

Article

Ethnologist as Foreign Body: A Systemic Explanation

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Abstract: During an ethnographic experience, which took place in a rehabilitation clinic, I had to deal with situations that required me to make a series of adjustments to my role in the clinic, so as to reduce my involvement with both patients and therapists. Although I expected to feel more at ease as the field progressed, instead, I felt as if my presence were more and more disruptive, and gradually becoming problematic. The systemic approach thus seemed the most relevant for clarifying the complexity of the interactions that were at play, and that shaped my experience, as I had to venture beyond reflexivity. The aim of this methodological article is to shed light on the need for constant adaptation in the ethnologist, in order to maintain their presence in the field, and obtain information to carry out research. In order to do so, a systemic triangulation has been performed based on the Donnadieu and Karsky method, leading to an analysis of some of the difficulties encountered, as highlighted via systemic thinking.

Keywords: ethnography; systemic thinking; clinical fieldwork; miscommunication

1. Introduction

The ethnographic approach is intimately linked to the ethnologist's personal experience, as it is built up over the course of interactions [1]. This explains the importance of reflexivity in anthropology, as it enables researchers to examine their relationships with their informants in such a way as to take into consideration how their own traditions [2] influence the knowledge that they construct. Fieldwork is a form of communicative interaction between ethnologists and their informants, who find themselves in the same space-time (what Fabian termed the coevalness of the ethnographic field, in 2014), but it also recognizes how this coevalness allows us (or not) to open up to the intersubjectivity of encounters during fieldwork. These relationships are only part of a larger whole, but it is through them that ethnologists can better grasp the systems that nurture them, and inform us of the social transformations that characterize, above all, contemporary societies.

Most anthropological texts include information on methodology, so as to meet certain criteria regarding the validity of ethnological materials, and the value of analyses. Ethnologists, thus, reveal certain elements about themselves, such as their self-defined identity and/or the one assigned to them by actors in the field, and/or the extent of their involvement in said field, elements which bear witness to reflections on their posture as observer, and the effect of their presence on interactions but, above all, on their own interpretations [3]. Nevertheless, even if the ethnographic approach is recognized as a path marked by pitfalls, crossroads, dead ends, forks, and topography [4], these elements rarely provide direct access to the doubts and strategies of adaptation and negotiation that characterize their approach [5–7]. Thus, they elude questions about the research process, or the dynamics of understanding [8].

Yet not only could it be interesting to look at the ethnologist's process of adapting to the surprises, unforeseen events, and obstacles that shape ethnographic work as a social phenomenon, but it could also be formative for aspiring ethnologists to glimpse at challenges others have had to face in their fieldwork, at the pitfalls involved, and reflections on how to deal with them, thus moving away from the notion that the ethnologist's role



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(in anthropology, we often refer to Linton's definitions, which described the ethnologist's role as orientated to others' patterned expectations. For the author, in every society and every group, each member has some function or activity with which they are associated. What the individual does or performs, we generally call their role [9]) is largely learned "on the job" [4,8,10]. It is around this premise that I wrote this article, which aims to explicitly describe one of my field experiences, during which I encountered several difficulties. Given the nature of the ethnographic experience, which is reminiscent of an initiation rite [11], as the ethnographer finds themselves in a liminal state, a stranger [12], separated from their own culture but not fully integrated into the host culture [13], I expected some of these difficulties, such as the feelings of entering unfamiliar territory, of being an imposter, and of embarrassment and discomfort. However, others were more surprising, notably because I expected to feel more integrated as the research progressed, whereas, instead, I found myself feeling more excluded.

During this ethnographic experience, which took place in a rehabilitation clinic, I had to deal with situations that required me to make a series of adjustments to my role in the clinic, so as to reduce my involvement with both patients and therapists. Although I expected to feel more at ease as the fieldwork progressed, instead, I felt as if my presence were more and more disruptive, and gradually becoming problematic. I experienced this series of events, which required me to make major adjustments to my role as an ethnologist, not only as an ethnographic ordeal, but also with a certain bitterness, as I was unable to find the "*posture d'inscription relationnelle*" (relational stance) [14] that would have enabled me to complete my observation process. A posture of reflexivity allowed me to understand certain elements, particularly regarding my own traditions [2,8], but it did not allow me to understand what was at stake in the field, which sometimes put me, as a researcher, in a position of failure. I had to go beyond reflexivity, to understand that the adaptation of my role as an ethnologist depended not only on the relationships I had succeeded (or failed) to forge with the actors in the clinic but, rather, on multiple interactions within the clinic. The systemic approach, thus, seemed the most relevant for clarifying the complexity of the interactions that were at play, and that shaped my experience. Not only did it provide me with the words to do so, but it also enabled me to take a broader look at the role of the ethnologist, which, far from being the result of a single individual's role, is, rather, the result of a system of interactions between multiple players. This insight constitutes the crux of this article.

The aim of this methodological article is to shed light on my role as an anthropological researcher in the field, specifically in a clinical context and, above all, on the need for the constant adaptation of this role, in order to maintain my presence in the field, and obtain information, to carry out the research for which I had been hired. This article, therefore, delves into this ethnologist's personal experience of the imperatives and constraints of the research field. It attempts to understand what ethnologists have to deal with in fieldwork, in order to grasp their role, and adapt it to the codes and norms of the field.

The article begins with an overview of the literature, to see what it tells us about the ethnologist's role in the field, particularly in clinical settings. This is followed by a description of my understanding of systems thinking, and what it offers as a tool for anthropology to explore this adaptation work but, above all, the mechanisms in place in the field that can make room for researchers, or that sometimes put them in a position of failure. Next comes a systemic analysis of my own field experience. This starts with an ethnographic account illustrating my day-to-day work as an ethnologist in a clinic, is followed by a systemic triangulation based on the Donnadieu and Karsky [15] method, and ends with an analysis of some of the difficulties encountered, as highlighted via systemic thinking.

2. Ethnologist in the Field

The ethnographic method has been developing in anthropology for over a century in different parts of the world, notably in Western Europe and North America. It provides an account of social facts [16], through empirical and descriptive results, and by setting

up a comprehensive study of phenomena [17]. Firmly established as the methodological cornerstone of social anthropology since Malinowski, who, in his own way, postulated a position of “ethnographic authority” (the ethnographer as a knowledgeable, disinterested, and trustworthy source of information) [18], the ethnographic practice was destabilized in the 1980s with the publication of *Writing Culture* [19]. This book, which has become an essential part of anthropological literature (and of the postmodern critique of ethnography), highlighted the implicit biases posed by the ethnologist’s very position in the field, the choice of voices to be heard, and the materials to be transposed into the monograph. Considered a “watershed in anthropological thought” [20], the ensuing debate shed light on the limits of the ethnologist’s objectivity and partiality. This led to a call for fieldwork with a greater personal component, including details of the researcher’s feelings and relationships with the actors in the field. The intellectual legacy of this crisis is complex, but the increased importance of reflexivity and literary, dialogical, and collaborative approaches are all part of it [21]. Nevertheless, it is clear that profound reflections on the relationships between ethnologists and their informants have left their mark on anthropology, even leading some to situate its epistemological space in the intermediary space between the ego and the alter ego [22], thus recalling the importance of the dynamics of field encounters in the production of ethnographic knowledge [23].

Today, as the importance of reflexivity continues to grow, some authors focus on its practical application, such as through ethnographic vigilance, which guides the researcher’s choices and influences their approach, while also serving as a lever for making their investigative practice more objective [4]. Indeed, in the absence of clear and precise guidelines for action in the field, the ethnographer must become hypervigilant, so as to closely follow the nuances of the environment, its uniqueness, and its contingencies. They must immerse themselves without being submerged, and wander without getting lost [24]. Ethnographic vigilance plays a dual role, as a theoretical concept and a methodological lever [4]. Practically speaking, ethnographic vigilance makes it possible to understand the evolving, meandering nature of field experience, as well as the researcher’s relationship with their investigative approach, the actors involved, and the contingencies encountered. Fieldwork, thus, requires researchers to make a series of adjustments and adaptations, which they guide through reflexivity.

The ethnographer must, therefore, constantly define and redefine their role in the field [25–27]. They must adapt it during the course of the investigation, in particular, to gain the trust of the people in the environment under study. Ideally, they should even manage to be forgotten or, somehow, to make their presence invisible [28], which is not without its paradoxes, especially as, by definition, researchers do not master the codes of the community where they are working. To do so, their role must be adapted to the situation, and negotiated with their informants.

Ethnographers have a number of options when it comes to their role in the field. Thus, important distinctions must be made between active and passive roles [29], as well as between observational and/or participatory roles, where researchers either explicitly disclose their intentions, or do not [30]. Gold [31] offers us a simple typology of four roles for the ethnographer: the participant, the participant-observer, the observer-participant, and the observer. Participants become a member of the group being observed. Participant-observers, who explicitly state their research intentions, find themselves in situations where they must participate by observing, while, at the same time, creating and maintaining relationships with their informants [32]. Observer-participants, on the other hand, maintain brief, formal, and explicit contacts with their informants [33]. Finally, observers essentially withdraw from the interactions among their informants. This typology is interesting but, in my view, the roles should not be seen as mutually exclusive. They need to be filled at different times, and in different contexts [33]. For example, during their first moments in the field, ethnographers might opt for the role of observer, to have time to integrate the information they receive, in order to better master the codes of the field and, thus, gain the trust of informants. This choice may also be imposed on them by the field, as

it is linked to the 'space' allotted to them as researchers. As the research progresses, researchers will be better able to adapt to the situation, and be sufficiently accepted by their informants, in order to play an increasingly participatory role. In addition, some contexts are more conducive to one role than another. For example, in a clinical context, researchers may prefer to maintain an observer role, so as not to interfere with the therapist–patient relationship or the intervention in progress. In another context, the observer posture might make participants uncomfortable, for example, during a social activity, which might lead the ethnographer to take on a more active role, in order to gain acceptance.

Ethnographers must adapt their role to the codes and norms of the environment under study. This can be a source of shock for them, as the codes and norms in the field may be far removed from the researcher's personal background and traditions, even when the ethnography is carried out in the researcher's own social and cultural environment. Culture shock refers to the feeling of being disoriented and losing one's bearings when the environment studied is far removed from the individual's familiar universe [34–36]. Often used to refer to interethnic relations, it also applies to disciplinary or professional cultures. Culture shock is a feeling of not fitting in, or being like a third wheel [28]. It is to be expected that this feeling will be momentary, and will fade away as researchers find their place and are accepted in the milieu, although certain difficulties and resistance may persist.

There seems to be a consensus that there is no general answer as to what role the ethnologist should play in the field, especially when problems arise, as each research situation involves specific circumstances that vary from place to place and time to time [37]. Nevertheless, certain ethnographies with accurate, precise, and rich analyses should serve to inspire and inform us about what awaits us in our own field experiences, especially as there are similarities between certain contexts. My interest is in ethnographies in clinical settings, which often occurs in hospitals.

Despite many accounts of hospital ethnography [38,39], the information that might help anthropologists prepare for fieldwork in clinical settings is sparse, and too often context-specific. The tools to help the ethnographer make sense of, and describe, their personal experiences seem to be lacking. Furthermore, methodological reflections are mostly used to make better sense of what is happening in the fieldwork [40,41], rather than helping the ethnographer to prepare, and are mostly about consent and/or gaining access to the field [42,43].

One of the first things an ethnographer needs to think about is the role they will play in the field. In a hospital, the choice of roles seems to be limited to patient, healthcare professional, and visitor [44,45]. However, this choice does not tell one how to act in the field. Fainzang [46,47] points out the risk of field actors attempting to use the researcher for their own ends, and explains the importance—for ethical and methodological reasons—of not taking sides. Nevertheless, how to achieve this objective of "neutrality" (the idea of neutrality is problematic in social science. It "is logically untenable and anthropologically naïve" [48], as the proponent of the "unmasking tradition" [49] would argue that scientific progress is determined by social factors, such as personal or group interests. Nevertheless, the researcher's appearance of neutrality in the eyes of the informant is important in certain ethnographic contexts, in order to foster a trusting relationship. The goal is to gain access to contrasting information from a diversity of actors, so as to understand a phenomenon from all perspectives) appears more complex, as clearly demonstrated by the author's example of how patients can interpret a non-response or gesture as information about their prognosis.

In an interesting account of hospital ethnography, Chartrand [50] explain how she had to adjust her research method to the different situations she encounters. Therefore, she offers the reader a grasp at how she learned to ask for consent in a matter appropriate to her field context, to make good use, or not, of a recorder, and what method was more appropriate for collecting data. Although her reflection brings matter to the question of how to conduct ethnography in clinical settings, it keeps the discussion on methods, and leaves aside the interpersonal side, and does not offer tools to reflect on the matter for future ethnographers.

Finally, in a special issue on ethnographic vigilance and methodological reflexivity [4], Lapointe [14] offers us a highly personal account of her experience in a hospital. She entered the field as “someone unlike anyone else, whose rights, responsibilities and obligations were ambiguous” (translated by the author) (p. 174), and tells us that the pace of work in these environments, and the high staff turnover, make informal exchanges difficult, and hinder the integration of researchers. Finding her place was a burden and an obstacle that Lapointe felt in her body (“Discomfort in my body, which I experienced as a burden” (p.176), translated by the author). Lastly, she explains how the role of “all-round caregiver”, which she herself cobbled together thanks to her experience as a personal care attendant, enabled her to find her place, and attain her research objectives.

These are some of the articles I found when looking for references on the role of the ethnographer in the field and, more specifically, in the clinical setting. To me, they remain very general and sporadic. Apart from a few hints about what to expect in the field, they did not give me any clear guidance on how to adapt to the field’s codes and norms. Indeed, there seems to be an implicit expectation that the anthropologist will adapt to the field without too many difficulties, through curiosity and humility [12]. It was these implicit expectations that awakened feelings of failure in me when it was implied that I had to put more distance between myself and my informants, as if it were a lack of personal humility and curiosity that had hindered me in my reading of the codes and norms of the milieu. In this case, the systemic approach was a very useful tool in helping me to understand that this was not the case.

3. Systemic Thinking

Systems theory, inspired by the work of Gregory Bateson, among many other academics inspired by the Palo Alto School in communication and related fields in social sciences and humanities, is based on the idea that behaviors and beliefs, often regarded as individual factors in certain disciplines, are, in fact influenced by a multitude of factors, which, taken as a whole, contribute to the construction of a “system”. From this perspective, systemic approaches are based on the interconnectedness and analysis of different levels of systems (individual, organizational, and societal, for example) in order to understand the singularity of each life course in relation to large-scale structures, patterns, and interactions [51].

For Bateson, human communication is based on contextual frames of shared reference, but also on complex everyday decisions about the type of information relevant to context-specific communication (“the difference that makes difference”) [52]. To support his theory, the author drew on set theory, notably the notion of logical types, as well as the concept of recurrence or patterns in social relationships, and various original concepts, to explain the dynamics of equilibrium and disequilibrium, such as homeostasis, feedback, and schismogenesis. Bateson’s systems theory approach emphasizes the dynamic and interdependent nature of systems, as they exhibit both continuity and change. According to this approach, studying interactions at the micro level is essential to understanding how systems evolve over time [53].

In the social and communication sciences, systems thinking is used by Gregory Bateson to put forward a general theory of mind, i.e., to model the relationship between a system of thought and the environment that enables it to survive [54]. A system of thought adapts to the environment that gives it meaning, just as the musicians in an orchestra tune themselves to their fellow musicians, or ethnographers adapt their role according to the actors in their milieu. For Bateson, understanding an individual’s behavior requires looking at the links that the person maintains with the other actors in the system, as these relationships are mutually influential. In these interpersonal communication processes, the question of interpretation becomes central, and can be the source of numerous communication breakdowns and cultural clashes.

Systems thinking is useful for representing complex objects, such as interpersonal interaction situations, characterized by imprecision, instability, ambiguity, and unpre-

dictability [15]. In my various attempts to make sense of my field experience, the notion of a system, seen as a set of elements in reciprocal action, and organized according to a goal [55], was useful, and enabled me to model the ethnographer's role during fieldwork, where there were, a priori, no pre-established role models. Indeed, ethnographers interact with the various actors in the field, all of whom have expectations and reactions that are largely indecipherable and unpredictable. It is with this incomplete and imprecise information that researchers must adapt their own actions, attitudes, and expectations, which may also seem imprecise, and even surprising to them.

Three systemic concepts, in particular, helped me to make sense of certain situations in the field: recurrence, the feedback loop, and the transducer. Recurrence is used to identify a pattern of behavior over time. This refers to regularities in behavior, which are used to create a pattern, and predict future behavior. The feedback loop is a process that depends on information sent by the environment and received by the system in order for it to adapt, so as to maintain its homeostasis (stability) [56]. Responding to its environment, the system then sends information back to itself on the result of this adaptation, in order to adjust, if necessary. For example, if informants in a fieldwork setting act with mistrust, then the researcher can act to reduce this mistrust (with transparency, or by addressing the subject of mistrust directly). Bateson calls it schismogenesis when the system instead engages in a process of differentiation that takes a symmetrical or complementary form [51]. For example, if the researcher becomes more transparent in order to reduce mistrust, but this transparency creates more mistrust, in turn bringing the researcher to respond with even greater transparency, the informants' mistrust could become so great that the relationship breaks down, with no possibility of being rebuilt. The result of such schismogenesis is not stability, but an imbalance in the system, and possible collapse. Finally, the transducer is what processes the transformation of an event (or a difference) into a signal. It is this transducer that captures the difference and transforms it into a stimulus that makes a difference, which potentially generates a new, more appropriate, adapted, or contextually sensitive tailored response. For Bateson [57], it is the difference that enables us to perceive. A difference must not only exist, but also trigger a volley of stimuli: the difference must make a difference.

Systems thinking is also used as a constructivist method for the artificial organization of a given complex situation [54], as a system is first and foremost a representation of an object based on observed patterns and models (which may differ depending on who is looking at it). This complexity stems from a wide variety of system components with specialized functions; non-linear interactions; the difficulty, if not impossibility, of exhaustively counting the elements that make up the system; and the wide variety of possible links [55]. Moreover, systems thinking is a construction of reality, and makes no claim to exhaustiveness. The knowledge acquired through modeling exercises, i.e., the presentation of a complex phenomenon in the form of a formal model, will never exhaust other possibilities of interpretation according to other purposes. Nevertheless, the approach remains rigorous, based on tangible clues of an analytical correlation, suggesting the presence of a pattern, and a system of interactions.

To give meaning to my experience in the clinical setting, I undertook a systemic exploration, as proposed by Donnadieu and Karsky [15]. This exercise consists of defining the boundaries of a system to be studied, in order to situate it in its environment, and understand the nature of the interactions involved. Systemic exploration also serves to sketch the internal architecture of the system, including its main components and relationships, to allow us to understand its evolution. Several tools are available for systemic exploration, including systemic triangulation, which I used to model my fieldwork experience. This tool requires us to observe the object from three complementary angles, each linked to the observer's point of view. It is through the study of these three aspects combined, and the ability to move from one to the other, that systemic triangulation provides an ever-richer understanding of the system:

(1) Its functional aspect: what does the system do in its environment? What needs does it meet? What is its purpose?

(2) Its structural aspect: how do the system's various components fit together? How are they related?

(3) Its evolutionary aspect: how does the system evolve? How and why does it change?

In the next section, I will draw on Donnadieu and Karsky's [15] methodology to model my role as ethnologist in my clinical fieldwork, elaborating on its functional, structural, and evolutionary aspects and, finally, taking a look at the difficulties encountered, to shed light on their nature.

4. Case Studies: The Ethnologist in a Clinical Setting

The first step in systemic modelling is to situate the phenomenon in a descriptive and quasi-photographic way [15]; i.e., to describe the system we are trying to model in as detached a way as possible, even if we recognize that it is impossible to totally subtract the interpretation of the researcher describing it.

An Ethnographic Tale

As a post-graduate anthropology student specializing in intercultural relations, I was hired to support a research team in collecting data for a project that had been in development for over two years. The project concerned the therapeutic alliance between injured immigrant workers and a multidisciplinary rehabilitation team. It aimed to document mechanisms and strategies for maintaining the therapeutic alliance, including the organizational, personal, and social factors associated with its weakening, breakdown, restoration, and stabilization. It was our shared interest in intercultural issues that led me to work with this research team, especially in a parallel project on intercultural skills. The other areas of this project—occupational health and safety and rehabilitation—were unfamiliar to me at the time.

To study the therapeutic alliance, we focused on its three components: trust between the therapist and the patient, partnership concerning the tasks to be accomplished, and partnership concerning the set goals [58]. To this end, the team implemented a mixed ethnographic design, combining qualitative (observation, interview) and quantitative (questionnaire for statistical analysis) research tools. Thus, we assured the continuous presence of a researcher in a rehabilitation clinic for the entire duration of a rehabilitation program (approximately eight weeks) for participating immigrant patients (for a total of 20 weeks), in order to observe and understand the clinical intervention context (e.g., clinical decision-making processes, day-to-day inter-institutional ties, and interpersonal relationships), the types of intervention (e.g., skills development, reconditioning, psychological follow-up), and the multiple interactions that take place among individuals (e.g., therapists, and managers). The researcher's observations were documented in a logbook. To complement the documented observations, semi-structured interviews with participants patients were conducted in two stages: at the beginning and at the end of the rehabilitation program. Lastly, a repeated measurement of the therapeutic alliance was taken every day, using the Working Alliance Inventory, a 12-items questionnaire addressing the three dimensions of the therapeutic alliance [59].

This research took place in a rehabilitation clinic offering an interdisciplinary return-to-work program with a high percentage of multi-ethnic clients. Access to the field was negotiated with the management of the clinic group that collaborated with us, including two members involved in research and development. These two individuals were only present sporadically at the clinic chosen for the fieldwork, but they ensured that our presence was accepted by the latter, both by management and therapists. It seems reasonable to assume that the level of acceptance differed from therapist to therapist. In addition, the consent of participants (the consent included three components: (a) participation in two 60- to 90-min interviews; (b) observation of rehabilitation treatment sessions; and (c) observation of clinical team meetings in which they discuss the patient's treatment), both therapists and patients, was obtained by means of a form, as required by our ethical certification.

The clientele was diversified, but the participants in our study had the particularity of being part of the clientele following a rehabilitation program. They have been on compensated sick leave for more than a year, and suffer a form of chronic pain and, in the case of some of them, some psychological distress. This program, covered by compensation, is followed when primary care and attempts to return to work have failed. The rehabilitation program is a multi-disciplinary program that seeks to not only address patients' physical needs, but also explore their psychosocial issues. This meant that our participants were there through obligation to the third-party payer, the Commission des normes, de l'équité, de la santé et de la sécurité du travail (CNESST), a government agency entitled to promote occupation health and safety, and compensation for injuries and rehabilitation; otherwise, they would have lost their income replacement benefits, in addition to having to live with chronic pain following repeated reports of failure.

The presence in the field was assured by three ethnologists. Of the five clinical field days per week, I was responsible for three or four, including the day of the interdisciplinary team meeting. The other two were mostly covered by the coordinator, while the principal researcher replaced us in the field when the coordinator and I had scheduling conflicts. This teamwork was also reflected both in the semi-structured interviews, which were conducted by different members of the team, and in the administration of the daily questionnaires, which were the responsibility of the person who had to be in the field that day. The following overview of a typical field day at the clinic provides a better understanding how our ethnography unfolded, at least from my part.

At the start of the week, I arrived at the clinic around 8:30. The clinic serves not only patients enrolled in a rehabilitation program, but also private clients who come for occasional treatments. The space is divided into public areas (a gymnasium, a reception) and private areas (administrative offices, and individual therapy rooms). The receptionist's desk bridges the two areas (see Figure 1). This is where the day began for rehabilitation patients, and where therapists came to distribute their individual programs for the day. They put all the programs onto clipboards, and placed them on the end of the front desk. When patients arrived, they found the clipboard belonging to them, and headed to the gym to start their day. The clinic is located in a building that serves other functions that we cannot name here for reasons of anonymity.

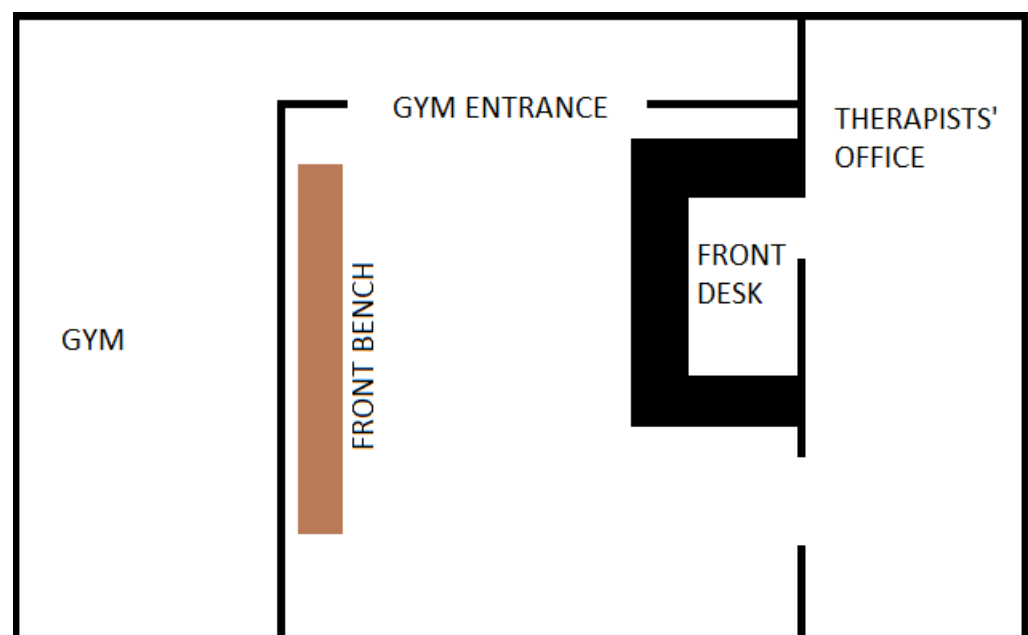


Figure 1. Plan of the clinic.

The kinesiologist in charge of the morning gym often made her first contacts with patients at this front desk. When I arrived in the morning, I also headed to it, as the research documents were stored there. I opened the drawer and selected the number of questionnaires needed for the day, based on the number of immigrant patients present multiplied by two (to include the version to be completed by the therapist). I then entered the immigrant participants' names and the dates, and inserted them into their daily plans. Next, I went to the therapists' offices, and placed the questionnaires on their desks. I then took an empty clipboard from the front desk, which I needed because it had a barcode that gave me entry into the gym, rather like a membership card.

I headed for the treadmills, which were lined up to form a line down the middle of the gym. This was where most of the patients started their day. Every morning, a series of accident victims crossed paths with people who had simply come to work out and keep fit. The difference was noteworthy: the patients' running speed was much slower, there was no sweat on their foreheads, and their faces often expressed the permanent pain, fear, and stress they felt. Moreover, a kinesiologist was often leaning in front of one of these treadmills, talking to one of the patients. They had to go there to see each patient, and catch up on their weekends or evenings, as well as to discuss their anxieties or goals for the day. Even if patients and therapists knew of, and consented to, my presence, I tried to fit in so that, paradoxically, my presence caused minimal interference. Therefore, I stayed active as they all did, by exercising. But first, I had to say hello to everyone, and check up on the study participants, in order to obtain information, and deepen the bond of trust. I then headed for a treadmill, preferably one near my study participants. This allowed me to observe their interactions with the therapists. From there, I could watch what was going on around me: which therapist was present? Had the participants in my study arrived on time? Were they interacting with other participants? At one point, I would see one of the participants looking at their plan for the day with the kinesiologist. I would approach them, grab a floor mat, and do some crunches next to them. Discreetly, so as not to disrupt the meeting, I tried to hear what was being said, and observe their non-verbal behaviors. It was a question of finding a complex balance in a paradoxical situation: being as invisible as possible so as not to be a disruptive element, while still being present enough to gather information. Following certain interactions, I would often go and see the kinesiologist, again discreetly, to obtain more information, check my interpretation, or add her perceptions to my notes.

At a certain point in the day, as my schedule dictated, the physiotherapist would take one of my participants for a private session. I would ask if I could join the meeting. I was welcome, but first I had to obtain the patient's consent. The patients usually did not mind, as they wanted to help us with our research. This is how I adapted my days to the rhythm of my participants' private appointments (physiotherapy or occupational therapy). These appointments differed greatly from one patient to another, and from one practitioner to another. For example, one member of the clinical team, who was very involved in research and development, included me a lot in his interventions and discussions. I noticed many demonstrations of cultural awareness in the way he explained his clients' symptoms to them. There were other days when the appointment was with an occupational therapist who had a much more personal approach to clients. During her meetings, she tried to create an atmosphere of trust and reassurance. My presence was less appropriate with this kind of approach, especially if the client was shy. I once saw a client cry. I felt that my presence was too much and, at that point, my role as researcher and my role as human being became more difficult to reconcile. I had to balance my curiosity as a researcher, my sensitivity as a human being, and my ethics as an observer. In any case, I decided to stay when more personal moments arose, so as not to rush or interrupt the unfolding of emotional situations, as this was a key moment in the creation of the therapeutic alliance.

I was often free to leave the clinic at lunchtime, as most of our clients were only on a half-day program, and finished at noon. I did, however, stay for lunch when a participant was present in the afternoon, or when there was a team meeting for the therapists in the

afternoon (Thursdays). There were very few of us in the gym on these afternoons, and the atmosphere was very calm.

When the day ended on Thursday, at 2:30 p.m., I waited for the weekly team meeting to begin. This was when the therapists would talk about each client, whom they had colour-coded according to the likelihood of them completing their rehabilitation mandate (from red to green). The therapists, thus, shared the snippets of information each had received from the patients. This enabled them to build a more complete picture of their patients, and to discuss intervention strategies that could help them achieve their goals for the day or for the entire program (e.g., endurance, pain management, functional capacity building, mobilization, return to work). Contextual, family, motivational, psychological, and other factors were discussed. This meeting gave me an overview of what the therapists were taking into account in their interventions, what was noteworthy for them, how they use the notion of culture in their understanding of their clients, how they felt about them, and whether they thought these feelings would have an effect on future interventions. This was how the day ended. I returned home exhausted from the multiple levels of effort involved in my day: physical, from the training; mental, from the research work; emotional, from the proximity to the immigrant patients' distress; and, lastly, psychological, from the level of self-awareness I had to maintain throughout the day to ensure that I took on the appropriate role at the appropriate time.

5. Systemic Triangulation

It was in this particular research context that I wanted to take a close look at my role as ethnologist, viewing fieldwork as an open system where the researcher receives information from their environment, and continually adapts so that their presence continues to be accepted, and they earn the trust of their informants. However, things are not quite so simple. This is a complex phenomenon, due to the many interconnections and interactions that characterize it, and the systemic approach is one way of unravelling it. Thus, I propose a systemic exploration of the ethnologist's role, which, according to Donnadiou and Karsky [15], is a method used to provide a reasoned and coherent knowledge of a complex object. To do so, I will use the systemic triangulation tool to look at the system from three different but complementary angles, as defined earlier: (1) functional, (2) structural, and (3) evolutionary.

5.1. Functional Aspects

Firstly, what do ethnologists do in their environment? In other words, what is the purpose of their work? The purpose of fieldwork is to gain access to a group's natural environment, in order to observe its actual everyday behavior, and analyze a given phenomenon [60]. The researcher's prolonged presence in the field enables them to describe in depth what they see as banal details, in order to convey what is going on in a particular socio-historical context [61,62]. In this case, the aim was to understand the therapeutic alliance between therapists and immigrant patients injured at work. The purpose was to describe, in a detailed and precise way, the interactions that attested to this alliance or to its absence, in order to refine our understanding of the concept of a therapeutic alliance in this particular context of immigrant worker rehabilitation, knowing, from the vast literature on the topic, the crucial importance of this alliance on the rehabilitation process and on positive clinical and occupational outcomes [63,64].

To this end, I had to earn my own informants' trust in order to gain access to their inner world and subjectivity. In a clinical context, where the very presence of a member of the research team is based on an agreement with the clinic, the therapists' adherence to the project depended on the ethnologist's discretion: I had to maintain an appearance of neutrality, and ensure that my presence did not constitute an additional burden for the clinical team and management, especially in a context of work overload, as observed in the clinic (during our days in the clinic, we observed several signs of work overload, including therapists' comments telling us that they could not address certain issues or carry out

certain interventions that they would deem relevant, due to lack of time, at the clinic level, but also in terms of the rehabilitation days granted to patients by the third-party payer) [28] and in other rehabilitation settings [65]. In addition, as the immigrants' participation was voluntary and consensual, I had to offer conditions for participation that were favourable, such as not adding a burden to the process (in terms of stress or time, for example), and being pleasant to be around.

5.2. Structural Aspect

Secondly, what is the structure of the system? In other words, how does the ethnologist relate to the various components of the system? Basically, ethnologists use their previously acquired knowledge and observations of the environment to adapt their words, actions, and attitudes to the key players in their research environment. A clinical environment must be seen as a place of care, expertise, and organizational and professional cultures [66], with its own rules, logics, and social structure. Despite this complex environment, this systemic analysis focuses on the two main categories of actors encountered by the researcher in the field: the clinic's therapists, and the immigrant patients.

As for the first category of actors, i.e., the therapist team, the researchers made a commitment that they would respect the ethical standards of the clinic and of their research, and that they would deliver the outcomes stated in the research specifications. On a day-to-day basis, I had to be transparent, and ensure that my presence in certain activities and my data collection techniques did not cause embarrassment, or hinder therapeutic activities. I had to exercise discretion and restraint, to ensure that I did not interfere with interventions. In addition to considering therapists at a professional level, I had to consider them at the personal level. For example, some people's personal insecurity or level of familiarity with the research world could influence my relationship with them. Thus, I had to be on the lookout for behaviors that might indicate discomfort, and deal with them sensitively.

Regarding the immigrant patients taking part in the research, several issues had to be taken into account. Firstly, they agreed to participate for different reasons. For some, it was to please and help the research team in our project. We also had to consider the possibility that some of them simply did not feel comfortable refusing to take part in the study. To ensure that this did not happen, I would repeat several times, and in different ways to make sure I was clearly understood, the optional nature of research participation, and the possibilities of opting out. In addition, I made sure to repeat this information if I observed, in their behavior, a desire not to be disturbed by our team (very short answers to questions, isolation, etc.). For others, participation in the project was based on the hope that their sometimes difficult, even chaotic, migratory, and professional integration path would be better understood by the research community, and that this would help improve government services and programs. I had a special relationship with them, as they were very generous with information. They tended to be more forthcoming about some of their difficulties or frustrations. Nevertheless, I had to be clear and transparent about the real impact of my research, so as not to mislead them.

Another issue that had to be taken into account was the participants' perception of my role within the clinic (this was not an issue for the therapists). It was understood differently by the participants, especially in terms of my degree of independence from the clinic, and from the compensation and rehabilitation system in general. I therefore had to ensure, as far as possible, that I acted in accordance with the researcher role that I wanted to assume, i.e., one who sought to paint a nuanced picture of the situation from the point of view of a diversity of players. I did this, notably, by demonstrating that I was not biased (in other words, to convince them that I was not in collusion with the clinic, the insurer, or their employer), in order to maintain the trust of all the players in the field. I had to gauge the degree of familiarity I needed to maintain with the immigrant patients in order to achieve my research objectives, while also respecting my ethical imperatives, and this degree depended on each person's personality and level of familiarity with the world of research. Above all, I had to listen to the participants, and pay close attention

to their non-verbal language, in order to understand their particular positions, and their perception of my role. Important interpretation issues play a major role in communication, particularly in an intercultural context, as the codes of immigrant patients could be very different from my own, such as physical proximity, emotional expression, eye contact, and voice intonation.

Moreover, as I was working in a clinical environment with a clientele living with chronic or persistent pain and long-term absence from work, I had to be very vigilant about my choice of words, and attentive to the worker's words, as they could be interpreted in a way that reinforced a worker's beliefs (e.g., that he was right in thinking his pain was due to an undiagnosed injury). The biggest part of the therapist's job was to try and change these beliefs, as there is a very strong link between perceptions and pain (e.g., there is no operation that will make the pain go away but, rather, constant work is needed on their part to manage it). At times, this became very difficult and paradoxical, as expressing support for immigrant patients, in my capacity as a good anthropologist who was demonstrating empathy and encouraging information sharing, could interfere with the therapists' work.

5.3. Evolutionary Aspects

The third and final question to ask when studying the components of the system involves its history, and how it has evolved over time. In my case, my presence in the field gave me an understanding of the clinic's norms and codes, and a better idea of what was expected of me. This learning process often required a series of trials and errors to adapt my role in such a way as to maintain good relations within the clinic, and obtain the information I needed. Over the course of my fieldwork, two main elements evolved: my degree of familiarity with the immigrant patients, and my discretion within the clinic.

Firstly, I had to consider the degree of familiarity to be maintained with patients and therapists. Lunchtime at the clinic was a good time to reflect on this. During gym hours, relationships were fairly formalized, as there was a relatively clear program to follow. Lunchtime, on the other hand, was a time when therapists took a break from their professional roles. Therapist/patient separation took place, and patients were free to dine where they wished, together or alone. In the early days, I spent my lunch hours with the therapists. Although I was not totally at ease, I felt that I had to be able to obtain some less formal information, and perhaps a glimpse of the therapists' subjectivity when they did not have to maintain a professional stance in front of their patients. Following a therapist's advice that I should go to lunch with the patients, as he believed that this was when they talked about their experiences with the third-party payer responsible for their compensation, I started going to the cafeteria. Nonetheless, I could see that the familiarity I was developing with the research participants over these lunches was creating discomfort among the therapists. They felt that our time together was having an effect on the workers' perception of their own situations. The fact that I was getting too close to the participants made the therapists suspicious that I was no longer neutral, that I was "taking side" [67]. This was particularly problematic, as our agreement with the clinic was to study the therapeutic alliance, which was a dual concept, including both patients' and therapists' perceptions. In this context, it was particularly important not to raise therapists' suspicions of me being biased in favour of the patients, which could expose them to criticism. The access to the field depended on it. I therefore had to readjust my level of familiarity, by distancing myself from the participants. I had to maintain the minimum level of contact necessary for data collection, but I could not become their friend.

Secondly, I had to consider the level of discretion a researcher needs to maintain so as not to affect the therapists' work. Private meetings between patients and therapists seemed a good time to think about this. In the early stages of the research, I followed the patients into their meetings. This did not appear to be a problem, especially at the beginning of the rehabilitation program, when interventions addressed single issues, such as how the program functioned, the work accident, and the symptoms of the injury. However, as the program for immigrant patients progressed, the issues addressed in private meetings

became increasingly delicate, especially when it came to discussing the return to work, which was often associated with a resurgence of symptoms, and sometimes led to a breakdown in the therapeutic alliance. Nevertheless, I continued to attend the meetings out of habit, until one particular encounter between a rather shy and secretive participant and the occupational therapist. The occupational therapist's very personal approach and discomfort made it clear that my presence was having an effect on the relationship of trust that they were trying to build. As this particular encounter had made us (both the research and clinical teams) aware of the undesirable effects of the researcher's presence during such an encounter, it became essential to adjust. Even if consent had been granted when participants were recruited for the research, at the beginning of the rehabilitation program, it seemed necessary to go beyond that first consent, and to practice a "continuous, situated and relational approach to informed consent" [68]. With the wellbeing of the participants in mind, it would not have been fair to only consider their first consent, as it would have been impossible for them, as it was for me, to imagine everything that the research was going to implicate. Therefore, as we became aware of this new facet of my implication, consent had to be reaffirmed by participants, which was not through the therapists. In order to continue pursuing the purpose of the fieldwork, i.e., access to the field, it was decided that the researchers would no longer attend private meetings, but would receive a debriefing from the therapist. Nevertheless, one of the consequences of our realization of the effect of the researcher's presence was that it set in motion a process of schismogenesis: the therapists became less and less tolerant of the researcher's presence, gradually reducing it, until its purpose—access to the field—could no longer be achieved.

In short, as a researcher, I maintained relationships with my clinical environment, mainly with the therapists and the participating immigrant workers. To continue pursuing my goal of building relationships in order to obtain information and carry out my research, my role evolved in relation to the aforementioned components. As the system observed was one of interpersonal communication, where the question of interpretation is central, communication breakdowns or cultural clashes sometimes occurred. Therefore, in the following paragraphs, I have chosen to use the systemic approach—in particular, the concepts of recurrence, the feedback loop, and the transducer—to examine three difficult situations that occurred during this research fieldwork, ultimately to make sense of them and, above all, to show that these difficulties were far from being the direct result of my own actions and words.

6. Communication Breakdown

6.1. Situation 1: Hypothesis Based on the Principle of Recurrence

To ensure that my role as a non-healthcare professional within the clinic was clear, and to maintain an appearance of neutrality with those involved in the field, I presented myself to the clinic's patients participating in my research as a student researcher with no connection to the clinical team or to the third-party payer at the heart of the compensation and rehabilitation system. I repeatedly mentioned that I was not trained in rehabilitation, and had neither the skills nor the qualifications to make a clinical judgment. I therefore adhered to the principle of never intervening or expressing an opinion about their rehabilitation programs or their injuries. In fact, I wore normal sports clothes, while the clinical team wore uniforms. In addition, when we took part in workshops, I always stood at the patient's side, and participated in a way that did not make me look like a facilitator. Regardless, there were a few occasions when participants in my study came to me for advice on their rehabilitation plan, such as how to do a difficult exercise. Of course, I reiterated that I was not in a position to answer, but I still wondered why they thought, for a moment, that I could.

Clearly, my behaviors with the immigrant participants were not sufficiently adapted to convey to them my desired role as a non-healthcare professional. Although Faizang [46] emphasized the importance of avoiding taking on a professional role, she gave no further indication of how to proceed. I therefore began to reflect on the signals I was sending to the participants that made some of them think I was part of the clinical team, despite my

efforts not to be seen as such. Looking back, I realized that, on a daily basis, I was picking up forms from behind the reception desk, which was reserved for the clinic's employees: no patients were ever seen on that side of the desk. I was also doing this at peak times when patients were arriving at the clinic, and the therapists—also behind the desk—were greeting them. This may be one of several possible explanations, but it seems relevant.

The systemic concept of “recurrence”, which identifies a pattern of behavior over time, may be useful in explaining this phenomenon. For example, in the clinic setting, patients may recognize regularities in therapists' behaviors or attitudes (wearing a uniform, giving advice, not working out), and expect these same behaviors in the future. Similarly, there are some regularities in patients' behaviors.

Thus, the fact that (1) at the start of the program, I introduced myself to the immigrant patients as unqualified in rehabilitation, and not as a member of the clinical team, (2) that I took part in few workshops as a participant, and (3) that I refrained from certain behaviors (e.g., giving an opinion) did not appear sufficient to create a recurring pattern of non-professional behavior. My daily presence behind or beside the front desk, and alongside the clinic's team members, sent out a contradictory signal. Conceivably, particularly because of our respective schedules, some participants might have seen me more often behind the front desk than they saw my other non-professional gestures (contrary to other participants, who did not see me behind the desk, because they arrived later, when I was already in the gym). It was this recurring behavior that served as a model for the former, which could also explain why they then associated me with a therapist. Thus, I had to find another place for the forms, to avoid sending a contradictory signal about the role I wanted to assume in the clinic.

6.2. Situation 2: Hypothesis Based on the Feedback Loop Principle

While my role in the field was to gather information from the study participants, I felt I had to act in a way that would make them want to share information. To do so, I tried to make them feel valued, important, and respected, while maintaining a constant aura of receptiveness and openness, in order to make them want to share what they were thinking and feeling. On one fieldwork day, I walked up to a participant at the end of the program to say hello and get an update on his situation. This was a worker I did not see very often, as he was more likely to be present during my colleague's field days. Therefore, without insisting, I sat down with him for a few minutes and asked him a few routine questions that were not intrusive, but left room for him to express himself if he wanted (e.g., how are you? How is your program going?). Nevertheless, at the end of the day, the therapists informed me that the participant was thinking of withdrawing from the study.

The outcome of this situation made me realize that I had overlooked something. Côté [43] notes the importance of exercising intelligence and dexterity in social relations in the field, but gives little indication of how to go about it. From my point of view, my actions that day with the participant had been adapted: I had spent a few minutes demonstrating my interest and openness, but without pressure, in order to remain respectful of his boundaries, and avoid being too intrusive. It was during discussions with my colleague, who worked with him every week, that I learned that he was going through a very difficult and anxious period in his rehabilitation. At the time, the presence of an additional player (me), virtually unknown to him as he was used to being around my colleague, was adding additional stress that he could not manage.

The concept of the feedback loop can help us to better understand one of the factors at play in this type of delicate situation: information. By observing the actions, attitudes, and words of therapists and immigrant patients in the field, I was able to adapt my behavior. However, I was not present at the clinic every day, as my colleague went there one or two days a week. This missing information, therefore, acted as a break in my feedback loop.

As there were three ethnologists in the field at different times, not all the information sent by the environment was received by the ethnologist on site, which meant that they were unable to adapt their role accordingly. For example, under normal circumstances, when a worker showed signs of anxiety due to my presence (even if unequivocally), I would

have a certain instinct about the situation, and move away from the patient momentarily, to better analyze the situation, and act accordingly. However, in this case, it was my colleague who was aware of the unequivocal signs of anxiety, and that information did not reach me. Thus, I had no reflex to distance myself from the patient, and my questions must have prompted the patient, already on edge, to consider withdrawing from the study. We therefore had to set up and reinforce a more effective communication system between members of the research team, to ensure a better feedback loop.

6.3. Situation 3: Hypothesis Based on the Transducer Principle

I had already been in the field for a few weeks. Every week, I attended the interdisciplinary team meeting, where information and professional opinions were shared regarding the clinic's various patients. Sometimes, the attendees would ask me for information, as I had different access to the patients, and my input could help complete the patient's profile. At the time, I always refrained from responding to this request, mainly because the information I had would have added nothing new to what was already being shared around the table. As the weeks progressed, my relationships with some of the participants deepened. At one team meeting, I finally took the liberty of expressing my thoughts on a complex case that was of particular concern to me. Above all, I wanted to readjust the profile (of the patient on the personal level, and not on a clinical or diagnostic level) that the clinical team was painting of the patient, a profile that seemed to me to be not entirely accurate, and even discriminatory. At that precise moment, I felt I had an ethical duty, as a humanist, to share my information, so that the patient could receive an intervention plan that was appropriate to his real and complete situation. In reality, I was probably overstepping my role as an ethnologist. Following my intervention, the clinic manager informed me that interventions from me were no longer welcome at multidisciplinary meetings.

In this instance, it appears that I had misread the norms and codes of the milieu. Similarly to Lapointe [14], I had wanted to fit in with the local players, and had felt it important to find some use for myself within the clinic. However, some contexts are evidently less suited to the author's self-concocted "all-round caregiver" role. Presumably, it is more difficult to integrate into small teams where everyone's roles and tasks are well distributed and assimilated, especially when we have no caregiving experience. Nevertheless, this desire to do something useful probably played a part in my positive response to some therapists' requests for information. What I did not realize, however, was that some therapists' requests for my reflections did not give me the organizational legitimacy to share them, even though I had not ventured into the therapeutic and diagnostic realms, which are governed by ethical standards. I thought that this legitimacy came from my level of knowledge of the files, and it seemed to me that, at this point in the program, I had a good understanding of the case and the issues at stake for the worker in question. However, I had not fully grasped the codes that governed the status and roles of each person within the clinic in this specific context of interdisciplinary encounters. Furthermore, my relationship with the clinical team may not have allowed me to confront them with their biases. I must point out that the more time I spent in the field, the greater my comfort level with the clinical team was. I almost felt at home, even though I had only been in the field for a few weeks. I felt like one of them, which made me forget our different statuses. I shared a lot in common with the team members, due to our common backgrounds, such as language, accent, expressions, and cultural references. Being in the field on home ground gave me a feeling of familiarity that interfered with my reading of the context; I had thereby diminished the importance of status, and accentuated the importance of knowledge of the files and the patients' wellbeing.

The concept of the transducer can help us to understand this situation. It is the transducer that captures the difference and transforms it into a stimulus, which, in turn, tells the system that something is happening. A difference must not only exist, but also trigger a volley of stimuli: the difference must make a difference. I therefore believe that when ethnologists go into the field in a foreign environment, the number of differences that

make a difference (creating stimuli) is so high that they come to experience “culture shock”. The difficulty in a home fieldwork setting lies in the fact that there are fewer differences. Hence, my transducer was not as alert when, in fact, I was a stranger in my own home [69]. I should have been particularly vigilant, and not trusted the level of comfort I quickly felt in the clinic. I did not doubt my knowledge of the milieu enough, and I allowed myself to offer my thoughts despite my non-professional status within the clinic. Everything was so familiar to me—the accent, the language, the way of doing things, the city I was in—that I allowed my transducer to work less hard, meaning that I was not totally aware of the differences in clinical culture, and did not adapt accordingly.

7. Conclusions

The systemic approach provided me with an invaluable tool for representing, in a comprehensible way, the various imperatives I faced during the research fieldwork. Although there is no recipe for fieldwork, as it is highly contextual, the systems approach is a tool that enables us to reflect on the interactions at play and to structure our thinking in such a way as to make it comprehensible and, above all, conscious. It should, thus, not only enable ethnographers to become aware of the different codes and norms at play in the field, but also make them explicit, so that they are able to give a personal detailed account of their experience. Systemics could help us to model the paths and journeys of understanding.

The systemic approach is also an important tool for the social sciences, as it provides a deeper understanding of certain phenomena and dynamics. In this paper, it could have led me to other equally interesting analyses, in order to answer questions such as the following: what are the codes, norms and customs in force in the system? Which actors are involved? Can they be involved in different systems? Are these systems interconnected, and how? Do they pursue different, divergent goals? Do these differing purposes ultimately render the overall system dysfunctional [52]? For example, I could have analyzed the situation where giving feedback to the multidisciplinary team was poorly received in terms of the encounter of several systems: the clinical system, which perhaps reacts negatively when confronted with its own limitations; the researcher’s system, which must avoid displeasing its informants, so as to retain access to the field; but also the human system, which finds itself faced with an ethical dilemma, where a lack of information on the part of the therapist could lead to certain discrimination toward the patient. I could also have delved deeper into the process of schismogenesis that led me, the researcher, to withdraw from the fieldwork. Which system is responsible? That of the clinic studied, or of the clinical environment in general? Or is it the system of therapy used with vulnerable patients?

In this paper, I have used the systemic approach to explain my personal experience as a field researcher. This has enabled me not only to explain scientifically what happened, but also to evolve professionally, personally, and emotionally.

At a professional level, using systemic triangulation, I was able to reflect on, and detail, the interactions I needed to maintain in order to achieve my goal as a researcher. These reflections, in turn, enabled me to transform certain difficult situations into long-lasting learning. Thus, the personal and team reflections presented in this article taught me, not just in the context of this specific fieldwork, but more generally, about the reality of the ethnologist. I learned, first and foremost, about methodology, as well as about certain dynamics that might be more present in milieus subject to organizational imperatives (such as fixed schedules or work overloads) or professional imperatives (professional order, ethics, etc.).

In terms of methodological learning, I firstly realized that it was important to return frequently to research objectives and observation grids, especially when the outcomes of a research project have been identified in advance with various stakeholders by mutual agreement. As memory is a faculty that forgets and changes according to the curiosities, sensitivities, and understandings we have of reality, objectives and observation grids serve as compasses for researchers. Secondly, as “home ground” does not offer sufficient stimuli to remind researchers that they are in unknown territory, it may be useful to create what I call a “transitional routine”. This consists of a routine that reminds researchers that they

are entering this unknown territory, and they need to be vigilant. Thirdly, when working with several people, the limitations must be recognized, and a functional method must be spelled out. In my opinion, it is preferable to be modest: it is better not to take any risks, even if the information obtained may be less rich, than to lose a bond of trust. In addition, a minimum of rigor and discipline is required when sharing information, to ensure that it is shared frequently, and does not embarrass a researcher [70].

From a personal point of view, as it is the actors' interpretations in an interpersonal communication system that make it complex, I acknowledge that my presuppositions, sensitivities, and personality had notable effects on my perceptions of the environment in which I found myself during the research fieldwork. My sensitivities as a human being and as a researcher sometimes led me in interesting directions, but away from the research objectives that were agreed with the clinical staff. Indeed, my interest in more vulnerable populations may have altered my observation of the therapeutic alliance as a dyadic concept (immigrant patients and therapists), as well as my ability to maintain an appearance of neutrality. They probably also influenced my interpretations of the situations I observed and, by the same token, the way I responded and adapted to these interpretations. In addition, my extroverted personality, and my ability to speak up in a group, certainly had an impact on my relationship with the therapists. Given this aspect of my personality, I readily accepted the invitation to give my opinion, and consequently learned that I will have to be particularly vigilant in the future about the codes and norms that govern the environments in which I will be doing research, especially as regards the ethnologist's right to speak.

The systemic approach also enabled me to put into perspective my role as an ethnologist during my research fieldwork in a rehabilitation clinic, and to understand certain particularities of the milieu. At a time when I felt "rejected" by the field, this perspective was invaluable in helping me to understand that it was not for lack of professionalism, competence, humility, or curiosity that I had experienced the difficulties I reported. Indeed, I had tried to adapt to the imperatives of fieldwork to the best of my ability, more specifically to meet the expectations of the various players with whom I was interacting, while at the same time attempting to meet the objectives of the research. However, clearly, the role in which I found myself was confronted with a major paradox: I had to make myself 'absent', so as not to hinder the alliance between therapists and patients, while, at the same time, be present, in order to collect rich data.

Thus, from an emotional point of view, the systemic approach enabled me to make peace with my experience, and to no longer feel as if I had not risen to the occasion. By explicitly identifying the issues encountered in the field, I was able to depersonalize the experience, notably by distinguishing between the researcher's personal and professional identities [71]. I even concluded that the problems encountered were not due to a system malfunction, but rather to the vital reflexes of any system to eject foreign bodies in order to ensure its survival or, at least, the continuity and regularity of its use and habits. This ethnographic research in a clinical setting required me to insert myself into an already well-established system: the therapeutic relationship. Like any other system, this already-functioning one had to fend off the intruder that might have led to its demise. The clinical system, which seeks to heal, requires a specific type of interaction between therapists and patients, where the latter often find themselves in a subordinate position, so that the reality of living with chronic pain can be accepted. On the other hand, the fieldwork system calls for an almost inverted type of interaction, where researchers find themselves subordinate to the patients, in order to gain access to their subjectivity. This type of interaction can even undermine the clinical system. Thus, all my goodwill would not have prevented me from encountering resistance to my presence on the part of the actors in the field. This is particularly true in organizational settings, where workloads and budgetary constraints make themselves felt; in clinical settings, where ethical and professional codes are tight and monitored; and in rehabilitation programs, where the difficulties of working with patients living with chronic pain are numerous and significant.

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References

- Demazière, D.; Horn, F.; Zune, M. Ethnographie de terrain et relation d'enquête. Observer les «communautés» de logiciels libres. *Sociologie* **2011**, *2*, 165–183. [CrossRef]
- Gadamer, H.G. *Vérité et Méthode: Les Grandes Lignes d'une Herméneutique Philosophique*; Seuil: Paris, France, 1976.
- Sarradon-Eck, A. Médecin et anthropologue, médecin contre anthropologue: Dilemmes éthiques pour ethnographes en situation clinique. *ethnographiques.org*. 2008. Available online: <https://www.ethnographiques.org/2008/Sarradon-Eck> (accessed on 15 May 2023).
- Morrisette, J.; Demazière, D.; Pepin, M. Vigilance ethnographique et réflexivité méthodologique. *Rech. Qual.* **2014**, *33*, 9–18. [CrossRef]
- Fassin, D.; Bensa, A. *Les politiques de l'enquête. Epreuves Ethnographiques*; La Découverte: Paris, France, 2008.
- White, B.W.; Yoka, L.M. Musique populaire et société à Kinshasa. Une ethnographie de l'écoute. L'Harmattan. In *Mémoires Lieux de Savoir*; Archive Congolaise: Paris, France, 2010.
- Favret-Saada, J. Les mots, la mort, les sorts: La sorcellerie dans le Bocage. In *La Nouvelle Revue Française*; Gallimard: Paris, France, 1977.
- White, B.W. Jamais deux sans trois: La situation ethnographique et le rapport à l'autre. In *Anthropologies Réflexives: Modes de Connaissance et Formes D'expérience*; Blondet, M., Mallet, M.L., Eds.; Presses universitaires de Lyo: Lyon, France, 2017; pp. 255–272.
- Linton, R. *The Study of Man: An Introduction*; Appleton-Century: Oxford, UK, 1936.
- Borofsky, R. The four subfields: Anthropologists as mythmakers. *Am. Anthropol.* **2002**, *104*, 463–480. [CrossRef]
- Caratini, S. Les non-dits de l'anthropologie. In *Libelles*; PUF: Paris, France, 2004.
- Agar, M. The Professional Stranger: An Informal Introduction to Ethnography. In *Studies in Anthropology*; Academic Press: New York, NY, USA; San Francisco, CA, USA, 1980.
- Johnson, N.B. Sex, Color, and Rites of Passage In Ethnographic Research. *Hum. Organ.* **1984**, *43*, 108–120. [CrossRef]
- Lapointe, M.-È. Le processus d'observation ou de la nécessité d'être «pris (e) au jeu». *Rech. Qual.* **2014**, *33*, 172–187. [CrossRef]
- Donnadieu, G.; Karsky, M. La systémique, penser et agir dans la complexité. In *Entreprise & Carrières*; Liaisons: Paris, France, 2002.
- Mauss, M. *Essai sur le Don: Forme et Raison de l'Echange Dans les Sociétés Archaïques*; PUF: Paris, France, 1925.
- Malinowski, B. Les argonautes du Pacifique occidental. *Rev. Française Sociol.* **1963**, *4*, 224.
- Clifford, J. On ethnographic authority. *Representations* **1983**, *2*, 118–146. [CrossRef]
- Clifford, J.; Marcus, G.E. *Writing Culture: The Poetics and Politics of Ethnography*; University of California Press: Berkeley, CA, USA, 1986.
- James, A.; Hockey, J.L.; Dawson, A.H. After Writing Culture: Epistemology and Praxis in Contemporary Anthropology. In *ASA Monographs*; 34; 1 Ressource en Ligne (ix, 273 Pages): Illustrations Vols; Routledge: New York, NY, USA, 1997; Available online: <http://www.myilibrary.com?id=6768> (accessed on 10 June 2023).
- White, B.W. From Experimental Moment to Legacy Moment: Collaboration and the Crisis of Representation. *Collab. Anthropol.* **2012**, *5*, 65–97. [CrossRef]
- Kirsch, T.G. From the Spirit's Point of View: Ethnography, Total Truth and Speakership. *Beyond Writ. Cult. Curr. Intersect. Epistemol. Represent. Pract.* **2010**, *12*, 89–112.
- Crapanzano, V. Textualization, Mystification and the Power of the Frame. In *Beyond Writing Culture. Current Intersections of Epistemologies and Representational Practices*; Berghahn Books: Brooklyn, NY, USA, 2010; pp. 39–56.
- Soukup, C. The postmodern ethnographic flaneur and the study of hyper-mediated everyday life. *J. Contemp. Ethnogr.* **2013**, *42*, 226–254. [CrossRef]
- Adler, P.A.; Adler, P. *Membership Roles in Field Research*; Sage: Newbury Park, CA, USA, 1987; Volume 6.
- Hopwood, N. Researcher roles in a school-based ethnography. In *Methodological Developments in Ethnography*; Emerald Group Publishing Limited: Bingley, UK, 2007; pp. 51–68.
- Silverman, R.M. Analysing qualitative data. In *The Routledge Handbook of Planning Research Methods*; Silva, E., Healey, P., Harris, N., Van den Broeck, P., Eds.; Routledge: New York, NY, USA, 2011.
- Côté, D.; Dubé, J.; Arsénault, M. L'approche ethnographique. In *Méthodes Qualitatives, Quantitatives et Mixtes, 2e Édition: Dans la Recherche en Sciences Humaines, Sociales et de la Santé*; Corbière, M., Larivière, N., Eds.; Presses de l'Université du Québec: Montréal, QC, Canada, 2020; pp. 97–126.
- Schwartz, M.S.; Schwartz, C.G. Problems in participant observation. *Am. J. Sociol.* **1955**, *60*, 343–353. [CrossRef]

30. Schwartz, H.; Jacobs, J. *Qualitative Sociology*; Simon and Schuster: New York, NY, USA, 1979.
31. Gold, R.L. Roles in sociological field observations. *Soc. Forces* **1958**, *36*, 217–223. [[CrossRef](#)]
32. Burgess, R.G. *In the Field: An Introduction to Field Research*; Routledge: London, UK, 2002.
33. Roy, D. The study of Southern labor union organizing campaigns. In *Pathways to Data*; Habenstein, R.W., Ed.; Routledge: New York, NY, USA, 1970; pp. 216–244.
34. Genest, S.; Gouin-Bonenfant, M.; White, B. *Choc Culturel*; Anthrope: Abitibi, QC, Canada, 2021.
35. Irwin, R. Culture shock: Negotiating feelings in the field. In *Anthropology Matters*; University of Oxford: Oxford, UK, 2007; Volume 9.
36. Oberg, K. Cultural Shock: Adjustment to New Cultural Environments 1. *Curare* **2006**, *29*, 3. [[CrossRef](#)]
37. Becker, H.S. Réflexions sur le travail de terrain et ses problèmes. *Rech. Qual.* **2021**, *33*, 1–8. [[CrossRef](#)]
38. Finkler, K.; Hunter, C.; Iedema, R. What Is Going on?: Ethnography in Hospital Spaces. *J. Contemp. Ethnogr.* **2008**, *37*, 246–250. [[CrossRef](#)]
39. Van der Geest, S.; Finkler, K. Hospital ethnography: Introduction. *Soc. Sci. Med.* **2004**, *59*, 1995–2001. [[CrossRef](#)]
40. Murphy, L.D. Murphy Out of Place: Ethnographic Anxiety and Its ‘Telling’ Consequences. *Anthropol. Action* **2006**, *13*, 78–86. [[CrossRef](#)]
41. Aberese-Ako, M. ‘I won’t take part!’: Exploring the multiple identities of the ethnographer in two Ghanaian hospitals. *Ethnography* **2017**, *18*, 300–321. [[CrossRef](#)]
42. Capurro, G. Witnessing the Ward: On the Emotional Labor of Doing Hospital Ethnography. *Int. J. Qual. Methods* **2021**, *20*, 1609406921998919. [[CrossRef](#)]
43. Côté, A. L’ethnologue au pays des cliniciens. In *Revue Internationale de Psychosociologie*; ESKA: Paris, France, 2013; pp. 197–207.
44. Wind, G. Negotiated interactive observation: Doing fieldwork in hospital settings. *Anthropol. Med.* **2008**, *15*, 79–89. [[CrossRef](#)] [[PubMed](#)]
45. Mulemi, B.A. On being ‘native’ and ‘outsider’ in hospital ethnography. *Vienn. Ethnomed. Newsl.* **2010**, *12*, 6–14.
46. Fainzang, S. La relation médecins-malades: Information et mensonge. In *Coll. Ethnologies*; PUF: Paris, France, 2006.
47. Fainzang, S. Champ-contrechamp: La relation médecin-malade entre anciennes et nouvelles normes. *Anthropol. Soc.* **2013**, *37*, 83–97. [[CrossRef](#)]
48. Vermeulen, C.J.J. Anthropology and politics: The logic of their interrelationship. *Dialect. Anthropol.* **1977**, *2*, 235. [[CrossRef](#)]
49. Horowitz, I.L. *Professing Sociology: Studies in the Life Cycle of Social Science*; Aldine: Chicago, IL, USA, 1968.
50. Chartrand, L. Theoretically Informed Hospital Ethnography: Reflecting on Method. *Int. J. Qual. Methods* **2022**, *21*, 16094069221142996. [[CrossRef](#)]
51. Bateson, G. *Steps to an Ecology of Mind: Collected Essays in Anthropology, Psychiatry, Evolution, and Epistemology*; University of Chicago Press: Chicago, IL, USA, 2000.
52. Côté, D.; White, B.; Dubé, J.; Gravel, S. Are Healthcare Systems Failing Immigrants? Transnational Migration and Social Exclusion in the Workers’ Compensation Process in Québec. *Int. Migr. Rev.* **2023**, *0*, 1–26. [[CrossRef](#)]
53. Genest, S. Constructivisme en études ethniques au Québec: Retour à la notion de frontières de Barth. *Anthropol. Sociétés* **2017**, *41*, 59–85. [[CrossRef](#)]
54. Genest, S. Le « changement d’état d’esprit »: Une étude à partir de la pensée systémique de Gregory Bateson. Ph.D. Thesis, Université de Montréal, Montréal, QC, Canada, 2022.
55. de Rosnay, J. Symbionomic evolution: From complexity and systems theory, to chaos theory and coevolution. *World Futures* **2011**, *67*, 304–315. [[CrossRef](#)]
56. Le Moigne, J.-L. *La Théorie du Système Général: Théorie de la Modélisation*; FeniXX: Paris, France, 1994.
57. Bateson, G. *Mind and Nature: A Necessary Unity*; E.P. Dutton: New York, NY, USA, 1979.
58. Bioy, A.; Bachelart, M. L’alliance thérapeutique: Historique, recherches et perspectives cliniques. *Perspect. Psy* **2010**, *49*, 317–326. [[CrossRef](#)]
59. Corbière, M.; Bisson, J.; Lauzon, S.; Ricard, N. Factorial validation of a French short-form of the Working Alliance Inventory. *Int. J. Methods Psychiatr. Res.* **2006**, *15*, 36–45. [[CrossRef](#)] [[PubMed](#)]
60. Brewer, J. *Ethnography*; Open University Press: London, UK; Philadelphia, PA, USA, 2000.
61. Bryman, A. *Quantity and Quality in Social Research*; Contemporary Social Research Series. 18; Unwin Hyman: London, UK, 1988.
62. Silverman, D. *Interpreting Qualitative Data*; 5E; SAGE: London, UK, 2014.
63. Ferreira, P.H.; Ferreira, M.L.; Maher, C.G.; Refshauge, K.M.; Latimer, J.; Adams, R.D. The therapeutic alliance between clinicians and patients predicts outcome in chronic low back pain. *Phys. Ther.* **2013**, *93*, 470–478. [[CrossRef](#)]
64. O’Keefe, M.; Cullinane, P.; Hurley, J.; Leahy, L.; Bunzli, S.; O’Sullivan, P.B.; O’Sullivan, K. What influences patient-therapist interactions in musculoskeletal physical therapy? Qualitative systematic review and meta-synthesis. *Phys. Ther.* **2016**, *96*, 609–622. [[CrossRef](#)]
65. Côté, D.; Gravel, S.; Dubé, J. Bibliothèque Numérique Canadienne (Firme). Relations Interculturelles Comprendre le Processus de Réadaptation et de Retour au Travail. 2017. Available online: <https://www.deslibris.ca/ID/10090955> (accessed on 30 July 2020).
66. Fortin, S.; Knotova, M. Présentation: Îles, Continents et Hétérotopies: Les Multiples Trajectoires de L’ethnographie Hospitalière. *Anthropol. Sociétés* **2013**, *37*, 9–24. [[CrossRef](#)]
67. Becker, H.S. Whose Side are we on? *Soc. Probl.* **1967**, *14*, 239–247. [[CrossRef](#)]

68. Klykken, F.H. Implementing continuous consent in qualitative research. *Qual. Res.* **2022**, *22*, 795–810. [[CrossRef](#)]
69. Caputo, V. At 'home' and 'away': Reconfiguring the field for late twentieth-century anthropology. In *Constructing the Field*; Routledge: New York, NY, USA, 2003; pp. 27–39.
70. De Sardan, J.-P.O. La rigueur du qualitatif: Les contraintes empiriques de l'interprétation socio-anthropologique, no. 3. In *Anthropologie Prospective*; Academia-Bruylant: Louvain-la-Neuve, Belgium, 2008.
71. White, B.W.; Grégoire, A.; Gouin-Bonenfant, M. The Intercultural Situations Workshop: Indirect Ethnography and the Paradox of Difference. *J. Intercult. Stud.* **2022**, *43*, 283–301. [[CrossRef](#)]

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