

Participant Observation

On the Past and Future of a Method

By Barbro Blehr

This article is an attempt to trace the history of participant observation in Nordic ethnology, as it appears in the journal *Ethnologica Scandinavica* from the 1970s onwards. My impetus is the impression that the method of participant observation has had its heyday, and that the reason for this is the combination of current rules and regulations of research ethics and the practical procedures for ethical vetting. At least, this is how it looks from a vantage point in Sweden.

Participant observation, then, refers to a method for studying culture and society which is performed by joining people in their everyday activities, following their interactions and practices in order to understand how they perceive and act upon the world, or some particular part of it. The method is inductive and research questions can be specified only step by step during the project, as the researcher's understanding of the field evolves. For that reason, research relying on participant observation cannot be pre-planned in detail. The word "participant" refers to the fact that researchers are present in the activities they follow (which, for that reason, must be contemporary with the researcher). Researchers may take a more or less active part in what is going on and be more or less marginal in the situations they attend. After the reflexive turn in the 1980s, they are expected to reflect thoroughly on how they contribute to the processes they study, and to integrate this awareness in their analysis. Prototypically, participant observation was used in the study of pre-industrial small-scale societies in the Third World and performed by anthropologists from the industrialised west. Over time, though, the method has been integrated in the toolkit

of a wide range of qualitative branches of disciplines in the humanities and social sciences and applied in the study of a variety of settings and societies.

As for research ethics, the immediate backdrop of the paper is the recent reinforcement of procedural ethics in the humanities and social sciences in Sweden – as a national instance of an international trend. Procedural ethics, then, refers to a model for action where a formal and/or juridical system is installed for identifying and handling ethical issues. This is often contrasted to "ethics in practice", referring to the continuous awareness of, and preparedness to handle, ethical issues in everyday professional life (Guillemin & Gillam 2004). Ideally, those two models might be seen as complementary. But scholarly debate testifies to a certain friction between them, and to a certain discomfort when qualitative research is confronted with the practical procedures of the procedural ethics.

In the Swedish context, the basic components of the procedural system are the Act on Ethical Review of Research Involving Humans (SFS 2003:460), the European General Data Protection Regulation, applying from May 2018, and the procedures of vetting as they are administered by the Swedish Ethical Review Authority (established in 2019, then replacing six former regional ethical review boards). The Act of 2003 is intended to protect the integrity, safety and health of human beings potentially being involved in research projects. Among other conditions, it states that participants in a research project should be informed about the objective, methods, potential consequences etc. of the project, that their consent must be documented, and

that they can terminate their participation at any stage in the process. GDPR, besides sharpening the general rules for handling information about identifiable living individuals, added the premise that some kinds of information should be handled with extra care: data revealing racial or ethnic origin, political opinions, religious or philosophical beliefs, trade union membership, genetic and biometric data, and data on health, sex life or sexual orientation. According to Art. 9 GDPR, the processing of such information is prohibited, unless consent from the person involved can be documented. Projects handling sensitive personal data must be vetted in advance. That is where the Ethical Review Authority steps in, with the task of securing that the benefit of a planned project outweighs the potential risks for the persons whose data are collected and processed, and that the researchers have taken all necessary measures to protect the integrity of research participants.¹

The Swedish Act of 2003 alone was not enough to make formal vetting imperative in the humanities and social sciences. Looking back, older researchers may recall the times when they thought vetting was required only for projects involving children, and for medical research. And if anecdotal evidence be allowed for a moment, later there were times when we contemplated the idea that if we were to take GDPR seriously, every ethnological project might need vetting, not only because “special” or sensitive data are often relevant to our projects, but also because they tend to crop up in our material even when they are not explicitly in focus. The prospect of having to apply for vetting of all projects was initially dismissed as unrealistic. But in the

beginning of the 2020s, it has almost come true.

The infrastructure of procedural ethics varies between the Nordic countries. In Sweden, researchers in all fields are referred to one single procedure of vetting, basically designed for medical and/or experimental research, and vetting is performed by a state authority.² In Norway, there are different advisory ethical boards for different kinds of research, including one for the humanities and social sciences. It is mandatory to apply, from the administrative body Sikt, under the Ministry of Education and Research, for permission to handle personal data in a project.³ The Danish system has no general requirement of applying for permission to perform research, and application is called for only in the context of medical or biological research; the same seems to be the case in Iceland.⁴ Finland has separate ethical boards, and rules, for the humanities and social sciences, and vetting of projects in these areas is not mandatory.⁵

These differences notwithstanding, researchers in various Nordic countries share the experience that GDPR and/or the routines of vetting of qualitative research are hard to reconcile with the epistemological grounds and the methodological ideals of their own disciplines.⁶ Norwegian anthropologists, well organised and deeply committed to participant observation, have addressed the issue repeatedly in the last few decades, at their own conferences as well as in print (Øye & Bjelland 2012; *Norsk Antropologisk Tidsskrift* 2020; Vike & L’orange Fürst 2020, 2021). They were quick to notice both that the standard vetting procedures cannot accommodate the open-endedness of participant observa-

tion, and that the formal documentation of consent to participate in a project does not fit well in many of the contexts they work with. Similar reactions have been reported from Denmark (accompanied by an accurate contrasting of formal ethical review processes and “in situ” ethics and substantiated by a description of the harm caused by a consent form in a study of a nursing home for elder people; Balkin et al. 2023). From his position as a Swedish sociologist working in the ethnographic tradition, Wästerfors (2019) has described the procedures of vetting as “insensitive” to ethnography, much for the same reasons as those highlighted by the Norwegian and Danish colleagues. Moreover, both the Norwegian anthropologists and Wästerfors remark that the focus of ethnographic research is not on human beings in their capacity of individuals, but on social phenomena; relationships, processes, behaviour, and practices (Vike & L’orange Fürst 2020:168). As Wästerfors puts it, ethnographic research is not about individuals with particular characteristics, but about social life (2019:184f); the aim of the research transcends, as it were, the persons involved. This takes us, no doubt, close to the core of the basic incompatibility of the formal system and the reality in which ethnographers/participant observers wish to operate; I will return to this aspect at the end of the paper.

In the winter of 2022, debate about procedural ethics in the humanities and social sciences spread from scholarly journals to daily newspapers in Sweden; for recent examples, see *Svenska Dagbladet*, November-December 2022 (specified in the list of references). And even more recently, in its assessment of the state of the art in the humanities and social sciences,

the Swedish Research Council voiced concerns about certain aspects of the procedural system, including the current understanding of the concept of “sensitive personal data” (2023:25). To put it briefly, the debate is expanding and advancing. But the basic incompatibility seems to be hard to address.

The sections above should give a glimpse of the situation that has inspired me to think about the history of participant observation in our discipline. Perhaps the metaphor of Minerva’s owl can be invoked as a reason: we do not reflect on what we have until we are about to lose it. The question to be explored in the following, then, is What did we have? How has participant observation been used in our discipline, and to what extent? What has it yielded, how has it been evaluated, and how has it developed over time? If we must give it up, what are we going to lose?

My source for reflection will be fifty volumes of *Ethnologia Scandinavica*; a journal intended to represent a broad range of ethnological scholarship in the Nordic countries. Since these volumes cover the period from 1971 to the early 2020s, they should allow us to revisit something close to the method’s pioneering phase in our discipline and follow it up to the present.⁷ The outline, after a brief introduction to the material, will be chronological; three sections will take us through the fifty years, and the article will close with some reflections on the findings, and on the future.

Sources

As is already well-known to its regular readers, *Ethnologia Scandinavica* is published annually since 1971. The most prominent genres in the journal are articles and book reviews, occasionally supple-

mented with presentations of conferences and workshops. The journal invites authors and reviewers from all Nordic countries, and as underscored in editorials (e.g. Jönsson 2021:3), it should be well suited for assessing the character and the development of Nordic ethnology as a discipline.

In order to trace the trajectory of participant observation in this context I have searched for all kinds of texts in the journal where the method of participant (or, in recent years, “participatory”) observation is mentioned. The first 39 volumes were scanned manually. From 2010 the journal was available in digital format and could be checked with the aid of automated search functions. Texts mentioning the concept were copied and entries noticed or checked were listed in a table. The table was helpful not least for providing a visual impression of when participant observation surfaced in the journal, and how frequently it appeared in a volume.

As I had some prior knowledge of monographs where participant observation was used to a considerable degree, I could notice that some of them were reviewed with no mention of the method. In many reviews, the method was mentioned but not further commented on. This is a reminder that the reviews are a filtered representation. They do not depict the actual frequency and use of the method, but the interest paid to it by the persons asked to write reviews, and their reactions to what they read.

The articles, in contrast, might be expected to give us more of a first-hand proof of how the method has been understood and utilised. In practice though, articles too differ as to their explicitness about the use of the method. Often, it is mentioned without being described or discussed.

The result of the search can be briefly summarised as follows. In the article section, the phrasing “participant observation” is virtually absent until 1999. At this time, the journal was still primarily devoted to the study of pre-industrial forms of life and, particularly, peasant culture. While articles on contemporary topics started appearing in the middle/end of the 1980s, the method of participant observation was not highlighted until the turn of the millennium, and it was not frequently mentioned in articles until later in the 2000s.

In the review section, on the contrary, the method is visible already in the first decade, and it is never completely out of sight during the years to come. As mentioned above, though, many reviews do little but mention that participant observation has been used. Apart from a handful of reviews in the first ten years, there is seldom any discussion of how it has been practised or what it has yielded.

A working hypothesis is that the more novel, and potentially controversial, the method has been to the author or reviewer, the greater the need or wish to comment on it. Inversely, merely mentioning the method might indicate that it passes as accepted, or at least has not provoked any reactions worth verbalising by a reviewer. My closer reading of the material has focused on the selection of texts where participant observation is more than merely mentioned, supplemented with a few texts where other words are used, but the method described is similar enough to participant observation to warrant inclusion.

Reviewers’ Reactions; 1970s–1990s

Somewhat surprisingly, the first traces of participant observation are brief comments

on the absence of the method. In 1972 and 1974, respectively, reviewers Bringéus and Stoklund note that even though the studies under review are influenced by social anthropology, observation/participant observation has not been possible to carry out – either because the topic of study belongs to the past (Bringéus 1972:182), or because the method has not been sufficient for getting a grasp of the area of study, a modern suburb (Stoklund 1974:192f). The tone is neutral, and there are no evaluations. Perhaps this is so since the method has not been applied. For when, next year, a work dominated by participant observation (focused on immigrants to a small Swedish town) is reviewed, the criticism is harsh. The reviewer is Sven B. Ek. He compliments the work for being a convincing “document of cultural journalism” (1975:192) but finds it lamentable that it focuses on the immigrants only, not taking account of how the Swedish population perceived them. He remarks that the author’s understanding of the term participant observation is too wide and that he has in fact not participated, since he has not taken part in the working life of the people he studies (*ibid.*). But the thrust of his criticism is that no evidence for the conclusions is presented (1975:193f). Quotations abound in the text, admittedly, but there are no maps and no pictures documenting objective evidence as to what kind of clothes, furniture, cars etc. various kinds of immigrants preferred in various stages of adaptation to their new place of living. As Ek puts it, “the reader has the right to demand a comprehensible account of the data on which the scholar builds his discussion” (1975:193); this would be easily accomplished “in the form of simple diagrams”

in an appendix to the text (*ibid.*). He adds that besides presenting the substantive evidence to the reader, such diagrams would help the researcher to control the accuracy of his/her own conclusions and prevent him/her from relying too heavily on singular impressions (1975:194). Summing up, his verdict is that the dissertation is highly readable but that it fails to meet the formal requirements of a scholarly work.⁸

Some years later, another monograph, dealing with a workers’ community, is criticised on the basis that the author has used participant observation as the sole method, and not a broader set of different sources that could have been combined to increase the reliability of the study (Pedersen 1981:168f). According to the review there is also a lack of clarity when it comes to analysis; “we see only his findings” (1981:169). Likewise, the reviewer would have liked to see more of a contextualisation; there are no comparisons with previous research, neither has the author tried to locate his findings in a “typological, historical or social context” (*ibid.*).

Thus, in the first phase, from the 1970s to the early 1980s, the method of participant observation emerged as an innovation, and it was not welcomed. It was associated with delimiting the subject under study in a way that was perceived as far too narrow, and with a style of writing and presenting results that clashed with the reviewers’ expectations of what a scholarly text should be like. The latter shortcomings were perceived as serious since they were related to the possibility of checking the accuracy of the author’s conclusion. Therefore, however vivid and readable the texts could be, they did not qualify as science for their reviewers.

The combination of writing well but not presenting one's empirical evidence or making one's analytical operations inspectable was considered an anthropological practice, obviously because that was where the inspiration for participant observation came from. Likewise, but with no explicit connection to anthropology, the early examples of works based on participant observation were associated with an ambition to address social issues and produce research that was relevant to the wider society (Thorsen 1981:172; Hodne 1981:183). To this cluster of associations, we can add Jonas Frykman's reflections – published in a comment to a paper on the role of fieldwork in tradition research (Honko 1977) – that in the 1970s, fieldwork consisting of participant observation and unstructured interviews was also related to taking a keen interest in theory – and to being in opposition to the ethnological establishment (Frykman 1977:95). According to Frykman, fieldwork of this kind even became “a condensed symbol for the new ethnology” (*ibid.*).

Another potential issue, not prominent in the earliest reviews, is the risk of bias in observations. It was raised as a problem on one occasion, by Bjarne Hodne in his review of a dissertation about institutions for care of long-term old patients (1981). Hodne's criticism referred partly to the possibility that the author's “personal views and attitudes” (1981:186) (such as his apparently positive attitude to euthanasia) had affected the situations he observed, and partly to whether the participants in the study ran the risk of being identified by readers. The topic of this study appears to have been emotionally and existentially charged to an unusually high degree; Hodne was visibly affected in his report.

So, in the first decade, the method was discussed in the review section only, and predominantly as an innovation of a negative kind. In these texts, there was no focus on what participant observation might yield, with one possible exception, a review of a study of bingo-players. Albeit briefly, this reviewer underscored the importance of results obviously stemming from the researchers' participating in the game (Fjellheim 1978:182f).

After this initial outburst of negative reception, there is a relative silence on the topic of participant observation in the reviews, as if the method has been incorporated in the standard toolkit without requiring much attention or comment. This is so both when texts obviously based on participant observation are praised (e.g., Åström 1984) and when they are criticised. One example of the latter is Balle-Pedersen's review of a monograph on dockers. He notes that the study is built on “genuine participant observation” and assumes that interviews do not play a big role in the study “... because the author did not want to spoil the participant observation” (1987:171). Later in the review he regrets that the study is concentrated on the workplace only, and that the analysis does not pay any attention to the family life of the workers, or their leisure interests (1987:172f). It does not seem to occur to him that there could be a connection; privileging participant observation might have been instrumental in producing the focus on activities that the author could share with the collective of workers he had set out to study. In a similar vein but reversed as to the arenas covered/not covered, the reviewer commenting on a thesis about family life in a small Swedish town would have liked the study to also

include data on working life (Andreasen 1992:183), here too without relating that effect to the choice of methods.

Having entered the 1990s, the criticism of lack of source references is no longer prominent. It surfaces once more in Lena Marander-Eklund's review of a thesis heavily dependent on participant observation; her remarks concern the fact that no references are given to primary data in the field notes (1995:139f). Slightly connected to the use of participant observation might also be an occasional lament several years later that the need for anonymising the site of fieldwork has made it impossible to include pictures in the monograph (Andreasen 2001). Still, these objections seem mild, compared to how the method was rejected twenty years before.

In the 1990s, participant observation is increasingly related to the practice of drawing on researchers' own experiences. The experiences in question may consist of many years of pre-research involvement, private as well as professional, in activities similar or adjacent to what is being studied (Liliequist 1994:142; Schulman 1997:144). Schulman does not comment on this way of understanding the method. Liliequist comes closer to evaluating, when she writes that the author is aware that her proximity to the field of study can give rise to problems in the analysis, and comments that her "personal experience is near the surface, but it is still handled with analytical detachment" (ibid.).⁹

On quite another scale, the experiences drawn on can also consist of immediate personal reactions in a specific research setting. Bringéus, when reviewing a collection of papers on public events, pinpoints this and (a little surprisingly) seems to ac-

cept it with no further ado. He notices that one of the authors takes on a double role as a researcher and a participant in the event under study, and that she "*dares to apply an emotional perspective*" (1996:161, italics added). He remarks that this is a contrast to the time when feelings were banned from our trade; scholars "were allowed to listen and observe but not to feel. Now, however, it is all right to analyse emotions and experiences" (ibid.).

Summing up impressions from the first three decades, it seems fair to say that the initial harsh criticism of participant observation soon gave way to a more accepting attitude. And in the 1990s, the method is also becoming visible in another part of the journal.

At the Turn of the Millennium; Appearing in the Article Section

Somewhat echoing the initial phase in the reviews, participant observation makes its entrance in the article section by being mentioned but not discussed. In one case the method is simply presented as part of the author's toolkit (Blehr 1999:31); in another the author introduces himself as a participant observer (Christensen 1999:106), thereby making the method stand out as more prominent. Neither of the two are explicit about how they have practised the method or to what ends. These are the first articles in the journal where participant method is explicitly presented as a method used. It might be that it is in fact referred to already in the previous decade, when Gösta Arvastson discusses a project documenting working life in modern large-scale industries (1983). His description of the methods used runs as follows:

The study was structured around qualitative collection methods: informal conversations, observations and journal entries from work-places, together with regular taped interviews. To shed some light on the amounts of material gathered from the various work-places, exactly 1,000 pages of interviews and 826 pages of informal conversations and notes can be mentioned (1983:53).

Later, he stresses how the fact that researchers have been around at the places of work, “participating in the shared life there” (1983:55), was essential for their understanding. He underscores that observing, then, meant more than watching, and pictures their work as “a way of entering others’ knowledge, and of wearing other people’s hats in order to discover new cultural landscapes” (1983:55). However, he never uses the term “participant observation”. And when a monograph obviously emanating from the same project is reviewed some years later, interviews are presented as the main source (Båsk 1991:190).

Shortly after the turn of the millennium, Maja Povrzanović Frykman extensively exposes and explicitly argues for a method called micro-ethnography (2001). The topic of her paper is transnationalism “from below” as experienced by “ordinary immigrants” (2001:47f), keeping up connections between a country of origin and a country of living. A significant portion of her text (2001:52–60, framing discussion included) consists of a description of travelling between Malmö and Zagreb by bus with her children. She is taking care to behave as a common traveller, she does not disclose her identity as a researcher and does not ask for personal information from her fellow travellers. And she pays particular attention to what it feels like, in physical terms, to be travelling in an inex-

pensive way; in a cramped space close to others, exposed to sensory impressions that are partly unpleasant. The importance of the bodily experiences, she notes, became salient for her “only after being involved” (2001:52). Her motive for documenting the trips (the description was based on two occasions of travelling) relates to the premise that “only fieldwork – necessarily multi-sited and preferably long-term – enables insight into non-homogenous practice within transnational groups” (2001:59); she also stresses that the ethnographic material is a valuable complement to interviews (*ibid.*). Furthermore, she remarks that the “field trip” described in the micro-ethnography should also be understood as a point of entry from which other strands of the topic studied can be explored, such as the personal histories of the travellers, the objects brought along etc. (*ibid.*).

Povrzanović Frykman’s framing of her micro-ethnography is the most explicit presentation in my selection of texts on how participant observation can be carried out and what it can yield – but for the fact that the author does not label it participant observation. Her reasoning, though – that this way of generating evidence gives insight into practice, in contrast to interviews – is echoed in two later papers where authors briefly state the reasons for their use of participant observation; Schousboe’s study of the making of Hessischer Handkäse (2014:39) and Kuoljok’s article on tending reindeer with the technological aid of GPS (2019:24).¹⁰

Yet another reason to highlight Povrzanović Frykman’s paper here, despite the fact that she does not identify it as participant observation, is that it is a harbinger of a steady flow of contribu-

tions to *Ethnologia Scandinavica* in the years to come: articles focusing on bodily practices and sensations, and interactions not only with human beings, but with material surroundings and objects as well. Overviewing the whole selection of texts, articles of this kind seem to represent the research where participant observation has been put most to use after the turn of the millennium.

A Standard Method at Work: 2000–2020s

From the beginning of the 2000s, participant observation appears to be truly established as one method among others in ethnology. Often it is merely mentioned in the texts. Sometimes it is not mentioned, even though it is clear from the text that participant (or participatory) observation has been an important method in the work presented or reviewed. It is not discussed in depth, neither is it subject to sharp criticism.

The use of the method in this period is particularly salient in institutional settings, such as schools, hospitals, or other situations where people are instructed, treated or taken care of (e.g. Tveit 2014; Ojanen 2015; Tiili 2017; Sløk-Andersen 2018; Silow Kallenberg 2019; and, for reviews: Suojanen 2000; Wikman 2002; Kayser Nielsen 2004; Gustafsson 2011; Nilsson 2020; Salomonsson 2020). These texts are far from dominant in a numerical sense. But they are numerous enough to produce the impression of a continuous flow in the journal, one where participant observation tends to be essential – albeit not as the single method used, and sometimes presented in other terms. For example, Suojanen’s review of a monograph on caring for people close to death presents, in a positive vein, a

study where the author describes her active involvement in caregiving during fieldwork, and her efforts to learn how to act by observing rather than asking. The review makes it clear that the “fieldwork ethnography” adds to the study in a substantial way (2000:200).

The focus on limited contexts is not entirely new. After all, when reviewers in earlier decades complained about researchers having omitted from their analysis either family life or working conditions (Balle-Pedersen 1987; Andreasen 1992), what they commented on was the result of the researchers having participated in one kind of context only: either working life, or life outside of work. What is new in the studies after the turn of the millennium is that many of the settings under study are not only delimited, but confined; some persons must stay inside, others are there to work with or serve those who cannot leave. Analytically, many of these studies focus on vulnerability, dominance, and power.

Another strand in the studies presented or reviewed after the turn of the millennium is the interest in bodily/sensorial/non-verbal experience, people’s interaction with material surroundings or objects, and/or emotions (Planke 2003; Tiili 2017; Sløk-Andersen 2018; Turpeinen 2019; for reviews, see e.g., Wikman 2002; Kayser Nielsen 2004; Damsholt 2011; Laukkanen 2011; Vakimo 2013; Salomonsson 2017; Stark 2017; Nilsson 2020). In some of these studies (not least those inspired by a neo-phenomenological tradition), researchers draw heavily on their own experiences and actions, or efforts to learn. Such a tendency is addressed as a potential problem in a couple of reviews; Byron (2004:178), Ristilammi (2004:141) and

Fjell (2013:174) raise the question whether, or to what degree, those experiences can be made relevant for understanding what other people feel or think. Similar reflections are verbalised in Terje Planke's article on ski wax (2003), based on a study where he alternated in the roles of "assistant, nuisance and test pilot" for ski waxers (2003:50).¹¹ The focus of Planke's study is on the relationship between "human, object and action" in what he terms a very narrow context: the shifting conditions of snow. Closing his article, he reflects on the difference between action and social action, and on what role the experience of a researcher can have when trying to comprehend other people, past or present (2003:59–60). Such reflections are however rare in the material.

Focusing on the last volumes of the journal, though, reviews more often testify to a successful integration of researchers' own perspectives in the analysis. One example is Stark's appreciative review of a study of a religious movement, which praises the author's ability to draw on her own experience to understand the universe she was exploring:

One of the great strengths of this study is the author's perceptive use of her own experience in participant observation, her recognition that when she saw research participants being moved by something, she was moved by it too, which aided her in understanding how bodily practice, social interaction, meaningful space, and feeling were all intertwined. Indeed, the author's systematic recording of her own emotions, sensations and impressions in the field is a methodologically innovative approach which represents the cutting edge of ethnological research (Stark 2017:196).

In a similar vein, several papers in recent years demonstrate how researchers can

perform participant observation by means of active involvement in the processes under study, as in Sløk-Andersen's article on the making of good soldiers (2018); how the interaction this entails and the cumbersome situations it may give rise to can be subject to a reflexive analysis (Ojanen 2015), and, last but not least, how participatory observation contributes to a study by giving access to dimensions that are not graspable by other methods (Silow Kallenberg 2019:116). Obviously, whatever the method of being present, taking part and registering what happens is called, it is successfully at work in these projects. The term auto-ethnography enters the scene late. As used in recent reviews, it represents the hitherto newest term for a practice where a participant observer makes use of all her senses and capacities for self-reflexivity (Nilsson 2020; Åkesson 2021; Bäckman 2022; Fjell 2022).

Finally, participant observation also tends to activate questions about research ethics. This is indicated, as mentioned above, already at the beginning of the 1980s (Hodne 1981), and it surfaces somewhat enigmatically in a review of a handbook of fieldwork twenty years later, when the reviewer states (with no further comment) that "it is in the participation that the ethical problems arise" (Mandrup Rønn 2000:180). Comments on ethical aspects often refer to the careful hiding, or not so successful attempts to hide, the identity of the places, units etc. that are studied, or the individuals involved in the project (e.g., Karjalainen 1988:171; Klinkmann 2003:120f; Koskinen-Koivisto 2009:154). But the selection also contains a review where the author is praised for her honest reflections on shortcomings that other re-

searchers seldom or never discuss in print (Kverndokk 2010:113).

Reflections: Established, but Marginal?

My first impression after going through the collection of texts is that participant observation is ever present in the contributions to the journal, but rather tacitly. It is seldom foregrounded, and seldom singled out for discussion. In a paradoxical way, it appears to be deeply integrated in our toolkit and somewhat marginal at the same time. If this is the case, there may be several reasons for its marginality.

The first and most banal one to state is that ethnology was for a long time a historical discipline; to a certain extent it still is. In such a tradition, a method suitable for studying only contemporary life will necessarily be marginal. That said, the tolerance for contemporary topics has increased considerably during the journal's fifty years, demonstrated not least by the relative prominence of articles on contemporary themes after 2000.

Secondly, ethnologists are expected to make use of a variety of methods and material in their projects, and the scope of potential methods and kinds of sources seems to widen with each decade. The time when it was possible – if not necessarily applauded – to use the combination of only participant observation and semi-structured interviews is long gone. When there are many methods, they may converge in the production of material and results, with the effect that no single methodological resource stands out.

Thirdly, from the 1980s onward, there have been many analytical approaches in the discipline calling for other methods

than participant observation. One example is the focus on identity and narrative in the 1990s (for reviews, see e.g., Liliequist 1994:142; Sjöholm 1996:119). Both topics are possible, in principle, to investigate by means of participant observation. But it is far more efficient to approach them through interviews, questionnaires, or written autobiographies. Likewise, when the interest in public discourse – initially understood as printed matter and opposed to everyday life – entered the scene in the 1990s, it called not only for new theories, but also for new kinds of evidence and new methods of documentation, sampling and analysis (see e.g., Hvidberg 1995; Magnússon 1997).¹²

Fourthly, one should ponder the possibility that marginality is contingent on the material explored. As mentioned above, *Ethnologia Scandinavica* initially privileged topics and research traditions where participant observation was bound to be marginal. This could contribute to explaining both that it took until the beginning of the twenty-first century before the method played an important role in the articles accepted, and that it was met with such vehement reactions in reviews thirty years before. Here, we might also keep in mind Frykman's remark that theoretically informed fieldwork consisting of participant observation and semi-structured interviews was once the hallmark of the new ethnology, intrinsically related to being in opposition to the establishment (1977:95). Seen against this backdrop, the most remarkable result of my reading is perhaps that the criticism in the first years faded away so fast.

Prospects

So, if participant observation is firmly es-

tablished as one method among many in our discipline, do we need to be concerned about its future? I will leave that for the reader to decide. But I will close the article by mentioning a couple of reasons for my own doubts. As briefly mentioned in the introduction, the backdrop is the formalisation of research ethics that has taken place in the last few decades, and the impression that current rules and regulations make participant observation harder to perform than before.

Two factors seem to produce this effect. The first is that the procedure of vetting (at least in Sweden; Norwegian rules seem laxer in this regard) requires researchers to specify in advance what kinds and amounts of data they need for their projects. This is hard to make compatible with the ideal that you should enter your field with an open mind and learn only as your work proceeds what settings and activities are most important to cover, and what persons (and how many) it would be wise to follow or talk to.

The second is the application of GDPR in research. The intention of GDPR is to protect individual integrity by preventing authorities, companies, or individuals from handling information about living human beings without having their consent, and to be particularly careful with information of a sensitive character. Sensitive topics are not prohibited ground for a researcher. But the rules require us to deal with them with the utmost care, and refrain from touching on them in our projects, unless they are crucial for our research objectives. In practice, then, we are supposed to either decide that we are going to investigate something sensitive or stay away from it. This is basically at odds with the attitude traditionally recommended in participant observation:

follow people wherever they go, listen to whatever they let you hear, and decide as your project evolves whether it is relevant for your understanding or not. (Needless to say, there is also the paradox that sensitive topics are not always sensitive to the people we study; they may want to expose what the regulation urges us to protect. But that is a topic warranting its own discussion.)

In addition, though, there is the more fundamental stumbling block that GDPR, and the general climate of juridification of which it is a part, invite/require researchers to treat social data as if they were personal property, and personal property only. For the time being, this appears to me as the most ontologically troublesome part of the situation. This is so not because I cannot see the value of protecting people's integrity, but because the idea that human beings can own every piece of information emanating from, or traceable back to, themselves is hard (if not outright impossible) to make consonant with the character of ongoing interaction, and with the social and cultural reality that can be grasped through participant observation.¹³ Because when people interact – in whatever setting or channel it may be – their actions and utterances are no longer theirs to control, once they are produced. When people react to what others say or do, their interpretations may be totally at odds with what their fellow human beings thought they said or meant to do. If somebody would like to erase something said or done, the process of attempted erasure is a new sequence of action, with its own unpredictable outcome. In short, contributions to interaction can never be reduced to one single person's intention or perception. For that reason, I

cannot see how individuals' rights to "their own data" can ever be secured in the process of studying what is going on between people. And participant observation is, or was, our prime method for finding out what is going on between people.

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Notes

- 1 For more information, see https://www.riksdagen.se/sv/dokument-lagar/dokument/svensk-forfattningssamling/lag-2003460-om-etikprovning-av-forskning-som_sfs-2003-460; <https://gdpr-info.eu/art-9-gdpr/>; <https://etikprovningmyndigheten.se>.
- 2 An overview of the procedure of vetting and a glimpse of the dialogue between the Swedish Ethical Review Authority and researchers can be gained by browsing the list of frequently asked questions at the website of the authority: <https://etikprovningmyndigheten.se/vanliga-fragor>.
- 3 <https://www.forskningsetikk.no/om-oss/>; <https://sikt.no/fylle-ut-meldeskjema-personopplysninger>. Sikt (Norwegian Agency for Shared Services in Education and Research), established in 2021, actually provides special instructions for researchers working with participant observation; such as explaining how data can be anonymised at the source and how third parties suddenly appearing in the field can be approached with requests for consent; <https://sikt.no/deltakende-observasjon>.
- 4 <https://www.datatilsynet.dk/hvad-siger-reglerne/vejledning/forskning-og-statistik/generelt-om-forskning-og-statistik>, and <https://nationaltcenterforetik.dk/ansoegerguide/overblik/hvad-skal-jeg-anmelde>, and, for Iceland: <https://vsn.is/um-visindasidanefnd/hlutverk/> and <https://vsn.is/spurt-og-svarad/>.
- 5 <https://tenk.fi/sv/etikprovning/etikprovning-in-om-humanvetenskaperna>.
- 6 As one of the anonymous reviewers reminded me, trouble arises not only for studies relying on participant observation, but also for researchers using interviews, social media etc. That is certainly true (for a discussion of difficulties concerning oral history, see Thor Tureby 2019). But for reasons that will be spelled out at the end of the paper, I do believe that among the various methods and techniques for generating contemporary material for qualitative analysis, participant observation is the one that is hardest to reconcile with the current rules and systems.
- 7 Thanks to the editor of *Ethnologia Scandinavica*, Lars-Eric Jönsson, whose invitation to reflect on our disciplinary history through the journal (at the RE:22. Nordic Ethnology and Folklore Conference in Reykjavik) provided me with a case to work with.
- 8 Similar criteria for judgement are echoed in a review of a monograph on mumming in the same volume; here the author is praised for *not* ignoring "...the demands for verification like a number of social anthropologists have done" (Bringéus 1975:195).
- 9 The main criticism raised in this review is that the author focuses more on one of the two main categories in the community under study than the other, and that more attention is devoted to men than to women. Reading between the lines, one might get the impression that the positions that are closest to the author's own are under-analysed. But that is not commented on in the review.
- 10 In Arvastson's earlier article mentioned above, on the other hand, there is no similar contrasting of observing and interviewing. In that text, all the methods applied seem to converge towards the overarching goal to "penetrate into other people's views of their surroundings" (1983:55).
- 11 Like Povrzanović Frykman above, Planke does not use the term participant observation.

But the actions referred to and the reflections presented at the end of his article nevertheless make the text relevant for the discussion here.

- 12 To this might be added the hypothesis that theoretical reorientations could make participant observation obsolete, to the effect that it has in fact been replaced with quite different methods (a neighbouring thought would be that I have underestimated the importance of the variation of terminology in the set of texts). One remark in the selection of reviews articulates such a view: “This is a report based on what was formerly called participant observation but has now been developed via Guittari’s ‘rhizome perspective’” (Karlsson 2016:206, cf. Høyrup & Munk 2007:6f for a comment relating the concept of rhizome to ANT and to the practice of multi-sited fieldwork). At the same time, though, the concept of participant observation continues to be in use in other texts, and there are no general signs of its abandonment.
- 13 Here is the connection to the remarks made by the Norwegian anthropologists and Wästerfors, referred to in the introduction. While my reflections are hyper-micro and experience-near, and theirs focus on the outcome of research at an aggregate level, we all touch on the problem that the rules are made to protect individuals, while the research focusses on phenomena that are social (cf. Wästerfors’ later clarification in a recorded seminar conversation; Edlund et al. 2021:462).

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