchers have to be cautious, since the same questions can be interpreted quite differently in the different local contexts. The American colleagues seemed to have assumed that there was a language or an existing discourse for people to talk about policy measures designed for poor people. However, the local researcher stressed that this was not the case since “this is a new democracy where discourse about policy options is new”.

Various political alternatives (for example: integrative/universalist approaches or affirmative action policies targeting specifically a minority) have been discussed in the US for several decades and have gradually become part of public narratives. However, such concepts and policies remained virtually absent in Central and Eastern Europe under state socialism, since social policy concepts were not an important or prominent part of public discourses and debates in the region. Under state socialism, general discourses on social policies were very limited, and there were very few legitimate alternatives of them on the political market: official narratives on poverty and ethnic conflicts could not really be openly discussed, even in the scientific community, until the fall of the Iron Curtain. The equivalent terms in the various languages of the region for what in English would be ‘social policy’ and/or ‘social care’ were often marginalized or excluded (see, for example, on Hungary and Poland: Aczél et al, 2015: 41-42). In accordance with the official ideology, poverty and social problems did not exist since the turn to communism following the Second World War, as the system purportedly provided work and thus a decent living for everyone. (In fact, they did exist, however, they were hardly mentioned: Ferge and Juhász, 2004: 234). With a perfect economic policy – according to the state socialist doctrine – social

policy or social care would become unnecessary. After the fall of the Iron Curtain, it took a long time until many Western social policy terms became part of the mainstream social policy narratives (Aczél et al, 2015: 51). Ferge (2001: 110) states that, for example in Hungary, the social policies of the first two governments after the regime change in 1989 did not have clear-cut political or ideological profiles.

Another difference between the discourses of the US and the local researchers lies in the fact that the emerging welfare policies of the CEE countries after 1990 cannot easily be compared to the existing Western schemes. They are hybrid formulations, as besides the new elements they incorporate various features of the previous welfare policies in Europe (Kuitto, 2016: 3). Manning (2004: 216) distinguishes three phases of changes in social policy after the fall of the Iron Curtain. Only in the second one, in the mid-1990s, did new policy debates begin to emerge. Before that, governments were characterized more by their distance from the successor communist parties than by the ideologies and policy alternatives they represented. As a consequence, real public narratives on social policy were barely a decade old at the time of the study.

To sum up: the local researchers contended that stances regarding social policy measures were not really formed at the time of the project. 'Social policy' as such existed during state socialism, however, the *idea* of social policy measures was not necessarily something meaningful for the general public, as envisioned by the US researchers. The local researchers linked the existence of positions regarding social policy formed in the broader public to the existence of public discourse about such topics, which did not really exist under state socialism. For the US colleagues, on the other hand, it was evident that social political stances existed as subjects of scientific inquiry, ready to be measured by a questionnaire. After much debate and negotiations, a compromise between the two groups was achieved. Thus questions about parties and their relation to social policy measures were included, however, only in a simplified form.

What is measured?

In what sense is the wording of a questionnaire shaped by the attitudes and knowledge elements of the authors? How does the text of a questionnaire gain meanings, making different scientific discourses possible? What are the questions used in a questionnaire able to measure? In this section, I analyze the content, composition and semantic features of the survey on social policy issues developed by the international research team, citing the research report written by the local researchers.

In the questionnaire, there are questions regarding the roles of different factors involved in the poverty of Roma and non-Roma populations. One of the questions was this: "In your opinion, what is needed (...) to ameliorate the situation of the Roma? (...) Is it necessary for the Roma to have fewer children?" This question was asked from both Roma and non-Roma respondents. When speaking about Roma respondents who agreed in their replies to the latter question, the local researchers wrote in the research report that "They (...) put the *reasonable* limitation of the number of children in a top place [emphasis by me]". The authors added the word "reasonable", not used in the questionnaire. With putting the question like this, they suggested that it was reasonable to believe that limiting the number of children helps to avoid poverty. By contrast, when commenting on the replies given by non-Roma respondents to the same question, the authors described those who replied "yes" to the question as to whether it would be necessary to limit the number of children if one sought to ameliorate the financial circumstances of Roma as a hostile attitude, a prejudice:

If we consider that behind the (...) the high number of children there is an attitude that emphasizes the responsibility of the Roma, we have to say that the majority society deprives the Roma of the solidarity that is due to the "innocent" poor who are vulnerable to external circumstances. (quote from the research report)

We might ask: why? Why is it necessarily a sign of prejudice when a member of the majority, non-Roma population thinks that having fewer

children would improve someone's financial circumstances? And why is a Roma respondent who suggests the same thing considered *reasonable*?

The question whether having children contributes to one's financial impoverishment might be regarded as directed at the respondent's opinion of how much children cost, how effective family subsidies in a certain country are, etc. Yet, in this research, when a non-Roma respondent answered "yes" to this question, this reply was seen by the local researchers as an indication of prejudice against Roma, not of the costs of raising children in that particular country. Here, the local researchers were drawing on their prior knowledge concerning a widespread prejudice, according to which Roma have "too many" children, i.e. they are unable to support (Orosz et al, 2018: 320). This knowledge is the decisive factor which explains why the same answer to the same question is interpreted in radically different ways, depending on the ethnicity of the respondent. In the researchers' assumption, the common prejudice concerning the large number of children born to Roma women is the context in which this question is interpreted by the ethnic majority respondents.

Another question formulated by the local researchers was the following: "If the budget of the country allowed it, whose situation would you ameliorate first? Please order the groups according to whose situation you would ameliorate first!" The groups were the following: "retired people, big families, unemployed people, Roma, refugees, people with disabilities, people belonging to the country's ethnic majority who moved to the country from abroad" (*anonymized by me*). When the local researchers discussed this question at a meeting, they defined it as a question measuring prejudice. Their discussion evolved around whether it was possible to interpret responses prioritizing the unemployed or families with many children as an indication that the respondent in question wanted to provide support for Roma. The researchers concluded that they could not assess whether these responses could actually be interpreted as indications that the respondents meant to support the Roma, since they did not have any information concerning how the respondents perceived people belonging to the Roma

minority in the first place. The local researchers discussed the possible conclusions they could draw concerning the respondents' prejudices of Roma on the basis of the respondents' answers to this group of questions. During the writing of this part of the questionnaire, one of the researchers wondered:

What the hell do I ask with this question? If she/he doesn't say that she/he would support the unemployed or those with many children, I do not know what she/he thinks about the Roma.

The US colleagues made the critical observation that the categories overlap: In other words, Roma can be unemployed or retired, may have disabilities, etc. So this group of questions – according to the US researchers – cannot measure prejudice towards Roma. At the end, the critical observations of the US colleagues were disregarded.

For the US researchers, the above cited question measured the subjective variable of *preferences*. This kind of inquiry is made in policy research projects, in which then items are compared according to the degree of preference expressed by the respondents (Saris and Gallhofer, 2004: 245). The local scientists tried to determine the extent of an *attitude/prejudice*, and they did so using the same question that was meant by the US researchers to measure the *policy preferences* of the respondents. We have seen that the local researchers aimed to assess prejudices against Roma, while the US researchers' purpose was to measure the respondents' policy preferences and how these policy preferences changed with the respondents having more "objective" information.

Nature or nurture? Latent knowledge structures and social political concepts

The American colleagues initially wanted to delete the above mentioned questions regarding possible reasons behind poverty altogether.³ At the same time, the local project leader insisted that these questions were backed up by standard theories, and have also been used in other surveys in the US. He was referring to questions to determine whether respondents attribute poverty to social / external, or personal / internal factors (see above on the five different ideal-types explaining

the situation of Roma). The US researchers argued that the answer categories (Roma, unemployed, living with disabilities, etc) were overlapping, i.e. not mutually exclusive (which is obviously the case, see Lepianka, 2009), a discouraged practice in social scientific questionnaire-writing. The local scientists were not aware of any such problems: for them, it was obvious that the answers were structured around the categories of either internal or external attribution of causes, and the fact that the categories are overlapping did not matter to them. The two categories of attribution were so important for the local scientists that the topic was included in the one-page briefing material about the project written by one of the local research heads, which was published on the project website:

There has been a shift in the causal attribution of poverty; views that blame the poor for their fate are still popular, but views that stress external, social circumstances and injustices have become somewhat more dominant, which points us to the fact that social solidarity has increased.

Another block in the questionnaire about the factors influencing the social status of ethnic groups allows some insight into the local scientists' attitudes towards the same topic: the reasons for the poor status of the Roma minority. In the section on non-Roma people, questions concerning the possible causes of *poverty* are asked, yet when it comes to Roma people, the respondents are asked to name the possible causes of their *disadvantaged situation*.⁴ This terminological difference (the use of the term 'poverty', on the one hand, and 'disadvantaged situation', on the other) suggests that, in the view of the local scientists, the social status of the ethnic minority (the Roma) is caused not only by lack of financial resources, but by social exclusion and prejudice against them: so not just financial problems, but social/structural ones as well (see Chapter 2). This notion determines how the questionnaire is phrased: while the ethnic majority of the country is "poor", the Roma are "disadvantaged". Another question that sheds light on the knowledge structure of the local sociologists was eventually excluded from the final questionnaire. It was taken from another questionnaire used in a similar project in

Australia, which also dealt in part with conflicts between ethnic groups. The question asked the respondent to compare the situation of Roma and non-Roma in different areas of life.⁵ In contrast with the Australian questionnaire, which offered evenly distributed potential answers to this question, the local, rather lopsided answer structure of the questionnaire under study looked like this: "much worse", "worse", "the same" and "better". In other words, there was only one positive response and one neutral one, while there were two negative options: an uneven distribution toward the negative side of the possible answers. Another question concerned how the respondents would improve the housing situation of Roma. The local researchers included several possible answers, like one that favored building new block houses for Roma on previously uninhabited city/town outskirts, or another one that suggested moving Roma to regions of the country, which were becoming depopulated. The US team members then simplified the answer categories in the following way: building new flats or moving Roma into existing flats. This shows that the US researchers were completely unfamiliar with two knowledge elements that the local colleagues considered crucial. The first of these was that a social policy measure encouraging Roma to move to city or town outskirts would result in a form of geographical segregation, which in many ways would reproduce the already poor housing conditions of the Roma. The second is the fact that the US colleagues failed to realize that the originally proposed answer categories included a latent reference to the well-researched prejudice about Roma being noisy people. According to the local researchers, the belief (or prejudice) that Roma are undesirable neighbors forms an important part of the discursive context of such questions.

The answer categories to questions in the survey were often formulated with the background notion of so-called latent variables. Latent variables are defined in various ways or by a combination of different approaches: they are regarded as hypothetical constructs put together by scientists as attempts to measure existing phenomena, as much as things that are impossible to measure or to observe (Bollen, 2002). Latent variables often help researchers arrive at explanations concerning

the relationships between two or more variables. Many variables in the psychological and social sciences cannot be observed directly. For this reason, they are considered latent, and the only option is to observe them indirectly, through the values of an observed variable. The exploration and study of latent variables are a central part of social scientific investigations, and discussions of the importance of latent variables are often regarded as the essence of such endeavors. The ultimate goal is to find causal structures that could explain how society works. Social sciences, as opposed to psychology or biology, use experiments as a methodological tool rather marginally. Experiments would try to ensure that only one (or very few) independent variables are manipulated between measurements. As experiments are not feasible most of the time, social scientists have to rely on other means (including references to latent variables) to narrate causal structures in their scientific texts.

Latent variable models have been used widely in quantitative social scientific research (Loehlin and Beaujean, 2017: ix), but they have rarely been analyzed scientifically (Bollen, 2002: 606). Latent variables in sociological and social psychological research can entail, for example, motivations, notions, prejudices, attitudes, etc. which influence interviewees in their answering patterns. They can serve as a means to summarize a number of variables, resulting in fewer factors (Bollen, 2002: 608). *A priori* latent variables are hypothesized prior to the examination of the data, while *a posteriori* latent variables are derived by researchers on the basis of the data analysis (Bollen, 2002: 615).⁶ In our case, an *a priori* latent variable influenced the ways in which the researchers envisioned the answer structure that was offered to the respondents: structural vs. individual explanations of poverty (see the first part of this article).⁷ In the above mentioned cases, where there was some dispute among the local and the US research teams regarding overlapping answer categories, it became obvious that knowledge on the latent variable of the possible prejudices about poverty and the causes of disadvantaged situations was guiding the local sociologists when writing the questionnaire and then interpreting the answers given by the respondents.⁸ One of the researchers

put it like this during the writing of the questionnaire, after going through all the possible answers in case of the abovementioned questions about how to ameliorate the situation of the Roma, and after categorizing them into structural/individual explanations: "We are in the hands of the respondents whether this typology will come out or won't."

The structural explanation of poverty echoes a very important notion of how the Roma minority is perceived by many social scientists in the CEE region: in modeling post-communist deprivation, many sociologists studying Roma minorities use the image of a social group segregated from the rest of society and stricken by discrimination. Their vision combines different aspects of minority existence. People belong to such a group when they are both socially excluded (also suffering discrimination: Barany, 2002, sometimes to an extreme degree, like in a caste system: Ladányi and Szelényi, 2006: 15) and economically excluded from other classes on account of their race/ethnicity. In this sociological understanding, the Roma minority has, due to structural causes, not been able to participate in economic growth, as it was unable to acquire the necessary education and skills (Stewart, 2002). This theory of Roma is only one of the many possible social scientific notions attached to this ethnic group (itself composed of socially and culturally diverse groups). There are approximately twelve million Roma living in Europe under widely varying circumstances. In CEE, the Roma minority typically consists of people who are settled, in contrast to more nomadic Roma groups in Northern and Western Europe (Ladányi and Szelényi, 2006: 22). Sociologists do not typically analyze the ethnic traits of groups, since this kind of research is usually conducted by ethnographers or anthropologists. Indeed, understandings and descriptions of Roma in mainstream CEE sociological literature tend to characterize them as a group facing racism, segregation, prejudice, and discrimination (Oblath, 2006; Vajda 2020). We have seen that it is precisely this notion of Roma that influenced the way in which the questionnaire was constructed and the data collected by the questionnaire interpreted by the local scientists.

Conclusion

A study of sociological practices that wants to be explanatory cannot escape certain realist commitments (Hammersley 2022:145). The difficulty, as Bloor (1999:92) puts it, is to decide which things should be topicalised for *investigation* and which others should be reserved as *resources*. It is a privilege to have social sciences as a field of study, because references to political or social factors often become explicit during the research process that the STS-scholar studies and thus serve as resources.

At the same time, besides such obvious references, there are contextual knowledges that have to be drawn into the study by the STS researcher in order to interpret what she sees. After all, knowledge about objects that we assume to have particular characteristics independently of our awareness of them (even though we know very well that this type of knowledge is an ever-contested one) needs to be included as long as we want to make STS an explanatory sort of scientific discipline.

I believe, and I tried to prove, that references to scientific, political and social contexts can be made in an STS study, without compartmentalizing into the traditional fields of “politics, ethics, epistemology, and ontology” (Lynch, 2013: 456). Dealing with both the conditions of production and the conditions of possibility (Lynch, 2013: 453-454), it seemed necessary to outline the large picture, containing some of the elements that seemed to me the most important ones. Some of the knowledge elements that I have discussed define the researchers’ assumptions about what entities exist in the “real world” and which others do not. They also inform both the process of data collection, i.e. creating questions for a survey, and the process of the interpretation of data, during the analytical phase of the research.

We have seen how notions regarding the prejudices prevalent in a CEE society as well as various ideas and discourses around social policy issues in the early 2000s impacted social scientific methods and data analyses in a particular social scientific research project, shaped by particular disciplinary, social and historical circumstances. Questions in the social sciences cannot always be easily arranged in measurable categories based

on the type of objects they intend to measure, since for different scientists the same question may measure different things. We have seen that researchers’ knowledge about latent dimensions of the possible prejudices concerning the disadvantaged situation of Roma and the causes of their poverty influenced the local social scientists when drafting the questionnaire and interpreting the respondents’ answers to the questions.⁹ The tensions behind these disputes emerged from different traditions, knowledge elements and scientific attitudes – some of these understandings and attitudes explicitly came up in the extensive discussions during the research process, contributing to changes in the applied methods and enabling specific research results.

Processes and practices of assigning meaning are by no means self-explanatory but, rather, contested and conflicting operations that enact different understandings of the research objects under study. More importantly, these understandings themselves are based on different (scientific, political and regional) traditions and discourses, potentially creating inherent tensions or even conundrums in a research project. Albeit certainly representing an important issue in the international science market, analyses of diverging contexts and traditions in the constitution of meaning in the course of research have rarely been conducted so far in the field of quantitative social sciences.

The project under study here, done in the mid-2000s in CEE, offers an example of the ways in which *a priori* and *a posteriori* latent variables can influence the composition and wording of a questionnaire, the types of data that are gathered, and how the data are interpreted. In this case, these variables included knowledge concerning local public discussions, notions of social policy, and images of a minority struggling with the effects of structural poverty and prejudices. As I showed, these variables can be sustained and reinforced by some of the scientific practices I analyzed, contributing to their perception as being parts of the objective reality.

Different understandings of the purpose of the project led the participants to different interpretations of what certain questions measured, and these differences in interpretation created conflicts during the process of drafting the ques-

tionnaire. For the local researchers, the assumed prejudices of the ethnic majority population concerning Roma constituted the most important context, framing both the questions and the interpretation of the *answers*. Thus, the influence of knowledge elements regarding prejudice towards Roma explained why the same answer to the same question was interpreted in fundamentally different ways by the local researchers when the respondent identified him/herself as Roma or as a member of the majority society. As for the US scientists, attitudes towards social policy measures were among the main scientific interests of the research project. However, according to the local sociologists, such attitudes often *did not even exist* among the local respondents interviewed in the project. Thus, the same answer to certain questions had different meanings for the two research groups, depending on what the teams thought the question measured.

The Roma minority, the unemployed, people with many children: these are categories of the population which emerge from specific historical circumstances in a given country. In the present case, for the local research team the historical context shaped the discourse on social policy that was essentially censored under state socialism and then allowed to burgeon after the fall of the Iron Curtain, resulting in (at the time of my field research) a decade-long heated debate concerning policy measures affecting visible, often stigmatized, minority communities and other

vulnerable groups. The local researchers' reference to this discourse influenced the ways in which questions were formulated, data created and then interpreted. However, ignorant of what the CEE historical context entailed, the US researchers did not share the same understandings of the named population categories. In general, different historical sensibilities represented by the two teams shaped the understanding of researchers concerning the things in the world that exist and can be measured. Or, using other, more constructivist terms: their divergent discourses created the very things that they thought could be measured.

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Notes

- 1 A practice to be avoided, as we have learnt from the Strong Programme.
- 2 In choosing this approach, I am aware of voices claiming that studies of ontology in STS, based on empirical observations about how realities are made or enacted in practices (Aspers, 2014: 2), do not fundamentally differ from constructivist accounts about how science works. Sisondo (2015: 2) argues that STS has always looked at the plurality of actualities, and we can talk of ontological turns only because explicit references to ontologies have been added to otherwise often more traditional lines of inquiry.
- 3 The questions were (in my translation): “We collected different opinions regarding the reasons behind poverty. Do you agree with these statements? People are poor when they: do not have sufficient education; do not like to work; live in disadvantaged areas; are physically or mentally disabled; do not do anything for themselves and expect everything from society; have many children; live irresponsibly; do not have job opportunities; cannot take care of their money; were born in poor families.
- 4 The questions were: “We collected different opinions regarding the reasons behind the disadvantaged situation of Roma. Do you agree with these statements? Those are in a disadvantaged situation who: do not have sufficient education; do not want to assimilate; live in disadvantaged areas; do not like to work; were born in poor families; live irresponsibly; cannot take care of their money; are the victims of prejudice; do not do anything for themselves and expect everything from society; have bad health; have many children.
- 5 The question was this: “Let us think about the health of the people. Do you think that the Roma have better or worse health than the others? And what about the level of education?” Other questions planned were related to income, unemployment, career chances, housing situation
- 6 I do not assess in my article whether these latent variables are “true” or “correct”.
- 7 Just to be clear: I am not trying to assess here whether such a hypothesizing or such a derivation of latent variables is “correct” or not. The literature about such assessments is vast. I am trying to show how the formulation of latent variables is shaped by knowledge elements that are independent of the research results of this specific project under study, and which, therefore, influence both the wording of the questionnaire and the collection and interpretation of data.
- 8 Neither here – nor elsewhere in this text — do I suggest that the methods of the researchers from the USA were in any way ‘better’ or ‘more objective’ than those of the other team.
- 9 Again: I do not discuss here at all whether researchers’ knowledge about these latent variables is ‘true’ or ‘false’.