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Authority, subjectivity and power: data devolution at a primary care unit in Guariroba (Ceilândia/Federal District/Brazil)

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Abstract *Devolution, restitution or sharing can mean, within other possibilities, to offer products to participants of a research or an extension project. Far from a new practice in Anthropology, returning results is still unusual, little organized and valued. This paper presents and discusses a devolution experience by an extension project in Anthropology that was developed in a primary care unit in the outskirts of Distrito Federal (Brazil). Local reactions were very different from what was expected by the project's staff, but still permitted dialogue with the health professionals and, more important, deepened our knowledge about work relations in this health institution. Even though IRB approval has been granted, negotiations about starting and continuing academic projects have to be negotiated continuously. Subjectivity, power and authority permeate any anthropological initiative from its beginning and much after it supposedly has been concluded.*

Key words *Anthropology, Devolution, Extension, Authority, Ceilândia*

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Introduction

Since 2008, we have conducted research in medical anthropology in Guariroba, a historic and working class neighborhood in Ceilândia, the largest and most populated city in the Federal District [Brasília]. At times, we conducted research in homes, on the streets and at neighborhood stores; at times we were at educational and health institutions that were established in the early 1960s. When I say "we", I am referring to the different and successive teams of undergraduate students that I have coordinated in research and extension projects in the region. These initiatives were converted into professional experiences, in senior theses, as well as in articles about long-term illnesses and aging. In early 2011, having observed our presence, a director of one of the 12 public health centers in Ceilândia invited us for a chat. She presented us with a demand to "better know and help reverse" the situation of employees' sick leave. Since we began our research there in 2008, we heard many reports from the nearly 70 employees of the institution regarding the high frequency of sick leaves and excused absences due to "health problems", the use of sedatives, alcoholism, and attempted suicides. The intense and precarious work routine (lack of material, low wages, deficient and outdated infrastructure), contact with patient suffering, and personal conflicts with colleagues and also with patients were among the main explanations for what they called work-related sickness.

As a result of this demand [from the public health center director], we developed an extension project that involved Anthropology students and local health center employees. Based on conversations and meetings with the employees, we designed the project where the first stage involved individual interviews with those employees and the second and last part, suggestions based on the analysis of the collected material could be transformed into activities to be tested and, if successful, incorporated by the respective health center team. Unfortunately, after the report regarding the first part was presented, when the suggested activities were outlined, the team retracted and did not implement the second phase of the project. Nevertheless, as the limitations of this extension project were discussed elsewhere[1], in this present article, the goal will be to take a short and unexpected stage of the project in order to reflect on the practice of devolution of the results of this research effort.

In the first phase of the project, 29 interviews were conducted among employees of this public health center. The schedule of questions was elaborated and the conversations were recorded in audio, transcribed and read by the research team. This reading involved highlighting especially significant segments and organizing them into "meaning cores"[2] that were discussed little-by-little in weekly meetings of the research team to enhance the understanding of work-related sickness. Finally, each of the interviews was printed, received a cover sheet with general information about the project, and returned to the participants. The idea was to reward them with the material generated by the project. In addition, sought to register the individual and institutional memory, the printed interview would be a way for the 29 interviewees to have, on the one hand, a registry of their participation in the project, and, on the other hand, material reflecting on their place of work. At the time, I considered that an oral form, so subject to dispersion and forgetfulness, might be perpetuated in some way in written and printed language. Furthermore, in this being an extension activity, each phase should have been discussed with the health center employees in order to create dialogue and accumulation that could have reverted into new data and advances in the project. I did not imagine that the return of the individual interviews would open a possibility of such intense dialogue. In fact, these unexpected repercussions were an opportunity to think about anthropology, anthropologists and services of primary health attention. Next, I present the main reactions to the return of the interviews to, in the end, discuss the potential results of this experience for the practice of anthropology. Not only because we obtained approval of an *Institutional Review Board* (*IRB*) and the local authorities to conduct this research that the negotiations regarding our entrance and continuity are continuously guaranteed[3]. Relations of power and authorship permeate any anthropological research project, before initiating and even more after, supposedly, terminating the project. Here I present, therefore, a very common and fundamental discussion in Anthropology that could be useful in other areas as well, such as health, known for its extension projects, rituals of sharing results and, also, hierarchical relations. In general, I follow the general orientation that "anthropological and qualitative research should stimulate the understanding of the processes and not only of the results"[3].

The ritual of returning the interviews

The return, restitution or sharing, as we are reminded by Rial in a recent article, might mean to deliver products and material to the participants research/extension project and/or disclose its results to wider publics[4]. Regarding the current case, I will dwell on the first format. The return of results is not a new practice in anthropology, although it is still unusual, unsystematic and undervalued[4]. Even so, I observe different strategies used in the field to communicate what anthropologists are thinking and writing about their interactions. Visual anthropology experiences[4], shared research results[5], exchange of texts[6], or the traditional offering of published products[7] are examples in this regard. The debate about the return of data is motivated by different aspects, like the criticism of colonialist and objectivist models at the beginning of the discipline, the compromise and established accords between research participants, the continuous revision of our professional practice, the possibility of the applied translation of knowledge that we facilitate, etc. I suggest that to anticipate, execute and reflect about the later phases of a project are actions that should be integrated into anthropological reflections and, in the scenario that I discuss here, it becomes even clearer to me that "later" is a relative and questionable qualifier. For us, "devolution" [return] to persons who hardly imagine when our ethnographic proposals begin or end might be just one step in the dialogue.

During the Guariroba extension project, the 29 persons received their interviews in individual envelopes from the researcher who conducted the interview, avoiding access to the interview by workmates so as not to violate privacy. We explained that by maintaining secrecy (with the use of pseudonyms and descontextualization), the underlined parts of the interview were central to us, and for this reason, tended to support our analyses. Several persons just thanked us for the interview. Others, when they ran into us in the hallways, said they liked reading the interview. Some of them criticized specific parts. Finally, some persons expressly asked that their interview not have public access, because they believed that even in a veiled form, they might suffer consequences in that work environment, at the State Secretariat of Health of the Federal District government (SES/GDF), or even in the neighborhood where their health center is located. It is important to emphasize that these reactions came from persons in the most peripheral areas of the health centers (considering that the supervisors and medical doctors, as well as the dentists, pharmacists and other college-level professionals occupied the most prestigious positions in these institutions). For example, one had a university degree, but was not as prestigious as a medical doctor, Another was part of the Community Health Agents Program (PACS), that has been systematically depreciated in the Federal District. Several persons were long-time employees of the SES-GDF, but only had high school education. The majority had been raised in the same Guariroba neighborhood, and had been treated in their infancy and adolescence at the same health center where they now worked. (For the effects of contrast and context of this field research project, the research team was composed of five undergraduate Anthropology students who were quite young and with higher levels of education *vis-à-vis* the majority of the employees [of the health center]; but more similar in terms of skin color – mulattoes –, residence – peripheral regions in the DF --, and family origin – fathers and mothers were migrants from Brazil's Northeast region). Apparently, the lower in the institutional hierarchy, the more these women were fearful of exposition and reprisal, but were attentive to the exactness of what was said, written and circulated. Thus, for me it was not surprising that the supervisors at the health center had said nothing about their printed interviews.

Since the beginning, these persons knew that they were talking in the context of a research/extension project, that a contract (Informed Consent Form) had been signed, that audio recorders were used, that there was list of questions, and that we were not persons affiliated with that health center – aspects that could have made them remember the atypical and artificial aspects of the interviews. These employees had observed the presence of many other researchers at their health center, because the University of Brasília and Catholic University of Brasília both consider Ceilândia to be their "territory" for research. Even more, they questioned the data that we presented to them. They were accustomed to being interviewed by researchers, but only very rarely had they had had access to the subsequent final reports, articles or published books, or the direct application regarding the offer of services at that health center. They saw the banners with research "results" that were sporadically posted on the walls of the institution. However, they had hardly reencountered a researcher or never knew what had become of that research project. I perceived

that they expected that, after the project was over, we would disappear as in previous research projects and for this reason they were surprised when we returned with the printed interviews. This was, however, an opportunity to understand the images of "research", "extension", and "project" in a primary care service.

To ask that certain parts not be made public, suggests that the correction of "errors" and disagreement with the transcribed text were the most common reactions related during the weekly meetings of our team. Inspired by Franchetto, I am also talking about "concurrent and contradictory representations of the written word"[8]. In the following section, I discuss the three types of reactions in more detail. What could have been just another thorny phase of the project, simply saved in the memory or field diary, became research data and an epistemological opportunity. The inherent methodological and ethical potential seems productive for thinking out our research practices and field interactions. The demands by these employees are entirely legitimate and point to the intrinsic and persistent problems in our research projects.

Questioning the reactions to the returned interviews

a) "Don't cite this underlined part"

Some employees came to us and pointed out some underlined parts that should not be included in our reports. The non-authorization to use these parts reinforces the dimension of authorship, but could also indicates that, after the oral statement, perhaps they no longer agreed with their idea, now in written form. Here, the power relationship established by anthropologists with a recorder and printer becomes clear; faced with the materialization of the transcribed interview, it becomes much more difficult for the interviewees to take back what was said, to deny or change their opinions. To print the text is to make these ideas become more permanent. Thus, believing in the trustworthiness of the transcription, the interviewee was not able to deny what she said during the interview, and thus could only negotiate the stage of public use of these ideas. Asking that the latter not become public, they were removing their consent and, in our reports, these non-authorized parts were not included.

I also began to understand that the option to underline certain parts could have caused a climate of suspicion, because this emphasized exactly phrases that centralized opinions and, over

all, dilemmas regarding work at the respective health center. By underlining, it became evident for these interviewees that we – within the parameters of Social Science – had chosen those parts as important in their discourse, but did not correspond to what they judged as priority – within the practice of their work as health professionals. The returned and underlined text communicated the sensation of estrangement about our presence and about the constant observations inside the health center. They seemed to suggest that the interview situation could occur in a intimate and even cathartic atmosphere that tends to vanish in a printed document.

Their reaction made me think that our practices establish an ambiguous game. If we had returned the interviews without any markups, an exchange of impressions free of initial contrition on the part of the researchers could have occurred. On the other hand, my intention was to explain each step of the project and present **our** *modus operandi* and thus approximate the two work practices that were beginning a dialogue – one produced in a health center and the other in a university. Only afterwards, when the project had ended and this writing had begun, I understood that returning the printed and underlined interviews might interfere in **their** *modus operandi*. By removing their consent and reviewing the relationship with us, they were showing us other important aspects, that we had not perceived. Now, little-by-little, I am trying to get to this point.

b) "I did not make these mistakes in Portuguese"

An interview navigates orally, with all the richness of spontaneity. Thinking becomes more and more linear in speech, but still lacks precision and contains replacements. Speaking is, above all, rehearsing, in a tentative and changing form. And, it is clear, that when we speak we are not totally concerned with the precision of the "normal standard"[9]. Nevertheless, a literal transcription would include both the lack of concern and imprecision [of the interviewee]. And some of the interviewees did not recognize themselves in the literal transcription of their interviews. This group hardly ever had had their voice transformed into a text and, even after we had made it clear before each interview that it would be transcribed, now I understand that this explanation was not totally understood, this was not a common practice in that environment. This group reacted *vis-à-vis* two aspects. First, they said that they did not recognize themselves as the

"authors" of the printed text, although they did recognize themselves as the authors of the ideas exchanged during the interview – proposing that we expand the idea of authorship concerning materials of a distinct nature. Second, they insinuated that this devolution scenario had a flavor of betrayal, in the end, "the transcription of the enunciated, becoming text, loses part of its sense"[10]. The researchers had "altered" the milieu of the conversation without this procedures being totally communicated by us or understood by them.

Here is a dilemma between the oral and the written. The oral form occurs every moment, via improvisation, via impulse. But with an audio recording and after the transcription of the content, the oral form is registered and becomes a text by an act of power of the researchers. Here we have an ethical and methodological disquieting about interview techniques, after all, the interview simulates a conversation, but the transcription proves the contrary[8]. The techniques subjugate the participants to the power of the researchers. Recording captures and crystallizes the oral with all the characteristic marks of this form of communication. We argued with these persons that the oral form has a specificity in relation to the printed form and, as we see it, is not something less[11]. This estrangement presented in the field made me remember how much anthropology is attached to the native form of saying things, as if, when opting for literalness, we get closer to a mythical ideal of truth, authenticity, originality[12]. But these interviewees did not accept the anthropological explanations. They explained that what is printed is an official and everlasting document. An "error" in verb conjugation, for example, that denigrates little if spoken in a meeting would, by contrast, testify against the competence of this employee if it appeared in print in an official document. These persons requested that the interview text be standardized in accordance to Portuguese grammatical norms and this was done with all the underlined parts that were cited in the final research report.

These interviewees remembered that, according to the logic of that world, when the interview appeared on paper it was transformed into a document. This group explained that these interviews were created in and would continue to exist in that health center even after our departure. A conflict, that apparently refers to the textual formality, revealed invisible feelings about rules and hierarchies of public health. Ideas, opinions, and analyses made individually in an interview, for example, would be considered very fragile if they were not in the form of memos, charts, etc. Within the state hierarchy, the individual opinion can create or accentuate interpersonal conflicts and thus generate a new process of work-related sickness. In addition, by requesting that texts be standardized, these employees alerted us regarding the possible stigmatization that they might suffer if, in the printed text, their "different" use of the Portuguese language in their oral expression became obvious[9]. As Bagno remembers, while the spoken word may have many meanings, the printed form is a caricature with the potential to classify people as "those who know" and "those who don't know". By leaving these differences apparent in the printed interview, we would help reinforce the social prestige of the local supervisors who, in general, pride themselves by their "correct" use of the language in their documents (ibid). Bit by bit, guided by the reactions of these women, I began to understand that the risks this devolution might generate were not adequately evaluated beforehand.

A solution appeared: the team could insert brackets in order to make the text more fluid and without grammatical "errors". But one of the researchers pondered that the brackets would be a visible mark of our correction of the native text. Our voice as researchers would appear to be superior, by over accentuating the "standardized norm" and by operating that correction. If on the one hand, the brackets made the author's voice clearer, avoiding interventions by the anthropologists; on the other hand, it would be explicit who, where and why the text was altered. Traditionally, for Anthropology, respect means to guarantee fidelity to the voice of others, during and after the interview. In a different way, these persons showed us that to correct the text was not mere perfectionism and consideration for the Portuguese language. Rather, the point was to adequate *vis-à-vis* the institutional structure, clearly revealing that anthropology's concern with reliability not always contemplates ethical concerns, but is simply a scientific practice and, this way, could become external to the encounter between these two women. And also, by aligning the prestige emanating from the "standardized norm", we might simply reinforce hierarchies. Respect in this context, they told us, would be not to show or emphasize inequality between the formal and the informal, between academics and non-academics, between "them" and "us". Thus, they were telling us about their world and about the abilities they needed to developed in order

to survive in that environment contaminated by power. Today, I perceive that both the literalization of the oral form and the artifice of brackets could become new modalities of violence in that field.

c) "What I said is not what is printed here. I said this another way. What is printed here is the wrong way. Who transcribed this, transcribed it wrong"

Until now the transcribed interview was seen as legitimate, both in terms of methodology as well as a printed result. In the first case, they requested that certain parts not be cited and, in the second case, that the Portuguese language be "corrected". Now, in the third request directed to us within the ritual of devolution, there was a clear contestation regarding the original veracity of the recording and/or the transcription. This demand exposed our belief in the audio and video recording equipment and also in our own capacity to listen to and understand the oral discourse and then transform it into a printed form. This also required us to take seriously the parts considered "inaudible". All the interviews contained parts that were not clear enough to be transcribed, due to equipment problems, voice tone or diction, acoustics of the environment and listening capacity of the interviewer. At times, another interviewer on the team would listen to it again. If doubts still remained, we would label this part [inaudible] and continue the transcription. Our interviewees instigated us to denaturalize our research techniques. Researchers and their equipment are prone to error, consequently the resulting texts are partial and all this can have repercussions for those we interact with in the field.

The parts of the interviews considered "wrong" were earmarked by these interviewees and, at that moment, they returned the interviews to be rectified. We produced a next printed version without "errors" and returned it to them. A give-and-take of statements and printed word, at first sight, might reveal a certain questioning or suspicion, but also might reinforce the dialogue within the project. I perceive that all these reactions presented us with opportunities to deepen our links in the field and the possibility of incorporating unexpected ethnographic discoveries, especially with regards to the discordant parts and the eventual attempts to silence them, on our part (with brackets indicating them as "inaudible") or on the part of the interviewees.

There was a similar request that did not refer to the use of the Portuguese language, but rather to the content of the transcribed interview. A nurse invited a member of the research team into her office and said, pointing to the printed version of her interview, "Here, I expressed myself badly. It was said erroneously. I am correcting myself and would like you to correct this also". On the reverse side of a prescription form she noted everything that needed to be changed. But here external responsibility was not attributed: "If it was recorded, that's what I said. I said it this way that it is", she reinforced at that time. Also it is interesting to observe what was noted down on the reverse side of the prescription form: the names of two former directors of the health center who had been mixed up during the interview. This nurse, already in her sixties and near retirement age, showed us how she believed in the recording equipment, a phenomenon previously observed regarding other equipment that investigate the human being[13]. Or at least she did not want to confront the transcription by the research team directly, after all, mixing up names is not an error that could have been made by the researchers, unfamiliar with successive health center administrations.

Here, the interviewee perceived that, faced with the transcribed interview, on one hand, she had something that she had helped prepare and, on the other hand, she had the chance to revise the text. Thus, she also perceived an opening for dialogue with the team, that had a common perception about the transitory nature of this material, and that we shared the purpose of a collective construction of the interpretation. This way, faced with this woman's request, I think of negotiations in the field in terms of successive approximations, but also of possible discomforts that might be generated by the ethnographic encounter[14]. Paying attention to both facets becomes a fundamental ethical requirement in our research activities.

Learning while doing anthropology

After this assemblage of reactions, my first impulse at the time was to question the strategy of conduct, underline and return the interviews. If instead they had only received the final report, perhaps these persons could have identified parts of their interviews, but they would have been within the contextual analysis elaborated by us, instead of perceiving evident and potentially compromising parts of individual transcriptions. But, on the other hand, they would have had access only to one type of context, created by us

for the final report, not contemplating another context, that in which their idea appeared in the interview. Later on, I questioned if the interviews were, in fact, the best methodological technique for the theme in question. Perhaps consulting the employees' files, encounter and converse with them outside the work environment, conduct observations of their work practices, for example, perhaps might have helped us observe work-related sickness with less impact and conflict.

Even with the unexpected repercussions, I perceive that returning the printed interviews made us think about invisible and unspoken aspects of doing anthropology. A good epistemological opportunity appeared since practices were seen as problematic, while, by us, they are considered very banal. In this devolution ritual, a new round of negotiations started and provoked us with unexpected ethical, political and methodological questions. Below, I discuss some of the learning that we acquired from this experience, and certainly there will be more to elaborate in the future.

First, these persons reflected on their own words and, especially on their position within the health center. Thus, the dialogue regarding the interviews can be considered a mutual pedagogical and political process of how to work at a health center and how to do anthropology. It seems that they perceived that reading and correcting the interviews was a moment of position and decision. Different from what these subordinates confronted on a daily basis in their work place, in their relation with us, they had the opportunity to communicate how they wanted to be treated and reflected in the texts, or, in a more pessimistic premise, simply reproduced the type of veto so common in that space. I had already observed several occasions when the persons felt the veto in the opposite direction, frequently having other texts as disagreements. For example, it is inadmissible that papers attached to the walls and murals in offices and corridors have mistakes. Frequently, the health center administrative employees complained about the old typewriters that still existed there, as a clear indication of the difficulty of correcting errors in documents, many times requiring that they be retyped entirely. Within a myriad of paper, it was notorious how only a few persons could find, sign and dispatch some types of documents, such as medical prescriptions, requisitions for pharmaceuticals, vacation authorizations, absence justifications, etc. Patient charts, when in a printed version, had gradual hand written additions, signed and stamped by several persons and always available for reading and – even worse – of retrospective corrections by supervisors. Thus, in addition to the research project, the interviewees told us about the centralization of power and the world of documents in their work space. Even more, they made me perceive how much our ethos – to package stories in anthropological clothing and, in this case, in printed texts – might classify narratives in versions more or less sophisticated, contributing to maintain the abyss between spoken and printed word, between who speaks and who publishes[8]. To put into practice a "graphocentric dictatorship"[8] in this ritual of devolution of transcribed and printed interviews, we were contributing to the subaltern alterity that exists in that health center.

Second, all the demands received in this health center were useful for us to review our participation and also for the data that we helped construct. We are accustomed to thinking of research materials as "ours", especially the field diaries, cautiously kept by us. I believe that, on the one hand, the employees of this health center indicated that we should not have a full decision about the materials produced, especially when they perceived that we are overly active when we read, select, underline, analyze, write about, and publish what they told us; but that we should assume more responsibility regarding the results generated, returned and made public. I suggest, however, that the ethnographical materials be of collective construction and property, including the readers who will begin a new round of dialogues with the text presented.

Third, this ritual of devolution installed a debate about authorship, inter-subjectivity and, finally, power. These women informed us how we could or not use the interviews as means for reflection. In the end, an interviewee might say that I cannot write and make public the ethnography I wrote about her? That even after granting initial access to her life and ideas, the later analysis and publication must also be negotiated? That the custom in anthropology has been not to bring up the negative aspects of field research? (Anthropologist Gretel Echazú reminded me that Miriam Grossi commented about the "hidden curriculum vitae"[15], where all of our rejected and failed projects should be registered. Everything that no one mentions. Echazú asks, is it true that our anthropology is really "addicted to success"?). Finally, to whom does the interview belong? Of course, to the person who lived the narrated story and was willing to tell it. The ideas, opinions and

her way of life are hers. But what about the experiences that were reconsidered, organized, articulated by the questions posed by the researcher? It is clear that this communication process could have occurred without the presence of the researcher, the research project, the list of questions, the tape recorder, the interview setting, etc. But, in some way, the presence of the researcher helped that the experience was retold in a linear fashion. (This supposes that linearity of an experience could acquire intense therapeutic aspects for the interviewee, as Maluf[16], reminds us, also in the context of anthropological researches). In the end, should not the interview be considered the property of both participants, the interviewer and the interviewee? This product was constructed by many hands and only exists because the two parties met and decided to stay together for a while (and afterwards a third party transcribed the interview and a fourth party was interested in reading the results, etc.). This consideration is important because while editing or prohibiting the publication of parts of the interview, the interviewees were demanding the exclusive authorship of that text that was oral and was later printed. With this articulation, the interview – that implies a dialogue – became equivalent to a monologue. The fact that the interviewees told us what they wanted (or not) to be published was a reverse game, in that they demanded for themselves the role that the researchers had assumed alone via the data they had collected in their field research.

Fourth, frequently used in health research, the Informed Consent Form generally is a condition for the approval of research projects that are submitted to the IRBs. In general, the form is presented to the interviewee *before* the interview, as one tries to explain how the research will be conducted. But, in this project it was clear that in this way, the form would be transformed into a "blank check", signed off by the interviewee before she knew exactly what she would say during the interview situation. Thus, the procedure adopted was to mention the consent form beforehand, but only *afterwards* offer it to be signed by the interviewee. This way, from the beginning, the person would know about a document to mediate the relationship and that, after knowing what was actually said, could then decide to authorize or not the use of her discourse. So, the Informed Consent Form is not the first or the last instance of negotiation regarding field access. Our reception by our hosts was parsimonious while they were understanding what we were proposing.

Deny the use of certain parts, demand certain linguistic corrections or question the quality of the equipment and of those who used these equipement are examples of successive negotiations and authorizations that were established with us, when analysis and publication of results began to appear. This experience in Guariroba reinforces the serious limitations of the Informed Consent Form, also experienced by other researchers[17-19] And, if its use was really necessary, the same should remain in constant evaluation and improvement, also seriously considering that other forms should be signed during the research project, as apparently was suggested by our interviewees at this health center. As a last resort, perhaps we should opt for discontinuing the use of the Informed Consent Form in many situations[20].

Final considerations

Marcel Mauss[21] was an important anthropologist who taught us how magic can stand on the basic principle of contagion: once linked to someone, that object (or idea) always will be in contact with that someone. These employees in Guariroba challenged us, by treating the interviews and the underlined texts in a zealous manner, and provoked us to think that the "field material" was not entirely "ours", but not entirely "theirs" either. This encounter and dialogue – always a negotiation and renegotiation – is what permitted the construction of the so-called "anthropological data".

The level of confidence established between the interviewer and the interviewee perhaps is clearer and more alive at the moment of the intertwining conversation. It is the *relationship* that impelled and constructed the possibility of this dialogue so that the interviewee could tell stories, intimacies, gossip, release their feelings and sufferings. These are secrets that were revealed to us in confidence. Afterwards, after seeing, for example, that same conversation printed on paper, the interviewee vacillated: the initial relation of confidence became more diffuse and distant. These interviewees, by saving this encounter in their memory and by not encountering us so often anymore, did not have the guarantee that the social relation which made possible that dialogue would be respected. And perhaps therein lies the fear that the initial confidence does not accompany the products later generated. The return of this material communicated to me not only about the opinions and tactics used by these women, but, in

a larger nature, about that specific place of work and about the type of roles that could become documents in a health institution. The demands for editing, correction and suppression by employees revealed how the confidence relationship still initial, unstable and budding, still lacked new and reiterated manifestations to be consolidated. In the strangeness that they revealed within our mode of work and by pointing out how the research relationship also needed duplicate the relationships in the health center, we learned not only how they defined "research", but the values that guided the daily contacts within the health center. Some of these values were, for example, trustfulness and suspicion; time and familiarity; transparency and gossip. This stage of the project, however, contributed to a deepening of our understanding about this institution, in terms of functioning, actors, hierarchies and conflicts, adding to the many anthropological studies on primary care centers[22].

The fear of an underlined part being cited out of context and outside the confidence relationship is what installed the feeling of insecurity and vulnerability that these persons presented to us on receiving the returned interview. We recognize that we transform the oral word into the printed word, literalizing what is generally formulated and maturated only in the oral form. Exactly as Mauss[21] taught us, a piece of the other person in wrong hands could be and opportunity for the production of dangerous magic. A piece of an employee is taken along with us after the interview and what was done with this piece will be of great importance because it is potentially ambivalent.

It seems that what was selected, underlined, fractionated and taken out of context, that part preserves even greater destructive power. The women employees at the health center told us about this space where they spent most of their days: they revealed to us the risks they ran with an action that I would call "institutional witchcraft". They demonstrated, therefore, the intense regimes of power and subordination that they had to deal with on a daily basis within the health institution, but that they also could be easily instigated by us, the researchers, during our field work, but not always with full perception of the consequences of these regimes and the risks involved. Rial[4], for example, defends the impossibility of restitution "because the article or film is perceived in a manner that we do not dominate and that might have little relation with what was carried out anthropologically". But we never have complete control over the production even less the consequences of a research/extension project and it would be presumptuous to ignore the dialogue that many times continues to occur in the encounter with the other. Anthropological data will always be reinterpreted, also by us over time. Lastly, I would like to suggest that the unpredictability of the situation of devolution should not restrict us from continuing to experiment to do it. Although in the present case, a clamor was configured, that strongly challenged our methodological choices, I believe that this intensified the mutual knowledge among us and the interviewees, opening the possibility of revising anthropology and of improving the understanding of the reality of work at Brazil's National Health System (SUS).

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