

THE FORMAT AS AN IRON CAGE:
WRITING IN SOCIOLOGY AND ANTHROPOLOGY

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ABSTRACT

THE FORMAT AS AN IRON CAGE: WRITING IN SOCIOLOGY AND ANTHROPOLOGY

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This thesis analyzes the prevailing formats of writing in sociology and anthropology that are considered scientific. For whom are sociology and anthropology texts written for, and who are the readers to these texts? How does the format of writing that constitutes a text as scientific influence the text-reader relationship?

In discussing this, the legitimate ways of writing of sociology and anthropology are presented together with the what scientificity brings. The reflexive critique that looks at sociology and anthropology with the very methods of these disciplines is explained in its main lines. Within this debate, the importance of the question “whom the texts produced in these sciences are intended for” is analyzed. This is followed by a discussion of the conditions that enabled the constitution of the conventional forms of expression in sciences. The concept of paradigm as proposed by Thomas Kuhn is used to explain the formation of these conditions.

Keywords: Sociology, Anthropology, Reflexivity, Paradigm, Format of Expression

ÖZ

SOSYOLOJİ VE ANTROPOLOJİ METİNLERİNDE YAZMA ŞEKLİ, BİR ALTIN KAFES

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Yüksek Lisans, Sosyoloji

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Bu çalışmada, sosyoloji ve antropolojideki bilimsel kabul edilen yazma biçimleri incelendi. Sosyoloji ve antropoloji metinleri kimin için yazılmakta, kimler tarafından okunuyor? Bir metnin sosyoloji veya antropoloji metni olarak kabul görmesini sağlayan yazma biçimleri, metnin okuyucuyla ilişkisini nasıl etkiliyor?

Bu tartışma içinde, sosyoloji ve antropolojinin meşru yazma biçimleri bilimselliğin getirdiği özelliklerle beraber sunuldu. Sosyoloji ve antropoloji içinde yapılan uygulamaları bu disiplinlerin kendi yöntemleriyle inceleyip eleştiren düşünümsellik tartışması ana hatlarıyla aktararak, bu tartışma içinde bilimsel metinlerin kimler için yazıldığı sorusunun yeri incelendi. Ardından, sosyoloji ve antropolojinin yerleşmiş ifade biçimlerini oluşturan koşullar tartışıldı. Thomas Kuhn'un önerdiği biçimiyle paradigma kavramı, bu koşulları açıklamakta kullanıldı.

Anahtar Kelimeler: Sosyoloji, Antropoloji, Düşünümsellik, Paradigma, Yazma Şekli

To Altan
and all my friends and relatives
whose journey in the academy I have been witnessing

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CHAPTER 1: Introduction

*“Ne kadar bilersen bil, söylediklerin karşındakinin anlayabildiği kadardır.”
(However much you know, what you say amounts to what is understood of it.)*

Mevlâna Celâleddin-i Rûmî

Sociology and social sciences in general produce very exciting ideas, researches and approaches. In studying sociology many of us feel that a whole new understanding of the social world we live in is opening up before our eyes.

We live in this social world and we have some understanding of it. If sociology and anthropology are basically about this social world, if what is talked about in these fields of science is about us and our lives, then anybody might be interested in what sociologists say. Of course nobody has to be interested in sociology, I'm not saying that it should be taught to everybody. Nevertheless, if a person is interested in what sociology has to say, can she easily reach it? Is the knowledge produced in sociology open to the reach of anyone? Does sociology or anthropology talk to everyone (including people who are not trained in that field)?

For me this question became a crucial one especially because I think that what is thought, proposed and discussed through sociology is important and valuable. I don't want to say that sociology is so great that everybody should be educated in it, that the emancipation of the human race lies in sociology, or that sociology should have an aim to diffuse itself and thus heal the world. It is just that I expect that something nice that tells something about our world should be reachable to our knowing if we want to learn about it.

1.1. A Book on the Shelf

My doubts on whether knowledge produced through sociology is open to the reaching of everybody have arisen with my experiences as a participant in anthropological and sociological researches. I have worked in social researches as an ethnographer or an interviewer in the “data gathering” part of the process: in direct contact with various people. Some of these researches were rather policy-oriented, others more on the side of pure science. Participating in a research on family values, the perception of honor and honor crimes has been especially influential. The research was directed by Filiz Kardam and carried on in İstanbul, Adana, Urfa and Batman, from January to April 2005. I was newly graduated from the Department of Social Anthropology of Istanbul University, and I wanted to take part in this research because of its very subject (a few years before that, someone I knew was trying to escape a situation related to the codes of honor and I had somehow got involved with the matter, which made the issue important for me). In the “field” we were a team of four, carrying out interviews with women and men with diverse backgrounds that we would meet in selected neighborhoods, with NGO staff, with related professionals such as lawyers, judges, imams, journalists or policemen, and with a small number of “victims”. What we were hearing from the people we were talking to often fell to the cruel, bitter side of social life. Many of the people that we talked with thought that one should live for honor and can kill and die for it; we were listening to stories of rape, torture, violence, forcing to marriage or suicide, and killings. You walk into the house of an old woman, she is kind and nice and you really like her, and then in an hour she tells you how a 17-year-old girl was killed in the streets of that neighborhood, having her throat slit by her husband, because she was seen with another man, and the old woman says this is exactly what should be done to her, that she deserved even worse. And we were also speaking to men and women (often holding values based on the perception of honor) who feared for the life of themselves or their relatives, who have suffered a lot, who were angry that their neighbors (were) killed “for honor”, who had close

escapes from being killed, or defended their relatives, sometimes saving them, risking themselves.

During this research and others, many people told us that they were happy to talk to us because they wanted to help others by telling what they have been through. Some people said they wanted all these things to be known so that measures would be taken to prevent honor crimes, while some wanted it in order to prevent people to transgress the honor code. Some said that they wanted their opinion and experiences to be taken into consideration while this topic is being discussed. Whatever the reason, many people spoke to us on this difficult matter because they wanted what they were telling to be known by others. For these people, making their opinions, feelings and experiences known to others was a main reason of our sociological researches.

In this research and in others, I have been asked by people what all this effort was for, and how the research results would be used. As it will be presented in the focus group discussions in this study, other graduate students who have done research with people have similar experiences. I remember the anger of one man in Batman, saying that some people come and ask them about their opinions on various matters from time to time saying that the research would be used for some ends, but he has never seen anything of this kind, he has never heard anything about the use of those researches. He felt cheated, and asked me “are the results of this research going to be yet another book on the shelf?” The same question was shared by other people, some times with the same way of expression.

Although, about four years after this research, as members of the research team we still don't think we've made a thorough use of the data (and we still talk on finding ways to work on it), it has been the most widely known study (including the non-scholarly circles) among all the studies that I have participated in. The report on the results of the study was first published in the end of 2005, printed 1000 copies then,

and reprinted last year. The man in Batman, if I could see him again, can still be dissatisfied with the change brought by this and other such researches. I think it is nice and hopeful to always demand more. It is also nice for sociology to be something that generates demands and expectations in circles outside of it. But I could tell him now that what we did has contributed to bringing the topic into public attention and even to some changes in NGO's and government bureaucratic organizations related to it. I have later met people who have read that report, including one important bureaucrat who said that the report had been of great use to him while he was organizing a circular for the ministry of interior to prevent honor crimes (2006).

Nevertheless, I am not satisfied with the answer I gave then. Regardless of what *is done* with the results of this or that research, an important question is *how they are known*. How are the results communicated and who can know about the "results"? As many people who become respondents to social researches say, they want what they say to be known by others. This demand, and the question of the man in Batman are the origins of the questions in this thesis. The question of the public or the audience to studies in social sciences is a crucial one. In Tayfun Atay's course of Anthropological Theory and Method (METU, 2007-2008 academic year fall semester), one of the students repeatedly asked "who does an anthropology write for?" As I will argue in this thesis, this question is related to how things are communicated when we are doing sociology or anthropology. Therefore it is not limited to research results, but valid for any theoretical discussion, any critique or new idea put forward in sociology, or questions and proposals raised from the field of social sciences.

The ways of expressing what is done in a field of science might be discussed for various fields of science, but my focus in this thesis will be on sociology (though I will not keep anthropology out of the discussion or make a clear-cut separation between sociology and anthropology). The questions of this thesis are thus rooted in

experiences of carrying out sociological and anthropological field researches in Turkey, but the answers will be sought in what I will claim to be rather structural characteristics of communication in sociology.

1.2. Research Question

The question of this thesis is “how does sociology communicate through the scholarly text it produces?” Focusing on the texts, however, shouldn’t imply that they are the only medium of communication employed in sociology. Discussions among people or in television and radio, lectures delivered in universities and elsewhere, articles sociologists send to daily newspapers or popular journals; books, websites, conferences and symposia that address not only scholars but a variety of people are some other ways of expressing what is done in sociology. But I have chosen to work only on the published texts for the purposes of manageability of the study and because these are the constitutive media of the discipline (Kuhn 1962, Burke 2000) and the conventional manner of expression of sociology (Smith 1991).

How is something told in sociology? How are theories or research results presented? Are there specific ways of speaking in sociology or by sociologists? If there are, how do they influence the relationship of sociology with the external world? (These questions take sociology as a body of knowledge as proposed by Foucault in his methods of discourse analysis; but the idea that there is an outside to this body of knowledge is something I take up from Dorothy Smith’s sociology). Are there rules to communicate for sociology? Who are the audiences to sociology texts? How does the format of communication influence or constitute communication?

The questions I tackle with in this thesis are not about what sociology is or to what good it is. The five theoretical approaches in sociology provide answers for why sociology is done (Neumann 2006). There are also answers to what the subjects of

studies are and how they should be studied (ibid.). Here my aim is not to discuss these perspectives and preferences. Good and bad practices of individual sociologists and anthropologists also are not within the scope of this thesis. I will not be focusing extensively on the discourse of sciences or sociology either. (Can we explain the features of communication within sociology and from sociology, only with the concept of discourse?) Instead I will be focusing on the format of expression used in sociology and anthropology, in its effects on the communication of what is produced in these social sciences. Nevertheless, how sociology is communicated is related to how it is practiced as a science and the various aspects of sciences, which will be discussed briefly in this thesis.

The relationship between texts and audiences can be expressed as a relationship between structure and agency. In this formulation the format remains on the structural side, though the parts of this relationship are not isolated from each other. The audiences or the everyday experiences are affected by the text and the format, from the way social sciences are expressed, but this everyday side of the relationship is at the same time the constituent of the format, the structure. Although in sociology structure and agency are sometimes conceptualized as different entities (often for the ease of discussion), it can also be argued that they are not in fact separate. However, in this thesis I will try to conceptualize this relationship as one of conversation, therefore with two sides, and focus on the structural side of it, on the formal ways of expression. I am aware that this is a reciprocal relationship; and agency, the individual practices, the everyday experiences can not be isolated out of it. Agency is of course a very important constituent of the communication of ideas as well. But for the sake of trying to make a more focused discussion on the implications of the structural side of the relationship, I will only look at the agency side in how it is influenced, and not on how it influences or constitutes this relationship. My question is how the ways of communication in sociology (the format) shapes the communication.

Another point I want to make clear is that while asking whether sociology speaks and to whom it speaks, I exclude the debates on the personal role and responsibility of the sociologist as an intellectual. There are many works on how a social scientist should behave as an intellectual, how he or she should be addressing a wider public in line with his/her political engagements, and ethical dimensions of intellectual and sociological works. The conscious writings of C. Wright Mills, a rather “activist” critical sociologist, on “the *need* for social scientists to address themselves to the great agonies and issues of our age” (Horowitz 1963:2) provide an example here. Such considerations are about individual intellectual roles sociologists should take. Edward Said’s *Reith Lectures* (1994) that he delivered in BBC radiobroadcasts is another example that discusses intellectual responsibilities, including those of social scientists, but these too rather concern the “*should*”s (in more scientific terminology, they are “normative”). I will not be dealing very much with these types of works in this thesis in favor of a more structural approach about what kind of relations social sciences establish within their fields and with the rest of the world.

A common claim in sociology textbooks or studies on method is that the findings are open, at least in principle, for the use and knowing of anybody, that sciences are not done for any particular group but for everybody. In this thesis I will be discussing whether this is the actual situation with sociology. Does a study become “public” when it is published?

Gillian R. Evans has a detailed work on the academy and the political economy of sciences, titled *Academics and the Real World* (2007 [2002]). The title may lead to an expectation that the book examines the relationship between the academy and the wider public, but the “real world” here refers to the companies, the capitalist system and its attempts to dominate the academic world, with an emphasis throughout the book on preserving the autonomy of the academia from this real world. Evans sees the non-academic world rather as a threat than an audience to be related to, or to be

communicated with. She asks what it is that makes a field of knowledge settle or transform, or what the relation is between making a discovery and having it known as a discovery, and answers:

We might try to answer this pragmatically, and suggest that the first stage is to see things from a new angle, making a novel juxtaposition or connection [...] The second stage is to get it said in a published form, so that it may be tested and perhaps begin to be adopted. (Here any delay or prevention of publication by a commercial funder becomes an important factor.) At the last stage of this process there is that integration of the new discovery into an existing system, or the formation of a new system for it to belong in, with which we are concerned, for a discovery or a new approach has to be accepted by an academic community whose members may at first be deeply resistant to, or uncomprehending of, its most fundamental manifestations. (Evans, 2007:20)

Here the second step, making a written publication of the discovery, is presented as a single move. It is not discussed as a process. This implies an assumption that an academic work is known at the moment it is published. Evans does not give any explanation for thinking that a scholarly study will be known by the public (whoever the public is for a certain work, be it only the academic community) when it is published. This approach is often repeated in studies on writing or printing and their consequences. This thesis is an attempt to challenge these assumptions that a text is known when it is published, and that sciences are open to the use of all.

However, in this study I do not argue for a popularization of science or sociology. Asking how sociology speaks and how it is heard by who shouldn't imply for any wish that sociology should be spread to wider circles. Although I do argue that sociology and anthropology should be concerned with and conscious of who they write for, this doesn't mean to say that spreading or diffusing sociology should be a concern in the discipline. I also don't claim that texts of sociology are the only ways of communication; cinema is another one, for instance, and it is not a sociologist's responsibility to make films. This thesis has no aim to suggest that sociologists

should be more visible in the media or being public should be an aim in doing sociology.

Moreover, I don't discuss whether the format of expression in sciences is necessary or inevitable or not. This way of expression may or may not be the best or the only way to communicate what is produced in sociology; the structure of knowledge or science might not enable any other forms, but this format nevertheless has implications on communications within and from sociology. My aim is not to open the necessity of the format into discussion here; this thesis is rather concerned with those implications.

1.3. Contents

How points are made in sociology can be understood in relation to the question of whom scientific studies address. In Chapter 2 I will try to develop this argument through the framework of a feminist sociology of knowledge, making use of the conceptualizations by Dorothy Smith and the methods of analyzing discourses developed by Michel Foucault. I will be making use of Smith's critique of sociology, which is more interesting for the purposes of this thesis than her proposals for a sociology for all. In her critiques she argues that "established sociology" creates a feeling of exclusion in women and relates this to the conventions of doing sociology. What is important in this critique for this study is not that *women* are excluded, but that the argument that there is *exclusion*, for which Smith finds reasons in the very way that sociology is done, locating it into the "relations of ruling," thus making a reference to the political economy of science.

I will make use of Michel Foucault's concept of power-knowledge and his methods of genealogy and archaeology of knowledge for the focus on how power is inherent to knowing and exercised through it. Genealogy and archaeology of knowledge enable us to look at the format of sociology texts as an act in its effects and at how

it has come to be as such. Thus I will try to take it as a process with various instances.

Although methodologically science works with principles, theories and methods, all these are carried on in practice by people and in institutions. This practical aspect of *doing* science is very much related to what it produces and how it communicates these products. Here, relations with the market, with governments, power and gender relations come into play. Knowing is more explicitly social, historical and political in science in practice. These aspects of science will be discussed (though not in much detail) in Chapter 3. This chapter also includes two focus group discussions with graduate students of sociology, anthropology, ethnology and media and cultural studies on what they observe and experience about the practice of their disciplines and its format of expression. This empirical part, however, only aims to present the opinions and experiences of beginner practitioners of these disciplines to enrich the discussions; it is not the main study of this thesis and it certainly has no claim of representativeness.

What have social scientists said about the relations of themselves with other people? This, the reflexive critique in anthropology and sociology, is discussed in Chapter 4. The critique, which has brought important changes to the practices, focuses on the relationship social sciences establishes with the people they study and the representation of this relationship in social science texts. Reflexivity is a self critique, turning the methods of anthropology and sociology to the disciplines, and is not based on criticizing others (other methods and perspectives or pressures one encounters in doing sociology, for instance). Briefly we can say that what is done is an effort to “make the other less other” (Abu-Lughod 1991). However, I will argue that in this trend of critique social scientists have not taken the relationship they have with audiences other than themselves and the people they have studied on/with, or the format of communication, into much consideration. But reflexivity is also important in that reflexive anthropologists and sociologists show that there are

ways to tell about their studies other than the regular scientific format (i.e. Lindisfarne 2000).

In Chapter 5, I will discuss the establishment of the format of communication in sciences through Thomas Kuhn's conceptualization of the paradigm (1962). Kuhn argues that with the acquisition of a paradigm in a field of science, the members of that field tend to communicate their studies to the other members of the community rather than a wider public. Thus he ties the format of communication to the very structure of science. This is an aspect of Kuhn's work that has not been studied on in relation to conversations within or from academy.

Kuhn's work is an attempt to explain the structure of sciences with the concept of paradigm, but it doesn't have an explicit focus on power relations. Thus I will try to discuss the arguments of Kuhn using the concepts and methods proposed by Foucault and Smith as presented in the theoretical framework of this thesis. In this chapter I will also try to present how the format has come to be in relation to the formation of scientific communities by making use of the studies on social history of sciences and the development of printing, and the concept of "imagined communities" as developed by Benedict Anderson (1991[1983]). The chapter ends with a more thorough description of the format in relation to the structure of sciences, and its implications as power. Chapter 6 presents a conclusion of this thesis.

CHAPTER 2: Theoretical Framework

“If social science is the answer, what is the question?”

(Garfinkel, 1981:vii, quoted in Hammersley and Atkinson, 1993:19)

In this study a feminist methodological perspective is adopted for its critical questionings of science itself and also for the flexibility it provides in handling questions. Feminist methodology has a claim to question the myths in science (Harding 1990[1986]), which makes it appealing to use this methodology to study the questions on sociology. I will be making use of feminist sociologist Dorothy Smith’s critique of sociology in her attempts for a sociology for people, starting from the women’s movement and the feminist efforts to establish a sociology for women. It is necessary to state in the beginning that Smith’s critique of sociology is more interesting to me and more relevant for the questions of this thesis than her proposals. I will also make use of Michel Foucault’s contributions to the sociology of knowledge with his analysis of power, especially through his methods of genealogy and archaeology of knowledge. Foucault argues through his concept of power-knowledge that power and knowledge are inherent to each other and they act as one.

2.1. A Feminist Sociology of Knowledge

Feminism has provided a new critical way of looking at the world and at sciences, and doing research. Feminism doesn't only propose a method for doing research, but claims to provide a methodology that challenges science itself. A core argument is that (non-feminist) science is based on men’s understandings and that women understand and express themselves in different ways than this (this way of understanding and expression is affiliated with positivism by many feminisms) (Harding 1990, Neuman 2006). Instead of aiming to replace a form of domination with another, feminism claims to struggle against all forms of domination. Feminist

perspectives are largely built on an understanding of the world from the women's / feminist point of view, with an aim to transform the world (freeing it from male-bias, androcentrism, gender discrimination etc.).

An important reason for feminist methodology to prefer basing itself on women's experiences or feminist standpoint is the argument that with non-feminist sociology we cannot see the gender situations (in doing a sociology *of* women, women will still remain the objects of inquiry (Smith 1987:74)). Instead of this, feminist methods see the world as gendered and aim to empower people to reach goals of gender equality or degendering (this doesn't mean putting feminine in the place of masculine). In scrutinizing on gender in science, feminist approach asserted that science is a social construct and therefore it is not free from the values of societies or from power relations (Harding 1990). "Feminist researchers argue that much non-feminist research is sexist, largely as a result of broader cultural beliefs and a preponderance of male researchers." (Neuman 2006:102). Basic theoretical assumptions, scientific concepts and the general working of the scientific community are seen as gendered cultural contexts. "Gender has a pervasive influence in culture and shapes basic beliefs and values that cannot be isolated and insulated in the social processes of scientific inquiry." (Neuman 2006:103). Therefore, challenging gender means to challenge race, class, culture, and knowing.

Women's movement's general refusal of the separation of the mind and the body is a refusal of the Cartesian subject. This Cartesian vision has been built into sociology (Smith 2005:23), which also makes the women's movement or the feminist methodology a challenge to sociology itself. Feminist approach refuses the scientific rationality that proposes that science and commonsense, or theory and practice are or should be two distinct fields (where science has a higher place in a hierarchy). Therefore, what is called "commonsense" or "experience" in the language of such dichotomizations should be a part of feminist studies without being separated from knowledge as a whole.

Like many critiques against positivism in sciences from various approaches, feminist methodology shares the argument that claiming value-neutrality is itself a value, claiming not to be political is itself political. Furthermore, such claims hide the inherent power relations in science and society, leading to mystifications. For a demystifying science, concepts and theories should be constructed with conscious and outspoken moral aims and political commitments (Harding 1990).

Feminist methodology challenges the main question of method itself: the question “How should we do research in science?” is under scrutiny (Harding 1990). It argues that the research process itself can and should orient the research, and that research shouldn’t be prescribed. In following feminist methodology, I will try incorporating my experiences into this study, telling how the things that I had done before and the things told to me by people have brought me into the questions of this thesis and still shape it. If this thesis is a journey, I will try to make that explicit throughout the text. I will try focusing on the social aspects of doing sociology, as feminist methodology proposes for all sciences, but in studying communication in sociology I will not be emphasizing gender relations especially.

2.2. Dorothy Smith’s Critique of Sociology

Born in 1924 in the UK, Dorothy Smith is among the earliest of feminist sociologists. Starting from her own feelings of a disjuncture between doing scholarly work and being a woman, she came to ask whether sociology addressed women, whether it availed for women’s speech within itself (Smith 1987). From here, through the feminist standpoint approach that she contributed in the making of, she wanted to build a sociology for women, which then developed into an inclusive sociology for all, as she later claimed that sociology should not be restricted to particular categories (Smith 1998, 2005). Dorothy Smith proposes “institutional ethnography” as the method of inquiry of her sociology. In this study I

will adopt this method and try to understand sociology through its texts, or rather through the way it is organized with texts.

At first, Smith's works seem to be in a coherent continuity and even repetitive. However, her first book *The Everyday World As Problematic* (1987), is based much more on experiences, even intuitions; it is a questioning and critique of "established sociology" more than a theorizing of a proposal for a new sociology (although she is also doing that). Her later books, *Writing The Social: Critique, Theory, and Investigations* (1999) and especially *Institutional Ethnography: A Sociology For People* (2005), on the other hand, focus on theorizing, even institutionalizing, her proposal for a new sociology, rather than criticizing the established one (the critiques she makes in the later works are usually summaries or direct repetitions of the old ones).

Smith says her work is "a reflexive critique of the ideological practices by which we create and express objectified forms of knowledge that are constituents of power in contemporary societies." (1990:11). Her first book, *The Everyday World As Problematic – A Feminist Sociology* (1987), is a collection of her essays written from 1970s onwards, that she claims to have originated "in the women's movement's discovery that as women we had been living in an intellectual, cultural, and political world, from whose making we had been almost entirely excluded and in which we had been recognized as no more than marginal voices." (Smith 1987:1). This is how she sees mainstream sociology and explains her own work:

In writing a feminist critique and an alternative to standard sociology, I am doing more than a work for specialists. A sociology is a systematically developed consciousness of society and social relations. The "established" sociology [...] gives us a consciousness that looks at society, social relations, and people's lives as if we could stand outside them, ignoring the particular local places in the everyday in which we live our lives. It claims objectivity not on the basis of its capacity to speak truthfully, but in terms of its specific capacity to exclude the presence and experience

of particular subjectivities. Nonetheless, of course, they are there and must be. (Smith 1987:2).

As part of the feminist methodology outlined above, Smith argues that the standpoint of men “implicit in the relevances, interests, and perspectives objectified in sociology” coincide with the standpoint of established sociology in the relations of ruling. “Established sociology has objectified a consciousness of society and social relations that “knows” them from the standpoint of their ruling.” (Smith 1987:2). Starting from her early works Dorothy Smith conceptualizes the standard forms of knowledge as representations of actualities to enter local lives into the relations of ruling. The practice of ruling, she says, “involves the construction of the world as texts, whether on paper or in computer, and the creation of a world in texts as a site of action. Forms of consciousness are created that are properties of organization or discourse rather than individual subjects.” (Smith 1987:3).

Smith comes to this point through looking at gender subtexts in knowing and the relations of ruling, based on her own experience of the exclusion of women’s ways of knowing and experiences from the academy and the dominant forms of sociology. However, the exclusive practices of sciences and ways of knowing don’t only exclude women. Smith’s arguments on exclusive practices and her efforts for a more inclusive “sociology for all” are applicable to many relations, as is stated for the method developed by Smith, institutional ethnography (http://faculty.maxwell.syr.edu/mdevault/Information_about_IE.htm). In this study I will try to make use of Dorothy Smith’s theory in this wider sense of exploring the practices of sociology in terms of inclusion and exclusion, and especially on what is done with texts.

Like many feminist researchers, Dorothy Smith considers a science detached from the rest of the society to be a myth. Smith takes the relations of ruling as a part of the world in which we live and experience as subjects, and she says she got to grasp

them through her own experiences as she was working as an academic in the sociology department of University of California at Berkeley and at the same time was a single mother:

When I went into the university or did my academic work at home, I entered a world organized textually [...] and organized to create a world of activity independent of the local and the particular. [...] But I went home or put down my books and papers to enter a different mode of being. I cleaned up after, fed, bedded down, played with, enjoyed, and got mad at two small children. I inhabited a local and particular world (Smith 1987:6).

Smith has experienced the abstract world of sociology and the practical world of her everyday life as “two modes of consciousness that could not coexist with one another.” (1987:7). She claims that the discourses embedded in the relations of ruling in the intellectual world, whose apparent centerless look was indeed centered, made women the other in it. The discourse that seems to speak objectively and dispassionately in fact has a center within the ruling relations and sets the rules of the game. The structure of the work sociologists do is thus determined through power. Putting the problem this way, Smith claims that she wants “a political economy that explicates and analyses just how our lives are caught up in political economic processes, including, of course, the ruling relations in which our own work as social scientists is embedded.” (1999:44).

Smith believes in the possibility of taking up a standpoint outside power relations, outside the “main business”. Departing from here, she proposes to do a sociology from the standpoint of women, starting from their everyday lives and “talking back” to the power that organizes the everyday. Although I do not agree with the possibility of a position totally “outside” or free of power relations, I find Smith’s conceptualizations of inside and outside useful for making discussions on communication.

2.3. Texts and Power

Marshal McLuhan argued that the media that carry messages change people's perception of the content of the message, that "the medium is the message" (Olson 1998[1994]:54). Smith conceptualizes texts ("words, numbers, and images on paper, in computers, or on TV and movie screens") as mediators of "relations of ruling" (Smith 1987:17). In contemporary Western societies, she writes, "texts are the primary medium (though not the substance) of power." (1987:17). (This claim that texts, or sociology for that sense, are not the substance of power is a main difference of Smith's theory from that of Foucault's, and it is an argument that needs further discussion. See below.)

Governing, managing, organizing are all communicative works coordinated textually: "images, vocabularies, concepts, abstract terms of knowledge are integral to the practice of power, to getting things done." (Smith 1987:17). Following Louis Althusser, Dorothy Smith also says that the ways we think about ourselves and the others are shaped through "ideological apparatuses". She argues that this way of organizing society has its own history in Western Europe in the last four or five centuries, and this "tradition" gives presence to men at the sake of the absence of women.¹ This way of thinking, developed in speech and in the written word, has its own questions and answers, it has its own standards and styles and themes. Smith argues that the concepts and forms of thought that we use are not shaped by all, they don't arise from the everyday lived relationships, but they are "the work of specialists occupying influential positions in the ideological apparatus (the educational system, communications, etc.)." (1987:19). Thus the sociological speech has become exclusive.

¹ Sciences as we know them now, the academy with all its organization, and the ways we practice sociology also originate in this Western tradition. Therefore, whether we are part of that tradition or not, it is relevant to discuss sociology in its relations with the Western tradition of thought.

Smith's works are important for the purposes of this thesis because her critiques maybe thought under the question of how sociology speaks, with subquestions like who speaks to whom and for whom in sociology, and how the ways of speaking in the discipline are formed. However, Smith generally looks at the organizations of discourses through texts (not at discourses themselves), how ruling relations are organized, or how disciplines are disciplined, rather than looking at how the format of expression is worked out and how it works out. She conceptualizes texts as media of the creation of authority, which "is a form of power that is a distinctive capacity to get things done in words" (Smith 1987:29). Authority means that one's words count; and through texts, circles are formed of those whose words count for another. Academy is one such circle which "excludes those whose words do not count, whose speakers have no authority." (Smith 1987:29-30).² So there is one scholarly world of sociology built through textually-mediated authority, and an outside to it, which is not free of these ruling relations but still is the place where those who are "excluded" live their everyday lives.

Smith argues that working inside a discourse that we as women have not been participants in the formation of, constitutes women as alienated figures to what they are doing within the academy as well as the intelligentsia. This forms what she calls a line of fault in the consciousness of women. Sociology is a part of the "ideological structure" where the intelligentsia of the ruling class produces the ideological definitions of the tools of thought for everybody, therefore sociology becomes an alienating practice. In writing in social sciences,

² Smith tells through her own experience how she first discovered that papers had communicative powers in the early seventies when she presented in a conference an unpublished paper titled "Women's Perspective as a Radical Critique of Sociology": "It made me aware for the first time that when I wrote a paper for an academic context of presentation or publication I might actually be speaking to people. [...] Previously I think I had always seen producing papers as a performance for invisible judges. Writing papers for publication made me nervous. Sometimes I used to take a slug of brandy to get myself going. My experience with how that paper traveled changed my view altogether. I saw that a paper could be a way of reaching other women, of talking to them. [...] I understand that a discourse could be organized differently than one organized around an establishment that judged and controlled and held its practitioners to conform to its notions of how sociology should be practiced." (Smith 1987:45-46).

The professional discourse has a momentum of its own. The canons of science as a constitutional practice require the suppression of the personal. The structures developed become the criteria and standards of proper professional practice. Being a professional involves knowing how to do it this way, how to produce work that conforms to these standards, addressing these topics, and following these methodologies. Further, doing it this way is how we recognize ourselves as professionals. We begin from a position in the discourse as an ongoing social process of formally organized interchange. We begin from a position within a determinate conceptual framework that is identified with the discipline (though they are many), and by virtue of our training and of what it means to do the professional work in our disciplines we begin from outside ourselves to locate problematics organized by the sociological, the psychological, the historical discourse. (Smith 1987:60)

We can infer from this paragraph that power is already inbuilt into the structure of sociology as we do it and learn it. As a Marxist, Smith recognizes this exercise of power as alienation and thus she proposes to start from ourselves against this alienation. Following the concept of discourse as developed by Michel Foucault, Smith describes the discourse of sociology as such:

The concepts, methods, relevances, and topics of sociology are accomplished in the social organization of discourse. A discourse [...] is like a conversation in which utterances are abstracted from particular participants located in particular spatiotemporal settings. Certain journals and occasions such as classes, conventions, and the like are *warranted sites* for the presentation of sociological work. Work is accomplished as sociological in part by its presentation at such sites. (1987:61, my emphasis)

According to Smith, a text becomes “sociology” through being published. And it is the literature of sociology formed by such texts that legitimizes what is sociology and what not. The discourse is maintained through decisions on “who can participate in it as fully competent members. It develops as a process of organization and reorganization of relations among participants through the medium of their work.” (Smith 1987:61). To be a participant in sociology, ones work has to

conform to appropriate styles and terminologies and it has to be locatable by these within the traditions or schools. The themes and conceptual practices of sociology are regulated through this process. Therefore, the relevances of sociological work are the relevances of its discourse. This is what Smith views to be the reason for women's inability to express their experiences within sociology:

To a large extent and until recently the nature of this relation has remained invisible precisely because sociology has operated with a conceptual apparatus that has served to detach phenomena from the working contexts of the social process constituting the phenomena thus named. (Smith 1987:63).

The subject's view from the center is replaced by a detached mode, and sometimes you can hardly express yourself in what you write.³ Smith points to the special situation of social sciences: here "both terms of the "knowledge relation" are human" (1987:72). Sociology, then, "is an organization of practices that structure our relations to others in the society of whom we speak and write, concerning who we make assertions, into whose lives and experiences we inquire, who are the objects of our study, and whose behavior we aim to explain." (Smith 1987:72). In such an organization, the particular subjectivities of knowers and known are suspended, and the social character of the organization also disappears. Smith thinks that this is just like the way activities of people disappear when the social relations acquire a form of commodity exchange. In such an organization of established sociology, the equivalent of commodity is the concept. Communication within and from sociology is therefore also done through concepts. As a sociologist, Smith writes that

we get into this mode very much as the driver of a car gets into the driving seat. It is true that we do the driving and can choose the direction and destination, but the

³ This, I think, expresses what happens as we write our theses. As we, many graduate students at the sociology department of METU say, we don't write our theses, they are inscribed on and through us ("*ben tezi yazmıyorum, tez beni yazıyor*", it sounds quite Foucauldian when translated to English). In the earlier drafts of this thesis, my supervisor Helga Tılıç had written a note here that nevertheless we do it, perhaps because we want to say something. Of course we do. This is why I do a thesis on speaking and hearing. When we put so much effort into saying something, and even more if we are asking the efforts of others as well to say it, why should the way we say it be a limit to the communication of what we say?

way in which the car is put together, how it works, and how and where it will travel structure our relation to the world we travel in. (1987:73)

This way of putting the car together and the way it works is what, I think, the format of expression does in texts. Theories produced in abstraction in sociology take for granted their conditions of existence, they claim to look at the everyday experience but they can't, as they don't adequately acknowledge the subjective, local, centered, particularistic or positioned characteristics of it (Smith 1987). The “object” of this way of knowing the world

appears to it as to Hegel's master, in a direct and simple relation. It is not capable of analyzing its own relation to its conditions nor of locating itself where the social relations organizing and providing for its existence can be seen. The sociologist as an actual person in an actual concrete setting, the sociological knower, had been “canceled” from the act of knowing by a procedure that objectifies and separates him from his knowledge. The essential linkage that is the first clue pointing back to the conditions of his knowledge is lacking. (Smith 1987:85)

To overcome this situation, Smith proposes “institutional ethnography” as a method of inquiry. Basically, this means understanding, or undoing, unlocking, the ruling relations and their organization from the place you actually are, from the local particularities of the subject in her everyday life. It is to understand the organization from the touching points that it organizes you. “The problematic of the everyday world arises precisely at the juncture of particular experience, with generalizing and abstract forms of social relations organizing a division of labor in society at large.” (Smith 1987:157).

In *Writing The Social*, Smith defines the ruling relations as “that internally coordinated complex of administrative, managerial, professional, and discursive organization that regulates, organizes, governs, and otherwise controls our societies.” (1999:49). Institutions, according to Smith “exist in that strange magical realm in which social relations based on texts transform the local particularities of

people, place, and time into standardized, generalized, and, especially, translocal forms of coordinating people's activities." (2005:101). As institutional processes themselves don't form a system of totality, institutional ethnography too, should not remain in any conceptual boundary that attempts at defining the institutional domain, it should have an open ended character (For examples of how Dorothy Smith practices institutional ethnography in many different cases, see Smith 1987, 1990, 1999 and 2005).

Smith's approach is critical in the points that she tells that "the everyday world is neither transparent nor obvious", "its inner determinations are not discoverable within it", its many variations are determined "by an organization of social relations that originate "elsewhere"." (Smith 1987:91-92). Her sociology aims to expose the working of powers beyond our control on our lives, to all of us, and from how we live it in our lives. Marxism is an important component of Smith's analysis. In tracing how she problematizes everyday experience, she quotes from Marx and Engels from *German Ideology*:

Individuals always started, and always start, from themselves. Their relations are relations of their real life. How does it happen that their relations assume an independent existence over against them? And that the forces of their own life overpower them? (Marx and Engels, 1973:30, quoted in Smith 1987:99-100).

Tracing the link between actualities and organizations, or from the other way round, between texts and experiences, means to look at the text in its effects in the everyday life where somebody *reads/lives* it. When we understand texts as "embedded in and organizing relations among subjects active in the discourse" (Smith 1987:214), then we are talking about actual people in actual relationships.

Smith points out that most sociology texts are not written from a standpoint that clearly takes sides with people. (She proposes to take the works of Marx and Engels, whose writings are locally and historically situated, as examples of good practice). In contradistinction to this type of writing, "institutionalized discourses"

create “a sphere of work and inquiry [...] with its own internal logic” (Smith 1999:36), where groups of texts “acquire mass, density, and referential power among themselves and thereafter in the culture at large” (Said 1979, quoted in Smith 1999:36). This, she says, is just what Edward Said is telling about in *Orientalism*: how ruling relations coordinate actual lives through texts, the Western perspective constituting “the orient” as other, as “oriental” through speaking about it.

The standpoint that Smith argues that one should take in looking at power and organizations doesn't have to be a single one and it doesn't assume that the ruling relations look the same from everywhere. She proposes to “seek from particular experience situated in the everyday/everynight world to explore and display the relations, powers, and forces that organize and shape it.” (1999:45-46). This is just how I would like to use Dorothy Smith's proposal of institutional ethnography; to understand the discomfort I feel with texts produced in sociology in the organization of it. This is not discourse analysis. Smith's emphasis on texts as the mediators of the ruling relations provides a legitimate ground for my intentions to look at how texts are produced. However, I don't wish to take texts as media or instruments of organization or to trace how “ruling relations” institutionalize through them. Texts do not just carry power like mediators, but power is inherent to their very structure of communication (in the way Foucault proposes we should try to analyze power and knowledge). In other words, while Smith proposes to look at how power (that she calls relations of ruling) organizes our lives through texts, I want to look at how power is organized into the text. The very format of expression of various texts is the visible side of it. A scholarly sociology text has a different format than a drug prescription, a law, a textbook, a letter or a novel. It is this format that makes a sociology text a sociology text, and it is this format that makes it address and reach some particular people (mainly the other sociologists) and not the others (those who are not sociologists). The exclusion and alienation that Smith finds in sociology texts are not the results of any particular discourse but of the

format of conversation. I will try to look at how this format makes up the way of expression of sociology in Chapter 5, especially through Thomas Kuhn's concept of paradigm.

All texts in sociology are intended to be read by some people, Smith acknowledges this and wants to incorporate this into her theory. Writing is a necessarily two sided relationship according to her. It doesn't end up in saying: hearing becomes an essential part of speaking. I would like to go a step further to say that the identity of the reader is an important constituent of the text, and this is often related to taking sides (see Lindisfarne 2008). Mikhail Bakhtin and Volosinov's work enable us to look at the different sides of a conversation. Smith privileges the works of Bakhtin and Volosinov over the post-structuralists who take Saussure's concept of language as a self-referential system, because Bakhtin and Volosinov's work incorporate the social into the systems of communications they propose. Volosinov conceptualizes the sign as arising only in the "inter-individual" territory and the word as "a two-sided act" (Volosinov 1973:86⁴, quoted in Smith 1999:112). Smith values Bakhtin's theory because he emphasizes the speakers of words, which Smith connects to the standpoint approach. In contradistinction to Ferdinand de Saussure's work that takes the sounds and words to be meaningful in relation to other sounds and words within the system, and makes a distinction between language as a system of signs (*langue*) and speech as people's actual use of language (*parole*); Bakhtin's literary theory is built on a dialogic relationship between language and the way people use it (in speech or writing) (Saussure 2006, Smith 2005:65). For Bakhtin, utterances are essentially dialogic, "an interplay between past determinations of meaning and their creative shaping to the speaker's or writer's current intentions" (Bakhtin 1981:272⁵, quoted in Smith 1999:113). In Bakhtin's theory an utterance is never an origin in itself, it is always a response to other utterances preceding it, words are always related to those spoken before and relate to those that will be spoken afterwards.

⁴ Volosinov, V.I. 1973. *Marxism and the Philosophy of Language*. New York: Academic Press.

⁵ Bakhtin, Mikhail M. 1981. *The Dialogic Imagination: Four Essays*. Austin: University of Texas Press.

Dialogue then becomes an activity that coordinates language, and discourse a socially coordinated activity.

Texts are written to be understood, but they can't respond to your reading even by saying "umm-humm". So the text-reader conversation as conceptualized by Dorothy Smith is "a special kind of conversation in which the reader plays both parts." (2005:105). Smith follows Bakhtin and Volosinov here. Therefore the texts come into action "*in* the readers who activate it" (ibid., original emphasis). The reader becomes the voice of the text and at the same time responds to it, interprets it, and acts upon it. Texts, according to Smith, are "activated" also in sociology, in writing, teaching, reading and learning it, coordinating local activities. "Textual discourses" organize social relations and at the same time they are socially organized. This notion is different from a discourse analysis that remains within the discursive field because it looks at how people take up discourses and how they are affected by them. To provide an example of this approach, Smith looks at how the standard North American family is established as an analytical concept in social sciences (like the nuclear family) and therefore researchers look for it and project it to different contexts whether it is relevant or not. The standard North American family thus becomes an ideological code that organizes the way different societies are understood. The concepts organize sociological works and census data, census data then organize different researches, and so on and so forth. Printed and published texts detach actions from their original local settings through replication and standardization.

Dorothy Smith's sociology is often getting parallel to the works of Michel Foucault, although it differentiates from it and contradicts in important ways as well. The works of both have aspects that I wish to make use of in this thesis. In the next section I will discuss the works of Foucault and the way they have influenced the works of Smith, as well as the differences among the two.

2.4. The Concepts of Power and Discourse in Foucault and Smith

The aim of this section is to discuss Michel Foucault's theory and the similarities and distinctions between his works and those of Dorothy Smith. I'm not doing this to base this thesis on one and discard the other and legitimize such a choice. Instead I think I can make use of both lines of arguments and I want to make clear how.

Foucault studied the discourses of various disciplines to expose how power is exercised through them. I might say he analyses and proposes methods to analyze how disciplines discipline and the processes by which they are disciplined. Of course his concept of discourse is not limited to professions and their specific discourses. He accepts no such boundaries, as he argues that power is exercised everywhere. He looks at how power shapes us and how we structure it: I can also say he has an inclusive, comprising concept of power. He looks at power from within.

Smith, on the other hand, uses the term "ruling relations" instead of "power". Ruling relations are an important component of her theory and like Foucault she also looks at how this works and how it is worked out. However, as a main distinction from Foucault, Smith builds her analysis of the ruling relations upon the experience of exclusion that she lived. Smith says she became aware of the discourse (of science, of sociology) through not being able to take part in it, through feeling like a stranger in it, and not being addressed by it, while at the same time sociology claimed to be encompassing the experience of all. So Smith looks at power from without, from a standpoint that she claims to be outside of this realm. This is also what makes working with her framework more relevant to me in this thesis, because my motivation to study how sociology works (through texts) has also come from a similar feeling of exclusion of people through the texts produced in sociology. As I have stated before, I see the format of expression (which Kuhn traces to the structure of science) employed in sociology to be the source for this

feeling. In trying to build a sociology that wouldn't exclude, and using the same format of "established sociology", Smith falls to the very trap she tries to get rid of. In trying to do a sociology of sociology, trying understand the ways of expression both to problematize them as the topic of this thesis, and to imitate them so that this thesis can become a thesis, I have the same problem.

Michel Foucault has two basic ways of studying discourse: the archaeology of knowledge and genealogy. Both are ways to understand history and use it as an analytical tool. Briefly, Foucault's work aims at writing the history of discourses. How does a discourse condition a body of knowledge? He looks at how discourses of professionalism are stages of power relations, but his theory isn't built on distinctions between scientific knowledge and everyday knowledge.

As classical historians look at documents to assess their truth value, Foucault looks at it from within, looking at how it came to be itself, at its conditions of occurrence, treating it like an event, with his method of archaeology of knowledge. Archaeology of knowledge looks for dispersions rather than regularities, and for discontinuities rather than continuities, opposing the classical approach that assumes that there is a kind of reason behind history (Foucault, 2000 [1980]). This of course is a critique to Hegelian concepts and Marxist theory, as it rejects a center or an aim or a law of history. Foucault's methodological proposals show a dissatisfaction with the classical sociology of knowledge approach or the notion of ideology. In his aim to disperse the conscious human subject, however, Foucault says he follows Marx, Freud and Nietzsche.

As a consequence, the author or the oeuvre are not relevant categories of analysis for Foucault's archaeology of knowledge. Instead Foucault looks at sentences: what makes the utterance of a statement possible? Every sentence should be understood in its relations to others, every statement is a way of saying something, by not saying all the others. So, the question is, why is it this statement, and not the others?

For the purposes of this thesis, if we think of the dialogism as proposed by Bakhtin and used by Smith together with the questions of archaeology of knowledge, we may ask, how a certain format is employed in sociology texts in relation to the dialogue it establishes with audiences.

Foucault's works challenge authority. He conceptualizes authority as the right to speak, and in this sense he influences Dorothy Smith. He asks how authority changes among agents: once upon a time it was the magicians who treated the madmen, now it is the psychiatrists. How come such a change was possible, what happened, and what did this bring along?

Genealogy is another method that Foucault uses and proposes. Through this method Foucault develops his famous concept of power-knowledge. Here Foucault tells that it is a mistake to take history as something linear, words as having a stable meaning, desires as having a direction and ideas as retaining their logic. Just as the archaeology of knowledge asks why a certain statement is used and not any other, genealogy should define the instances that an event remained unrealized.

Genealogy "opposes itself to the search for "origins"" (Foucault, 2000[1971]:370). In contradistinction to a historian who believes in origins and essences of things and tries to "discover" these in history, a genealogist, in Foucault's words, "listens to history, he finds that there is "something altogether different" behind things: not a timeless and essential secret, but the secret that they have no essence or that their essence was fabricated in a piecemeal fashion from alien forms." (Foucault 2000[1971]:372). Knowledge, according to Foucault, "is not made for understanding; it is made for cutting" (2000[1971]:380). Like archaeology, genealogy too, as a search for descent, finds no reason in the flow of history, it is just chance; no truth behind appearances; the quest for a hidden truth and the concepts attributed to the nature and essence of things are inventions of man. With genealogy we can study theory as a practice in itself, like an event. In its use as a

strategy, genealogy should disturb and fragment the things that are considered unified and consistent. Relations of domination leave their mark everywhere and conflicts are not oriented to any consensus.

In his lectures at College de France in 1975-1976, which are transcribed in *Society Must Be Defended* (2004[1976]), Foucault tells that genealogy is a re-combination of what is separated as “specialized knowledge” and “local knowledge”, which only becomes possible on condition that the despotism of the comprehensive discourses, together with all the privileges of institutional avant-gardes, is put to an end. Genealogy is not an attempt to place the concrete multiplicity of phenomena against the abstract and definite monism of theory, it’s neither a kind of positivism, it’s rather an attempt to mobilize the local, discontinuous, disqualified, non-legitimized knowledge against the monist thought that is handled to few under the name of science (Foucault 2004:25). Genealogy is counter-science, it is an uprising of knowledges against science. Foucault makes clear that genealogy’s uprising is not against the contents, methods or concepts of science, but against the effects of a centralizing power that establishes itself through the establishment of a scientific discourse. “Genealogy should struggle against the factors of power specific to a discourse that is thought to be scientific” (Foucault 2004:25, my translation). This is what Foucault does in *Discipline and Punish* (1977).

In *Discipline and Punish*, where Foucault analyses the discourse taken up by the penal law with a genealogical strategy, he exposes how the body becomes an instrument and at the same time an intermediary of power through a scientific discourse. In this process and all other implications of power, it is knowledge that produces power and power that produces knowledge. These two are therefore so inseparable that Foucault studies them as “power-knowledge”, taking the technology of power to be the common principle of the history of sciences and the history of penal law (1977:24). In the “microphysics of power”, at every point we meet power, there is knowledge. There is no “power-free” realm; besides, we need

power to resist against power too. By studying the history of sciences and the history of penal law together and in their effects as techniques of power, Foucault shows how the body of the individual becomes both an object of penal intervention and “an object of knowledge for a discourse with a ‘scientific’ status” (Foucault, 1977:24). According to Foucault, a passion to power is brought with the claim of being scientific, and this should be questioned: What kinds of knowing are being disqualified under the claim of being scientific? In the moment that a subject claims that he holds a scientific discourse and is a scientist, which other knowing subject does he intend to attach unimportance? (Foucault, 2004:26).

In looking at how the practices of science silence women, Smith uses Foucault’s ways of analysis, but reverses them, looking *at* power from the way we experience it, rather than looking at how it works. Smith frequently goes back to the everyday world rather than taking the discursive realm as a closed system; she compares the discourse with the experience. This enables a conceptualization of two sides of a conversation, from within sociology and from outside of it. Smith looks at discourse as a social activity and criticizes all those who she thinks have disregarded this. She criticizes Foucault because she thinks that his theory defines the subject as created by the discourse and this doesn’t allow the subject to take on different perspectives: “I do not accept Foucault’s view,” says Smith, “that knowledge is necessarily a relation of power.” (1999:5).

Foucault tells that archaeology is the method to analyse local controls, and genealogy should be the strategy to mobilize the knowledges that are freed from the yoke of these controls (Foucault, 2004:27). Genealogy does not seek to define what power is, because that would be a too comprehensive approach, instead, the aim of genealogy is to determine different instruments of power that operate on various levels of the society and in various fields, within their processes, effects and contexts. The first methodological principle that Foucault adopts in *Society Must Be Defended* to grasp the concept of power in its limits, where its legitimization is

gradually lessening. His second principle is not to examine the power on the level of its intention or decisions, and not asking who has got the power and what he aims (such questions are, says Foucault, like labyrinths). On the contrary, power should be studied in its actual applications, on its outer surface where it shows its real effects (in this sense Smith's proposal of institutional ethnography is quite close to this claim, with the difference in the emphasis on *experiencing* the "relations of ruling"). Thirdly, power should not be taken as a massive, homogeneous block of domination of one on another, it is not something that is possessed but something that is exercised (this is the perception Foucault holds in studying power-knowledge in *Discipline and Punish*). Power works as a web and it passes through the subject that it has created, and the individual is a factor and at the same time a transmitter of power (therefore not like an endpoint as implied in Smith's studies). Lastly, power should be studied upwards from below; its examination should start from the infinite small pieces that have their own histories, techniques, strategies and orbits. A study of power should focus on how these fragments at the bottom are affected, transformed, changed and used by the methods and techniques of power (the micromechanics of power). With these principles, by not supposing that there is a knowing subject of history and of power, by not examining the actor but the act in its effects, by adopting a strategy that examines procedures from bottom to top, Foucault takes a genealogical path.

The concept of power-knowledge is a key one for the work of Foucault and one that Smith contests explicitly. She finds his concept of power-knowledge to be an ideological practice (in the negative sense of ideology, in contradiction to materialism) where both power and knowledge are mystified (Smith 1990:70). I don't agree with this critique, besides I don't think it is developed well in Smith's works. The idea I get from reading her is that she doesn't like Foucault's concept of power because she's a Marxist and she wants to open up space to look at power from a position that doesn't hold it, if not free of it (defending the privileged position of the slave in Hegel's dialectic of the master and slave). In Foucault's

analysis, discourse has varieties; it has multi-meanings. He does not judge the discourses, he analyses them. Is this preference what Smith had found mystifying? Foucault might seem way too postmodern to Smith (she defends the standpoint approach against postmodernism, see Smith 2005). She takes sides (so does Foucault). Rather than looking at the multiple meanings of a discourse, she looks at its effects on one side: the women, the oppressed, the working class, etc.: She takes sides with the slave. Foucault of course is not at the side of the master. But Smith, I think, wants a more explicit adoption of the standpoint of the slave and probably finds Foucault's way of thinking too detached in that sense. After all, she's an activist. But in this sense her approach is more limited than that of Foucault. She stabilizes the standpoint (that there should be a standpoint, and it should be like that of the slave), though she says there can be various standpoints/perspectives (Smith 2005).

Foucault's genealogy could be criticised of leading to nihilism by saying that everything is constructed, that all understandings are invented or fabricated. I think Foucault's saying that certain ways of perception are established through discourse doesn't imply that nothing is meaningful; but just that truths and meanings depend on the historical discursive means of truth and meaning production. Genealogy is not a method that *aims* to arrive at nihilism; on the contrary it opposes teleology. This is one of the reasons why it is important to analyse events within their specific contexts. Otherwise, genealogy itself can become another way of constituting discourses. I think this is the point that Smith misses. Foucault's overall perception of philosophy is a key to the way his methods can be used: he sees the problems of philosophy to be the problems of present, which is why, according to Foucault, "philosophy today is totally political and totally historicist. What is inherent to history is politics, what is necessary for politics is history" (Foucault 2004:294, my translation from the Turkish).

Take Foucault's concept of episteme: it is the total set of relations that unite at a given time; it should not be reduced to the speech of a certain author or a certain period, because episteme brings together all that can be said. An archaeological analysis of discursive formation is an analysis of the episteme, whereas genealogy is an examination of history in series of time with breaking points, where each epoch has its own episteme, and it is this episteme that conditions the discourse. (Thomas Kuhn's study on the structure of science can be taken as a study of the episteme, where the concept of paradigm is working just like an episteme, as the shared perspective that constitutes a field of science. Practice of normal science is conditioned by the paradigm, like episteme conditioning the discourse, and it breaks in paradigm shifts that Kuhn calls the scientific revolutions. See Chapter 5.)

Dorothy Smith uses Foucault's methods of understanding power and domination and how these work in discourses, but from a feminist perspective that wants to understand power relations from a particular standpoint, from how we live power. This is not a possible stance from within Foucault's theory. In this sense it is original to Smith.⁶ For me this is valuable because it makes it possible to ask how sociology speaks in a way different from discourse analysis: enabling to look at texts in the dialogues established through them, including the question of who sociology speaks to. This is the question I had in mind in starting to work on this thesis, and this is the question that underlies Smith's work, especially her critiques to established sociology in her early works. Asking similar questions from Foucault's work hasn't been possible, at least for me, because with his work I think it isn't easy or maybe not relevant to make distinctions between the parties to a communication. He analyzes speech in relation to conditions, but rather not in relation to speakers or audiences (this comes from the Saussurian way of conceptualizing relations within the system, traceable in Foucault's works). I think this is a disadvantage in trying to study the ways of communication employed by

⁶ Other feminists (i.e. Elisabeth Grosz) who study Foucault's theory rather emphasize the gender relations directly, criticizing him for being gender-blind, or trying to insert gender into his theory or use his works to look at power in gender relations.

sociology. In exploring the discussion of how sociology speaks through texts, it is important to understand how formats of expression are organized and how they organize conversation. Looking at the audience of sociology texts is an important component of this discussion. This is how Dorothy Smith conceptualizes texts (though she doesn't look at the format), when she makes a distinction among the text and the rest.

Foucault's work on discourse and Smith's work on texts and conversation provide answers and new ways of asking the questions. Following Foucault's method of genealogy, I will not examine the actor but the act in its effects; I will not focus on how a certain sociologist presents his/her works, but how works of sociology are presented in certain formats. Are there certain methods, rules, or structured ways for this communication? My experience (the use of which is legitimized by the feminist methodology) as a graduate student of sociology, and Smith's analyses of text-reader relationships and the "conventions" of writing in sociology (see Chapter 3) suggest that there are. In looking at the ways of communicating sociology, I will look at their effects and I will study from bottom up, following Foucault's genealogy or Smith's institutional ethnography—which seems to be just a different naming in this sense. We can conceptualize texts in relation to how readers hear them, through their format, and from the touching points of the texts in our lives, as argued by Bakhtin. Here, the distinction Smith makes between the text and the rest, and the writer and the reader, while keeping the text as the medium that connects them into each other is useful.⁷ Following Volosinov, Bakhtin and Smith (and not Foucault here) I will take dialogue as a central concept in understanding texts and communication, although my focus remains on the writing side of it (the format) rather than the reading (the everyday world). However, here I will not be looking at the text itself or the discourse as Smith and Foucault do. Instead I want to focus on

⁷ The problem is, as Erdoğan Yıldırım pointed out in discussing previous drafts of this study, if experience is conditioned by the discourse, then how can we understand the discourse from the way it is experienced? This involves an isolation of theory from practice, texts from everyday experience, or power from its effects.

the format of the texts. Neither Foucault nor Smith focuses on this aspect of texts or discourses, but their works provide a framework to study it.

Smith says that there are “conventions” in writing sociology, “methods of writing the social into texts, making them recognizable to readers as sociology, and generating the phenomenal worlds that organize the multiple theoretical enclaves of the discourse.” (1999:46). However, throughout her works, Dorothy Smith neither questions the format of sociology texts nor leaves it or changes it.⁸ Proposing another way of doing sociology based on “the everyday experience” still doesn’t change the way it is communicated. Is the format, or the conventional ways of expression for sociology able to reach the “everyday life” of a person who is not educated in sociology or a familiar branch of science?

When I gave an earlier draft of this chapter to my thesis advisor Helga Tiliç, one of the important comments she made was this: Smith says she proposes a sociology that is built on the everyday life, she bases this proposal on the feeling of exclusion she experienced in sociology, you say you take this as your framework, and then you say you will look at texts, at how sociology speaks in texts. If texts are excluding the everyday experience, then how are you doing what you say you will be doing? How does this come from the lived experience? How does it relate to that? Oh, I see, I said; to say you don’t want to be detached is not enough not to be

⁸ We know, however, that there are scholars who also write social sciences in forms other than scholarly books or articles. One such book that I had read was *Dancing In Damascus* (2000) by Nancy Lindisfarne, comprising of stories she wrote based on her ethnographic studies with the educated upper-class women in Damascus, Syria. *The Innocent Anthropologist* (1983) by Nigel Barley, tells about his own experiences as an anthropologist in the field in a rather funny way. Another example is J.M.Coetzee’s novel *Foe* (1987[1986]). When you know about postcolonial theory, you can see that this novel is relevant for the discussions within it, and that it does have theory in it (DiMichele 1996, Spivak 1991). If you haven’t heard about postcolonial theory, you can still read and understand the book, interpret it your personal way, which can never be “false”. In reading Coetzee’s *Foe*, a reader who is not familiar with sociology may nevertheless get the ideas written down in some scholarly texts of sociology, although the book is not written in an effort to “diffuse” the discussions of postcolonial theory or sociology to the non-scholarly readers; it has no emancipatory aims. But the book itself is a contribution to theoretical discussions, although it is not put forward in the format of a scholarly text of sociology. Coetzee’s book is also read by the scholars of the field and discussed by them in scholarly articles.

detached. I think the question above is not only about the theoretical framework of this thesis but to the proposals of Smith in general. Now I think I can respond by saying that my experiences had been the starting point for this thesis. I wanted to look at how sociology speaks because of the discomfort I felt with that speaking as I took part in field researches in Turkey. Smith's proposal to look at the relations that organize our lives through the way we experience them thus provides a link to the everyday world, and when these relations are organized through texts (as she proposes for all ruling relations, and I find relevant at least for my case with speaking in sociology) it legitimizes studying texts to understand the actual world. This is what institutional ethnography proposes to do. Although I criticize Smith for not speaking to outside the community of sociologists whose relations are organized by the scholarly texts because she doesn't deal with the format, I do think that her theory is linked to the everyday world from its starting point in the experience. For this thesis, the focus group discussions with graduate students have been another link with the everyday experience (see Chapter 3).

Dorothy Smith's theory doesn't propose that we write about our experiences like we're writing diaries or novels or biographies. She proposes that we take them as the starting point to look at the organization that constructs, enables or affects that experience. "The aim is not to explain people's behaviour but to be able to explain to them/ourselves the socially organized powers in which their/our activities contribute." (Smith 1999:8). So I look at the texts, or rather at their format, because it was the texts and mainly their format that "organized" the discomfort I and others feel at producing written materials that are read mainly by the scholars.

Smith's concept of the text-reader conversation aims to link texts to the rest, the actual world where we read them. However, this conceptualization presupposes that there is a relation between the text and the reader, which gives no space to any possibility that a text cannot or does not communicate with the reader or a certain group of readers (or she takes non-relation also to be a type of communication),

although Smith herself had started from the experience of not being a part of established sociology. Smith loses the focus on who the reader to the text is, which, I think, can be an important component of the conversation. Thus, I will try to look at sociology texts with the method of institutional ethnography as proposed above by Smith, but I would like to rewind it a step back, taking communication with texts as a possibility rather than something taken-for-granted. Taking this communication for granted, I think, is one of the problems of writing in the scholarly world of sociology as well as the theoretical frameworks built to look at it. So I will try to insert the possibility that texts might not be speaking to everyone into this final version of Smith's sociology. In fact, in those first works of Smith that seemed to me to be written more intuitively and more enthusiastically, the "feeling" of not being addressed and not being listened to have been an important starting point in her effort to establish a sociology *for* women. In the end I think her works look more *established*, like the "established sociology" she criticized in the beginning.

I think there are formalized ways determining how one should be speaking in sociology, there are written and unwritten rules you have to know and keep in mind when you are writing a thesis or a paper, if you want your text to be legitimately accepted as a text of sociology. This has an effect on the conversation of sociology through its texts. Historians study the format of texts from the history of writing and of the book to and through the development of the print media, the print revolution in Europe (Trujillo Moreno 2008, Olson 1998). Discourse analysis in general and Smith and Foucault as well, don't incorporate this type of investigations. In this thesis I want to look at the development of the format of communication within the academy; namely that of the academic texts, more specifically the scholarly articles and books in sociology. In the next chapter these aspects of writing in sociology and anthropology will be discussed in their relations to doing science at large, based both on written sources and experiences of graduate students as they have expressed in focus group discussions. In Chapter 4 I will be presenting some important arguments of the self-reflexive critique in sociology and anthropology with an

attention to what they have said about the relations of their disciplines to audiences, and the established ways of communication of the discipline. Then, in Chapter 5 I will be discussing how this format of expression has come to be. History of sciences has important contributions to the discussion. I will try to make use of such studies like *The Structure of Scientific Revolutions* by Thomas Kuhn to look at the features of communication in and from the scholarly world of sociology, in which he states that when a paradigm is settled a discipline is established and only then the scholars of that discipline start writing papers that mainly address those who are within the field, instead of writing books that intend for the reading of the general public. I will follow Michel Foucault and Dorothy Smith in looking at the format of sociology texts, especially papers and books, as acts in their effects, organizing the everyday experience.

CHAPTER 3: Doing Science

“Nowadays in circles of youth there is a widespread notion that science has become a problem of calculation, fabricated in laboratories or statistical filling systems just as ‘in a factory,’ a calculation involving only the intellect and not one’s ‘heart and soul.’ First of all one must say that such comments lack all clarity about what goes on in a factory or in a laboratory. In both some idea has to occur to someone’s mind, and it has to be a correct idea, if one is to accomplish anything worthy. And such intuition cannot be forced.”

Max Weber 1919
(Weber 1995[1948]:136)

Discussing the format of expression employed in sociology and anthropology requires a brief discussion on how sociology works as a science. This chapter is thus intended to provide a glance at some aspects of doing science, both to understand the ways of expressing sociology and to lay a ground for the further discussion on reflexivity in sociology and anthropology. To follow the lead of Antonio Gramsci:

The problem of what ‘science’ itself is has to be posed. Is not science itself ‘political activity’ and political thought, in as much as it transforms men, and makes them different from what they were before? (Gramsci 1971:244).

This chapter aims to discuss sociology and anthropology as sciences in their historicity and through the ways they have been practiced, and the relations of science with life at large. Here science will be discussed as a historical, economic and political act; not through a thorough literature review or a detailed discussion but just as setting a background to the further discussions of this thesis. Thus it doesn’t include a presentation of various perspectives in sociology and their particular stances on how sociology should be studied, or their critiques to other schools of sociology.

3.1. Claims to Science

Science, briefly, is a type of knowledge, or better, a specific way of knowing. It is this “way” of knowing, the “scientific method” that makes science different from other knowledges. The method, and the emphasis on method, is what makes science science. The claim on behalf of science that it is certain and thus somewhat superior to other ways of knowing (Gulbenkian Commission 1996:5), generally rests on the specific methods of knowing science employs. It is these methods that are claimed to make scientific knowledge more systematic, reliable, valid, objective and universal, with respect to more local, particular, individual knowledges as well as knowledges based on traditions, subjective experiences or various types of authority, from religion and law to the fathers words (Neumann 2006). The “legitimacy” of science also rests on such assertions. And it is this claim of superiority and legitimacy of science over “other”, “unsystematic” or “invalid” ways of knowing that irritates Foucault and makes him promote genealogy as an uprising of knowledges against science: In the moment that a subject claims that he holds a scientific discourse and is a scientist, which other knowing subject does he intend to attach unimportance? (Foucault, 2004:26). Saying a knowledge, science, is legitimate with respect to other knowledges is an exercise of power and authority through the very claim. In thinking about science,

there had always been an unexpressed but quite real assumption. It had been implied that science was more rational, “harder” and more precise, more powerful, more serious, more efficacious, and therefore more consequential than philosophy or arts and letters. The latent premise was that it was somehow more modern, more European, and more masculine. [...]

Basically, the same issue emerged in the question sometimes framed as the local versus the universal, sometimes framed as agency versus structure. Structures/ the universal were asserted to be impersonal, eternal, or at least very long-lasting, and beyond control by human effort—but not quite beyond everyone’s control: structures seemed manipulable by rational, scientific experts, but not by ordinary

people, and not by groups that were less powerful within the structures.
(Gulbenkian Commission 1996:65-66)

Antonio Gramsci (who is not a “scientist” by profession, but a partisan) says that every theory bears speculation, and criticism “must resolve speculation into its real terms as a political ideology and an instrument of practical action.” (1971:370). Skepticism, an important part of scientific methodology, “is connected with vulgar materialism and positivism” according to Gramsci (1971:374).

Science is linked to the idea of progress (Weber 1995[1948]). This profoundly distinguishes it from art, as Max Weber points out: scientific works are intended to “surpass” the accomplishments made before them, and then to be “antiquated” and surpassed by other works, in a progressive quest for the better: “We cannot work without hoping others will advance further than we have.” (Weber 1995:138)

Weber argues that science is a part of “the process of intellectualization” (1995:138). Intellectualization and rationalization, historically rooted in the West, means “the knowledge or belief that if one but wished one *could* learn it anytime.” (Weber 1995:139, original emphasis). Science, as an outcome and a constituent of the trend of intellectualization and rationalization, rests on the assumption that humans are capable of understanding anything that goes on in this world with their intellect, which means a “disenchantment of the world” in Max Weber’s famous words. The second premise of science is the Cartesian dualism that assumes a distinction between nature and humans, matter and mind, the physical world and the social/spiritual world, and subject (knower) and the object (known) (Gulbenkian Commission 1996:2, Özlem 1998b). Scientific method is what systematizes these. It operates with some tools, of which Weber specifies “the concept” as the first one, that he traces back to Plato’s *Republic*, and “the rational experiment” as the second, achievement of which as a “principle of research” he attributes to the Renaissance.

Science answers questions in certain systematized ways of its own. But not all the questions are valid to be asked within the sphere of science. Science validates some questions, and thus some fall to a realm considered illegitimate to science. Medicine, as the example Weber (1995) provides, doesn't ask whether life is worth living, but it asks how life can be made to go on; aesthetics takes the existence of works of art for given, it doesn't ask whether they should exist. In other words, there is no scientific explanation of the choice to do science, because there is no scientific way to "prove" or to "argue" that sciences *should* be practiced, or that this or that individual should be a part of a scientific practice and a scientific community.

Modern science is also characterized with its obsession with "exactitude" (Nalbantoğlu 2009: 70). With modernity, if the social world, "order" and "social change" was also to be organized and rationalized, then it first had to be "understood" scientifically:

There was not only space for, but a deep social need for, what we have come to call social science. Furthermore, it seemed to follow that if one were to try to organize a new social order on a stable base, the more exact (or "positive") the science, perhaps the better. (Gulbenkian Commission 1996:8-9).

Thus "social sciences" imitated physics, which represented the domination of science over "philosophy", reflected in the belittling of the faculties of philosophy in the universities at the advent of the faculties of natural sciences and then engineering. As history as an "ideographic" discipline (that was not after general rules, but recognized events in their uniqueness and specificity) was closer to faculties of arts and letters, "nomothetic" social sciences were closer to natural sciences by their establishment. Doğan Özlem (1998b:58) points out that those who used the name "social sciences" were those who adopted a universalist perspective in contradistinction to particularism or any ideographic approach. He thus criticizes "social sciences" from the perspective of hermeneutics. August Comte who "invented" the name "sociology", aimed it to be a "positivist" science of "social

physics”. Such tendencies were power-laden and politically charged throughout the constitution and practice of “social sciences.” The local and particular Western viewpoint in all its historicity was presented as universal through social sciences (Özlem 1998a, 1998b). The very method of social sciences disguises its own historicity (Özlem 1998a). Universalism is not only an epistemological standpoint, but also a mentality, of those who believe that everything has a single truth and that they can understand and control it, wanting it to be held true by all others (which Karl Popper sees as the source of all totalitarianisms and despotisms) (Özlem 1998b:63).

Social scientists, no less than political and religious leaders, have missions; they seek the universal acceptance of certain practices in the belief that this will maximize the possibility of achieving certain ends, such as knowing the truth. Under the banner of the universality of science, they seek to define the forms of knowledge that are scientifically legitimate and those that fall outside the pale of acceptability. Because the dominant ideologies defined themselves as reflecting and incarnating reason, both presiding over action and determining presumptively universal paradigms, to reject these views was said to be choosing “adventure” over “science” and seemed to imply opting for uncertainty over intellectual and spiritual security. (Gulbenkian Commission 1996:53).

In short, universalism itself is historical and particular. Criticizing social sciences by saying it is in fact parochial thus lies on science’s claim of universality. It is this claim that makes it valid to criticize sciences by saying that some people are excluded. Doğan Özlem criticizes the Gulbenkian Commission for trying to strengthen the ideal of universalism by incorporating the excluded others, instead of recognizing its impossibility and “incorrectness”, and abolishing it altogether on behalf of particularism. (In the discussion on universalism here I wish that my intent is not taken as an effort to strengthen the ideal of universalism in a similar sense.) The Commission, it seems to me, also aims to defend and strengthen social sciences’ claims to objectivity (after all the commission was an initiative to reorganize science, one can also claim that it’s a defense against postmodernism).

Would giving up on objectivity mean to give up on science? There are approaches (like that of Özlem's) that say it won't. (The discussion will be carried out in more detail in Chapter 4.)

The social world was studied in a division of “disciplines” of science. The classification of these rested upon the antinomies between studies of past and present, idiographic and nomothetic methods, and the objects of study as the west and the rest. Though many of these antinomies have been challenged and rejected, the disciplines still bear the traditions built on them. One reason is that being part of an established discipline gives a sense of safety to its practitioners (Gulbenkian Commission 1996:97). As Foucault says, “Disciplines constitute a system of control in the production of discourse, fixing its limits through the action of an identity taking the form of a permanent reactivation of the rules.” (1972:224). For Neale,

the division into disciplines is perhaps the most important way that ideology confuses social scientists and makes many of their observations trivial and irrelevant. [...] There is no economy separate from class and the state, no family insulated from power and money. (2008:231).

If we look at the topics of this thesis, sociology and anthropology, very broadly we can say that sociology, rooted in the will to understand and organize the “modern” Western life with all the urbanization, migration, industrialization etc., studied the modern “self”, while anthropology studied the “other”, “discovered”, dominated and colonized by the West. The method of “participant observation” employed by anthropology from its onset can be seen as an “inevitable” result of its birth in this strange encounter (Gulbenkian Commission 1996:21). This method “always threatened to violate the ideal of scientific neutrality” (ibid.). The Gulbenkian Commission specifies the anchoring of anthropologists in the university as the most important fact to keep ethnography within the normativity of science (1996:22). Sociology and anthropology were criticized for being bourgeois endeavors or for their Eurocentrism, masculinism, objectivism and universalism, as, unlike the relationship natural sciences have with their objects of study, in social sciences

people are under study, and it potentially makes a sense when they object to social sciences by saying “this doesn’t reflect me” (Gulbenkian Commission 1996). Between 1850 and 1945, the disciplines of “social sciences” were institutionalized by establishing chairs and departments in the universities providing degrees to disciplines, and also

by the institutionalization of research: the creation of journals specialized in each of the disciplines; the construction of associations of scholars along disciplinary lines (first national, then international); the creation of library collections catalogued by disciplines. (Gulbenkian Commission 1996:30).

This institutionalization of disciplines, together with the fact that they had institutionalized in the universities as a profession for the academics, created a demand for segregation and specialization. However, this practice has been contested by scientists themselves, which has become a strong trend with specific outcomes from time to time (like forming various interdisciplinary programs). Change brought by such criticism has often been in the form of adaptations, “a continuous fine tuning of both the universal lessons supposedly transmitted and the ways in which they are transmitted” (Gulbenkian Commission 1996:49). Thus, once a discipline is institutionalized, it hardly gives up on its foundational premises and claims (see Gulbenkian Commission 1996 for a discussion of critiques to social sciences and the changes they brought in doing social sciences, and Özlem 1998a, 1998b for a critique of social sciences from the perspective of hermeneutics). Nevertheless, we can say that separations and tensions between natural sciences, social sciences and humanities are now softer, more diminished.

3.2. Science as a Vocation

The “scientific method” is not the only “structured” side of science. In practice, science is carried on in institutions. University is still the major institution to do social sciences (Özlem 1998b:65). Through such institutions, science is carried on as a profession, which also has implications on it. Science doesn’t go on by itself on

an ideal or imaginary plane. It is carried out by scientists in this world with all its historicity. Through institutions and through scientists, sciences are linked to political interests, capitalist relations or ethical questions, and the relations of working and teaching. It is the organization of science as a practice (learning, teaching, and producing it—mostly through writings) that reproduces its assumptions and claims. We are not talking about science as a *sui generis* entity here, but about *doing* science.

These aspects of science are pointed out by Max Weber in a speech delivered to students in 1919, named “Science as a Vocation” (Weber 1995). Weber points out that the need to fund research in science inevitably makes it a capitalist institution, where “we encounter the same condition that is found wherever capitalist enterprise comes into operation: the ‘separation of the worker from his means of production.’” (1995:131). The scientist is not just a researcher, but also an employee, an assistant or a professor, and a teacher, more or less dependent to the head of the department or the university administration and state and capitalist relations in general as much as any worker, and thus in a precarious situation. “Anthropologists often write as if they worked for a discipline and their job was called anthropologist. In fact, the great majority are teachers in universities and colleges.” (Neale 2008:226). For Jonathan Neale, a radical anthropologist, universities do three important things in the capitalist system: with the division of disciplines, they justify the division of labor in society, they train new professionals useful to the system, and they “confuse people about reality in order to keep the capitalist system going.” (Neale 2008:227). Through fundings, various institutions with their own interests have many words to say on what will be researched and what will be produced as knowledge:

Universities are run by the government, by the Church, by boards of rich people, or by some combination of the above. More important, in Marx’s phrase, ‘the ruling ideas are the ideas of the ruling class.’ (Neale 2008:237)

What is done as science is very often linked to what is learned by the scientist in life and in the university. What is learned and taught in the university is also social and historical, from the selection, design and teaching of courses to the determination of fields and the formation of the disciplines. Teaching is a very important part of being employed in the university and thus being able to carry on research; however, being a good teacher and being a good scholar are two different things (Weber 1995).

For Ünal Nalbantoğlu, the world of university is rooted in the ideal of *universitas*, but university-the-institution is drifting apart from it. Nalbantoğlu's focus is often on how science is practiced in the academic scene; against the degeneration of universities and the loss of position of philosophy, he makes a defense of the ideal of *universitas*. He asks how free thinking, an inseparable part of the *universitas*, can be continued in the universities of today which are more and more tied to the world integrated to markets and technologies. University, after all, is a modern institution (Nalbantoğlu 2009:28). It often works as an ideological state apparatus (Althusser 1972[1971], Nalbantoğlu 2009:30). Universities in many places are under the pressure of the state power; even if this is not very visible at “easy” times, you know that that coercive force exists and will be enacted “if necessary.” The “autonomy” of the university is often not gained through struggles, but granted like a gift that can be taken away (Nalbantoğlu 2009:52), as we have experienced in Turkey with various state interventions and military coups and *YÖK* (Institution for Higher Education), the very institution of state control over universities. (As just a little symbolic example, during the time that I have been a graduate student at METU (2005-2009), at the same time of every day there were military helicopters flying over the campus; I can't speculate here on where they go.)

At the same time, the marketing relations turn knowledge into a commodity to be sold, and rather require technical abilities and “information” from the scholars than “thinking” or “knowledge”, management and engineering rather than philosophy or

humanities (Nalbantoğlu 2009:31). The loss of philosophy in the modern universities brings as a consequence to the other disciplines that they become more and more the slaves of the techno-science world metaphysics (Nalbantoğlu 2009:32). By early twentieth century Weber was saying that “science has entered a phase of specialization previously unknown and that this will forever remain the case.” (Weber 1995:132). Thus, acquiring something in science meant, and often still means, being a specialist in a field.

Rationality, the principle that the university is built on, can turn into a tool for anything. Ünal Nalbantoğlu, in his “Cultures of Modernity” lectures (at the Sociology Department of METU, 2007-2008 spring semester), had told that although the roots of university lied in everyday practices, thanks to doing abstractions, it had lost its roots and become a realm of fetishisation of theory. University now has turned into a “mass university” rather than an institution training free-thinking people. Specialization, and the practical question of “what is it good for?” have contributed to the making of scholars-as-“white-collar-workers” (Nalbantoğlu 2009). Against all these, both Özlem (1998a) and Nalbantoğlu (2009) argue that scholars should represent and defend the *universitas* in their universities. In the long and hard road to produce knowledge in freedom and to share it with others, to overcome the difficulties as those cited above, Nalbantoğlu says we should struggle to speak a different language and change the organizational patterns of science accordingly.

According to Özlem (1998a), science today is produced in the Anglo-American world to a great extent, and through certain formats controlled by this center, while the “periphery” is expected to consume science and follow it, but not to lead it. The standardizations brought especially by scholarly journals serve to maintain this relationship. Academics of the periphery may send articles written in the “universal” language of science (mostly English) and get their works published in journals that appear in the citation indexes, they can thus be promoted in their own

countries. But the material and intellectual income of this effort is gone to the big specialized business of scholarly publishing developed in the center countries, which is ready to serve the world. A stronger mechanism of control on the organizational structure of sciences than being trained in a discipline has been “the fact that disciplines have controlled the career patterns of scholars”: positions in universities by and large require a doctorate or an equivalent of it, publication in official or semi-official journals of a discipline is the other must of career advancement in the scholarly world. (Gulbenkian Commission 1996:71). Funding research is yet another mechanism of domination and control in the academy.

On the other hand, we can talk about an “*invard* calling for science” (Weber 1995:134). There is this *daimon*, an irresistible force that draws one into thinking, which Nalbantoğlu (2009) describes as something that doesn’t *happen* to everyone. “Intuition cannot be forced” says Weber (1995:135). In doing science there is this tension between the social organization of science and the “intuition”, the “calling”, or the “daimon” that scientist as a humanbeing has or has not. For Weber inspiration is as important in science as it is in arts:

Ideas occur to us when they please, not when it pleases us. The idea is not a substitute for work; and work, in turn, cannot substitute for or compel an idea, just as little as enthusiasm can. Both, enthusiasm and work, and above all both of them *jointly*, can entice the idea. (Weber 1995:136, original emphasis).

Thus doing science involves a risk: “does an idea occur or does it not?” (Weber 1995:136). But of course nobody has to take the risk, one doesn’t have to be a scientist if s/he doesn’t want to; but if one has “chosen” to be one, s/he is expected to fulfill the requirements it brings (Nalbantoğlu 2009: 156). However, the question “who can become a scientist?” is not only about inspiration, risks and responsibilities. It is also related to one’s background, her class position or her cultural capital (Bourdieu 1988 [1984]). Although it seems that jobs are rationed on the basis of education and qualities, “[i]n practice, how well people do in education is more dependent on who their parents are and how much money they have than it

is on anything else.” (Neale 2008:227). Academic circles and positions in the universities tend to be more open or available to those born to educated families; it is much easier for the daughter of a professor to become a scholar than the worker or the villager or their children. And the answers to the question “scientific knowledge is produced by whom?” are certainly meaningful to what is produced as knowledge (see the discussion below).

Max Weber discusses teaching as part of the job of a scientist. He says that a “useful” teacher should teach his students to distinguish between the “convenient” and “inconvenient” facts to serve for their “parties.” In the disenchanted world of our times, it is an error of the youth to expect from the teacher something more than “mere analyses and just statements of fact”, to seek in him something more than a teacher, to seek in him a leader (Weber 1995:149). Weber says that

The American’s conception of the teacher who faces him is: he sells me his knowledge and his methods for my father’s money, just as the greengrocer sells my mother cabbage. And that is all. (1995:149)

In teaching, the quote above probably describes an experience of “poor teaching”:

Poor teaching, pedagogic routine, a style of instruction which is, consciously or not, cynical in its merely utilitarian aims, are ruinous. They tear up hope by its roots. Bad teaching is, almost literally, murderous and, metaphorically, a sin. It diminishes the student, it reduces to great inanity the subject being presented. It drips into the child’s or adult’s sensibility that most corrosive of acids, boredom, the marsh gas of ennui. (Steiner 2003:18⁹, quoted in Nalbantoğlu 2009:82)

Nalbantoğlu writes that a lecture delivered to a crowded “mass” is not a real talk or a dialogue, but rather a monologue of the teacher (2009:78). Nevertheless, the *daimon* has a place in any oral communication, where even mistakes bear creativity and bore insights into the speakers. Word that becomes script, however, no matter

⁹ Steiner, George. 2003. *Lessons of the Masters*, Cambridge, Mass.: Harvard University Press.

how satisfactory it is, doesn't listen to the person who reads it (Nalbantoğlu 2009:80).

Personal values or opinions of the scientist / teacher surely exist, but for Weber the classroom is not the place to demonstrate these and be the leader that the students apparently expect, although it is a situation present in the academy (1995:150).¹⁰

This is a political and ethical question:

Some claim that 'science' cannot be moral lest it lose its neutrality and autonomy as a distinct realm of knowledge (Hammersley 2005¹¹). This has stood against the claim that science is always already culturally and historically situated, and that disinterested knowledge is neither possible nor desirable. (Armbruster 2008:4).

In this sense, Nancy Lindisfarne argues that "inaction is nothing more than political action that supports prevailing [...] policies." (2008:24). However, as one of the "fathers" of sociology Max Weber argues that one's activism should be kept out of his/her scientific work (this is a point contested by others, as seen above, nevertheless it has many proponents), and asks what science contributes to practical life then. Firstly, "science contributes to the technology of controlling life by calculating external objects as well as man's activities" (Weber 1995:150), which is basically technical and thus being a scientist is just like being a greengrocer in this sense. But "methods of thinking, the tools and the training of thought" and "clarity" are, for Weber, and certainly for many others, things that sciences can provide but the greengrocer can't (1995:150). The "end" of a process is given to the technician,

¹⁰ "Fellow students! You come to our lectures and demand from us the qualities of leadership, and you fail to realize in advance that of hundred professors at least ninety-nine do not and must not claim to be football masters in the vital problems of life, or even to be 'leaders' in matters of conduct. [...] For those who most frequently think of themselves as leaders often qualify least as leaders. [...] And if [the professor] feels called upon to intervene in the struggles of world views and party opinions, he may do so outside, in the market place, in the press, in meetings, in associations, wherever he wishes. But after all, it is somewhat too convenient to demonstrate one's courage in taking a stand where the audience and possible opponents are condemned to silence." (Weber 1995:150).

¹¹ Hammersley, Martin. 2005. "Is Social Research Political?" in C. Pole (ed.), *Fieldwork. Volume III: Ethics and Politics in Fieldwork* (Sage Benchmarks in Social Research Methods), London: Sage.

but it shouldn't be given to the scientist. Whether science is a worthwhile vocation is certainly a value judgment that Weber thinks should be kept out of the lecture room, but he says he is affirmative of this question for himself by the very work that he has done. But "[n]o science is absolutely free from presuppositions, and no science can prove its fundamental value to the man who rejects these presuppositions." (Weber 1995:153).

When we are talking about science as a practice, the intellectual role and responsibilities of the scientist becomes an important question. This is especially important with sciences that deal with people, and Max Weber's views above are only one side of the discussion. Theories may be crucial for the hegemony of a certain group (Gramsci 1971) and those (like feminists and Marxists such as Antonio Gramsci, Wright Mills, Edward Said or Pierre Bourdieu) who advocate that the intellectual or activist side of a scientist is inseparable from what s/he does in the field of sciences emphasize theories of praxis or the collision / inseparability of theory and practice. Gramsci (1971) emphasizes the importance of the role of an intellectual who comes from within a class (the organic intellectual) for that class to be hegemonic (acquiring consent). This takes us to the class dimension of doing science. If scientific / "legitimate" knowledge is produced dominantly or exclusively by a certain class or group of people, what does this mean for the others? In Gramscian thinking this is also a form of domination. Whether sciences, or sociology as the topic of this thesis, are open to the knowing or contribution to anybody, or the question of publicity of science, becomes a crucial one in this sense. Foucault's claims that power is transmitted through discourses including the scientific one imply that leftist discourses of liberation are also parts of the economy of power (Armbruster 2008:10). In any case, taking sides is a part of doing science.

Another question we can ask is what the method of knowing brings as a consequence to the content of the knowledge, politically and epistemologically. The

scientific method can be taken as a structure, which the format of expression also is a part, and which has implications on the knowing itself. The next section on the format of written expression in science will be followed by focus group discussions on the topic with those who are newly starting to do science (the graduate students of sociology and anthropology).

3.3. The Format of Written Expression in Science and Sociology

Being published in a certain place and in a certain format legitimizes a text as a text of sociology. Authority appears “as the differences between the credibility granted to some sources and the treatment of others as mere opinion or as lacking credibility in some way.” (Smith 1990:101). In studying sociology, we learn to become sociologists, “we learn to think sociology as it is thought and to practice it as it is practiced.” (Smith 1990:15). Therefore the primary world of sociologists as professionals is already made up (1990:54). Dorothy Smith thinks that this process is a sort of conceptual imperialism, where the limits of inquiry are drawn within the already established framework (1990:15-16). Although sociological procedures claim to be referential to the social reality, they often “legislate a reality rather than discover one.” (1990:53). “Concepts and categories reflect social relations mediated and organized by concepts and categories. [...] the universe on paper that social scientists encounter and rely on is *already ideologically structured*.” (1990:57, emphasis in the original).

Ünal Nalbantoğlu writes that there is this language prevailing in “scientific” writings and meetings, which was constituted with the desire for objectivity in the beginning, but now has turned to an ordinary and boring requirement (2009:143-144). Smith says there are “conventions” in writing sociology, methods that make a writing of the social world recognizable as sociology to the readers. In order to be admitted as a “member” to sociological writings, an account has to provide a legitimization, presenting that it has a proper claim for representation. The account

is in the “textual time” which hides the construction of itself and its factuality; it is fixed and certain. Publication is one important process that fixes an account in an official form and makes it the same for every reader in each reading. I would like to add to this assertion that *where* a text is published is an important aspect of its communication, and a determinant of its format. As stated before, making publications in scholarly journals is a must for scientists employed in universities, and it is the main written way of communication in sociology. This principle of “publish or perish” often results in thoughtless researches and papers written for prestige more than anything else (Nalbantoğlu 2009).

If we look at how sociologists communicate their work through articles sent to journals, we encounter a very strict format. There is an Institute for Scientific Information (ISI) which keeps the “Web of Science citation index.” (<http://science.thomsonreuters.com/press/2009/8499916>). Journals are ranked by the citations the articles published in them get, according to different measures. The “impact factor” of is one measure of a journals “influence”:

It is a weighted measure – of citations per paper – and as such it is an attempt to compare journals of the same subject area that publish different numbers of papers each year. Journals producing many articles would typically attract more citations than those publishing comparatively fewer articles. The impact factor is calculated as citations in year C to a journal’s contents in years A and B, divided by the number of regular articles and reviews published in years A and B. (www.timeshighereducation.co.uk).

Even the measure itself is set according to a journals impact *within* the academic field and through its own ways of communication. In *American Journal of Sociology*, which is at the top of the ranking for journals of sociology by its “impact factor” in 2007 (ibid.), the format of an article is described as such:

Text: Readable copy for the purposes of peer review is set in a serif typeface (e.g., Times Roman or Courier) at a font no smaller than 11 points (we prefer 12). Manuscript text should be double spaced with margins of at least one inch all

around the page. In addition to the main text, every submission must include (1) an abstract, (2) a reference list, and any (3) notes, (4) tables, or (5) figures mentioned in the text. *A word count that includes text, notes, and references must be included in the cover letter.* While *AJS* does not have any word-count limit, we encourage authors to be as concise as possible. Authors should note that many referees balk at reading papers larger than 10,000 words (i.e., 50 pages at 200 words per page). Please organize your paper so that the elements are gathered in this order: abstract, text, references, notes, figures, and tables. If your paper places figures and tables where they are discussed in the text rather than at the back of the manuscript, the *AJS* editorial board *may decline to read it.*

Abstract: Your abstract should be as close to 100 words as possible. It should include your research question or puzzle, identify your data, and give some indication of your findings. Your abstract is likely to be sent by email to potential readers: giving an accurate and efficient statement of your project is likely to increase your chances of enlisting their aid. Unfocused, verbose abstracts may make it harder to place your paper with referees.

2. References: *AJS* uses the author/date style of references, but it also allows notes for substantive commentary (see below). *Papers without a reference list will not be sent to readers for peer review.* [...] ¹²
(<http://www.journals.uchicago.edu/page/ajs/instruct.html>, emphases in the original)

The limits to the number of words an article should have also limits to what it tells about and how it tells it. This is quite a structured way of communicating something, implying that there is a single meaning in the sentence that should be understood, in contradistinction to ways of expression in arts, for instance, that are often open to multiple meanings and multiple understandings, or intended for personal interpretations. The “scientific” way of speaking in sociology and

¹² Similar requirements for the journals second and third on their impact factors, *American Sociological Review* and *British Journal of Sociology* can be found at <http://www2.asanet.org/journals/asr/submission.html> and <http://www.wiley.com/bw/submit.asp?ref=0007-1315&site=1> respectively. TÜBİTAK – ULAKBİM provides a list of the “qualified periodicals” published in Turkey that are in the Web of Science citation index of ISI by May 2009 (http://www.ulakbim.gov.tr/cabim/vt/uvvt/isi_dergiler.uhtml). Among these 70 periodicals, none are directly related to sociology or anthropology.

anthropology texts also often makes references to prior knowledge of the topic. Thus there is an implication in these texts that they are not written for other people who are outside of the realm of science.

The texts mark the limits of the scientific field, with all the (...), [...], (sic), (!), (?), (i.e. xxx 2009), (cf. xxx), quoted in xxxx, original emphasis, my emphasis, personal connection, (xxxx 2009:xvi), (xxxx and yyyy), (xxx et al.), ^{17, 35, 90, +, *, **}, art., vol., a priori, ad hoc, etc., etc. Like “a tone of feathers” that Caplan (1993) argues for the pressure created through gender discrimination, the conventions of expressing oneself in the scholarly world create exclusions bit by bit. Scholarly journal articles, scholarly books on sociology, and sociology theses for masters and PhD have their norms, their particular format that makes them texts of sociology (see METU Thesis Manual 2009 as an example). Articles have their abstracts in the beginnings, and all serious scientific works end with references. Citing references have their own specific rules, and just like footnotes and endnotes (Burke 2000) this is not only about intellectual property rights, but it also has to do with “proving” that the cited information has sources.

In discussing the implications of writing, David Olson (1998) rejects the idea that writing is not a language but just a record of it. He argues that writing is not a representation of speech but it makes up another system of communication with its own modes of thought and perception. The format of communicating thus creates its own world, which, for writing in general, Olson calls “the world on paper”. Attempts to represent the world on the paper, he argues, have altered the very structure of knowledge (1998:xvii)¹³. Similarly, sociology and anthropology have their own worlds on papers, in which you learn to live and express yourself.

¹³ According to Olson, “our understanding of the world, that is our science, and our understanding of ourselves, that is, our psychology, are by-products of our ways of interpreting and creating written texts, of living in a world on paper.” (1998[1994]:19).

Olson also argues that “it was the printing press that made it possible for a multitude of readers actually to look at the same text at the same time” (1998:58). The printing press “was primarily an instrument of standardization, accumulation and dissemination” (Olson 1998:59). A main effect of a conventional format of expression is to stabilize the meaning. Accuracy and strictness are important ideals for scholarly texts. Writing is praised for its stability with the famous phrase in Latin: *verba volant scripta manent*, the word flies but the script remains. *Söz uçar yazı kalır*. Stability of meaning is a necessary end for scholarly texts, but having stability simultaneously means losing flexibility and losing the flight of the word. This stable meaning, the format also implies, can only be conveyed in *this* very format itself, of conveying meanings in science in general and in sociology in particular. The format presents itself as the unique way to write sociology, and it reproduces itself.

The format of writing sociology is not limited to the aspects provided above. The regular format for speaking in sociology and anthropology is not only “heard” in texts as you read them, but also in writing them. As will be discussed in the focus group interviews presented below, many graduate students of sociology and anthropology (including myself) have difficulties and pleasures in writing texts that are part of their graduate education and (possibly) their future profession. Other aspects of doing science, the format, and writing in sociology and anthropology will be discussed below with the valuable contributions of the graduate students.

3.4. Doing Sociology, Learning to do Sociology: From the Beginners

All that is said above on science and how it is practiced is from the world on papers. The authors cited there are themselves practitioners of sciences, sociologists, philosophers, historians and anthropologists (though, in a distinction from the trend of reflexive criticism discussed in the next chapter, the discussion here focuses on more general traits of science and its practicing, and criticism isn’t directed at the

authors' own identities as scientists). In this sense the discussion above is from the practice, but it is from the side of the faculty, the teachers. When our topic is the format of expression in sociology and anthropology, it is also interesting to look at how it is perceived and experienced by the students of the disciplines. After all, in learning to become sociologists and anthropologists, one has to learn to fulfill the requirements of the disciplines, which includes understanding and conforming to this format of expression. And, while the senior faculty staff is practicing the format for a long time, the students with their liminal status still have the outsider's ability to make striking observations on both the format and the academic scene in general. Undergraduate students may be yet too new for this, but the graduate students of sociology and anthropology tend to have more mature choices on their disciplines, and are beginning to practice sociology and anthropology as they do research for their own thesis. Incorporating the students' experiences to the discussion is also in line with Smith's (1987, 1990) proposal for institutional ethnography to try to understand the organization from the way it is experienced by the people.

For this end I have conducted two focus group interviews with graduate students of sociology and anthropology in September and October 2009, in Ankara. The respondents are master and doctorate students in the departments and programs of sociology, anthropology, ethnography and media and cultural studies of Ankara University and Middle East Technical University. They have received their undergraduate education from various departments of these and other universities. Some of them are currently employed in universities as research assistants. The educational backgrounds of the respondents or any other personal information (including the thesis topics) cannot be given in any more detail because it directly undiscovers their identities. Similarly, I will not be able to present many of the "delicious" examples they have given from their departments, as these are also often specific and personal. The names of the respondents are also changed with pseudo-names. The questions of the focus group discussion can be found in

Appendix 1. All interviews are recorded. Original Turkish quotations can be found in Appendix 2, where indicated in this text by numbers in parentheses.

The first group consisted of 6 people, 3 men and 3 women, all of whom are masters students just starting or about to finish their theses, and 5 of them employed as research assistants and one temporarily unemployed (Betül, Aylin, Zeliha, Ayşe, Murat, Levo). The second group also had 6 people, 4 women and 2 men, one of them almost done with her doctorate thesis and employed in the university, others continuing with their master's or doctorate studies, 2 of them currently employed (İlyas, Samet, Mualla, Cemal, Meryem and Leyla). The pseudo-names, some of which are chosen by the respondents themselves, don't reflect gender. All respondents are between 25 to 31 years of age. All participants are people I already knew, some being friends for years, with me and with each other. Many of them are activists and/or they have strong political opinions. In the script below I try to present what they have said in all the dynamism, contradictions and sincerity of the focus group discussions.

I would like to note again that this empirical data is not the main aim of this thesis. The aim here is to make a contribution to the discussion with the experiences of those who learn to practice science and thus enrich it. I would like to thank my advisor Helga Tılıç and committee members Erdoğan Yıldırım and Tayfun Atay for this valuable recommendation.

What kinds of texts do we call texts of sociology or anthropology? What is it that makes a text scientific? According to Murat, the texts produced on tribes by European conquistadors or Jesuits can be considered as data for sciences, but they aren't scientific texts, because they don't have explicitly scientific analyses. However they have contributions to the constitution of the literature of anthropology and thus influence what is a text of anthropology and what not.

Anthropology texts become more explicitly anthropology texts with the writings of Malinowski.

Ayşe recalled that in taking her first courses on sociology in her first days in her undergraduate education, when in the sociology textbook she was assigned she saw a photo of two women having a chat and read under it something like “this is how people act”, she was rather disappointed with the simplicity. Is this what she was to learn as sociology? Although at that time she looked down on the everydayness of this presentation, she now finds this type of a rootedness into the everyday life very meaningful and valuable. She says she personally finds an account presented in a more literary way much more delightful and useful than rigid texts with lots of tables and ratios.

Cemal thinks asking what kind of texts we consider scientific takes us to discussing what science is. He told that in discourse analysis, the concept “intertextuality” looks at how a text is constructed through including and excluding other texts, thus creating a social identity, as Norman Fairclough explains. The members’ resources that a person brings along in her background and the power relations in which a text is created is also important in the making of a text (1).

Mualla said that the formal properties of a science text catch her eye first:

In a scientific texts there are footnotes, there are quotations, there are some names and some numbers that seem to be years in parentheses.

Meryem: (laughing) and there should be.

Mualla: There should be, the more of these the better, but if you exaggerate then they say “what is it that *you* written?”, it will be that you didn’t write it. It takes such a form that the footnote is longer than the text itself. These are the formal aspects of it (2).

The properties of a science text are not limited to its formal aspects. İlyas said the need to express what you mean in theoretical contexts and in a presentation of

chapters and all such standardizations “empty” her, so that she can’t write anymore. Mualla pointed out that texts are taken more seriously when they are written in academic formats by people with academic titles. We write texts with the purpose of producing something scientific, as İlyas noted. Cemal said his criterion isn’t whether a text is scientific or not, he’s not interested in that. What he looks for in a text is how it makes use of discourses and creates them, and how it thus influences life. The more open a text is, the better you can make politics through it. Science is thus done for its implications in life, be it political, economic or whatever (3).

According to Betül the very question of whether a scientific text is distinct from other texts is rooted in the field of authority of science (authority as in saying that a person is “ignorant” when s/he doesn’t believe in what the doctor says). Cemal similarly said that this question attributes authority to science as it makes it a separate field of inquiry. Betül said science is very strongly alienated from the everyday life and science loves this very status, that’s why it wants to distinguish itself from other texts. This strengthens its authority by creating an “other” (4). Science is successful in doing this, according to Betül. Science makes you believe it is true through its apparatuses, its hegemony and its power (5). Scientific texts impose themselves through their methods by excluding other types of texts:

Because you add some data to something and claim this is scientific, do we all have to believe in this? Or say I want to read something on the history of the left in Turkey. Do I *have to* read Zürcher? Vedat Türkali’s novels are much more analytical. Now do I have to say to myself “no, Vedat Türkali hasn’t done any experiments or any observation of scientific quality, well he has no objective viewpoint he writes from the within because he’s a leftist; skip this text then, read Zürcher”? (Betül) (6).

How does science convince people that it is true? According to Betül the predictions science can make successfully is contribute to this convincing, when you see the concrete data in this sense, you believe in it and then you take in the rest as well. When Betül said science puts definite lines, Levo added science does this

by its format. Ayşe agreed, and Betül said that science does it with its method. “[Science] builds its format, builds its methodology and builds rationality” (Levo) (7). And then, as Leyla expressed in the second group, science authoritatively claims “this is what you are experiencing.” (8). When I asked the first group how they made the distinction between Vedat Türkali and Zürcher, they said that Türkali as a novelist communicated his observations through literature, he didn’t make quotations, he could leave rationality aside and talk about his own feelings, and could say “I”, and his works were not “checked on” (9). It was also stated that novelists were free to write fiction, even when they are talking about realistic groups, while scientific texts were institutionalized and that meant they are accountable, as Zeliha pointed out. The first group didn’t have an agreement on whether the mechanism of accountability in sciences is more valid than being responsible to the readers of a novel, as Betül and Aylin argued that the checking mechanisms in science exist precisely because they are manipulable. Zeliha pointed out that the same rigidness and control through method nevertheless exists in literature and theatre as well, while for Betül this is just another reflection of scientificity. Zeliha was doubtful and critical as always.

According to Aylin, we just shouldn’t say either science or other ways and expressions of knowing are better in reflecting realities, and Zeliha emphasizes that it is still important that one of them has a claim to reflect truth: it is a difference in assertiveness. An approved thesis is much more authoritative than just saying “I know this”, said Betül. İlyas argued that when some people make definitions and propose concepts and then the users and makers of those concepts and definitions are *scientists*, and because you can’t make this s/he becomes superior than you (10). Thus medicine becomes something superior to the herbs that your grandmother boils because it has the measures and the experiments, because it is science, and hence the scientist is separated from the non-scientist (İlyas).

A question that came out of the discussion is, if we are to give up on these claims of science, then why do we do it? Why don't we write novels? (Zeliha) "But then you won't have any relevancy within this system!" (Betül) (11). Even if you want to do something different, as a student you are have to fulfill some requirements, your thesis will not be accepted if you don't write it according to a format, and you thus reproduce it (Ayşe). But as Murat later stated, this is just what brings the legitimacy to what you do; creating a distinct realm may also increase the influence of your study (12). Besides, as Meryem says, reliability is important: "how can you say this? What is it based on?" are powerful and legitimate questions when you want to defend something. These attitudes settle down in the academics and in a while you're having difficulties in expressing yourself in other ways, which is something Meryem hates. This, moreover, is an international system (13). International criteria shape how you write here, and the very format of stabilization changes in time. The format is not an arbitrary shape according to Mualla. It is the result and reflection of a certain systematic way of thinking.

The participants weren't against theses having theoretical frameworks, but they had the idea that the reason a theoretical framework should exist is not because it is constrained so by the format; they were against the meaningless reproduction of mere formalizations. Leyla, who has been struggling with her thesis for the last 2,5 years, finds the efforts of standardization very absurd. You shouldn't write that Durkheim has said something just to show that you have read Durkheim, it should be there if it is meaningful to have it there (Ayşe). Otherwise, participants of the first discussion agree that one should know about what other people studying in the similar fields have been doing. They have complained a lot from writing theoretical chapters which have loose connections with research. İlyas, from the second group, said she has all her thesis written in her head, but she can't write it because she can't do it the way a thesis is expected to be. In the first group Ayşe said it would be nice to be able to communicate our theoretical and political choices (like being a Marxist or a functionalist) between the lines, in a mature fashion, but that we are

not yet able to do that as students, so we imitate the frames and formats, and this is also imposed on us. We don't always understand what is written from what kind of a perspective.¹⁴

Moreover, Samet points out that while we are complaining that the format constrains us, it is also not true that we would be making wonderful novelties as very original individuals, as if we ourselves are full of authenticities, etc. This is not like that in the academy, or in the everyday life. After all, we are also formed by the academy, we're not so independent from it. We're made in the academy and this is what we are, this is what we've got (14). As Samet later put it, this is why we make references, to acknowledge what is already there, already into what we think. Even when we criticize science, we use the concepts generated by science, and with references to works of science such as those of Foucault, Gramsci or Said, as Meryem put it. Moreover, we can't do it otherwise, because we are trained in this way (Meryem). Besides, it is here that we can produce hope, and this is why we are here, as Cemal, Samet and Meryem said in agreement. On the other hand, if you want to reach publics wider than the circles of scientists, writing a book that is in the native language and not in the scientific format will serve you better, according to İlyas (15).

Being suitable to the aim is another important criterion, as Zeliha pointed out: you can't blame a paper that aims to tell about demographical data for being full of tables, because that's what it aims to do. Acknowledging subjectivities, as proposed by reflexive critiques, may be a way to do better science, while at the same time destroying scientificity (Zeliha) (16).

Deadlines to turn papers in are also seen as a format by Ayşe. A text that is given on time with poor content is received better than one that is late but richer in content,

¹⁴ Aylin expressed that once he was shocked to learn that a paper he was very satisfied with reading and that he thought was postmodern turned out to be written by a functionalist who was a student of Parsons.

written with care and enthusiasm. What Ayşe criticize here is not the existence of the deadlines, but the artificiality and meaningless rigidity this sometimes acquires. As students we also complain of ourselves that we are always late with everything, as stated in the group discussion. But imposing timeliness is found parallel to teaching to be a good worker for the boss, whereas an academician should rather be a little absent-minded (as Ünal Nalbantoğlu says), in contrast to careerism (Ayşe). Zeliha thinks the criteria to criticize here is not when you turn a text in but where you have it published, the publicity of the place you present the text (17). The legitimate sites to “publish” sociology texts are often not very public. And the requirements to make publications in the legitimately scientific places (scholarly journals) often mean preferring to use some trendy concepts, which looks funny when they are used inadequately and by wrong people (18). The individuals’ acts are not the reasons for the academy to receive such critiques, on the contrary such acts are the results of the structure of science that acquires power through its method: the individual does these things exactly because s/he’s so close to the power, these moves are ways to gain power (Betül). When the scientist as the teacher expects you to do things according to the convenient forms, even if he’s too strict it’s not because he’s bad, he in fact teaches you to reach that power. This is a sharing of the power with a community. He imposes the method (Betül said she uses this word as synonymous with format) because this is what the power imposes (Betül).

For the participants of the first group interview, one should have questions on or maybe discomforts with the social world (*dert*) to study social sciences, “a “curiosity” as they say in bigger words” (19). The sincerity of what you do comes from this inner curiosity, anger or discomfort you feel, like the *daimon* mentioned above, it is these feelings that make you write (20).

Betül argued that many scholars don’t have such a relationship with their studies. As an example, Betül (identifying herself as a feminist) and Ayşe said that they

really hated those doing women's studies in the academy, because these people have never really been hurt for anything related to their or others' gender but they just do gender studies because it's a high trend in the academy (21). Such people do their theses and their studies on feminism and then when it's finished it has nothing to do with the rest of their lives. Ayşe's thesis advisor explicitly stated that he is very fond of her topic because this is something that would "sell" greatly in America. We should always question ourselves on whether the topics we study really touch us. But, as Aylin pointed out, this is not only about the academy, it is about life and our stands in it (22).

On the other hand, you can work on your discomforts with life and express these things in ways other than doing science, but you don't have to give up on science for this, as Murat said. You can work as a sociologist and also do cinema. But if you're working under harsh conditions 12-hours-a-day, as Murat did, you can hardly do anything about your questions and the topics that draw you to themselves, let alone any scientific study (Murat).

Zeliha linked the topic to activism and one's political choices. She thinks being an intellectual, including activism or dirtying your hands, and being a scholar are different things. When someone shouts slogans during a lecture, we say this is not the place for it. The format is this very line of separation (Zeliha) (23). Nevertheless, activism is also rooted in your anger and discomfort, the boundaries are not so certain when we think that doing sociology can also be a form of shouting (Aylin). In the second group discussion, Cemal pointed out that doing sociology of a worth is a matter of discipline as well, of studying systematically in archives, for example. An organic intellectual of such a sort is very valuable. Overcoming elitism is not just about where you live, reading and writing are also important ways for it, and as Samet and Cemal emphasize, reading Gramsci shows you the way to do this. "Being able to write even in prison. This is also a matter of hope." (Cemal) (24):

Samet: Emphasizing practice in all that detachment, this is just what fascism is. This is in fact what fascism does, it says “no”, it says “I’m practice”, the mentality that says “reading and all that stuff, this is all nonsense” is in fact... this is the mentality held by the fascists.

Meryem: Because then it will say I will think for you.

Cemal: Yes!

Samet: The mentality that says everything is practice, is bad (25).

The attitude criticized as insincerity or distance (not really dirtying ones hand with what s/he’s doing) is also felt as a kind of elitism of the scholars. Ayşe and Zeliha have stated that they have often experienced their own lives objectified by their teachers who appeared so foreign to their realities:

The teacher says that the foreigners are very surprised at, and this she talks about as some data from the outside, the jumps in the education levels [between generations of the same family] in Turkey. (...) So interestingly in Turkey the sons and daughters to an illiterate mother can be university graduates! I was shocked as she was talking like “this is really possible, isn’t it” (...) My mother also doesn’t read or write but I’m doing masters too (Zeliha) (26).

It gives quite a strong sense of not belonging to the academic world when you are asked by your teacher if you have ever heard a word (“*emmi*” was Ayşe’s example) that is in fact so much into your life: it feels distant and surprising to see that a sociologist is so alienated from people (like themselves) according to the respondents. Murat was annoyed as a socialist to see that his teacher in the university didn’t want to sign a document which said “worker” under her name; this sociologist couldn’t think of herself as a worker. Objectivity sometimes feels like “we don’t care about anything”, and because of this some sociologists sometimes seem incredibly ignorant (Betül, Ayşe), an ignorance rooted in politics, elitism and class differences. (The incredible examples given in this topic I unfortunately cannot present here.) Making legitimate accomplishments in science brings respect to the person who does them and to what s/he says; be it in a hierarchy: Meryem

told that when she finished her masters and started doing PhD, the attitudes of other students as well as teachers changed towards her. Rather than saying “read this”, they started to ask whether she has already read it, instead of expecting her to cite others’ work in her studies, she was now expected to do something original. She finds this direct change funny.

In discussing how the format influences the content, Mualla argued that the format is decisive on what you ask, what you discuss, your context and your approach to what you’re studying. But this is a reciprocal relationship, the format is never independent of the content, on the contrary it is created by the content as well.

Cemal: But it can hide the content.

Mualla: (...) As the format distances itself from the everyday language and narrows the audience of readers, by limiting the touch [with the everyday], prevents the interaction and contributions from a wider potential environment.

Cemal: (...) The dialectical relationship between them... Let me put it this way... If you have something great in format, this doesn’t mean it’s content is also great. The format of that thing can even hide and distort its content. (...) Money is the format of something for example, under it lie exploitation of labor and it hides this content (27).

Here Leyla told that in science texts an inner coherence is expected. In writing her master thesis her first advisor said that everything she said should be based on an argument someone made in another science text, that she shouldn’t make her own inferences or interpretations on her data but support it with studies done before hers. Her second advisor wanted to see that every paragraph should end in a way that would hint the next paragraph, and there should be a balance between chapters as well, which means if one chapter is about 20 pages, the other should be more or less equally long. Such demands were found absurd by Meryem, she thinks this doesn’t represent what is expected from science texts. Nevertheless, academic writing does have such demands, and there are academic writing centers to teach to fulfill such expectations. What you write shouldn’t be eclectic. But when you are thinking

about something, is this how your thoughts progress? Don't we, at least some of us, think and understand in eclectic ways sometimes? Don't we jump from one topic to another in conversations? When we put our way of thinking into systematizations imitating perfection, doesn't the format influence the content of what we write? Thus it determines the content by including and excluding things. These are what came out of my mouth with the questions the participants asked me in the second group. Then Cemal and Samet said I have made my own work very difficult by writing things that challenges writing itself, it's a study that eats itself, like trying to fill and empty a glass at the same time. İlyas argued that it is a misperception of academy to expect that everybody thinks and expresses their thoughts in the same way. This comes to say that if you don't conform to this you can't produce a science text (28).

Cemal said that at such a point one can resort to poststructuralist theory, to destroy the centers for example. You can't abolish methodologies but you can push their limits. But then that will establish as yet another methodology, another school, as Samet points out (29). Meryem gives the example of Nancy Lindisfarne's *Dancing In Damascus* where she writes what she has learned in fieldwork in stories of fiction as another genre of anthropology. Mualla asked whether Lindisfarne was an academician when she wrote it (yes she was) and Cemal asked whether she's a professor (yes she is), then saying that being a professor enables her to be freer in writing. What the authority of the format or the method wouldn't avail, the authority that comes from being a professor can legitimize. For Meryem the "legitimacy" of *Dancing In Damascus* still comes from the method: Nancy Lindisfarne did go to Damascus, she did fieldwork there and this is the outcome.

In discussing the same question in the first group, Betül said when she couldn't "dare" to say something herself as she writes her thesis, she makes a reference to someone who had written it beforehand. And then if there's a problem with this, you can always say "I didn't say it, he did!" (Betül). But your choices in using the

works of this person and making this quotation is also meaningful: it is *you* out there (Zeliha). Still, as you make the quotation you share in the power that the person you make reference to has acquired: he is powerful thus he can say it, but you can't; when you say it it's biased, when he says it it's scientific (Betül). What I am now doing with the quotations from the focus group discussions is a parallel to this. If I *only* talk about my own experiences, it becomes personal and coincidental, and as it was expressed by Erdoğan Yıldırım in my thesis committee, "we are not interested in this in a thesis", because science should be reliable rather than coincidental. In this sense the format is not arbitrary, it is a way to assure reliability. Making focus group discussions doesn't only enrich the thesis with other people's experiences, it is also a legitimate way of incorporating people's personal ideas or everyday life into the sociology text. This, I think, is true for most sociology and anthropology texts using data from research done with people.

Making references to prior works is important and useful as this is one of the ways that scientific texts become meaningful. It shows to the reader what you know about your topic and it's a way to share it at least as background information. But it often becomes instrumental: Murat exemplified the dialogue of two friends where one of them, without knowing what topic the other is writing about, suggested him to just "put some Giddens" (30). It is the responsibility for a researcher to be familiar with what is done in the field s/he's interested in, but setting this as a criterion is a result of the process of standardization (Murat). Besides, as we are all students, we are just learning to write, and don't these standards also help to check what we learn? (Zeliha). In Zeliha's words "of course this is bad when it becomes an aim in itself, but it also has an instrumental side." (31). We do need thesis committees, but we should also have relationships with them which might be more emotional than rationally defined and standardized according to Aylin.

Criteria related to format may become points of reference for the respondents, like trying to find different sources to use just to make the bibliography section look large:

For example this is how I decided the thesis would be finished: I said that when the references make 100 the thesis would be over. And for example there are two people saying the same thing, I confess that I took one from each so that the references would be larger! (Betül) (32).

References, on the other hand, are also criteria to decide how serious and relevant a study is, and Ayşe says this is also how she judges a sociology text: when references are made to second hand sources especially in crucial arguments of a text, this implies that the writer hasn't read enough on the topic or doesn't have a good command on what s/he's doing.

Moreover, all these things we talk about are historical:

But we also have a historicity. (...) These conditions that we are tied to, etcetera. For instance that novel, Vedat Türkali's novel isn't accepted as a proof in the bourgeois court. But a mediocre thesis, just because it's a research, can be used as a proof (...). And there is this situation of making an effect, in the short term, it's because of that that I always talk about legitimacy. (Murat)

Aylin: Not refusing the legitimacy problem, but to withdraw from making a universal description of this (...)

Ayşe: For me when we say historicity, you're mentioning another aspect of it but, I don't want to read any unhistorical text. For me what makes a text strong is when it is not detached from its historicity. Because both the writer and the thing I'm reading have that history and I want to see that.

Betül: You want to see it within its context. (33)

A parallel discussion is carried on representativeness. How can we make the people we speak to in the field speak in our texts? How do we link their experiences and perspectives to the rather macro narratives on that group, that place, or history?

(Ayşe). Zeliha gives Leylâ Neyzi's "Gülümser'in Öyküsü"¹⁵ as an example, which is a study of oral history, presenting history through the life of a Kurdish Alevi woman from Dersim. The violence she has lived through, for example, is representative of historicity according to Zeliha: this is not an experience a woman lives on her own anymore, when you present it within its historical context. When discussing whether this is relevantly "scientific", the respondents say no matter what scientificity claims, they *personally* accept it as scientific (Zeliha) and that the academic sense of historicity shouldn't be fetishized (Aylin).

My question here with the first group was, as you are thinking how to present what you have done with people in its historicity, do you think on who the text is written for? Ayşe said that who the audience is is an important part of the discussion, and in writing she knows that she isn't writing for the people in the field, and this is also told to her by the faculty. "I write for the academic circles", says Ayşe, and the reader of the text she thinks *within* this assumption: how would a postmodernist understand this text, what would an anthropologist say, what would a positivist think? (34).

The personality of the sociologist or anthropologist is reflected in the choices s/he makes in writing up the text with such considerations. Betül, on the other hand, told that her thesis advisor told her to think of the quotations she makes from the people she spoke to as if they are tables in a quantitative study. But Betül thinks that presenting what people say in their own words is something that distances a scientific text on people from seeming to be talking about a foreign entity like a herd of bucks. Nevertheless, we choose how we represent our topics (Aylin), I select the two paragraphs out of all that is said, and try to represent a social universe with colors, laughter, touch and smell in a text on paper. "We're talking about

¹⁵ In Leyla Neyzi, 2004, "Ben Kimim?": *Türkiye'de Sözlü Tarih, Kimlik ve Öznellik*, İstanbul: İletişim Yayınları.

something impossible, we're just seeking in our consciences how much we can extend it." (Aylin) (35).

In the first group Betül said we write only for the colleagues, in the second one İlyas said sociology teachers read sociology texts, and her mother doesn't (and her mother doesn't have to read a sociology text, as Meryem later said, she might better like to read a novel). In both groups participants agreed that the audiences to social science texts are other social scientists and the people from related professions. Ayşe told that because of this she sometimes doesn't want to write. We write to satisfy ourselves, we write for our careers, other than that "it's great if 5 people reads my thesis" (Ayşe) (36). Many others don't even know that there is a such a study, says Leyla.

As another problem, Ayşe says she sometimes writes in great enthusiasm, but this is not transmitted to the scholar/teacher reading it at all, because s/he reads only the introduction and the conclusion. This I think is also a sign that we as many graduate students still don't have a good command of the academic writing skills and we can't make a good use of the format as an instrument: otherwise, we could write introductions and conclusions that could better express ourselves. Nevertheless, just reading some parts of texts is not a good way to look at a whole picture, as Ayşe said, and it is disappointing not to be able to express what you mean. It feels useless to write when you think that you're writing just for some three scholars who are trying to write something in a similar topic, or when you are writing just to get into the citation index so that you'll progress with your career (Betül) (37).

At this point I asked the group what my advisor Helga Tılıç had asked me for this thesis: everything seems so pessimistic and nihilistic when you write like this; is there no reason for you to write other than to graduate? Knowing the political topics some had chosen to work on and their identities as activists, I asked the first group why, then, were they working on such subjects? Didn't it have anything to with

their political opinions or their own lives? Zeliha said that these certainly have to do with our writings, it's inseparable. Betül said we just have to earn our lives somehow and there's nothing to be angry at here. Ayşe told she felt she is using the people's experiences for her own aims (38). For Zeliha, what you write becomes meaningful when you make it flow, when in some way it reaches wider circles (39), but it doesn't flow when it remains in the scholarly format (i.e. remains as a thesis):

In a mail from Gülizar to Betül there was something I liked a lot. She sent a mail that said "Just when I was saying "well I'm finished with my thesis and here it stands in the shelf, so what good it is," you wrote to me asking "can we meet one day, I'm writing a thesis in a similar topic, can you share some points with me." I'm really happy to receive this mail just at this point." Yes there are such things, but that's not enough. What we do is not that subjective, why don't we go and make drawings then (Zeliha) (40).

Betül questions why we have so high hopes on ourselves. Scientists think they will do something and the world will never be the same again, but it's not like that, because it has no ties to their own lives, none of them throws a stone like Edward Said did (41). Precisely because they can't do it in life they think they can do it in writing, they think they'll be a radical sociologist in writing an article (Betül). You try to reach power by using a scientific format but because you use this format nobody knows about what you're doing and it never grows out (Betül). Zeliha and Murat argued that it doesn't make a change when you don't use the format either, and Betül explained that by the format she didn't mean the format of scholarly texts but the format at large of doing science.

For Ayşe this all goes back to what your problem is (*dert*). The very experience of writing is important for Ayşe, to problematize the social world and take the great challenge to express it in your writings. She expressed very sincerely and enthusiastically that learning how to understand and convey what you mean to texts is great in itself and for now she doesn't have a hope higher than this, as learning and knowing are sometimes so satisfactory (42). Cemal said he loves to read the

works of a person he likes and really says “this is it” when he reads a good analysis or a good idea put forward well. Meryem says that sometimes making a good analysis feels even better than orgasm (43). On the other hand, many of the participants of both groups were activists or had strong political stances, which influenced their choices to do graduate studies on social sciences. Cemal, for example, said he studies sociology because of the depth of perspective and analysis Marxism brings, İlyas also says that studying sociology contributes a lot to one’s understanding in life (44).

Samet, with his holistic attitude, said that a person shouldn’t be limited to what he writes for the academy, his personality in activism cannot be isolated out. The problem, according to him, is not that somebody writes an academic text and this is so heavy that only the academics can read it. The sociologist/author can carry what he has done in his texts to outside academic circles with his attitudes outside. What keeps him from doing this, or what keeps sociology within university, shouldn’t be looked for in texts but in the tendencies and choices of their writers or the structures that effect these. Samet and Meryem say that for Turkey with the 1980s (the military coup in 1980) the academics tend not to have something to do with life at large, they go to universities as a job and don’t do anything else with what they have learned and produced there (45). Meryem called these people asocial social scientists. For Betül, many sociologists working in the academy don’t put themselves into what they’re doing, which means there’s no difference for them in being a sociologist or a bank clerk. This is also related to the academy’s integration to market economy and the alienation this brings along. Samet later emphasized that it is the detachment itself that makes us ask about the touching points. Betül told that neoliberalism influences academy too, reflected also in the working conditions of the academics.

In the first group, as Aylin said it’s a little too harsh to expect from the scholars to all be throwing stones in this big world (Edward Said was an example mentioned by

respondents in both groups), Betül told that the word is sometimes an act, and sometimes it's the embodied form of not acting. Continuing a critical perspective is also very meaningful in itself for Aylin. All along the one year that he had been a member of a political organization, he never believed that a socialist society would be established one day, but he found it very important to have that tradition of thinking going on. For Murat, utopia is worth being protected as it is, but it is also the base for him to make his objections in the everyday life, it is meaningful to be part of a struggle (46). He makes clear that he doesn't want to romanticize the academy, but here can also be a field of struggle. For the participants of the first group, what keeps you going is not a belief that one day the world be a degendered, ecological, socialist, "wow, perfect!" place, if you believe that you exist, whether your aims will be fulfilled doesn't condition your acts today but you just have to do what you believe, your words and your activism being a part of it. You have to see opportunities in crises. Even if the academy here and for now is not a sphere of opposition, it has the potential to be so. It bears the opportunities to make objections, which is very valuable.

Now, in increased optimism, Ayşe said that doing research in anthropology you don't only take from people, they may also be happy with this relationship, as the women she has been observing have expressed to her. This is a relationship, a communication with various dimensions. Ayşe finds it very nice that one of the women she has spoken to wanted her daughter to study and learn English so that she could one day read Ayşe's thesis. Betül on the other hand feels a responsibility sourced in her relationship with the people she spoke with during her research: they call him "Hocam" and demand her to tell what they have been through to other people (just as I was told by other people during researches) (47). The "influence" of a text of sociology or anthropology in circles outside of the sphere of science, is something that I try to problematize in this study. In the focus group discussion it turns out that the researcher's feelings about the meaning and influence of writing in sociology or anthropology differs from person to person and in relation to the

topic being studied. Betül, who otherwise seemed more pessimistic about the practice of science, here told that we shouldn't deny the contributions of activists, in relation to the academic spheres, on the cases of Hüseyin Üzmez (accused and sentenced for sexually assaulting a teenage girl) and Şahin Öğüt (charged with raping various women, arguing that a woman wearing jeans can't be raped). Words sometimes turn out to be powerful acts, when research results influence decisive processes (Betül)¹⁶. Sociology texts might have a role in the hegemony of different perspectives in the public sphere. It is important to contribute in the making of discourses to be hegemonic (Betül). This is how people realize that some things are not absolute or compulsory, or that they don't have to do some things (Murat). This might also be a matter of time and patience, nevertheless it is hopeful to demand more, and to be demanded something. Mualla emphasized that after all we are speaking with the concepts put forward by some people, and we think with the analyses of others who might have died long ago; if we are using these than there is a flow, ideas generated in social sciences must have somehow gained currency, which is meaningful.

In that case, if what we produce is important for the formation of discourses in the public sphere, but it is at the same time limited by its format to the academic sphere, then would it be better for the sociologists to try to communicate using mass media? Betül and Zeliha say it certainly would. Ayşe has doubts whether these media are suitable for the presentation of any sociological subject. Betül said she would certainly accept to speak on TV if she was invited; those few minutes there are very important (48). Although this involves an arrogance of the sociologist, it is also a way to push things with our activist identities. Nevertheless, as Aylin pointed out,

¹⁶ As an example, after the release of the research report on the perspective of honor (Kardam 2005), as I was participating in another research on how the circular of the Prime Ministry of Turkey to prevent violence towards woman and children and honor killings was enacted (2006), I interviewed a police/bureaucrat in the Ministry of Interior. He was carrying out a careful work to overcome the difficulties with the enactment of the Prime Ministry circular, writing another one for the Ministry of Interior, and he told me he made wide use of our report on the honor crimes, and also told that his dialogues with some feminist institutions have influenced his perspective on the matter. It really felt good to hear these.

mass media is not a very good place that we can express ourselves in our subjectivities. Ayşe hopes we will not turn one day into the expert sociologist commenting on election results on TV with all the authority of science. She is disturbed by the hierarchizing of the sociologist as an expert on TV. She also said that as the sociologist-on-TV Emre Kongar has been an example for a long time for her family to understand what she's studying and to explain it to other people. Zeliha thinks that using the mass media makes you a part of the market relations: you are there if this sells.

Another discussion is who the audience is to TV and each channel—do we see critical sociologists speaking in religious TV channels? The language of expertise that the sociologists use on TV makes them seem like they've come from outer space (Murat), it's completely unintelligible (Ayşe). It is an important qualification to be able to express yourself to wider publics without losing the meaning of your message. But this is not what we learn in the academy, we rather learn the jargon expertise, which Ayşe says is a part of the format.

Very importantly, Aylin points out that using mass media to communicate what is done in sociology doesn't have to be done by the sociologist. Don't the serials on TV also do this from time to time?

İlyas, on the other hand, argues that many of the discourses produced by sciences on social life have ideological dimensions, and presentations of these in mass media may lead to false understandings. When a psychologist, for example, comes and says that 90 percent of gays have been raised without fathers, basing this on a research with statistic etc (apparatuses of the claim to science), we know that this is not true, but people reading it in mainstream newspapers might make out that they shouldn't get divorced so that their children won't be gay. Even if you're another psychologist who would say this is false, basing yourself on another research you made, how will people choose among two different viewpoints between two

scientist? (49). İlyas's point, I think, is not that non-scientist audiences can't understand this or that people shouldn't be left for their own ways of making meanings out of what texts on science say, but that the media has the power to manipulate and science has the power to legitimate anything. And, İlyas said, not being familiar to the field of sciences, what is obvious "for you and me" is not so obvious for the reader of that news in the paper. Samet related such situations to degenerations and detachment of academics. Meryem said that the habits brought by doing social sciences, like looking at phenomena from various perspectives and speaking at length, disables the social scientist from communicating herself: she can't make simple and unilinear arguments as expected by many people, and this detaches her from them. She thus remains in her shell (50). For İlyas this is what such a sociologist does to herself, and then she acquires elitist attitudes, looking down on people, preferring to live away from them in isolated upper-class neighborhoods: "I know so many things and I'm so above everyone else I can't live with them."

Mualla argued that our regular format of communication in sociology might also be a way of protection for ourselves. She used to think that one can explain something to everyone once she understands it herself, but now, after starting to study sociology, she has doubts that we can make explanations without using terms and concepts that are unfamiliar to non-scientists. Or maybe, when we are able to express ourselves, what we will find out is that what our say doesn't make sense to anybody else. When we present our works to the people we studied, they might say "so what?", because in fact we are learning for them, and for Mualla it is bad if it means nothing to them. It is also bad if we have difficulty in explaining what we do to other people. The prevailing language of sociology keeps us away from taking this challenge (51).

This chapter presented various aspects of claims to do science and how they are experienced by graduate students of sociology, anthropology and related fields.

Sociology is discussed as a profession in its historicity and as a political practice. Claims to be scientific are claims to authority, and as it is also put forward by participants in focus group discussions, the format of expression in sociology is a way of legitimizing this claim. Nevertheless this doesn't have to be taken in a negative sense. Sciences and sociology are found as good ways to understand the world and to contributing into the making of discourses, which is found very important by students with activist affiliations. The next chapter discusses how sociology and anthropology as practices studying on people are analysed by using the very methods of these disciplines.

CHAPTER 4: Reflexivity in Anthropology and Sociology

This chapter looks at what sociologists or anthropologists have said about their own practices and discusses what they say about their relations with the audiences to their works. It is mainly the trend of “reflexive criticism” in sociology and social anthropology that concerns this chapter.

The question of reflexivity, the role of the sociologist or the social anthropologist in relation to his/her subject of study has been debated widely since the 1970s. Some scholars have advocated sociology to be more reflexive, to take the relation between social sciences and what they study more thoughtfully. However, many of these studies challenging the way sociology and social anthropology is done have studied the relationships of the sociologist to “her subjects” of study, or to the field she belongs to, largely disregarding sociologists’ relationship to people who are not members of the scientific community, or related to it. In other words, reflexive sociology reflects on two kinds of relationships: sociologists’ relationship to/within the academic field and to the people they study. I will argue that the relationship of sociology to its audiences is also very important, but not considered enough in the reflexive trend.

4.1. “Observers Observed”: A Reflexive Anthropology

Classically, anthropology is a “science” “on” people. More accurately, it is the study of “different people”. Güngören (1986:16¹⁷, quoted in Atay 1996:221) defines it as the “science of the different”. In this classical meaning, anthropology aims to understand and introduce the different societies and cultures. But from whom are these cultures “different”? They are different to the anthropologist; the “other” to the West, or to the “Westernized” people of the rest of the world. So

¹⁷ Güngören, A. 1986. *Cadıların Günbatımı – Bir Antropoloji El Kitabı İçin Notlar*, İstanbul: Yol Yayınları.

anthropology rather studies the small communities like villages versus the people in big cities, the tribes of the “developing” part of the world versus the “developed” societies. A classical anthropological study would involve the long-lasting participation of the anthropologist to the lives of the people in the community she has decided to study, called the participant observation. With this process the anthropologist is expected to reach a good understanding of this different life and to document it. This documentation is supposed to be a full and exact transfer of information on that social world to the reader. The documentation part is ethnography (this is rather a method that can be used by various sciences), and the following analysis and evaluation of the culture “under study” is anthropology (Atay 1996:221). Reflexivity is, briefly, a questioning of the possibility of this process: is such an objective understanding possible? Is it possible, and right, to represent an “other” in ethnographic writing? What kind of a process and what kind of a relationship is this, between the “participant observer” and the community he observes?

The four years of undergraduate education that I had in the department of “Social Anthropology” in İstanbul University (2000-2004) was a caricaturized replica of classical anthropology. Here I would like to briefly present what we were taught there as an extreme example to illustrate classical attitudes, to provide a background information on what is criticized, to better assess the meaning and importance of later reflexive criticism. Although my experiences and the practices in this department are not representative of anthropology as a discipline in its classical form, I present it here to give an idea on how far it could get.

The department which consisted of two people as teaching staff had a holistic attitude which meant making descriptive studies on different people, aiming to present literally all aspects of their lives. Our teacher that gave most of our courses had no interest in anything theoretical. We were literally “taught” what the prominent anthropologists had found out about the people of the world (prior to

1940s, nothing newer). So Bronislaw Malinowski had done a research on the people of Trobriand Islands and we learned that “the Trobrianders are such and such a people.” It was not a sentence like “according to Malinowski, the Trobrianders are...” The “according to the anthropologist X” phrase was never uttered as it implied that the views of the anthropologist X could be questionable, that the anthropologist X might not be perfect; even a possibility that there is no single, absolute “Trobriand reality”.

In this department of anthropology we were expected to do a fieldwork by the end of our third year in the school, and “count” the results and write up a thesis consisting of some hundreds of tables in our fourth year. Our teachers always said that such a fieldwork was the only way to become anthropologists. The fieldwork was already designed for us by the department; it had been applied for more than ten years before us. We were going to go to villages in Turkey in teams of two or three, “choosing” the villages by some rules provided by the department. We had an allegedly all-encompassing questionnaire to “apply” to every household in the village, the same questions had to be asked in all the villages. The villagers of course had no choice of refusing to “answer”.

Thus the villagers had quite a lot of fun with our questions: “I already told you that there are no chicken-breeding farms in the village. Why are you asking it again to him [another villager] whether he has a chicken-breeding farm?” Naciye Abla and her daughters-in-law, in whose house we stayed, often explained laughing, “Their work is like this, they say they have to ask these questions to all of us!” And the “respondents” often said “You had already asked my wife yesterday when it was that we got married and how old we were then, just copy her answers to my sheets, it’s the same marriage you know!”

Adding anything theoretical or analytical to our thesis was strongly discouraged. Anthropology should be descriptive, according to the department (but how can you

describe anything without any analysis?). Analytical works were not considered “scientific”. Once, our “senior” teacher went to a conference on anthropology in Istanbul, and told us about it in class, saying “some people are arguing for an anthropology which is not positivistic. What do you say, children?” And one of my classmates gave the “correct” answer: “Oh, what nonsense!”

In writing up our undergraduate theses, it was strictly forbidden to write “I” or form any sentence in active voice. Instead we were to write things like “X village of Y province was visited between the 1st to the 30th of August, 2003.” But who did that visit? It was not “scientific” to talk about any “anthropologist” as a subject, because, for the department, anthropologists are all objective and absolute and perfect, they all “apply” the same perfect, unique, unquestionable scientific method of anthropology wherever they are. The expected outcomes of our fieldworks were briefly something like this: “The village x consists of 450 people, %48.7 of which is women and %51.3 men, with a median age of 42.4 for men and 46.7 for women. In the village there are a total of 89 cows, 12 water buffaloes, 0 sheep, 0 goats, 100 chickens, 10 roosters and 45 chicks.” However, with some classmates, we also put photos of certain people we had met in the villages, trying to present them in person and tell about their lives and relationships with other people, even making quotations from them. This was something that the department couldn’t think of forbidding by then, and it was received with hostility. I remember that I had the comment by the senior teacher “you’d better make it look more scientific than a novel!”

Although anthropology is usually practiced much better ways than my undergraduate education, it was quite widely accepted in classical anthropology that a social anthropologist is not affected by the work s/he does as a participant observer in the field, as if s/he is immune to the environment, taking the methodology of the discipline as a perfectly working machine, and the anthropologist as a mere applicant of some principles, rather than a human being.

Classical anthropology was based on the field experience, but banned this experience to be told openly in the outcome text. In Rabinow's words:

As graduate students we are told that 'anthropology equals experience'; you are not an anthropologist until you have the experience of doing it. But when one returns from the field the opposite immediately applies: anthropology is not the experiences which made you an initiate, but only the objective data you have brought back. (1977:10¹⁸, quoted in Tedlock 1991:72).

This attitude resulted in a situation where the anthropologists were not really prepared to do their "job", and tended not to talk about what they have been doing.

To quote from Malcolm Crick,

Evans-Pritchard says he was told to take quinine and to keep off women (1973:1)¹⁹. Beals (1970:38)²⁰ related the story of a senior anthropologist who was only advised to take plenty of marmalade. Beattie spoke of the Oxford view as "sink or swim." (Crick 1982:18)

[Lévi-Strauss] went to the ends of the earth (1976:436-7, 544)²¹ to find savages and having finally stood near to them found them mute and unintelligible; his text ends with a painful reflection on the sense of understanding one sometimes has in a chance exchange of glances with a cat. (Crick 1982:23)

Gradually, it came to be seen that this "unique methodology of anthropology", the participant observation, causes stress because of the contradictory position it requires from the anthropologist: "Participation implies emotional involvement; observation requires detachment. It is a strain to try to sympathize with others and at the same time strive for scientific objectivity." (Paul 1953:441²², quoted in

¹⁸ Rabinow, Paul. 1977. *Reflections in Fieldwork in Morocco*, Berkeley: University of California Press.

¹⁹ Evans-Pritchard, E.E. 1973. "Some reminiscences and reflections on fieldwork", *Journal of the Anthropological Society of Oxford*, 4.

²⁰ Beals, A.R. 1970. "Gopalpur 1958-1960" in Spindler (ed).

²¹ Lévi-Strauss, Claude. 1974. *Tristes Tropiques*, Hammondsworth: Penguin.

²² Paul, B.D. 1953. "Interview Techniques and Field Relationships", in A.L. Kroeber (ed), *Anthropology Today*, Chicago: University of Chicago Press.

Tedlock 1991:69). Joke Schrijvers tells about her experiences in a village in Sri Lanka while doing research oriented to action:

The more I felt at home [...] the more uneasy I became about my research. I began to dislike conducting interviews in which I asked a particular person everything I wanted to know. [...] I experienced 'fieldwork depression', I felt tired and useless, and became ill. What was the use of collecting all this information, even if the dialogues had provided new knowledge? How could my research become a meaningful contribution that would benefit the women for whom I now really cared? (Schrijvers 1991:164-65).

Many of the common practices in anthropology and sociology prior to this period of self-reflection were found problematic through this re-thinking. Anthropologists are now expected to tell more about how the information is gathered and how they participated in people's lives than what they have observed, and more on how they are representing "the other" than the representation itself. Reflexive critique questions the very possibility of representation.

The general idea of reflexive thinking in social sciences is to apply the method of the discipline not only to its subject of study, but to the discipline itself as well. If the world of history and society is one that people constitute and are constituted in, if people are inevitably a part of this world, then here the "objects" are necessarily constructs of the "subjects", thus one cannot "posit" subjects against objects (as implied by "positivism") because these categories already include each other (Özlem 1998a:27). A sociologist thus cannot study the society of which s/he is a part from "outside" of it (ibid.).

A well known example of studying science itself with the methods in science is Bruno Latour and Steve Woolgar's study of a biology laboratory (1979), which is famous for applying the anthropological method to a prominent medical biology (neuroendocrinology) institute in California, U.S.A., whose coordinator Jonas Salk was a public hero of science as he had found the vaccine for polio in 1955.

Laboratory Life – The Construction of Scientific Facts can be considered within the reflexive debate as it is innovative in applying the method of anthropology to the field of science and therefore leading to discussions about the methodology of social sciences as well. Perhaps the best known example of studying a community of social scientists with reflexive sociological methods is Pierre Bourdieu's *Homo Academicus*, where he made an analysis of the academic field in France in 1960s (1984).

Reflexivity is not only about how the observer affects the observations and the social environment of the research. This is a reciprocal relationship: the social scientist is also affected by what s/he studies and the people s/he works with. The key concept here is subjectivity: when we accept subjectivity as something that is and should be a part of the process of research rather than a source of problems to be avoided, then comes an epistemological difference. (This may also be discussed as part of a broader epistemological turn from modernism to postmodernism.)

Something that had contributed to these questionings of the practices of social sciences and their relations to other people was the publication of Bronislaw Malinowski's diaries that he kept during his famous fieldworks in Mailu and Trobriand (1967). When classical anthropological texts were taking the anthropologist as the authoritative figure, the hardworking and value-free scientist telling the public all the truth about a certain group of natives, Malinowski had been the profound figure of such a position. But what the diaries brought out was not quite like this: Malinowski in fact hated the Trobriand life; he was looking down on the Trobrianders and wanted to leave this horrible place as soon as possible (Geertz 1990:45, cited in Atay 1996:227). This was a surprise for the community of anthropologists and in effect contributed for the textual presentations of fieldworks to get off the classical track. Now there were new questions on ethnography and the voice of the ethnographer:

That Malinowski was hypochondriacal, arrogant, racist, is not the point. The real issue (Geertz 1975a:47)²³ is epistemological: what understanding can there be when normal empathy and feeling are lacking? If one feels alone when in the presence of others, as Malinowski confesses, what sort of account can one give of their lives [...]? (Crick 1982:20)

What desires and confusions was it smoothing over? How was its “objectivity” textually constructed? (Clifford, 1986:14)

The readers of texts of classical social sciences usually have no idea of all these procedures that affect the researcher, the field and the results of the work. Classical anthropology was not really interested in asking questions *on* the fieldwork process and answering them, as the discourse that participant observation, the technique of anthropology which makes it unique among social sciences, is the most reliable source on gathering information about the social world, does not allow for such a questioning (Atay 1996:226). In fact the part that is lost here is the part that might bridge the gap between the community of those who “study” (the sociologist or the “sociological self”) and the community that “is studied” (the famous “other”). This part is now considered very important for sociological / anthropological work by many proponents of a reflexive social science. The re-inclusion of these excluded processes to sociological texts is one of the solutions proposed to have a sociology that makes the other less other (Abu-Lughod 1991). Tedlock writes:

Beginning in the 1970s there has been a shift in cultural anthropological methodology from participant observation toward the observation of participation. [...] The shift from the one methodology to the other entails a representational transformation in which, instead of a choice between writing an ethnographic memoir centering on the Self or a standard monograph centering on the Other, both the Self and Other are presented together within a single narrative ethnography, focused on the character and process of the ethnographic dialogue. (1991:69).

²³ Geertz, Clifford. 1975a. “On the nature of anthropological understanding”, *American Scientist* 63 (Jan/Feb).

4.2. Sociology, Anthropology, Colonialism, Power

Relations of power and domination have always been inherent to sociology and anthropology, as discussed in Chapter 3. Edward Said's *Orientalism* (1978) is a seminal work in arguing that how the West perceives the Orient (as the other) becomes constituent of this relationship, how an idea of the Orient is constituted *by* the perspective of the West towards it:

The effect of domination in such spatial/temporal deployments (not limited, of course, to Orientalism proper) is that they confer on the other a discrete identity, while also *providing the knowing observer with a standpoint from which to see without being seen, to read without intervention.*" (Clifford, 1986:12, my emphasis).

To use this perspective with respect to the conversations established in texts, to their readers, we can say it is a relationship of speaking without being heard, or writing without being read, by certain people. In another very important work, *Anthropology and the Colonial Encounter* (1973), Talal Asad argues that the Western "functional anthropologists" and "orientalists" studies of African and Islamic cultures have been constituted by the relations of power. European colonial interests have shaped to a great extent the studies of cultures, politics of domination masking and facilitating theoretical inadequacies (ibid.). According to Asad,

we need to see anthropology as a holistic discipline nurtured with bourgeois society, having as its object of study a variety of non-European societies which have come under its economic, political and intellectual domination—and therefore as one such discipline among several (orientalism, indology, sinology, etc.). All these disciplines are rooted in that complex historical encounter between the West and the Third World [...].The bourgeois disciplines which study non-European societies reflect the deep contradictions articulating this unequal historical encounter, for ever since the Renaissance the West has sought both to subordinate and devaluate other societies, and at the same time to find in them clues to its own humanity. (Asad, 1973:103-104).

In his critique towards anthropology and orientalism, Asad argues that these disciplines have ignored the European coercive power in the colonized territories. He directly relates selecting some phenomena to study and not selecting some others, asking some questions and not asking certain others, and the perspective to look at history, to Western power and domination (1973:114). This doesn't have to be done deliberately, what matters here is not the intention but the result, which nevertheless reassured Western colonial power.

4.3. Communicating and the Format

According to Crick there has been a “duality” in anthropological texts, coming from the fact that the people in the field and the people who read the texts have been two very distinct communities:

Those who were informants in the field rarely saw the finished anthropological texts; those for whom the texts were largely written, namely academic colleagues, rarely had knowledge of the precise field situation nor even saw the notebooks from which the anthropologist created his account. (Crick 1982:17)

This observation by Crick is shared by others, and it's one of the bases of reflexive criticism. However, neither in the work of Crick nor in others who have produced reflexive accounts on anthropology and sociology this “duality” is linked to the format of expression used in scholarly texts. It is discussed under the relationship of self and other or in contexts of power relations. This duality or the relationship between social scientists and the people they claim to study has been criticized, challenging the detachment of science, the notion of objectivity, the political economy of doing sociology, or the ties of sociology and anthropology to modernism and colonialism. These, however, are not the only reasons for the duality. The conventions of writing in sociology, the very format of communication, is a source of detachment between the anthropologists and the people they study with, between sociologists and those who are not related to the community of social scientists.

In the twentieth century, people started to study their own cultures from the inside. Next to the ethnographers who have gone native, there emerged a “native ethnographer”.²⁴ This has had the impact of making explicit once again that the identity and subjectivity of the producer of an account mattered. On the other hand, Atay says that the ethnographical texts have started to become more available to the “natives” concerned in these works, who might then evaluate anthropological studies on themselves (1996:227-28). In parallel to this argument, Tedlock writes that

If today’s ethnographers are writing not only for various academic audiences [...] but also for *the educated public*, including members of their host communities, then they are no longer in a position to write as if they themselves were the only active members in cross-cultural exchanges. (1991:81, my emphasis).

I don’t think that it has been a change in the formats of sociology or anthropology texts that made them “reach” or “speak to” “the natives”. Instead, this is an outcome of the diffusion of a Western type of education (that Tedlock also hints) that produces audiences to scientific texts out of various people (this thesis can also be seen as an outcome of this move: a person from the Middle East with Turkish as the mother-tongue writes a thesis –a modern western form of expression— of sociology—a Western institution— in English—the new universal language that most people in Turkey don't know, but almost a must language for scholarly practice in Turkey). Nevertheless, the presence of the native reader puts pressure on the writers of scientific texts on people, which changes the power relations between the “writer” and the “studied”. It is no longer possible for the anthropologist to leave the field as soon as his study is finished, to go to an office hundreds of kilometers away and write up a text on a community based on his own notes about them, without feeling

²⁴ Martin Yang (1972) wrote an essay on his own graduate education and the production process of his famous ethnography *A Chinese Village* (1945) that he made in his own village. He says “My fieldwork was my own life and the lives of others in which I had an important part” (Yang 1972:63, quoted in Tedlock 1991:78-79). Tedlock considers this to be the first self-reflexive essay on doing a fieldwork among ones own group.

anxious for any pressure or demand by the group (Atay 1996:228). This new situation jeopardizes the dominant position of speech of the anthropologist:

The critique of colonialism in the postwar period—an undermining of the West’s ability to represent other societies—has been reinforced by an important process of theorizing about the representation itself. [...] What is at stake, but not always recognized, is an ongoing critique of the West’s most confident, characteristic discourses. (Clifford, 1986:10)

This is another point where the question of the audience to texts in sociology and anthropology gains importance. If being read by the people they study on “jeopardizes” the domination of the speech of the scientists, then speaking only to the members of the community of anthropologists or sociologists and related professionals is a way to exercise power. Not speaking to some people is a legitimately scientific way of silencing and dominating them.

4.4. Writing Cultures

Malcolm Crick has pointed out in 1982 that texts

are not just the end of the process of meaning creation which occurs in the fieldwork context, for they also purport to be knowledge. It is important that we ask, therefore, how meanings become knowledge. If meanings are not seen as properties but as processes, and data not as given but produced, so too we must look at knowledge in the same light. Knowledge is a social achievement: it consists of meanings that have “made it”. (Crick 1982:28).

Within the reflexive move, some accounts present the discourse of both the ethnographer and the informants in the form of a dialogue, telling openly about the interpersonal relationships in the field, including the ethnographer her/himself. In *Writing Culture*, which is a manifest of the new reflexive stand on anthropology especially in relation to texts, following the thread provided by Asad and Said on

the relationship between anthropology and sociology with domination, James Clifford argued that

These fictions of dialogue have the effect of transforming the “cultural” text (a ritual, an institution, a life history, or any unit of typical behavior to be described or interpreted) into a speaking subject, who sees as well as is seen, who evades, argues, probes back. In this view of ethnography the proper referent of any account is not a represented “world”; now it is specific instances of discourse. [...] the principle of dialogical textual production [...] locates cultural interpretations in many sorts of reciprocal contexts, and it obliges writers to find diverse ways of rendering negotiated realities as multisubjective, power-laden, and incongruent. In this view, “culture” is always relational, an inscription of communicative processes that exist, historically, *between* subjects in relations of power (Dwyer 1977, Tedlock 1979). (Clifford, 1986:15, emphasis in the original).

Dialogism and polyphony are now established forms of producing ethnographic texts. “Informants” of classical anthropology have now come to be taken as co-authors of ethnographical texts. Schrijvers states that the terms “dialogical, dialectical and reflexive research” are interchangeable, arguing that fieldwork is a two sided process.²⁵ In this sense dialogism, reflexivity, or polyphony if you wish, focuses on the relationship anthropologists have with the people they live with, or they work on. (This kind of a dialogism is not about the relationships sociologists or anthropologists establish with the audiences to their works.) Nevertheless, Clifford points out that “However monological, dialogical or polyphonic their form, [all ethnographic texts] are hierarchical arrangements of discourses.” (1986:17).

A related dilemma is the problematic relationship between writing (publishing) and the power to define: ultimately the person who writes up the findings has defining-power: the established researcher who presents her construction of reality to the various audiences selected by her: ‘the question must be asked: “Who speaks? Who

²⁵ “In my mind the term *dialogical* refers to a specific, reciprocal manner of exchange and communication during the research interaction, between the researcher and the subjects of research.” (Schrijvers 1991:169).

writes? When and where? With or to whom? Under what institutional and historical constraints?" (Scholte, 1987:39, quoted in Schrijvers 1991:172).

The principle consideration of *Writing Culture* is not with what the ethnographers are doing in the field, but with the writing of it. The authors of this compilation discuss this process, contesting to the reduction of writing into a mere methodological act of keeping nice field-notes and then writing up an account. They hold that "academic and literary genres interpenetrate", and that "science is in, not above, historical and linguistic processes." (Clifford 1986:2) All ethnographic texts are constructed systems or economies of truth: "Power and history work through them, in ways that their authors cannot fully control." (Clifford 1986:7).

[C]ultural fictions are based on systematic, and contestable, exclusions. These may involve silencing incongruent voices ("Two Crows denies it!") or deploying a consistent manner of quoting, "speaking for," translating the reality of others. Purportedly irrelevant personal or historical circumstances will also be excluded (one cannot tell all). Moreover, the maker (but why only one?) of ethnographic texts cannot avoid expressive tropes, figures, and allegories that select and impose meaning as they translate it. (Clifford, 1986:6-7).

As a consequence, all "full" accounts will inevitably appear partial. The epistemology that the discursive partiality implies

cannot be reconciled with the notion of cumulative scientific progress, and the partiality at stake is stronger than the normal scientific dictates that we study problems piecemeal, that we must not overgeneralize, that the best picture is built up by an accretion of rigorous evidence. Cultures are not scientific "objects" (assuming such things exist, even in the natural sciences). Culture, and our views of "it," are produced historically, and are actively contested. There is no whole picture that can be "filled in," since the perception of filling of a gap lead to the awareness of other gaps. (Clifford, 1986:18).

In *Writing Culture* Vincent Crapanzano discusses ethnographical texts as a process of translation, as a "somewhat provisional way of coming to terms with the

foreignness of languages—of cultures and societies.” (1986:51). However, the ethnographer has no text to translate, he creates the text himself. According to Crapanzano, like Hermes the god of speech and writing, associated with the boundary stones, the ethnographer “also marks a boundary: his ethnography declares the limits of his and his readers’ culture.” (1986:52). Crapanzano traces in ethnographical texts strategies to convince readers. The need of convincing the reader is what the ethnographer has in common with Hermes the trickster god.

The ethnographer must make use of all the persuasive devices at his disposal to convince his readers of *the* truth of his message, but, as though these rhetorical strategies were cunning tricks, he gives them scant recognition. His texts assume a truth that speaks for itself—a whole truth that needs no rhetorical support. He does not share Hermes’ confidence. When Hermes took the post of messenger of the gods, he promised Zeus not to lie. He did not promise to tell the whole truth. Zeus understood. The ethnographer has not.” (Crapanzano, 1986:53).

Among the strategies of conviction of the ethnographer, the constitution of his own authority comes the foremost:

his presence at the events described, his perceptual ability, his “disinterested” perspective, his objectivity, and his sincerity. [...] He assumes an invisibility that, unlike Hermes, a god, he cannot, of course, have. His “disinterest,” his objectivity, his neutrality are in fact undercut by his self-interest—his need to constitute his authority, to establish a bond with his readers, or, more accurately, his interlocutors, and to create an appropriate distance between himself and the “foreign” events he witnesses. (Crapanzano, 1986:53)

What Crapanzano, and Clifford as well, argue about texts in *Writing Culture* are rather about the relationship between “subjects” (ethnographers) and “objects” (the people studied) of an ethnographic study. The questions Clifford and Crapanzano raise against the constituted authority in ethnographical texts are not related to the question of the audience; to how a reader receives a text, how a text addresses its readers. They don’t make it explicit who they assume the reader of an ethnographic

text to be. Clifford is rather concerned with how the text is constructed, how cultural accounts are evaluated, or how are separations drawn between science and art, realism and fantasy, or knowledge and ideology.

4.5. Writing Against Culture

5 years after *Writing Culture*'s call and recognition that ethnographies are partial truths (Clifford, 1986:6), in an article named "Writing Against Culture", Lila Abu-Lughod calls for the recognition that they are also "positioned truths" (1991). Positionality here is not something that disturbs anthropology, instead it is something that contributes for a more "humanistic" anthropology. For an anthropology without hierarchies, Abu-Lughod proposes to write against the concept of culture and radically alter it, to write "ethnographies of the particular".

In his introduction to *Writing Culture*, James Clifford apologizes for not having included feminism into the book. Palestinian-American anthropologist Lila Abu-Lughod starts her article by saying that the feminists are not the only critical group that is excluded from the collection and that the "halfies", the people from mixed cultural identities (due to mixed parentage, migration or overseas education), and the indigenous anthropologists are not even mentioned (1991:137). According to her, these groups have dilemmas coming from their special position, which reveals the problems with the distinction between self and other, inherent to the practice of anthropology (ibid.).

Abu-Lughod argues that the self-other distinction and the hierarchies it brings along are based on the concept of culture. We can always ask why in the social science texts the "self" is often the West and the White-Christian-Middle Class-Male, and the "other" is the rest. Following Said and Asad, Abu-Lughod argues that although anthropology now "seeks explicitly to give voice to the Other and to represent a dialogue between the self and the other, either textually or through an explication of

the fieldwork encounter”, it is still the study done by the West on the rest, and this relationship is constituted by the Western domination since the beginning (1991:139). The anthropologists studying Western communities have an uneasy feeling on whether they blur the “boundaries” of anthropology with sociology. What is it that makes their work “anthropology”?

One way to retain their identities as anthropologists is to make the communities they study seem as “other.” Studying ethnic communities and the powerless assures this. [...] There are two issues here. One is the conviction that one cannot be objective about one’s own society, something that affects indigenous anthropologists (Western or non-Western). The second is a tacit understanding that anthropologists study the non-West; halfies who study their own or related non-Western communities are still more easily recognizable as anthropologists than Americans who study Americans. (ibid.).

Anthropology, for Abu-Lughod, has many things to learn from feminist theory, which also concerns selves and others. As Sandra Harding points out, the problem is that “once ‘woman’ is deconstructed into ‘women’ and ‘gender’ is recognized to have no fixed referents, feminism itself dissolves as a theory that can reflect the voice of a naturalized or essentialized speaker.” (1990:246). Lila Abu-Lughod argues that anthropology should learn from feminism that “the process of creating a self through opposition to an other always entails the violence of repressing or ignoring other forms of difference.” (1991:140).

Feminist anthropologist shares with the halfie a discomfort in assuming the self of anthropology. “For both, although in different ways, the self is split, caught at the intersection of systems of difference.” (ibid.). In these consequences, Abu-Lughod asks an important question: “What happens when the “other” that the anthropologist is studying is simultaneously constructed as, at least partially, a self?” (ibid.). Their in-between position makes it clear for the halfies and feminist anthropologists that

everywhere is a view from somewhere and every act of speaking a speaking from somewhere. [...] What we call outside is a position *within* a larger political-

historical complex. No less than the halfie, the “wholie” is in a specific position vis-à-vis the community being studied.” (Abu-Lughod, 1991:141)

Abu-Lughod goes on with addressing this question of who the audience is for the work of the anthropologist. She writes:

Split self-hood creates for the two groups being discussed a second problem that is illuminating for anthropology generally: multiple audiences. [...] Rather than having *a primary audience, that of the anthropologists*, feminist anthropologists write for anthropologists and for feminists, two groups whose relationship to their subject matter is at odds and who hold ethnographers accountable in different ways. Furthermore, feminist circles include non-Western feminists, often from the societies feminist anthropologists have studied, who call them into account in new ways. (1991:142, my emphasis).

The importance of these statements for my argument does not only lie in Abu-Lughod’s arguments on the multiplicity of the audiences, but that she explicitly says that anthropologists write for anthropologists. Other possible readers of the texts are the “educated” members of the groups that are studied, and the members of the groups that the anthropologist is affiliated with (i.e. feminists):

Halfies’ dilemmas are even more extreme. As anthropologists, they write for other anthropologists, mostly Western. Identified also with communities outside the West, or subcultures within it, they are called to account by educated members of those communities. (Abu-Lughod, 1991:142).

Here Abu-Lughod is not in fact discussing the question of the audience. It is just a side-data of her discussion on the position of the feminist and halfie anthropologists. And the information from within that anthropologists write for other anthropologists is just routine information.

Against culture, Lila Abu-Lughod tells that

culture is important to anthropology because the fundamental distinction between the self and the other rests on it. Culture is the essential tool for making other. As a

professional discourse that elaborates on the meaning of culture in order to account for, explain, and understand cultural difference, anthropology also helps construct, produce, and maintain it. Anthropological discourse gives cultural difference (and the separation between groups of people it implies) the air of the self-evident. (1991:143).

The concept of culture cuts just like race, but it “allows for multiple rather than binary differences.” (1991:144). For Abu-Lughod, the main advantage of the concept of culture over race “is that it removes difference from the realm of the natural and the innate. Whether conceived of as a set of behaviors, customs, traditions, rules, plans, recipes, instructions, or programs [...] culture is learned and can change.” (ibid.). When contesting the dominations based on cultural discrimination, the anti-movements often fall to doing the same kind of discrimination on the reverse, rather than being able to abolish it. These movements are “attempts to reverse the power relationship proceed by seeking to valorize for the self what in the former system had been devalued as other.” (ibid.). Such attitudes overlook the connections between the self and the other, and the differences within each category. With the concept of culture and a holist methodology, anthropology constructs coherent others, which are defined by their discreteness. Against these assertions, Abu-Lughod writes,

If “culture,” shadowed by coherence, timelessness, and discreteness, is the prime anthropological tool for making “other,” and difference, as feminists and halfies reveal, tends to be a relationship of power, then perhaps anthropologists should consider strategies for writing against culture. (1991:147).

As a strategy to write in such a way, she finds the discussions with the terms discourse and practice (in the meaning associated with Bourdieu’s theory) as moves away from culture: these concepts were “intended to enable us to analyze social life without presuming the degree of coherence that the culture concept has come to carry.” (1991:147). Her second suggestion is to establish historical and contemporary connections and interconnections between the anthropologist and the

community s/he studies. She sees this as a political project. The community studied cannot be taken as a separate entity, disconnected from the larger political structures and the world. Referring to global relations of power, Abu Lughod says “We need to ask what this “will to knowledge” about the Other is connected to in the world.” (1991:148). As a third strategy, asking whether there are “ways to write about lives so as to constitute others as less others”, she proposes writing “ethnographies of the particular”, avoiding generalizations. One problem with generalizations is that they result in abstraction and reification.

Citing the work of Dorothy Smith, Abu-Lughod proposes to avoid creating a self-referential field of conceptual entities. Speaking with generalizations is a part of the professional discourse, of expertise, and thus “it is inevitably a language of power. On the other hand, it is the language of those who seem to stand apart from and outside of what they are describing.” (1991:150). According to Abu-Lughod,

The very gap between professionalized and authoritative discourses of generalization and the languages of everyday life (our own and others’) establishes a fundamental separation between the anthropologist and the people being written about that facilitates the construction of anthropological objects as simultaneously different and inferior. (Abu-Lughod 1991:150-151).

Against such characteristics of generalizations and professionalized texts, Lila Abu-Lughod proposes to bring the language of the text closer to the everyday language. She also says that such examples of anthropologies exist, but they are overlooked, partly because they are not written by professionals, but the wives of anthropologists. Abu-Lughod considers “Elisabeth Fernea’s *Guests of Sheik* (1965), Marjorie Shostak’s *Nisa* (1981), Edith Turner’s *The Spirit of the Drum* (1987), and Margery Wolf’s *The House of Lim* (1968)” to be excellent examples of ethnographies written in everyday language (1991:152):

Directing their works to audiences slightly different from those of the professional writers of standard ethnographies, they have also followed different conventions:

they are more open about their positionality, less assertive of their scientific authority, and more focused on particular individuals and families. (ibid.).

I think the main difference between these texts written by anthropologists' wives and those written professionally by anthropologists is not only the use of everyday language instead of a scientific discourse. The choice of language, the choice of audience (which is directly related to where and how a text is published), any conventional way of writing and the discourse of the texts are all parts of the format of expression employed in a text. These texts that Abu-Lughod cites she finds good (not othering), probably because they are *not* texts of sociology or anthropology. These are not scientific texts by their format. They have no claim to be scientific, their authors probably don't cite works by scientists and don't write to communicate with a community of scientists. This is why they are "overlooked" by the scientists. In this sense, what Abu-Lughod proposes is to change the very format of anthropology. But then what she would be doing would not be a "science of anthropology", but maybe an "art of anthropology." The reasons for that will be discussed in the next chapter, through the works of Thomas Kuhn.

Moreover, we can also discuss who this "anthropological other" is. The "anthropological others" here refer to the people studied by the anthropologists, and not to the general public of non-scientists. This implies that the people who might be interested in anthropological texts are either anthropologists or "anthropological others". Neither Clifford or Crapanzano, nor Abu-Lughod who criticizes them, think about addressing an unspecified audience who in principle can be anyone. Abu-Lughod is concerned with who audiences are, but as her examples show us, her audiences are only the ones who are from the community (anthropologists, feminists) or who are studied (natives, locals).

Abu-Lughod says that generalizations not only facilitate authoritative professional discourses, but they also produce "effects of homogeneity, coherence and

timelessness” (Abu-Lughod, 1991:152). Generalizing flattens the difference among peoples and turns them into a collective entity. Instead of such a stance, she proposes

that we experiment with narrative ethnographies of the particular in a continuing tradition of field-work based writing. In telling stories about particular individuals in time and place, such ethnographies would share elements with the alternative “women’s tradition” discussed above. (1991:153).²⁶

In our own lives, we balance the account of ourselves that social science purveys with the ordinary language we use in personal conversations to discuss and understand our lives, our friends and family, and our world. For those who live “outside” our world, however, we have no discourse of familiarity to counteract the distancing discourses of anthropology and other social sciences, discourses that also serve development experts, governments, journalists, and others who deal with the Third World. (Abu-Lughod, 1991:158)

According to Abu-Lughod, a more familiar way of writing in social sciences might come with a more “humanist” stance. She is of course aware of the multisided critiques toward humanism, but sees certain advantages in adopting it as a writing method:

Because humanism continues to be, in the West, the language of human equality with the most moral force, we cannot abandon it yet, if only as a convention of writing. In advocating new forms of writing—pastiche, dialogue, collage, and so forth—that break up narrative, subject identities, and identifications, antihumanists ask their readers to adopt sophisticated reading strategies along with social critique. Can anthropologists ask this? Already, complaints about boredom and resistance to being jarred have been leveled against experimental ethnographies. Humanism is a

²⁶ Rather than telling that a certain group is polygynous, for example, anthropologists may focus on the particular experience of one man and his wives, with all the discussions, justifications, disagreements or solidarities it bears within, showing the constructedness of such institutions. “It would show that although the terms of their discourses may be set (and, as in any society, include several sometimes contradictory and often historically changing discourses), within these limits, people contest interpretations of what is happening, strategize, feel pain, and live their lives.” (1991:154)

language with more speakers (and readers), even if it, too, is a local language rather than the universal one it pretends to be. (ibid.)

Another way of “seeing the “other” as more than a representative of a ‘culture’” and thus not as the other is to take sides with struggles and the oppressed, according to Heidi Armbruster (2008:18).

4.6. Taking Sides

It is impossible to remove “the anthropologist and her mode of learning from the composite site of ethics and politics.” (Armbruster 2008:1). Anthropology now seeks for ethics in a scene of politics within structures of power and domination (ibid.). The documents on the ethics of the discipline, like those codified by the American Anthropological Association and the Association of Social Anthropologists argue that ethical responsibilities bind anthropologists primarily to the people they study, and then to the sponsors, to the discipline (the colleagues) and the students and trainees, to the own and host governments, with “no obvious reference to the link between ethics, politics and power in anthropological research” (Armbruster 2008:3). It is also worth attention that the readers to anthropology texts are not cited among those that the anthropologists should be related to with ethical considerations (the parties cited above can also be taken as audiences to texts, but these are specified audiences; what about the others?). Armbruster points out that it is mystifying to turn ethics into the personal responsibility of the individual (ibid.), which hints to the use of the word “ethics” a disguise that what is done is rather unethical (Nalbantoğlu 2003). “[A] critical interest in relations of domination and oppression puts moral relativism into question.” (Armbruster 2008:18). In the context of reflexivity, as how we see others is related to how we see ourselves, “one could not just claim to take someone else’s side without not also, in some way, taking one’s own.” (Armbruster 2008:11).

When looking at ethics and politics in sociology and anthropology, postmodern questions shifted the attention “from the political to the conceptual: Power seemed no longer lodged or primarily sought in nations, governments, economic systems or institutions but in rhetoric, representation, discourse and epistemology.” (Armbruster 2008:6). Postmodern perspective makes it more complicated to take sides or activism in science (Armbruster 2008). In *Taking Sides* (Armbruster and Laerke 2008) the contributors defend taking sides and discuss the role of anthropology in relation to ethics, politics and activism, while acknowledging the importance of questions brought forward by postmodern critique (like *Writing Culture*). Nevertheless, postmodern critiques reflect the era of “right-wing individualism” in which it was developed according to Armbruster (2008:8).

Nancy Lindisfarne points out that anthropology works with “the lives of ordinary people,” which means it works “from below”, but this only works when looking upwards from below (2008:23). For her, many contexts cannot be understood adequately without explicitly naming American imperialism and its influences in different localities. This is at least as important as naming colonialism and the relationships of power in knowing and in the social world at large. Lindisfarne writes that in this sense there is a difference of agendas between the scholarly circles and the rest of the world: for the academics it is almost a taboo to talk about American imperialism, they often fail to make the “causal connections of the kind that are perfectly familiar to journalists, generals and ordinary people.” (2008:26). In an era of many ongoing local civil wars as well as wars between states, Lindisfarne finds it remarkable that there are very few ethnographies that challenge these tyrannies around us, reflected in dress codes, gender relations or war rapes (2008:41). The taboo against American imperialism “obfuscates, and reproduces new kinds of highly gendered capitalist/Orientalist discourse”: “Uncle Sam needs us to keep looking the other way.” (Lindisfarne 2008:41). According to radical anthropologist Jonathan Neale, in reflexivity too, some feelings are allowed to be expressed and some are not:

Indecision, guilt, confusion, identity politics and moralism are encouraged. The key injunction in the reflexive turn is to make the native other, and then wallow in discomfort about difference. Commitment, identification with the oppressed, solidarity, rage and political economy are discouraged. (2008:223)

Neale points out that anthropology used to study people in a more holistic way before, they could interlink the systems of kinship to nutrition and landowning and to belief systems (though not the colonial interventions) when the people they studied were yet “on the periphery of the periphery of the global system” (2008:230). But now, when the subject is not the tribe but the workers in a factory,

[t]o study these people in a holistic way would be to mount a serious challenge to the prevailing ideas that support capitalism. [...] To say the same thing in the United States today is to say that if we want to understand what a 50-year-old working woman feels in a Baptist church in Baltimore, we also have to understand her job, the power of the corporations in the country, the federal government and American foreign policy. Moreover, we cannot understand what religion means for her if we ignore the religions of George W. Bush, Martin Luther King and Osama bin Laden. However, to put all that together would be dynamite, and not only in the United States. (Neale 2008:231)

4.7. Epistemic Reflexivity

As an anthropologist, sociologist and philosopher Pierre Bourdieu has a lot to say on reflexivity, with a claim that much of his own work has been reflexive. He argues that reflexivity should be embedded within all social science, as the title of his book *An Invitation to Reflexive Sociology* (1992) suggests. He also has strong critiques against some of the claims discussed above for reflexive anthropology.

Bourdieu makes a distinction between the “commonsensical perception” of social reality and the sociological one. He sees the perception that gives primacy to either the structure or the agent; to the system or the actor, etcetera, as part of the

commonsensical perception. In contradistinction to this, he suggests that sociology should give primacy to relations. (Wacquant 1992b:15). Here, the primacy he gives to relationism is important to understand Bourdieu's theory, but for the purposes of this thesis, equally important is his distinction between the commonsense approach and the sociological approach that he prefers. Bourdieu makes a parallel distinction between the everyday language and the language of social science. Of course, Bourdieu does not say that sociology should build a language separate from that of the people. But he says that the antinomies that plague sociology are rooted in the society and the everyday language, also because such dichotomies are promoted by politicians and media experts who have their own interests in commonsense thinking. This is the opposite of what Abu-Lughod wants to promote, to write closer to the everyday language in order to make the other less other. Is the society, the other of sociology?

Relationism is central to Bourdieu's thinking as

his key concepts of *habitus and the field designate bundles of relations*. A field consists of a set of objective, historical relations between positions anchored in certain forms of power (or capital), while habitus consists of a set of historical relations "deposited" within individual bodies in the form of mental and corporeal schemata of perception, appreciation, and action (Wacquant 1992b:16, emphasis in the original).

Thus we can think of sociology as a field. Bourdieu conceptualizes the field as a ground for a play of competition and conflict, where the participants try to have more of the type of capital of that field: "cultural authority in the artistic field, scientific authority in the scientific field, sacerdotal authority in the religious field, and so forth" (Wacquant 1992b:7). This is at the same time a struggle over setting the rules of the game. There is always a space for indeterminacy, a space for the change individuals can make²⁷: "Even in the universe par excellence of rules and

²⁷ This is important also because Bourdieu insists his theory always has a space for resistance, while he is often criticized that it is too rigid (examples listed in Bourdieu and Wacquant 1992:79). He

regulations [referring to his study on the French bureaucracy], playing with the rule is the part and parcel of the rule of the game.” (Bourdieu 1990b:89²⁸, quoted in Wacquant 1992b:18). To continue with the same analogy, habitus is the actor’s sense of the field in playing the game in his own way²⁹. Habitus and field only function in relation to each other, they shape and they are shaped by each other.

Bourdieu’s sociology, with its stand against antinomies, is a total and monist science. When we can grasp the social world as a dynamic totality, we can see

how artificial the ordinary oppositions between theory and research, between quantitative and qualitative methods, between statistical recording and ethnographic observation, between the grasping of structures and the construction of individuals can be. These alternatives have no function other than to provide a justification for the vacuous and resounding abstractions of theoreticism and for the falsely rigorous observations of positivism, or, as the division between economists, anthropologists, historians and sociologists, to legitimize the limits of *competency*: this is to say that they function in the manner of a *social censorship*, liable to forbid us to grasp a truth which resides precisely in the *relations* between realms of practice thus arbitrarily separated. (Bourdieu and de Saint Martin 1978:7³⁰, quoted in Wacquant 1992b:27-28).

Theory is a practical activity of generating knowledge. Bourdieu is against theory as an institution, as a self-referential realm. He thus proposes a change in the social organization of science. But what makes Bourdieu’s studies relevant for this chapter is, above all these, his deep concern with reflexivity. Noting that “re-flectere”

claims that “*The dominated, in any social universe, can always exert a certain force, inasmuch as belonging to a field means by definition that one is capable of producing effects in it (if only to elicit reactions of exclusion on the part of those who occupy its dominant positions).*” (Bourdieu and Wacquant 1992:80).

²⁸ Bourdieu, Pierre. 1990b. “Droit et passe-droit. Le champ des pouvoirs territoriaux et la mise en oeuvre des reglements”, *Actes de la Recherche en Sciences Sociales*, no 81-82, p. 86-96.

²⁹ When forming an analogy between a field and a game, Bourdieu explains that unlike a game, “a field is not the product of a deliberate act of creation, and it follows rules or, better, regularities, that are not explicit and codified.” (Bourdieu and Wacquant 1992:98)

³⁰ Bourdieu, Pierre, and Monique de Saint Martin. 1978. “Le Patronat”, *Actes de le Recherche en Sciences Sociales*, no 20-21, p.3-82.

means “to bend back”, Wacquant defines “Bourdieu’s brand of reflexivity” as “the inclusion of a theory of intellectual practice as an integral component and necessary condition of a critical theory of society.” (Wacquant 1992b:36). According to Wacquant, Bourdieu’s reflexive sociology differs from the others in three ways:

First, its primary target is not the individual analyst but the *social and the individual unconscious* embedded in analytic tools and operations; second, it must be a *collective enterprise* rather than the burden of the lone academic; and, third, it seeks not to assault but to *buttress the epistemological security of sociology*.³¹ (Wacquant 1992b:36, emphasis in the original).

Bourdieu’s reflexivity does not oppose objectivity as a goal, as some other reflexive sociologists do. He takes reflexivity as something that should be an essential part of sociological thinking. When the reflexive thinking only deals with the individual, his/her place within his/her branch and the relation of that individual scientist with his/her studies, it fails to deal with the limits of knowledge concerning a particular field (i.e. sociology or the social sciences). Yes there is a bias that comes from the individual, or more precisely, the social origins and place of the individual researcher, such as gender, ethnicity, class, etc.; here Bourdieu agrees with many other thinkers who advocate reflexivity that the individual researcher does have an effect on the “outcomes” of his studies; but the second bias concerns him more than the first:

it is that linked to the position that the analyst occupies, not in the broader social structure, but in the microcosm of the *academic* field, that is, in the objective space of possible intellectual positions offered to him or her at a given moment, and beyond,

³¹ Bourdieu explains in his interview with Wacquant: “the sociology of sociology I argue for has little in common with a complacent and intimist return upon the private *person* of the sociologist or with a search for the intellectual *Zeitgeist* that animates his or her work, as in the case with Gouldner’s [1970] analysis of Parsons in *The Coming Crisis of Sociology*. I must also dissociate myself completely from the form of “reflexivity” represented by the kind of self-fascinated observation of the observer’s writings and feelings which has recently become fashionable among some American anthropologists (e.g., Marcus and Fisher 1986, Geertz 1987, Rosaldo 1989, Sanjek 1990) who, having apparently exhausted the charms of fieldwork, have turned to talking about themselves rather than about their object of research. When it becomes an end in itself, such falsely radical denunciation of ethnographic writing as “poetics and politics” (Clifford and Marcus 1986) opens the door to a form of thinly veiled nihilistic relativism (of the kind that I fear also undergirds various versions of the “strong program” in the sociology of science) that stands as the polar opposite of a truly reflexive social science.” (Bourdieu and Wacquant 1992:72).

in the field of power. The points of view of sociologists, like any other cultural producers, always owe something to their situation in a field where all define themselves in part in relational terms, by their difference and distance from certain others with whom they compete. Social scientists are furthermore situated near the dominated pole of the field of power and therefore under the sway of the forces of attraction and repulsion that bear on all symbolic producers (Bourdieu 1971d³², 1988a³³, 1989a³⁴). (Wacquant 1992b:39, emphasis in the original).

The third bias is what I find more original to Bourdieu's work and more important for my thesis. This is the "intellectualist bias",

which entices us to construe the world as a *spectacle*, as a set of significations to be interpreted rather than as concrete problems to be solved practically, is more profound and more distorting than those rooted in the social origins or the location of the analyst in the academic field, because it can lead us to miss entirely the *differentia specifica* of the logic of practice (Bourdieu 1990a, 1990e). Whenever we fail to subject to systematic critique the "presuppositions inscribed in the fact of thinking the world, of retiring from the world and from action in the world in order to think that action" (Bourdieu 1990e:382), we risk collapsing practical logic into theoretical logic. (Wacquant 1992b:39, emphasis in the original).

Bourdieu argues for a reflexive sociology that does more than objectivizing the class background, gender or race of the "cultural producer", the sociologist; a reflexive sociology must be concerned with "the *invisible determinations inherent in the intellectual posture itself*, in the scholarly gaze that he or she casts upon the social world." (Bourdieu and Wacquant 1992:69, my emphasis). But I think the problem of the intellectual bias is not only that "we" cannot see the logic of practice. It is also a constituent of the situation that social scientists speak among themselves excluding the others, as the intellectualist bias, in Bourdieu's

³² Bourdieu, Pierre. 1971d. "Le marché des biens symboliques", *L'Année Sociologique*, 22, p. 49-126.

³³ Bourdieu, Pierre. 1988a. "A Long Trend of Change", *The Times Literary Supplement*, 12-18 Aug. 1988.

³⁴ Bourdieu, Pierre. 1989a. "For a Socio-Analysis of Intellectuals: On *Homo academicus*", *Berkeley Journal of Sociology*, 34, p.1-29.

terminology, is one of the chief reasons of doing “sociology for sociology’s sake,” speaking with no references to “outside of sociology.”

Consequently, Bourdieu does not like the reflexivity of the textual type concerned with ethnographical representation process. For him reflexivity is not about opening up the field notes or thinking about it afterwards; neither is it about writing in passive voice or with “I” s and “you” s. “It is not the individual unconscious of the researcher but the epistemological unconscious of his discipline that must be unearthed” (Wacquant 1992:41). According to Bourdieu, there is a resistance towards practicing sociology in an epistemically reflexive way, and this is a rather social resistance than an epistemological one:

Sociological reflexivity instantly raises hackles because it represents a frontal attack on the sacred sense of individuality that is so dear to all of us Westerners, and particularly on the charismatic self-conception of intellectuals who like to think of themselves as undetermined, “free-floating,” and endowed with a form of symbolic grace. (Wacquant 1992b:43-44).

Here, Bourdieu’s reflexivity shows the social within the individual, therefore aims to free sociology from these kinds of delusions.³⁵ Therefore Bourdieu’s “epistemic reflexivity” is not egocentric or logocentric, it is turned to the scientific practice itself and it is embedded within this practice. In *An Invitation to Reflexive Sociology* Bourdieu says:

Indeed, I believe that *the sociology of sociology is a fundamental dimension of sociological epistemology*. Far from being a specialty among others, it is the necessary prerequisite of any rigorous social practice. In my view, one of the chief sources of error in the social sciences resides in an uncontrolled relation to the object which results in the projection of this relation onto the object. What distresses me when I read some works by sociologists is that people whose profession it is to

³⁵ “A genuinely reflexive sociology must constantly guard itself against this epistemocentrism, or this “ethnocentrism of the scientist,” which consists in ignoring everything that the analyst injects into his perception of the object by virtue of the fact that he is placed outside of the object, that he observes it from afar and from above.” (Bourdieu and Wacquant 1992:69-70).

objectivize the social world prove so rarely able to objectivize themselves, and fail so often to realize that what their apparently scientific discourse talks about is not the object but rather their relation to the object.” (Bourdieu and Wacquant 1992:68-69, emphasis in the original).

In *Homo Academicus* (Bourdieu 1984b) where Bourdieu has made a sociological study on the sociological field itself, on the academic world (of social sciences) in France, he made a self-conscious epistemological experiment.

The idea behind this research was to overturn the natural relation of the observer to his universe of study, to make the mundane exotic and the exotic mundane, in order to render explicit what in both cases is taken for granted, and to offer a practical vindication of the possibility of a full sociological objectivation of the object *and* of the subject’s relation to the object—what I call *participant objectivation*. But I ended up putting myself in an impossible situation. Indeed, it turned out particularly difficult, if not impossible, to objectivize fully without objectivizing the interests that I could have in objectivizing others, without summoning myself to resist the temptation that is no doubt inherent in the posture of the sociologist, that of taking up the absolute point of view upon the object of study—here to assume a sort of intellectual power over the intellectual world. So in order to bring this study to a successful issue and to publish it, I had to discover the deep truth of this world, namely, that everybody in it struggles to do what the sociologist is tempted to do. I had to objectivize this temptation and, more precisely, to objectivize the form that it could take at a certain time in the sociologist Pierre Bourdieu. (Bourdieu and Wacquant 1992:67-68, emphasis in the original).

Bourdieu holds that scientific knowledge is possible and important. But he also agrees with Derrida and Foucault that knowledge, especially knowledge on the social world, is never free of power relations, that it is itself political; and should be subject to a deconstruction and historicizing. He argues that scientific knowledge is different from other types of knowledge also because of the historically established autonomy of the scientific field and its socially privileged claim to truth. It is this autonomy, or relative autonomy, and the institutionalization of the scientific field

that allows for deconstruction itself, as well. Bourdieu seeks to ground scientific rationality in social history. Reason is not a natural faculty, but it originates in the struggle of actors “in the name of the universal, for the legitimate monopoly of the universal, and in the progressive institutionalization of a dialogical language which owes its seemingly intrinsic properties to the social conditions of its genesis and of its utilization” (Bourdieu 1990e:389, quoted in Wacquant 1992b:48). For Bourdieu, a sociologist’s work is “to denaturalize and defatalize the social world, that is, to destroy the myths that cloak the exercise of power and the perpetuation of domination.” (Wacquant 1992b:49-50). This is why sociology is a political science. The academy can be influential in political matters. As it is already a part of political struggles, it should and can be a more self-conscious one: not just an arena or an unconscious player in power relations, but a player in a conscious and aimed political act.

Cultural producers of today face threats (such as state intervention, penetration of economic interests, the tendency to lose the ability of self-evaluation among the intellectuals...) that push them to choose between being an expert in the service of the dominant, or staying as “an independent petty producer in the old mode, symbolized by the professor lecturing in his ivory tower” (Wacquant 1992b:58). Bourdieu’s sociology, with such assertions, may be considered as an “attempt to transform the principles of vision whereby we construct, and therefore may rationally and humanely shape, sociology, society and, ultimately, ourselves.” (Wacquant 1992b:59).

In this chapter I tried to present some important texts of reflexive anthropology and sociology. These can be taken as examples of how sociologists and anthropologists think of what they are doing, talking through their own way of speaking *within* their profession:

The diverse post- and neo-colonial rules for ethnographic practice do not necessarily encourage “better” cultural accounts. The criteria for judging a good account have never been settled and are changing. But what has emerged from all these ideological shifts, rule changes, and new compromises is the fact that a series of historical pressures have begun *to reposition anthropology with respect to its “objects” of study*. Anthropology no longer speaks with automatic authority for others defined as unable to speak for themselves (“primitive”, “pre-literate”, “without history.”) (Clifford, 1986:9-10, my emphasis).

The excerpt above is like a summary of the story of reflexivity. It has a lot to say both on the personal experiences of the practitioners of social sciences, and more importantly, on the field of science itself. However, what concerns reflexivity about the field of social sciences is how knowledge is made here. As a consequence the reflexive move discusses social sciences “with respect to its “objects” of study”, arguing for changes in the related practices. But it is not limited to that only. As we see through the works of Edward Said, Talal Asad, Leila Abu-Lughod, Dorothy Smith and Pierre Bourdieu (and Antonio Gramsci from the activist front) sociologists and anthropologists are also concerned with the power inherent to what they do, and they problematize it.

However, the discussion lacks a very important aspect of doing sociology or anthropology in that it does not deal explicitly with their relationships with their audiences and how they communicate with people. The field of sociology is not limited to the relationships it has *within itself* and with its relationships with its “objects of study”. Social sciences are neither only constituted with their (power) relations with what they study, but also with the relationships established or not established with the people who are not social scientists and who are not directly studied by any particular research.

One way reflexivity addresses the question of the audiences of texts is the claim that now the ethnographic works are available to the natives themselves, as discussed

above. It is important to say that the availability of social science texts has consequences. However within the reflexive debate who else are the audiences of these texts is not discussed thoroughly, with the general consequences of the relationship of a social science text with its audiences. Moreover, the consequences of the availability of an ethnographic text to the natives are discussed with respect to the “new” responsibilities it puts on the researchers. “Now” that the texts are more available to the natives (which makes it explicit that they were not available before), there is an important “judge” to speak about the works of anthropologists and sociologists and to question their authority.

The ideas on the audiences to texts are not discussed in relation to the format of speaking in social science texts in the trend of reflexive critique. However, through the proposals of Leila Abu-Lughod we can trace the problem of availability of sociology or anthropology texts to wider publics in relation to the ways of communication. Although she doesn't put the problem this way, she suggests that we abandon some conventional ways of speaking in science in favor of a more humanist way of writing. Her aim in this proposal is mainly to overcome the problem of constituting some people as others in anthropological studies. The proposal she makes to change this situation is simultaneously one that would alter the way of communication in writing texts of anthropology. Writing about people, the subjects of anthropology, in another format, can be considered anthropological by its topic. This is in line with the argument of Clifford (1986) that anthropology texts are not fictitious but are fictions. In the next chapter, working through Thomas Kuhn's work on the structure of science, I will argue that they are not anthropology texts because of their format. For, we can infer from Kuhn's works that the format of communication is a constituent of a field of science. Nevertheless, if we think of the format of communicating in disciplines of science like an iron cage that keeps the information inside, away from the reach of outsiders, then finding other ways to communicate it, like Abu-Lughod proposes, is the gate for it.

CHAPTER 5:

Paradigm, Community, Format: Writing in Sociology

“The whole of science is nothing more than a refinement of everyday thinking.”
(Albert Einstein 1936, quoted in Hammersley and Atkinson, 1993:v).

In this chapter I will be looking at the established ways of expression in sociology through an examination of how texts are produced in the field. The question is not about the discourse any specific paper takes up, but about the format of communicating through sociology texts and how it affects the relations among insiders of sociology and relations to the outsiders. Thomas Kuhn (1996 [1962]) proposes that the concept of paradigm is a key one in understanding the pattern of change in sciences. I will argue that it is also useful in understanding the emergence of the prevailing regular ways of expression in sociology in their historicity. Here I will start by introducing Kuhn’s concept of paradigm and how he uses it in relation to communication and the formation of papers.

5.1. Kuhn’s Concept of Paradigm

The physicist Thomas Kuhn’s major work on the history of sciences, *The Structure of Scientific Revolutions* (1962) was groundbreaking (in epistemology as well as history) because of the different way he looked at how theories in science replaced one another and how they got accepted. Kuhn tells that we generally have opinions like science develops by accumulation, it progresses towards a single true end (*aklım yolu birdir*, “rationality is single”), because of the way the history of sciences is presented in textbooks. The pre-Kuhnian history of sciences implied that in science there is a puzzle to be solved and this or that scientist at some time has found the place of a piece of that puzzle because that piece belongs to that place; therefore sciences “find” something that exists somewhere, and this is discovery rather than

invention. Thomas Kuhn points to the teleology inherent in this understanding (instead of historicity, or doubt that science claims to be resting itself upon).

All the out-of-date theories that some historians of science don't call science but myths, beliefs, errors or superstitions now, were done using the same methods as those who are called science:

[The] once current views of nature were, as a whole, neither less scientific nor more the product of human idiosyncrasy than those current today. If these out-of-date beliefs are to be called myths, then myths can be produced by the same sorts of methods and held for the same sorts of reasons that now lead to scientific knowledge. If, on the other hand, they are to be called science, then science has included bodies of belief quite incompatible with the ones we hold today. Given these alternatives, the historian must choose the latter. Out-of-date theories are not in principle unscientific because they have been discarded. (Kuhn 1996:2-3).

In this case, Kuhn concludes, scientific development is not a “process of accretion” as it is implied to be. There are more than one lines for the development of science. Science has had different and changing images in different times and different societies. Applying the scientific method doesn't lead to one single true conclusion. Reaching one of the possible solutions is related to the personal experiences and choices and beliefs of the scientist. It is not a failure in method that distinguishes various schools in science from each other, “but what we shall come to call their incommensurable ways of seeing the world and of practicing science in it.” (Kuhn 1996:4)

Scientific research is based on some answers given to basic questions. These shared answers constitute the paradigm. According to Thomas Kuhn, paradigms are “universally recognized scientific achievements that for a time provide model problems and solutions to *a community of practitioners*.” (1996:x, my emphasis). “Effective research” doesn't begin before the paradigm is established. It is sharing these answers that form the community of any field of science.

When a paradigm is settled, then it is more or less certain what can be asked within that field of science and how the method is to answer that question. This means to put a problem in a certain way and to solve it accordingly. This is what Kuhn calls “normal science” and he thinks that the basic activity of normal science is just like “puzzle solving” in the standard sense of the term.³⁶ Science normally goes on its way like this, working according to its dominant paradigm until it no longer can. This happens when there are questions that cannot be answered through the paradigm constituent of that branch of science, questions that are at the same time too fundamental to be ignored.

[T]hen begin the extraordinary investigations that lead the profession at last to a new set of commitments, a new basis for the practice of science. The extraordinary episodes in which that shift of professional commitments occurs are the ones known in this essay as scientific revolutions. They are the tradition-shattering complements to the tradition-bound activity of normal science. (Kuhn 1996:6)

Kuhn claims that the scientific revolutions have “[e]ach transformed the scientific imagination in ways that we shall ultimately need to describe as a transformation of the world in which scientific work was done.”(ibid.). It is this change, the shift in the paradigm that makes a scientific revolution. Science, therefore, does not develop by accumulation of knowledge: the developmental pattern of what Kuhn calls mature science is through paradigm shifts. But this has not always been so and not for all fields of science (Kuhn claims this for contemporary natural sciences, he has doubts that social sciences are mature sciences in this sense, that have settled single paradigms. It is true for sociology that there is no single paradigm but different

³⁶ Kuhn calls the activities within normal science puzzle solving, as the paradigm provides and expectancy for the outcomes of these researches:

Puzzles are, in the entirely standard meaning here employed, that special category of problems that can serve to test ingenuity or skill in solution. [...] It is no criterion of goodness in a puzzle that its outcome be intrinsically interesting or important. On the contrary, the really pressing problems, e.g., a cure for cancer or the design of lasting peace, are often not puzzles at all, largely because they may not have any solution. [...] Though intrinsic value is no criterion for a puzzle, the assured existence of a solution is. (Kuhn 1996:36-37)

approaches. But having more than one paradigm, as in the case of sociology, doesn't mean that it is not a mature science.)

The absence of a paradigm is the absence of any tool to guide you in interpreting facts. Then you end up in a butterfly collection. “No wonder, then, that in the early stages of the development of any science different men confronting the same range of phenomena, but not usually all the same particular phenomena, describe and interpret them in different ways.” (Kuhn 1996:17). This diversity disappears as the science develops through the establishment of paradigms (this claim of Kuhn doesn't hold for social sciences, where there are different theoretical stands). When a person or a group is able to make a proposal that attracts most of the practitioners/researchers of the same field of interests, this proposal gradually settles as the paradigm and the discipline is formed or redefined:

The new paradigm implies a new and more rigid definition of the field. Those unwilling or unable to accommodate their work to it must proceed in isolation or attach themselves to some other group. [...] it is sometimes just its reception of a paradigm that transforms a group previously interested merely in the study of nature into a profession or, at least, a discipline. In the sciences [...] *the formation of special journals, the foundation of specialists' societies, and the claim for a special place in the curriculum have usually been associated with a group's first reception of a single paradigm.* (Kuhn 1996:18-19, my emphasis)

Paradigms, when they are adopted, are then acquired by others while studying in science, reading the achievements of their field from textbooks which often present them with their applications. In other words, a scientist acquires a paradigm by applying it, and often without being aware of it. Achievements that become classics do so because they are “sufficiently unprecedented to attract an enduring group of adherents away from competing modes of scientific activity” and they are at the same time “sufficiently open-ended to leave all sorts of problems for the redefined group of practitioners to resolve.” (Kuhn 1996:10). Paradigms are achievements that have these two characteristics. These “accepted examples of actual scientific

practice” then constitute traditions of scientific research. “To be accepted as a paradigm, a theory must seem better than its competitors, but it need not, and in fact never does, explain all the facts with which it can be confronted.” (Kuhn 1996:17-18)

5.2. “Normal Science”

When paradigms are settled, they provide conceptual “boxes” to “force nature” into them, into the way the paradigms work. The boxes are preformed and rather inflexible, and they impose the choice of problems. So some questions are relevant to the paradigm and some are not: “No part of the aim of normal science is to call forth new sorts of phenomena; indeed those that will not fit the box are often not seen at all.” (Kuhn 1996:24). Therefore a paradigm may “even insulate the community from those socially important problems that are not reducible to puzzle form” (Kuhn 1996:37). These problems are seen (from within the paradigm) as distractions for the development of science, therefore within normal science remain rather the problems that “only the lack of ingenuity” of the researchers can keep them from solving, and therefore normal science develops quite rapidly (ibid.).

Textbooks of normal science (written from within) hide the historical development of science through revolutions. According to Kuhn, history is written backwards in these texts, making references to past heroes of science as if they are parts of the same normal-scientific tradition, together with the professionals and students that practice it today. This hides the paradigm-based structure of science³⁷ by making it seem as if the same types of questions have always been asked, therefore presenting a cumulative progress of science. According to Ludwik Fleck, “Whatever is known has always seemed systematic, proven, applicable and evident to the knower. Every

³⁷ Peter Burke, in *A Social History of Knowledge*, discusses such practices as central phenomena to cultural history, “the naturalization of the conventional or the presentation of culture as if it were nature, invention as if it were discovery. This means denying that social groups are responsible for classifications, thus supporting cultural reproduction and resisting attempts at innovation.” (2000:86).

alien system of knowledge has likewise seemed contradictory, unproven, inapplicable, fanciful or mystical.” (1935:22³⁸, quoted in Burke, 2000:2). Thomas Kuhn says that it is in fact not possible for the past paradigms to have asked the questions of today, because that is not how they have viewed the world: one cannot have possibly asked the questions that have led to theorizing gravitational force if he saw the falling of a stone according to the Aristotelian paradigm that says stones fall because they are impelled by their nature to reach a final resting point.

In Kuhn’s view the proponents of the old paradigm replaced by a scientific revolution may never be able to make the convergence to the new, not because they are stubborn to accept their error but because they see no error in the normal-science that they had been successful practicing. Therefore, as Kuhn’s examples with Copernicus, Darwin and Maxwell suggest, the new paradigm is established rather not (always) when it is first proposed, but through the change of generations. As he quotes from Max Planck’s autobiography,

a new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it. (Planck, 1949:33-34, quoted in Kuhn 1996[1962]:151).

5.3. Paradigms and Communication

How is the concept of paradigm related to communications within and from sciences? Having a common paradigm enables scientists to take a body of knowledge for granted. (Although sciences involves a critical thinking of itself, this critical perspective also rests on a paradigm. This of course doesn’t mean that it doesn’t work at all.) This brings along the comfort of not having to start from the beginning in any attempt to explain something, scientist from the same field (constituted by the shared paradigm) don’t have to explain all the aspects of their

³⁸ Ludwik Fleck, *Genesis and Development of a Scientific Fact*, Chicago, 1979 (1935).

work, but they can focus on details of problems, tell only about the parts they criticize or what their contribution or study is. Thus the work of the scientists gains speed and efficiency. This, however, is only possible when one is speaking to the people from the same field of science. Then, when one tells about his research as such,

No longer will his researches usually be embodied in books addressed, like Franklin's Experiments ... on Electricity or Darwin's Origin of Species, to anyone who might be interested in the subject matter of the field. Instead they will usually appear as brief articles *addressed only to professional colleagues*, the men whose knowledge of a shared paradigm can be assumed and who prove to be the only ones able to read the papers addressed to them. (Kuhn 1996:19-20, my emphasis).

The situation seems clearer when one thinks of how studies had been before they could base themselves on a common paradigm. Practitioners of a field of science don't become scientists by the settlement of a first paradigm, they are still scientists before that. Yet Kuhn thinks that the result of their studies is "something less than science":

Being able to take no common body of belief for granted, each writer on physical optics felt forced to build his field anew from its foundations. In doing so, his choice of supporting observation and experiment was relatively free, for there was no standard set of methods or of phenomena that every optical writer felt forced to employ and explain. Under these circumstances, the dialogue of the resulting books was often directed as much to the members of the other schools as it was to nature. That pattern is not unfamiliar in a number of creative fields today, nor is it incompatible with significant discovery and invention. It is not, however, the pattern of development that physical optics acquired after Newton and that other natural sciences make familiar today. (Kuhn 1996:13)

Dialogues of scientists (of "mature sciences" whose paradigms are settled) are not directed to others but rather to scientists from the same field, since they have the grounds for it, thanks to the common paradigm. It's also important to note that Kuhn doesn't say that science was not science before its paradigm was set. Science

only had a different pattern, and a different dialogue, before the establishment of its paradigm. It is the paradigm that brings the consensus to a certain field of science, though it also brings strictness in dialogue along. In a differentiation from pre-paradigm stages of a science, books that communicate to the “layman” (in Kuhn’s words) are not common in mature sciences:

only in those fields that still retain the book, with or without the article, as a vehicle for research communication are the lines of professionalization still so loosely drawn that the layman may hope to follow progress by reading the practitioners’ original reports. (1996:20)

The settlement of a paradigm provides ways to expertise in science, it enables knowledge to get more profound in the discipline. This specialization at the same time means compartmentalization in sciences. In other words, Kuhn’s studies imply that if a field of science is established this means that it is somehow set apart from the rest of the world (by its paradigm), so that the non-members to that field can only learn about it through textbooks. The communications and means of communication of this field of science are now separated: scientific texts only address the members of the community.³⁹ “Except with the advantage of hindsight, it is hard to find another criterion that so clearly proclaims a field of a science” than having articles only for the reading of professionals, writes Kuhn (1962:21-22).

Kuhn claims that these practices are inherent to the structure of contemporary science from the settlement of the paradigm. This means that speaking to the insiders and excluding the outsiders from the dialogue is also inherent to sciences. This, I think, is the main reason behind the feeling of exclusion Dorothy Smith tells she has felt. This is why I find it absurd that she can propose to make a sociology for all while staying within the conventional forms of communication in sociology

³⁹ Those who are not members are not addressed by science texts, but they are reached by other ways of communication. Science museums, art works, popular science books and magazines –like TÜBİTAK’s publications- and some internet sites are examples. <http://www.emergentuniverse.org/STAGING/090109/> is a recent very creative work of telling about natural sciences to related and non-related people.

and not questioning the “structure” of communication of sciences that excludes non-scientists. Kuhn’s theory is disregarded in this very important argument he makes for sciences, not only in the works of Smith but throughout the critiques social sciences make to themselves as discussed in Chapter 3 and Chapter 4.

Paradigms, we can say, are a part of the “conventions” of sciences, or sociology at least, as Dorothy Smith proposes. It is not the result of any paradigm that sociology (or natural sciences if we follow Thomas Kuhn) doesn't speak to those outside but only to the insiders. It is the result of the conventions that were formed throughout the history of sciences, which includes the pattern proposed by Kuhn. Not speaking to outsiders about science (i.e. not writing books that address anybody but only articles that address the community of scientists in their own language) may be a sign of the establishment of a common paradigm for that field of science, as Kuhn says, but it has also become a very important part for the conventions.

When we think of communication, we might ask how paradigms are accepted and settled. The answers Kuhn provide to such a question (cited above) are rather on how the paradigm is acquired by the students of a discipline, or if it is not a first paradigm but a shift in paradigms, then on how it becomes the dominant paradigm for practitioners of the discipline. This is also because paradigms are known to their practitioners only, and those who are not insiders aren't a relevant audience on the settlement of paradigms. This is why Kuhn claims that “Scientific revolutions [...] need seem revolutionary only to those whose paradigms are affected by them.” (1962:92-93)

There are two things told above: first, that textbooks are those that present sciences both to their newcoming practitioners to be recruited, and to those outside the community, those who are normally not addressed by the insiders. Second, that textbooks are authoritative on how we perceive sciences. Kuhn writes more explicitly that

Both scientists and laymen take much of their image of creative scientific activity from an authoritative source that systematically disguises—partly for important functional reasons—the existence and significance of scientific revolutions. [...] As the source of authority, I have in mind principally textbooks of science together with both the popularizations and philosophical works modeled on them. (1996:136)

Let's think of this argument together with the claims made by Foucault and Smith that authority is the right to speak. Here we see again that we, the outsiders to the speaking in sciences, are not addressed by the papers who make up the main body of communication in sciences, but by the textbooks (and their "popularizations" that "attempt to describe these same articulations in a language closer to that of everyday life", Kuhn 1996:136) who are the authority to talk about sciences. What are sociologists doing when they write addressing just other sociologists and other social scientists? Take this as an event in its effects as Foucault proposes. What we (as sociologists) are doing, then, is excluding all the rest, all those who are not sociologists. This is how authority is exercised by excluding, also pointed out by the participants in focus group discussions. This exclusion is all the more striking when what sociology is talking about is basically the society. Sociologists talk about people *by* not talking to people. Resorting to sciences is not just a way of basing an argument on authority. It is a direct way of exercising the authority, creating it at the same time. Besides, not being addressed in this way doesn't only make the outsiders to sociology subject to authority; it also deprives them from any means of benefiting from all the nice things sociology provides the insiders with.

We may think the other way round and say that those who are not somehow familiar with sociology do not have the necessary means to be addressed by texts of intra-communication. This can also be taken as a side-effect of the settlements of paradigms. It can be discussed as another way of the exercise of power as well.

Kuhn himself, on the other hand, does not discuss the settlement of paradigms with respect to the dialogues between science and society, nor does he problematize authority established through science with the settlement of paradigms that enables speaking inwards. He just tells such effects of the settlement of paradigms and the authority through textbooks to explain how scientific revolutions are not visible because the dominant discourse of textbooks and history of sciences have looked at sciences from within the settled paradigms, within the outcomes of scientific revolutions, which made invisible the processes by which they took place. Kuhn looks at how texts shape ideas about science, but just in this sense. As a consequence he discusses “The Resolution of Revolutions” (the name of one of the chapters in his book), only with respect to how a paradigm is received in the scientific community, i.e. verification or falsification of theories. He asks how a paradigm replaces the one before it, but doesn’t discuss how it comes to be known by people as a new candidate, instead he tells the processes by which the scientific community accepts a new paradigm (remember the example with Max Planck above).

Thomas Kuhn argues that proponents of different paradigms are not able to communicate with one another about their studies. “pre- and postrevolutionary normal-scientific traditions”, according to him, are “incommensurable” (Kuhn 1996:148). Here the paradigm is defined as quite strong in its effects on communication. In Kuhn’s theory, disabling communication to outsiders is something inherent to the concept of paradigm, therefore inherent to the field of science itself. It is not arbitrary, neither is it a negative practice. On the contrary, the establishment of a format of expression with the acquisition of a paradigm is a necessity which may have positive implications as well. Kuhn argues that “the unparalleled insulation of mature scientific communities from the demands of the laity and of everyday life” increases the efficiency of professional life in sciences (1996:164):

The most esoteric of poets or the most abstract of theologians is far more concerned than the scientist with the lay approbation of his creative work, though he may be even less concerned with approbation in general. That difference proves consequential. Just because he is working only for an audience of colleagues, an audience that shares his own values and beliefs, the scientist can take a single set of standards for granted. (ibid.)

Therefore, the scientist does not have to think of how his work is received by other groups and he can work faster than the people who work with a more heterodox audience. The scientist does not have to study the problems that *need* to be solved, he/she can just focus on the ones he/she is *able* to solve.

In this respect, also, the contrast between natural scientists and many social scientists proves instructive. The latter often tend, as the former never do, to defend their choice of a research problem [...] chiefly in terms of the social importance of achieving a solution. Which group would one then expect to solve problems at a more rapid rate? (ibid.)

5.4. Community Structure

In the postscript to *The Structure of Scientific Revolutions* that Thomas Kuhn wrote in 1969, he discusses the “community structure” in sciences in relation to his concept of paradigm. Here he points out that a scientific community is generally taken to be consisting of the members practicing the same branch of science.

Usually the boundaries of that standard literature mark the limits of a scientific subject matter, and each community ordinarily has a subject matter of its own. [...] the members of a scientific community see themselves and are seen by others as the men uniquely responsible for the pursuit of a set of shared goals, including the training of their successors. Within such groups communication is relatively full and professional judgment relatively unanimous. (Kuhn 1996[1969]:177).

It is not only the subjects of study that make up a scientific community but rather the way they perceive the problems. “Both normal-science and revolutions are,

however, community-based activities.” (Kuhn 1996:179). Consequently, “members of a given scientific community provide the only audience and the only judges of that community’s work.” (Kuhn 1996:209):

Scientific knowledge, like language, is intrinsically the common property of a group or else nothing at all. To understand it we shall need to know the special characteristics of the groups that create and use it. (Kuhn 1996:210)

In *Imagined Communities*, Benedict Anderson (1991) discusses the emergence of nations and nationalisms, but the concept applies well to the working of science through communities:

It is *imagined* because the members of even the smallest nation will never know most of their fellow-members, meet them, or even hear of them, yet in the minds of each lives the image of their communion. (1991:6, original emphasis).

Anderson describes the print revolution and the emergence of newspapers and novels as important processes that enabled the imagination of nation, and in reading some first novels he notes that with making reference to familiar places and times or naming the characters as “our”, novels also imagine their readers to be recognizers of such references (1991:27). In a similar way, references made in academic texts to other works of the community are also intended for the understanding of the insiders, and thus they simultaneously constitute some people as the insiders. Anderson notes that in such novels the community is often not specified by name, similarly the scientific community of sociologists is also not specified: “it is already there” (Anderson 1991:32). This of course doesn't mean that anyone who doesn't belong to the community or isn't a candidate to it cannot read scholarly texts. But it rather feels like listening to the conversation of some people that are not speaking to you, like hearing the chat of the people sitting in the next table in the cafeteria, or putting your ear on the neighbor's wall to listen to their quarrels. In another sense it is like the “hey you!” hail by the police officer that makes the person a state subject, as Althusser (1972) explains, making you part of the community in the moment you feel the scholarly text is addressing you too.

Thus, Anderson argues that texts intended for the understanding of a group of people is central to the making of a community. For the academy, the possibility of this kind of a communication arose historically, through the settlement of paradigms, and also through the facilities brought by printing. If Thomas Kuhn provides a reason and a structural pattern of development for this situation, historian Peter Burke provides a detailed account of its emergence and effects in *A Social History of Knowledge* (2000). From the first scholarly journals to the concept of plagiarism and the use of footnotes, many conventional ways of practicing sociology in the academy can be traced back to early modern Europe, and Burke's work shows that the processes by which they came to be have been quite different from what we imagine them to be today.

5.5. Printing and the Republic of Letters

Peter Burke, a professor of social and cultural history, says that it only became "normal" to study science from a social point of view from the 1960s onwards (2000:10). In *A Social History of Knowledge* (2000) he studies science and other knowledges in the early modern period from this perspective. The period he is studying is *from Gutenberg to Diderot*, "from the invention of printing with movable type in Germany around the year 1450 to the publication of the *Encyclopédie* from the 1750s onwards." (Burke 2000:11). The era is a crucial one for this thesis because of the invention of printing; Burke's discussions are important in this sense to understand the format of texts produced in sciences.

In looking at the formation of the format of communication in sociology, it is relevant to look at what happened in the West, because sociology as we practice it today in Turkey and many other places is a product of that culture and we can claim that it is still under that domination, not only in languages but even more so in its

methods and format of communication. Burke's work is a contribution to understand how this came to be in historicity.

Burke makes a distinction between knowledge and information, “‘knowing how’ from ‘knowing that’, and what is explicit from what is taken for granted.” (2000:11). What is raw he takes as information, and knowledge is rather cooked, processed or systematized by thought. Burke tells that in early modern Europe, elites, claiming the authority on knowledge, identified it with their own knowledge: “they sometimes argued, like cardinal Richelieu in his *Political Testament*, that knowledge should not be communicated to the people, lest they become discontented with their station in life. The Spanish humanist Luis Vives was relatively unusual in his admission that ‘peasants and artisans know nature better than so many philosophers’.” (2000:13)

As Burke's work and other studies on the history of knowledge suggest, the separation between the knowledge of “the learned” and that of the “ordinary people” is one that is historically constructed, and there had been debates over it, supporting the separation or not. Diderot and d'Alembert's big project, *Encyclopédie*, was one of spreading knowledge. There, in the article on ‘Art’, “the distinction between the liberal and mechanical arts is described as an unfortunate one because it lowers the status of estimable and useful people.” (2000:17). As the distinction settled in, some people (exclusively men) started to call themselves “men of learning” or “men of letters”:

From the fifteenth century to the eighteenth, scholars regularly referred to themselves as citizens of the ‘Republic of Letters’ (*Respublica Litteraria*), a phrase which expressed their sense of belonging to a community which transcended national frontiers. It was essentially an *imagined community*, but one which developed customs of its own such as the exchange of letters, books and visits, not to mention the ritualized ways in which younger scholars paid their respect to senior colleagues who might help launch their careers. (Burke 2000:19-20, my emphasis).

The term “schoolmen” (scholastici) “was a term of contempt invented by the supporters of a new-style university curriculum, the ‘humanities’.” (Burke 2000:22). The teachers of this curriculum were called the humanists (*humanistae*) and they were earning their lives from teaching or depending on patrons. “The emergence of the word ‘humanist’ suggests that in universities at least, teaching the humanities encouraged a sense of a common identity among the teachers.” (ibid.). The emergence of printing widened the opportunities for making a living out of knowledge for the men of letters. Some of these people became printers, and some were translating, correcting or writing books on commission, which brought freedom from the dependence to patrons.

Indeed, Norbert Elias portrayed the humanists in general and Erasmus in particular in the manner of Mannheim as examples of the free-floating intellectual, their detachment linked to their opportunity of ‘distancing themselves’ from all the social groups in the world. (Elias 1939: 1, 73)⁴⁰ (Burke 2000:22).

“By 1600 or thereabouts a process of social differentiation within the European clerisy had become apparent.” (Burke 2000:25). Now there were writers, and other people who acted as knowledge managers, putting scholars in touch with each other and organizing information. University professors were by now becoming a distinct professional group:

Their sense of a separate identity is revealed by an increasing concern with academic dress and titles as well as by the rise of galleries displaying portraits of professors at the University of Uppsala and elsewhere. [...] the early modern professorate embodied intellectual authority. [...] Early modern scholars were coming to view their work as a vocation. [...] And librarians of the early modern period were important as “crucial ‘mediators’ in the republic of letters. (Burke 2000:25)

⁴⁰ Elias, Norbert. 1939. *Civilising Process*, Oxford.

The scholars started identifying themselves as a community. Gradually came into being what Pascale Casanova calls a “literature-world”,

a literature universe relatively independent of the everyday world and its political divisions, whose boundaries and operational laws are not reducible to those of ordinary political space. (2004:xii)

Casanova argues that the international literary space was formed in the sixteenth century when literature became a way of contention in Europe (ibid.). The world republic of letters then emerged has its own “mode of operation”, with its specific economy, hierarchies and history. For the literary world Casanova argues that there is an invisible “aristocracy”, a community who sets the framework, whose authority

consists in the supreme power to decide what is literary, and lastingly to recognize, or to consecrate, all those whom it designates as great writers: those who, in a strict sense, make literature; whose work incarnates (in some cases “for centuries after their death”) literary greatness itself in the form of universal classics, and sets the limits and standards of what is and will be considered literary—thus becoming the model for all future literature. [...] [The republic of letters] form a society that, in conformity with the law of autonomy, disregards political, linguistic, and national divisions [...] and sanctions texts in accordance with an analogous principle of indivisibility in literature. By rescuing texts from imprisonment within literary and linguistic boundaries, they lay down autonomous—that is, nonnational, international—criteria of literary legitimacy. (Casanova 2004:21-22).

Casanova’s claims for the literary world apply well to the republic of letters in general and publishing sociology and anthropology texts in “warranted sites” (Smith 1987:61) as well. The men of letters were publishing the books and the journals, and through these books and journals they developed a self-identity:

The European clerisy also defined themselves as the citizens of the Republic of Letters, a phrase which goes back to the fifteenth century but was employed with increasing frequency from the mid-seventeenth century onwards. *Nouvelles de la République des Lettres* was the title of a journal founded in 1684, one of an increasing number of learned or cultural reviews which were published from the

1660s onwards and helped create a new identity for their readers: the *Journal des Savants* (1665), The Royal Society's *Philosophical Transactions* (1665), the *Giornale de' letterati* of Rome (1668), the *Acta Eruditorum* of Leipzig (1682) and many others. (Morgan 1929⁴¹, Gardair 1984⁴², Laeven 1986⁴³)” (Burke 2000:29)

The term “journalist” came to use in French, English and Italian around 1700, distinguishing the writers of learned journals from the *gazetiers* who were reporting news for daily or weekly papers and had a lower status (ibid.). “In 1725, Daniel Defoe had already declared that ‘Writing... is become a very considerable Branch of the English commerce.’ He compared booksellers to ‘the Master Manufacturers’ and writers to ‘workmen’.” (Burke 2000:164). Burke argues that the press in Europe, and the periodical press especially, can be regarded as an institution

which made an increasingly important contribution to intellectual life in the eighteenth century, contributing to the spread, the cohesion and the power of the imagined community of the Republic of Letters. (2000:48).⁴⁴

In Europe the reference books came out in the fifteenth century as there were now too many books for one to find out in which one he/she could find out what s/he was looking for. Dictionaries that were few in the 1500s proliferated in seventeenth and eighteenth centuries.

An increasing number of reference books were produced for the particular sections of the public, such as the clergy, merchants, physicians, lawyers, women and so on. [...] Attempts were made to cater for general readers and to sell them encyclopaedias on the grounds that it was impossible to read the newspaper without their aid, or even to converse intelligently (hence the idea of the *Konversationslexikon*).” (Burke 2000:172).

⁴¹ Morgan, B.T. 1929. *Histoire du Journal des Savants depuis 1665 jusqu'en 1701*.

⁴² Gardair, J.-M. 1984. *Le 'Giornale de' letterati' de Rome (1668-81)*, Florence.

⁴³ Laeven, A.H. 1986. *Acta Eruditorum*. Amsterdam.

⁴⁴ “No fewer than 1,267 journals in French are known to have been founded between 1600 and 1789, 176 of them between 1600 and 1699 and the rest thereafter.” (Burke 2000:48, calculated from Sgard 1991). The point of publication in periodical form was precisely to provide news of the ‘Republic of Letters’, including obituaries and famous scholars and, for the first time, reviews of new books.

5.6. Knowledge of These, Knowledge of Those: Public and Private Knowledges

In 1451 there were about 50 universities in Europe, and just like corporations they had privileges “including independence and the monopoly of higher education in their region, and they recognized one another’s degrees.” (Burke 2000:33). Heretics, on the other hand, “who multiplied at about the same time as universities”, were “textual communities” formed through discussions of “ideas written down in books.” (Burke 2000:34). The universities were rather scholastic by the time of the Renaissance, when the ideas of the humanists gradually influenced “the unofficial curricula”, though rather not the institutional regulations. “The challenge to the establishment now came from ‘the new philosophy’, in other words from what we call ‘science’.” (ibid). With exceptions, science was rather growing outside of the universities when it first came into scene, and it was quite “public”:

In London, for example, the lectures at Gresham College, which began in the early seventeenth century, were open to all, and most of them were delivered in English, not the Latin customary at the universities. In Paris, Théophraste Renaudot organized lectures on a wide range of subjects for a wide public at his Bureau d’Adresse from 1633 onwards. The Royal Garden in Paris, which opened to the public in 1640, offered lectures on anatomy, botany and chemistry. (Hill 1965⁴⁵, Mazauric 1997⁴⁶, Ames-Lewis 1999⁴⁷) (Burke 2000:42-43)

“Public” lectures, botanic gardens, observatories, cafés or informal salons under aristocratic patronage became the new places of learning, which encouraged innovation and discussion. Learned societies were organized in such places. During the enlightenment, less formal spaces, such as the salons and coffeehouses had a part in communicating ideas. The printing-house bookshop was another place where people could meet and speak and at the same time look at the new books.

⁴⁵ Hill, C. 1965. *Intellectual Origins of the Scientific Revolution*, Oxford.

⁴⁶ Mazauric, S. 1997. *Savoirs et Philosophie a Paris dans la Premiere Moitié du 17e siecle: Les Conférences de Bureau d’adresse de Théophraste Renaudot*.

⁴⁷ Ames-Lewis, F. (ed.) 1999. *Sir Thomas Gresham and Gresham College*.

Learned societies were organizing expeditions, offering prizes, and exchanging visits, letters and publications, forming an international network of semi-professional organization, from which emerged the “professional scientist” of the nineteenth century (Burke 2000:46-47). At the time of the enlightenment, universities worked as teaching institutions, but rather not the places for new ideas: “They suffered from what has been called ‘institutional inertia’, maintaining their corporate traditions at the price of isolation from new trends.” (Burke 2000:48). At this point Burke presents isolation and resenting change as inevitable consequences of institutionalization of universities, with the domination of groups who have interests in the system and do not want to loose their intellectual capital: “There are social as well as intellectual reasons for the dominance of what Kuhn calls ‘normal science’.” (Burke 2000:51)

the social history of knowledge, like the social history of religion, is the story of the shift from spontaneous sects to established churches, a shift which has been repeated many times over. [...] On the one side we see open circles or networks, on the other institutions with fixed membership and officially defined spheres of competence, constructing and maintaining barriers which separate them from their rivals and also from laymen and laywomen. (Kuhn 1962, Shapin 1982⁴⁸, Elias 1982:50⁴⁹)” (Burke 2000:52).

In the seventeenth century, knowledge was multiplying and becoming more specialized in relatively large cities of Europe, which were home to the “open circles” as well as the closed ones:

The public spaces of cities facilitated interaction between men of affairs and men of learning, between gentlemen and craftsmen, between the field and the study, in short between different knowledges. Forms of sociability had, and still have – their influence on the distribution and even the production of knowledge. (Burke 2000:56)

⁴⁸ Shapin, S. 1982. “History of Science and its Sociological Reconstructions”, in R.S. Cohen and T. Schnelle (ed.) *Cognition and Fact*, Dordrecht.

⁴⁹ Elias, Norbert. 1982. “Scientific Establishments” in N. Elias, H. Martins and R. Whitley (ed.) *Scientific Establishments and Hierarchies*, Dordrecht.

It was such “forms of sociability” that, I think, have affected the emergence of a certain format for telling about ideas and studies in sociology and other sciences. The format used now for this communication, whether in writing a thesis or a paper for a scholarly journal, thus emerged from the communication of insiders, thus it serves for the legitimization and reproduction of this very type of communication, and not any other.

Intellectual producers work within the frameworks of traditions of academic institutions. Institutions, as we also see in Smith’s works, don’t have to be universities. The format of conversation, and especially the type that takes place through texts can also be regarded as an institution. David Olson writes that “One of the dramatic facts about Renaissance literacy was the impact writing had when texts came into the hands of the ordinary readers, the wrong people!” (1998 [1994]:xvi). So, texts are not always seen by all to be intended for all. In this aspect printing has had implications in different senses at the same time: institutionalizing knowledge, specializing it, closing it to circles and formatting its communication, thus limiting it to some; while simultaneously making knowledge available to greater number of people, diffusing it through the printed book. The replacement of the manuscript with printing literally created a much wider public, a much diverse audience, to communications in textual form (Trujillo Moreno 2008). With the invention and growing use of printing, knowledge was centralizing and decentralizing at the same time. Burke links the centralization of knowledge with the advent of printing to the rise of a world economy and the centralization of power (2000:57). He also tells that in early modern Europe,

cognition was linked even more closely to production via printing, and this led to a more open system of knowledge. The invention of printing effectively created a new social group with an interest in making knowledge public. (Burke 2000:176).

In the 1600s there were many newspapers that effectively spread information. The audience of these publications was now “the learned”. The libraries increased in

importance as well as numbers (Burke 2000). Burke already uses terms like “scholars” and “scholarly knowledge” as he speaks about the libraries of the 17th century, which demonstrates a separation between types of knowledge – the scholarly and non-scholarly. And now the processing of knowledge was also institutionalizing:

As items of information moved along the route from the field to the city, many different individuals added to their contribution. By this means knowledge was ‘produced’, in the sense that new information was turned into what was regarded – by the clerisy, at least – as knowledge. (Burke 2000:75).

After processing in the city, knowledge was distributed or re-exported through print, a medium which weakened geographical barriers, ‘dislocating’ knowledges from their original milieux. (Burke 2000:77-78)

Discussions on knowledge included the relationship between theoretical and practical knowledge (*scientia* and *ars*: science and art)⁵⁰, and private and public knowledge (Burke 2000). “Private” here is rather not in the sense of “personal” knowledge but in the sense of information restricted to a particular elite group. In this sense, private knowledge included the secrets of the state as well as secrets of professions, which included “scientific” ones:

Alchemical secrets, for example, were transmitted, sometimes in cipher, via informal networks of friends and colleagues or within secret societies. Technical secrets were shared within guilds of craftsmen, but outsiders were excluded. [...] The question of what kinds of knowledge ought to be made public was a controversial one, answered in different ways in different generations and in different parts of Europe. [...] Some learned societies were more or less secret societies, while others, like the Royal Society of London, were concerned to make knowledge public. Over the long term, the rise of the ideal of public knowledge is

⁵⁰ “A vivid example of the employment of these categories in a practical complex comes from the building of Milan cathedral around the year 1400. In the course of its construction a dispute developed between the French architect and the local master masons. A meeting of the masons argued that ‘the science of geometry should not have a place in these matters since science is one thing and art another’. To this argument the architect in charge of the enterprise replied that ‘art without science’ (in other words, practice without theory) ‘is worthless’ (*ars sine scientia nihil est*). (Ackerman 1949)” (Burke 2000:83).

visible in the early modern period, linked with the rise of the printing-press (Yates, 1979⁵¹; Stolleis 1980⁵²; Eamon 1994⁵³) (Burke 2000:83).

The reformation, for example, was also a debate on public and private knowledge, on sharing the religious knowledge with the laity (as Luther argued it should be). In the early modern period there was also a separation between higher and lower types of knowledge, a “hierarchy in the intellectual organization of knowledge,” as Burke calls it. “Male knowledge, including knowledge of the public sphere, was regarded, by males at least, as superior to female knowledge, more or less limited to piety and the domestic realm.” (Burke 2000:84). Similarly, liberal knowledge was considered superior to useful knowledge. Theology, law and medicine were the higher faculties in Europe, and thus considered more “noble,” while in the Muslim system the distinction was made between the Islamic sciences and the foreign ones (arithmetic and natural philosophy), between religious and secular studies. These are examples of “the projection of the social hierarchy onto the world of the intellect.” (Burke 2000:92). The organization of faculties and curricula and the articles in the encyclopedias reflect a view of knowledge which is at the same time a view of the world; after all, the world was often described as a book from middle ages onwards (Burke 2000:94).

Through “the advancement of learning”, one of the changes was the use of figures and statistics, “associated with the new ideal of impersonal or impartial knowledge, of what would later be called objectivity” (Burke 2000:110). A second change was in the increase in importance of useful knowledge in its relation to liberal knowledge. “Reversing the dictum of the French architect in Milan in 1400, the Baconians might well have said in 1700 that ‘theory is worthless without practice’, *scientia sine arte nihil est.*” (ibid.). Useful knowledge was now something respectable and science aimed more to promote knowledge with applications.

⁵¹ Yates, F. 1979. *The Occult Philosophy in the Elizabethan Age.*

⁵² Stolleis, M. 1980. *Arcana Imperii und Ratio Status*, Göttingen.

⁵³ Eamon, W. 1994. *Science and the Secrets of Nature: Books of Secrets in Early Modern Culture*, Princeton.

Novelty, something that had been looked down before, started to become an important part of science in the beginning of the seventeenth century.

In the eighteenth century, the ideal of intellectual exploration was sometimes summed up by a quotation from Horace, taken out of its original context and turned into the slogan ‘Dare to know’ (*sapere aude*). (Venturi 1959⁵⁴, in Burke 2000:114).

5.7. Power, Control, Knowledge

The main story to be told here is one of the accumulation of information, both responding and leading to the increasing desire of rulers to control the lives of the general population, whether to tax them, conscript them into the army, or feed them in time of famine. (Burke 2000:119).

Knowledge is power, as Foucault says (1977). In addition to the contexts studied by Foucault and others of how power-knowledge is exercised, Burke gives examples of the ways early modern states, empires, and rulers (including the church) collected information, led by their desire for control. Censor was also practiced widely in the early modern period. Monopoly of information meant monopoly of power.

Governments sometimes needed to make information public for their own purposes. It was obviously to their advantage to spread the knowledge of laws and other regulations, which were regularly proclaimed aloud as well as printed and posted in public places (Fogel 1989)⁵⁵. The problem, from their point of view, was to keep a balance between giving the public too little information, a situation which encouraged the spread of wild rumours, and giving them too much, which encouraged ordinary people to comment on affairs of state. (Burke 2000:146)

Foucault doesn't discuss power or knowledge as possessed/shared but as exercised. But if we follow Burke's arguments that there have been distinctions among knowledges, especially the distinction between public and private knowledges, we can ask whether sharing knowledge, whether making it public and open, would

⁵⁴ Venturi, F. 1959. “Contributi ad un dizionario storico”, *Rivista Storico Italiana* 71.

⁵⁵ Fogel, M. 1989. *Les Cérémonies de l'information*.

challenge power. Smith emphasizes that “established sociology” excluded women’s experiences. Thus, knowledge, science, can build itself upon exclusions. This is what makes “normal science” through the acquisition of a paradigm. Then power might not be exercised at the same level through knowledge in all its forms, but rather more strongly with the more private forms of knowledge. Anything known by everyone would be much more “powerless” in contrast to knowledge appropriated by some, not told to the rest, or told only in authoritative ways.

Kuhn’s presentation of a paradigm is something that makes normal science possible, in its private, specialized, and close form. The format of communication within this type of knowledge is also one that is specific to insiders, thus excluding the outsiders. The format comes through the establishment of the paradigm and puts the boundaries to the science. Thus, a science is unreachable to the knowledge of those outside its realm not because it is kept secret. On the contrary, scientists claim transparency to be a principle of the working of sciences (i.e. Neumann 2006, Wallace 1971). But science is a kind of private knowledge through its format which works like a crypto, making it unintelligible from the outside. It is not the paradigm that makes the communication so, though it is the paradigm that enables the format to operate this way. Like a book distributed for free, sciences often claim that they can be read by anyone, that it is public and transparent, but the alphabet or the language by which the book is written is known only by some.

Think of censorship in the contemporary world: scholarly journals and papers are rather not subject to censorship as are films and non-scholarly books and even music and people themselves, which are probably harder to censor. Censorship is essentially a process of hiding something from some people; academic materials of social sciences are not relatively free of censoring because science claims transparency and everyone respects that, but because scholarly works are already not public. Today, the “closeness” of knowledge is not a matter of permission, but of specialization.

For the field of literature Casanova claims that those who assess the value of literary works, like literary critiques and translators,

are naively committed to a pure, dehistoricized, denationalized, and depoliticized conception of literature; more than anyone in the world of letters, they are firmly convinced of the universality of the aesthetic categories in terms of which they evaluate individual works. (Casanova 2004:23).⁵⁶

Similarly, sociologists do not question the universality of the format they use for communicating their work. Scholarly texts of sociology speak on a ground of universalism, even when a particular text is in fact criticizing or rejecting universalism; the text seems to be speaking to everybody, whether American, Korean or Kurdish, but actually it speaks to the (usually English-speaking) Korean, American, Kurdish or Brazilian *sociologist / scholar*, and not to the villager or carpenter. Sociology as a science is thus put in an iron cage that isolates it from the knowing of those outside it, making it power-knowledge. And those that can exercise this power-knowledge are those who know the format, the language of the book, the privacy of which is assured through its acquisition, the education. Thus we can also say that the format puts knowledge into a *golden* cage, rather than an iron one (I thank my supervisor Helga Tılıç for the naming “iron cage”, and my friend Süha Ünsal for the following suggestion of “golden cage.”)

⁵⁶ Casanova proposes to look at texts in themselves and within the world literary realm as a whole, while dominant approaches in literature usually prefer either one, not both at the same time. In the quote above she argues that it is these approaches that are responsible for such a settlement of the field, and an understanding with such lacks to prevail in it.

CHAPTER 6: Conclusion

*First come I. My name is Jowett.
There's no knowledge but I know it.
I am the master of this College,
What I don't know isn't knowledge.*

(Anonymous, attributed to Benjamin Jowett,
vice-chancellor of Oxford University at 1882. Quoted in Liberman 2008)

In this thesis I have tried to make a discussion about how sociology and anthropology are communicated in the textual format that is legitimated as that of sociology and anthropology as sciences, through publications made in “warranted sites” in the ways required (Smith 1987:61). The discussion is based on the assumption that because sociology and anthropology speak on the social life of people, what they say should be open to the reach of everybody. This doesn't mean that everybody should read sociology texts or anthropology should be popularized to attract more attention. In this study I have just tried to question whether sociology and anthropology texts are at the reach of a non-scientist subject who could nevertheless be interested in what sociology and anthropology as sciences based on social life have to say about his/her world.

This question challenges the assumption that sociology and sciences in general are public when they are published (as in Neumann 2006, Evans 2002). The questions of this thesis originate in the discomfort I felt with the reaching or not reaching of the work done in sociology to audiences other than sociologists and the like, crystallized in the expression “is this going to be yet another book in the shelf?” I have tried to frame this question with Smith's critiques of sociology based on the arguments that it excludes some people. Then, by making focus group discussions with graduate students of scientific disciplines on social life (sociology,

anthropology, ethnology, and media and cultural studies) the experiences and opinions of other people in related topics was briefly presented and discussed. This empirical part, however, is not the base of this study, it is rather an effort to enrich the thesis by providing it with the experiences of students who are both beginners in practicing in their disciplines and also learners and observers of it.

Doing science is not only about principles, rules or aims. Doing science has historical and social dimensions which are seen in the practice and in universities. Science is related to capitalist relations of production, gender, power and domination through the ways it is constituted and practiced, especially in the universities. These certainly determine to various degrees what is produced and legitimized as science. Briefly, what is produced in sociology and anthropology is never separated from how it is produced. Although “pressures” in doing science is not a central theme in this study, a brief discussion on these aspects was provided to understand the communication between sociologists and other people within the structure of the practice of science.

There are many ways to communicate what is done in sociology, from conventional forms in science such as theses, journal articles, books, conferences or symposiums to other ways of expression that are not considered legitimately scientific, like speeches delivered on the radio or TV, internet websites, and works of art from cinema and theatre to songs and novels. I have chosen to work only on the published texts of sociology or anthropology for the purposes of manageability of the study. These are also the constitutive media of the discipline (Kuhn 1962, Burke 2000) and the conventional manner of expression of sociology (Smith 1991). As it was stated in the focus group discussions as well, the format of expression in these texts is a way of legitimizing these texts as science and thus a way to make what is said authoritative. The participants have cited various aspects of doing science to be parts and constituents of the format of expressing it; from the authority in science texts through exclusions they make to the alienation of sociologist-as-a-worker from

what she does; and from the sites to make publications to the institutional pressures on the academic to make such publications.

The discussion on doing science is followed by a discussion of the self-critique by sociologists and anthropologists on their own disciplines. The practices of doing science in general lay the background for this discussion, where sociologists and anthropologists, instead of writing on pressures on the universities and science as a vocation (Weber 1995), criticized their own practices as anthropologists and sociologists in relation to the people they study. This reflexive criticism concerns the relations they establish with the “objects” of their works, as part of a general trend of criticizing objectivity, the claim of universality, of reaching single truths for social realities and positivism in social sciences (Tedlock 1991), proposing ways to have more humanist (Abu-Lughod 1991) relations with the people they study on/with and making this explicit in their texts.

However, reflexive criticism has a two-sided focus: on what the sociologists and anthropologists are doing in relation to the people they study. This two-sidedness thus excludes any possible other parties that could be interested in the knowledge produced through social sciences. Reflexive critique also disregards the structure of communication in sciences. However, with a concern on representations and textual politics (Clifford 1986), and refusing the suppression of subjectivities in social science texts, it opens ways for producing various other types of texts. These texts can be very creative, plus able to communicate with different circles outside those of scientists, although this was not the main concern of the critique.

What is missed in the reflexive critique is the relationship a text establishes, or not, with audiences, and how the format of conversation influences this. I have tried to think of this conversation as a dialogue as proposed by Bakhtin and used in Smith’s concept of text-reader relationships. I argued that sociologists should think more on who they are addressing in their works. In an addition to the questions of for whom

sociology is *done* for, whose interests it potentially serves, and who it represents, sociology should be discussed in relation to its audiences. This study has been an attempt to contribute into this discussion.

I discussed the establishment of the format of communication in sciences through Thomas Kuhn's concept of paradigm (1962). Kuhn argues that with the acquisition of a paradigm in a field of science, the members of that field tend to communicate their studies to the other members of the community rather than a wider public. Thus he ties the format of communication to the very structure of science. This is an aspect of Kuhn's work that has not been studied on in relation to conversations within or from academy.

As Kuhn's work on the structure of science has no focus on power, I have tried to understand its relations to power through Michel Foucault's methods of genealogy and archaeology of knowledge. Genealogy and archaeology of knowledge enable us to look at the format of sociology texts as an act in its effects and how it has come to be as such. Thus we can see that format itself becomes an exercise of power as it includes and excludes speakers and audiences.

Kuhn's presentation of a paradigm is something that makes normal science possible, in its private, specialized, and close form. The format of expression in science, constituted through the acquisition of a paradigm, thus works like a boundary that keeps the practitioners of a science inside, and contributes for them to become a community (Burke 2000, Anderson 1991), while at the same time excludes those who are not members from reaching what is produced inside the circles of sciences. The format then becomes an iron cage, limiting sociology as a science to speak only to itself. Thus, a science is unreachable to those outside its realm not because it is kept secret; it is its format that makes it a private knowledge.

This certainly doesn't mean sociology is “useless” or “worthless”, it just means that it could have been more “public.”⁵⁷

At the point of discussing the publicity of sociology, a rather new perspective, “public sociology” comes in. This is not a particular theory or method but a way of doing sociology that would aim to engage in audiences wider than the academy. Although not a dominant trend in sociology, public sociology provides important openings to the discussions on sociology. The concern of Michael Burawoy (a critical sociologist who had been the chair of American Sociological Association in 2004, addressing the issue in his presidential speech) with public sociology, made it discussed more widely. In 2000s, *Contexts*, the magazine of American Sociological Association, was founded for the purpose of “bringing sociology to general readers” (Burawoy and VanAntwerpen, 2005). Burawoy contrasts public sociology with academic sociology that primarily addresses itself. He argues that the passions that originally draw one to study sociology, like social justice or human rights, are then channeled to working for academic credentials. For public sociologists, sociology should engage more explicitly with political activism, social movements or debates in the civil society. According to them,

Social science as public philosophy is public not just in the sense that its findings are publicly available or useful to some group or institution outside the scholarly world. It is public in that it seeks to engage the public in dialogue. It also seeks to engage the ‘community of the competent,’ the specialists and the experts, in dialogue, but it does not seek to stay within the boundaries of the specialist community while studying the rest of society from outside. (Bellah et al. 1985⁵⁸, quoted in Burawoy and VanAntwerpen 2005).

⁵⁷ Analyzing the situation of not speaking publicly but rather to oneself would be very meaningful with Hannah Arendt’s thinking. Very briefly, in *The Human Condition* (1998[1958]), she argues that acting and speaking publicly (i.e. to humans as one’s equals) is a condition to being free and being human. (Interpreting the ways of speaking in sociology with Arendt’s claims was my initial project in deciding to study the communication of sociology, but then I couldn’t take the challenge to do that.)

⁵⁸ Bellah, Robert; Richard Madsen, William Sullivan, Ann Swidler and Steven Tipton. 1985. *Habits of the Heart*, Berkeley, University of California Press.

For Burawoy, sociology has multiple *potential* audiences, from media audiences to policy makers, from silenced minorities to activists and social movements, and incorporating these audiences to the debates within sociology would “inspire and revitalize” the discipline in a revolutionary way (Burawoy 2004). This proposal is certainly not without critiques, based on the argument that sociology as a discipline should not be “handed over” to activism (see Deflem 2004). The discussions in this thesis demand further studies on public sociology. It has been a limitation of the study that I haven’t done it here.

The relationship between activism and doing sociology and the question of the audience of a sociology or anthropology text is reflected also in the problems of writing sociology texts, as expressed by the participants to focus group discussions. Jonathan Neale, a radical anthropologist and an activist, addresses these relations in “Ranting and Silence: The Contradictions of Writing For Activists and Academics” (2008). In contradistinction to the proposals of public sociologists to address wider publics in doing sociology and relate it directly to activism, Neale suggests to split the texts for different audiences. He puts it from the start that academic texts are read by the academics and thus should be written for them, and if one wants to reach other audiences she should write another text⁵⁹ (this was told me by teachers and friends as well). Marxism, or activism in a more general sense, is a different project from anthropology or sociology according to Neale. In a distinction from a critical anthropology or a critical sociology where the subject and the aim would be the discipline, for a radical anthropology (or sociology) the subject would be the world, not the discipline (Neale 2008:225).⁶⁰ As it is obvious in the group

⁵⁹ For example, in reading Marx and other Marxist writers, Neale recalls that French Marxist philosophers like Althusser and Balibar

were much harder to understand than Marx, partly because Marx is just very clear, but also, I think, because Marx is honest. We were looking for revolution in Marx, we found it, and we understood him. We were looking for it in Althusser too but couldn’t find it, so we had difficulty making sense of what he was saying. (2008:224)

⁶⁰ “Then there is the discipline ploy”, writes Neale:

‘That is interesting, but it is not anthropology,’ or ‘not history’, or ‘not sociology’, or ‘not my field’. This tactic seems ideologically neutral. What it actually does is forbid holistic thinking. You cannot think about connections as long as you think within a discipline. Yet

discussions with the graduate students, we often want to put our political considerations into the sociology and anthropology texts we write. It is true that sciences are inseparable from politics, and so are our identities as activists and students or sociologists, but it is also true that as Neale points out it makes it harder to communicate in writing. Neale's point is,

You need not harbour illusions: an article in an academic journal will not change the world. However, it will inform some people and be valued by those academics who think. (Neale 2008:247)

Moreover, doing sociology and anthropology changes with the pressures and constraints from everywhere. The pressures "silence some people within the academy" (Neale 2008:241). The pressures are real and as they leak into the texts they change the readers' understanding of sociology and anthropology. Anthropologists, for example, aren't likely to criticize the state policies of the countries they study in, for if they do they and their students aren't likely to be accepted to enter that place again. "This produces students who have read the monographs and think that the national politics, corruption, the American embassy and the secret police are unimportant in understanding village life." (Neale 2008:239).

Neale proposes that we write separately for separate audiences, to academics in their language and in their media, and for the social movements and activist in their journals and languages. I infer from Neale's text that he puts the audience in the very important position of validating a text (as in psychoanalysis, his example, where you know what you put forward is correct if the person under analysis benefits from it). "In the end, the patient is the judge" (Neale 2008:243). Then again, if in sociology and anthropology you write for the academics as your audience, the people you write about are not your judges (unless you're writing

this appeal to disciplinary loyalty is often met with a general chuckle. [...] The professor is saying, 'We won't talk about it, because we do not talk about things like that. And if we have to discuss them, we do not do it that way.' (2008:241)

about sociologists themselves, like I do). Academy writes for the academy, it validates itself, it legitimizes itself. A homemade circle.

On the other hand, the extensive focus on the structural side of the communication has been another limitation to this study. For the sake of making a more focused study on how the format of expression constitutes communication of discourses produced in sociology, I have largely disregarded how the audiences to texts influence the communication and the discourse in turn. Thus this thesis presents only one side of a reciprocal relationship.

Another question concerns the relationship between universality and locality. Özlem points out that within the scientificist discourse of our day, the desire to make (use of) knowledge as economic and political power is present in the term “information society” (1998a:32). Propagating knowledge on the grounds of its universality, transparency and openness to all thus serves for the dominant classes (ibid.). I have tried to question these assumptions in discussing the implications of the structured format of written expression employed in anthropology and sociology. Reading sociology and anthropology texts from various parts of the world, their format of expression is more or less consistent, so are the ways they are and they are expected to be published. In this study I look at these phenomena from Ankara, Turkey, by the year 2009, with the experiences of my and other graduate students from the universities in Ankara. The study has no claim on universality, but it discusses phenomena that have a claim of universality (science and its format of expression).

In writing this thesis about the format of writing in sociology, I myself have been learning how to write a thesis, which is also about the format itself. In beginning this study I was familiar to the format as a reader. In writing it I have to practice it and conform to it, while at the same time trying to problematize and criticize it. A tension that arouses from these contradictions prevails in the thesis. As it was

expressed in the focus group discussions by Cemal, it has become a thesis that eats itself. It has been problematic that I have tried to problematize something that I am not thoroughly able to practice. But I hope that this has also added the strength of making observations from the liminal.

In this study I might have over-emphasized the limiting aspects of the format. Thus I would like to stress again here that it is not an arbitrary formal constraint. The format is also necessary for communication, and not only in sociology and anthropology or sciences, but for any way of communicating. Having common ways for expressing things, just like the working of languages, is a must to for communication. The positive aspects of the acquisition of a format can be seen in Kuhn's works on the paradigm. I never mean to propose that writing in sociology should have no format at all. We can even say it is inevitable to have formats, all texts are closed to some extent, and knowledge is structural and limited by its constitution, as Erdoğan Yıldırım argued in the committee of this thesis. Nevertheless, in this study my aim was not to discuss whether it is possible to do sociology without a format, or to speak without jargons and technical terms. I have only wanted to question the implications of having this structure of communication on the relations of sociology with the rest of the world.

Moreover, the format of writing sociology and anthropology is also historical. A sociology text written in 1920s or 1950s communicates in a different way than it does today. While focusing on the format of contemporary sociology and anthropology texts I disregarded that the format itself is not stable in time. How formats are changing could be a revealing question to ask.

If the format keeps the speech of sociology in an iron cage (my friend Süha Ünsal recently suggested calling it a golden cage), the arguments of Leila Abu-Lughod and Thomas Kuhn provide hints to open up a gate in the iron cage of the format. A trait of excluding and including might be one that holds true to all types of formats,

or more clearly, all ways of addressing audiences, from the science museums to films. But these formats other than those of science might be more inclusive in that they don't operate on grounds of being a member of a scientific community or not. Kuhn's claims are on the structure of science: when speaking "scientifically", the paradigm is taken as a common ground. But people don't have to have a shared paradigm to be addressed by a novel. And the novel or other forms of communication don't have formats that aim to stabilize messages as the conventional formats of expression in science do. Thus, the iron cage of the format is one that keeps sociology closed inside *as a science*. It is not closed to creative ways to taking it out or opening it up in "unscientific" ways. What keeps sociology limited to the sociologists, then, is its claim to science though its format. Nevertheless I want to stress here that what I tried to do has not been a problematization of any popularization of sociology. I have no assumption or claim that taking sociology out of scientific circles is or should be a responsibility of the sociologists. If any reader to this study gets the idea from this text that I wish sociologists or anthropologists to be more "mediatic" or write popular books, this is only for any deficiency of my command on expressing oneself in a sociology text.

Although initially I imagined circles outside the academy as audiences to this study, in the end I find myself speaking again to sociologists and anthropologists (more specifically, students of these disciplines). I still find it worth problematizing that in academy it is difficult, if not impossible, to write to other audiences. The discussions of this study are in no way final, but only pieces of a process in continuity.

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APPENDICES

Appendix 1: Questions for Focus Group Discussion

- How old are you, when did you graduate from which program? In which program are you studying now, when did you start your studies there?
- Why did you want to study sociology / anthropology?
- To talk about texts; what makes you think that a text is a text of sociology? What makes it a text of science, can we make such a distinction?
 - Do you remember the first sociology books or articles that you have read? When was it, what do you remember? Was it easy reading it, and was it different from other texts in any aspect?
 - Who reads a sociology text? Can anybody interested read and understand it? How do you tell your family and the people around you what you are working on?
- Do you think there are formal aspects to learn in producing sociology texts? What are these?
 - When did you first write a paper? How was it received? Have you learned anything about writing a paper? Is it something to be learned, and how does one learn it?
 - What about writing a thesis? How was the prosthesis seminar? Did you ask anybody how a proposal is written? What would you say if someone asked you now?
 - How is a sociology thesis like? Where and how did you get your ideas about this?
 - The process of thesis writing: What are the things that make it easy or hard? How did you learn to write a thesis? What are the recommendations from your supervisor, committee members and other people? What would you recommend to others?
- How does using English affect you in reading and writing sociology texts?
 - Did the English education at METU change your way of speaking?
 - Why is English the language of education here? Do you agree with this?
- When you are producing sociology texts, how do you choose your resources? (English/ Turkish, original/second hand sources etc.)
- Thesis subject: How did you choose your topic? How important have considerations on your career been in this choice? How about your political opinions? How is your thesis related to your own life and your activism? If you are an activist, how does your thesis contribute to your activism, what are your expectations?
- Have you done any empirical research? How do you explain what you are doing to the people you meet there? Have they seen the results? If it involves social movements, how do the activists receive your work?

- (If an opinion like there is a format of writing comes out) Does the format determine or influence the content of a text? How does it influence the relationship between sociologists and those who aren't related to sociology?
- With whom can you share the knowledge you produce? How?
 - Those who have finished their theses: who has read it? Do you receive feedback?
 - For whom did you write your thesis? Do you think on who you are addressing as you write?
- How is sociology presented in the mass media such as television, newspapers, radio or internet? What can you say about the sociologists who use these channels? If you were one day invited by a TV channel to speak about your topic, would you go? Why?
- Anything else you want to say?

Appendix 2. Turkish Originals of Quotations from Focus Group Discussions

1. Bence bu soru bizi biraz şeye getiriyo, teoriyi tartışmaya getiriyo yeniden, yani yeniden bilimsellik ne, dönmeye getiriyo falan. Neden dersin mesela, senin bahsettiğin soruya birden fazla cevap verilebilir ama, mesela *intertextual* diye bi ilişki var biliyo musun bilmiyorum, söylem analizinde falan. Mesela o *intertextual* ilişkide şeye bakıyo söylem analizcileri, bir söylem, bir *text* geliştirilirken kendini hangi *text*leri dışlayarak ya da hangi *text*leri içererek kendini geliştiriyö diye bakıyolar ya; ve bi de bu *text*in şeyini öneriyolar, bu *text*in bi sosyal kimlik ürettiğinden filan bahsediyolar, böyle olduđu zaman. (...) Mesela onların bakış açısından bu şeye bağlı, tamamen kişinin *background*unda getirdiği işte *member's resources* dedikleri şey, ta o eğitiminden ailesinden ne biliym daha böyle yapısal bişey alarak getirdiği ve onu kendi dünya görüşüyle harmanlayıp üretmesiyle ilgili bişey. Ama bu tabii ki şeyle alakalı, içinde bulunduđu güç ilişkileriyle alakalı. Bunu üreten, ne biliym bir sivil toplum örgütünün başkanıysa bambaşka bi şekilde üretebilir, ne biliym bunu üreten illegal bi örgütün işte muhalif bir topluluğun bambaşka bi şekilde üretebilir gibi geliyo bana. (Cemal)
2. Mualla: İlk bakışta, hani ilk aklıma gelen şeyi söylersem, işte bilimsel bir metinde ne olur footnote olur, alıntı olur, işte parantez içinde bir takım isimler ve bi takım yıla benzeyen numaralar olur.
Meryem: Hatta olmalıdır (gülerek).
Mualla: Olmalıdır, ne kadar çok olursa o kadar iyidir, ama çok abartırsan da bu sefer “oğlum sen ne yazdın?” derler, sen bişey yazmamışsın olur. Yani böyle, öyledir ki artık nerdeyse footnote’un kendisi metinden daha uzun olur, öyle yani. Biçimsel özellikleri bunlar.
3. Yarattığı etkiye bakarım ben *text*in. Tamam mı. Benim için hani bilimsel olması çok önemli değil *text*in. (...) Bi *text* yazılırken diğer *text*lerden nası faydalaniyo, yani o direkt belirleniyo mu yoksa belirsiz mi kalıyo birinin söylediği şey *text*in içinde? Nası alınmış yani o *text*in içine nası yerleştirilmiş. Bu biraz da şey bişey yani o bilimsel olmanın ötesinde nesnellik arıyosun, gerçeklik arıyosun. (...) Ve bunun yarattığı etki önemli. Zaten ancak öyle bir *text*, yani belirsiz olmayan açık olan argümanı açık olan bi *text* etki yaratabilir ya da onun üzerinden siyaset üretebilirsin. Sosyoloji konuşuyosak, içinde bulunduğun gruba karşı ya da topluluğa karşı sorumluluğunu o *text* üzerinden gösterebilirsin. Böyle bişeye bakıyorum ben. Daha böyle politik uzantılarına, siyasi uzantılarına bakıyorum. Akademinin bi nevi böyle bişey olduğunu bunun için bilim yaptığını düşünüyorum. (...) Ya bu ekonomik olabilir politik olabilir sanatsal olabilir, böyle uzantılara sahip olmadığı zaman, yok olup gitçek yani. Onun için *text*in öyle bi etki yaratmasını bekliyorum ben bilimselliğinden. (Cemal)
4. Mesela hani şöyle bişey vardır ya hani, ya doktor bi şey derse ve bi insan ona inanmazsa cahildir, yani orda o bilimin bi iktidarından dolayı aslında senin bize bu sorunmuş gibi geliyo. Hani bi metnin bilimsel olup olmamasının sorulması, bence o bilimin kendi iktidar alanından çıkmış bi soru yani sana yönelttiğim bi eleştiri bu yani ben bu soruyu sormazdım. (...) Bana şöyle gibi geliyo; bilimin çok acayip bi yabancılaşması vardır ya gündelik hayata, ve bilim bu statüsünü çok seviyo. Ve o

yüzden kendini diğer metinlerden çok ayırd etmek istiyö. Edebiyat eserinden ayırt etmek istiyor, ya bilim kendini faldan ayırt- yani psikiyatri kendini faldan ayırt edebilir mi? Ama bunu yapmak istiyö, yani bu iktidarını güçlendiricek bişey, bi öteki yapmak zorunda. (Betül)

5. Seni inandırıyor doğru olduğuna. Araçları var. Yani, hegemonyası var, araçları var, iktidarı var, egemenliği var. (Betül)
6. Yani sen ona iki üç tane veri ekledin diye o bilimsel oluyor diye haydiii hep beraber buna inanmak zorunda mıyız, ya da, ne bileyim ben, örneğin Türkiye’de sol tarih üzerine bişey okuycam mesela. *İlla ki* açıp Zürcher mi okuycam? Mesela Vedat Türkali’nin romanları çok çok daha çözümleyici. Şimdi ben şöyle mi demem lazım “hayır işte Vedat Türkali deney yapmamış, işte gözlem- bilimsel anlamda gözlem yapmamış, tarafsız bi bakış açısı yok içinden yazıyor çünkü solcu bi insan şudur budur; hadi bakalım bu metni geç, sen Zürcher oku” mu diyecem ben kendime? (Betül)
7. Yani formatını kuruyo metodolojisini kuruyo bi de rasyonaliteyi kuruyo. (Levo)
8. “İşte senin yaşadığın şey *bu*” diyo. (Leyla)
9. Levo: Çünkü Vedat Türkali, işte edebiyatçı; tamam evet o toplumu gözlemliyo ediyo falan ama bunu edebiyat yaparak, edebiyatı kullanarak yapıyo, bi takım formatları kullanmıyo, alıntı yapmıyor mesela başkalarından-
Betül: Denetlemiyö kimse onun eserini mesela.
Levo: Sadece duygularını filan katabiliyor, ki hani rasyonalite burda çok önem arzetmiyo-
Betül: “Ben” diyo.
10. İlyas: Burda bilimin kendisi bence sorgulanması gereken şey. Bişeylere hani işte tanımlamalar yapıyosun, bi kavram koyuyosun, ve o tanımlamanın ve kavramın sahibi olan kişi *bilim* insanı ise, hani sen onu yapamadığın için senden üstün oluyo. Çünkü ona sahip o bilgiye birikime bilmemneye sahip.
11. Zeliha: E o zaman niye bunu yapma telaşı içindeyiz biz? Roman yazalım...? (...)
Betül: Ama o zaman, bu sistem içinde senin bi geçerliliğin olmıyacak ki. Yani pragmatik bi anlamda.
Danende: Seni ne geçerli yapıyo tatlım sistem içinde?
Betül: yani sistemin koyduğu kurallar geçerli yapıyo.
Zeliha: Tez jürisinden geçemezsin mesela.
Betül: Evet.
Zeliha: Ama zaten edebiyatçı olsan tez jürisine gerek kalmaz.
12. Ben mesela, yazsam otursam, hani, kendi saha çalışmamı (...). Mesela şey yazsam, atıyorum “Asiye, işte şu yaşta şöyle şöyle bi kadındır” filan; bunu mesela ne kadar uzatabilirim bilmiyorum. Tam burda bana mesela müdahale gelir tamam mı hani. Asiye’nin, “sen Asiye’nin güzel bi kadın olduğunu düşünüyosun”. Ya da bilmiyorum işte kocasıyla ilişkisinin bilmemesini, tabii ki ben kendi öznelliğimi oraya katıyorum yani. Ama, çok bi mesela ben şunu bekliyorum yani, bana şu

kritik gelebilir yani, ben tezimi verdiğim zaman komite üyelerine, “hayır Ayşe bu çok hani *edebi* olmuş. Senin hani böyle bir teorethical hani background’a ihtiyacın var.” Bunlar belli yani hani. Her tezi açın, işte *Introduction, Theoretical Background*, ondan sonra işte methodology. Budur yani, akış bu ve bu bekleniyö yani hani, ben her seferinde şeyi tekrar etmek zorundayım, “işte Marx böyle böyle demişti”, paraphrase hani reproduction yani gereksiz, ben hep bunun içindeyim yani başka türlü kabul göremiyorum çünkü karşı taraftan. Ben yaparım ama yapılamıyo. Böyle bi durum var yani. Hani şeyde bile, herşeyi bi kenara bırakıyorum bu yazmak konusunda. (Zeliha)

Zaten o derdimiz yüzünden o dili kullanmıyör muyuz biraz da, meşrulaştırmak için o derdi aslında. Tüm o safsatalar dediğimiz şeyler. Biraz da, tabiri caizse raconuna uyuyoruz. (...) Bilimsel olanı ayırd edip çekmek, bilmiyorum ben hâlâ öyle bi yerdeyim, belki de biraz bu işe yarıyo, sadece. (...) Belki de etki alanı ya da meşruiyet zeminini daha fazla genişleten bişey bilim ayrı olarak. Yani sanat da aynı şekildedir... (Murat).

13. Elimize bi metin aldığımızda mesela “Türkiye’de bu böyledir”, “eşcinseller şöyledir” gibi bi genelleme. Otomatikman, “neye dayanarak söylemiş ki bunu” diye bi bakış atarsın. Atmasan bile, bu niye böyle, yani sinirlenirsin, neye dayanarak söylüyorsun bunu? (...) Ama biz de onu ararız, bi yerden rahatsız oluruz yani, işte mesela akademisyenlere yerleşir o. Ben mesela normal bi metin bana yaz deseler şimdi, normal işte gezi anılarını anlat filan, böyle bi bakıyorum bi anda böyle hakkaten makale yazmaya başladım ben. Tavrı farklı, cümleler farklı “yapılmaktadır edilmektedir” filan. (...) Bi footnote koyasın geliyo mesela koyamıyosun. O çok yerleşiyö ve o düzeni almaya çalışıyosunuz. (...) O hakkaten içime içime oturdu ve bundan nefret ediyorum! (...) Hocalarda da o var, bence, yani onlara yazık acımak lazım. Onu arıyolar, haliyle. Ve bu , işin komik tarafı, uluslararası bi düzen. Yani hani git Almanya’ya Almanya’daki de öyle bakıcak. (Meryem)
14. Şimdi çok böyle biçimsel sıkıntılarını dile getirdik falan filan... ama hani sanki bunlar olmasa, biz kendimiz böyle, yani hani biçimselliği falan savunucağımdan diil de, bunlar olmasa biz çok özgün bireyler olarak çok inanılmaz çığır açıcaz aslında, ve hakkaten kendimiz böyle özgünlüklerle doluyuz filan, öyle bişey diil, kesinlikle diil. Gündelik hayatta da öyle diil. Yani biz yazıdan, kendimizi yazıdan ayrı bi yere koyduğumuz zaman, tutup da öyle gündelik hayatımızda çok özgün şeyler, ilk kez söylenmiş şeyler üretmiyoruz yani. (...) Ama akademi tabii ki bişeyi kurumsallaştırdığı için bütün o şeyleri kurumsallaştırdığı için, (...) hani orda bi takım sıkıntılar doğuyo tabii ki yani. (...) Biraz akademiyi savunalım. İsteyerek bi şekilde hepimiz sosyoloji bölümünde filan okuduk yani. İşte o da bizi şekillendirdi, yani tamam mı biz ondan bağımsız diiliz. (...) Orda oluştu yani, ve o biziz yani, eldeki malzeme aha bu. (Samet)
15. Ben [] hareketiyle ilgili bi tez yazıyorum, tamam işte okul raflarında durucak bu. Ama o hareketin içinde bulunan bireylerin kaçı bu kitabı okuyacak, benim derdim. Muhtemelen kimse okumıyacak. Eğer o zaman ben onu Türkçeleştirip bi kitap olarak yayınlamadığım sürece okumıyacaklar yani “aa burda İngilizce bi tez var heea”. Bu işte tam da heralde senin sorun ettiğin şey yani ne kadar ulaşabiliyoruz ki

o insanlara? Ve hani bence teori yazmak yerine belki de enerjimi gidip orda çalışarak harcasam çok daha faydalı olacak. (İlyas)

16. Zeliha: Refleksivite meselesi benim için bir kriter olabilir. Şöyle ki yani bunu yazan kişinin de, bunu kim yazıyo, yani bunu bana şey demişti Levo demişti, mesela Durkheim'in bi lafi varmış: "bütün yazdıklarımı, bi hahamın oğlunun yazdığını bilerek okuyun." Yani bu, bi taraftan hem bu işi bence bilim yapıyo, hem de bi taraftan da bilimi de çürütüyo, böyle bişey olduğu için, öznelliğe o şey iddiasından pozitivist hani nesnel tanrısal bilgimiş gibi yani her yerde tek değişmez falan ondan da uzaklaştırıyo.

Danende: E ondan uzaklaşınca bilim çürümüş mü oluyo?

Zeliha: Çürümüş olmuyo elbette ama, hani şu an belki konuştuğumuz bilim çürümüş oluyo.

Danende: Yani bilim diyince pozitivistimden mi bahsediyoruz o zaman?

Zeliha: Bana öyleymiş gibi geliyo biraz.

17. Ayşe: "Ben biraz daha iyi vermek ama biraz geciktirmek istiyorum"; "daha iyi bişey vericeğine daha kötü bişey verip zamanında vermeni tavsiye ederim." Yani ve ben zamanında verdiğim ödevden daha yüksek not alıyorum. Bence bu da bi tür format ama neyse (...).

Zeliha: (...) Tüm ödevlerini geç vermiş her şeyi son güne bırakmış bi insan olarak, zamanında vermenin yine de önemli bişey olduğunu düşünüyorum ben, ve bu ne kadar formatla ilişkilendirilebilir bilmiyorum kafamda bi soru işareti var açıkçası. Bu disiplinle ilgili bişey ve gerekli bişey bi yerde hani.

Ayşe: (...) müthiş hani böyle katı, esnek olmayan bişeyin içine seni böyle tıkiyo olması hani şey diil, anlamlı diil yani. (...) Bu çok Amerikalı bi akademi anlayışı yani hani. Çok competitive olacak, her şeyi zamanında böyle düzgün yapmış olucaksın, ona yetiştiriceksin ve bu sana bu ahlaki vermeye çalışıyo. hani zamanında düzenli falan filan.

Zeliha: E bunda ne kötülük var ama?

Ayşe: Bence biraz da akademisyen dediğin dalgın akademisyen olur, yani bu kadar da şey takıntısı olmaz, aman da şu paper'ım olacak bu bilmemesi olacak falan olmaması lazım hani.

Zeliha: Dalgın derken?

Ayşe: Dalgın derken aman da işte şurda şu makaleyi yazıcam ordan şu kadar puan alıcam; çok kariyerist yani hani. (...) Bu seni direkt bilmemkaç puan alıyo olabilmen üzerinden filan, ya da şu makaleyi yazıyo olabilmenden tanımlar yani.

Zeliha: Bence o noktada haklısın ama kriter bu değil de şu olmalı, yazdığın bişeyi hele bi de biraz daha önceki konuya bağlarsak derdi olan bi kişi illa ben İngilizce yazıcam şu kadar puanı olan hakemli bi dergiye yazıcam filandan ziyade, daha çok işte, daha geniş kitlelere açık olan bi yere yazmak, yazmanın derdi başka bi yere denk düşüyo. Yani bunu zamanında vermek takip etmek vesaire değil de.

18. Dün ünlü bi sosyolog profesörle bi miktar monolog geçirdik, maruz kaldık. (gülüşmeler) Kendisi pozitivistin allahı. Ama bi taraftan da şey dedi, Türkiye'de en fazla yayını olan akademisyen olduğundan da müthiş bi övünüyo. Böyle bazı kavramları ders anlatışında kullanma gereği duyuyo. Hani abuk subuk kullanıyo ama hani. Hani refleksiviti filan diyo. Öyle bi yerden ele verdi ki, çok bombaydı bence, "yani yurtdışında yayınının olması için bazı kavramları kullanmak

zorundayım” dedi. Pozitivist aslında! Yani bi de tersinden böyle bişeyi var sanırım bu konunun. Akademi de bi ölçü de en azından bazı coğrafyalarda biraz daha liberalleşmiş olabilir. Kendisi onun da daha muhafazakar tarafında durduğu için, ama uyum sağlamak zorunda. Altını dolduramadığı için de bazı kavramları kullanma ihtiyacı hissediyö. Ve bazı kavramları kendi dersine de sokma çabası içinde aynı zamanda yani (Aylin).

19. Bir derdinin olması gerekiyor. (...) Yani dert ediyorsun annenin çektiğini. Nasıl anlarım annemin çektiğini, falan. (...) Ya da hani, yani Kürtlerin halini anlamak, falan. (...) Şey diyorlar ya hani cilalı anlamda “curiosity”, falan. (Ayşe).
20. Bir derdi vardır, bişey batıyodur ki ona, yazar yani. (Betül)
21. Mesela bana okulda bir arkadaş şey demişti “ya Betül *woman in war* çalışalım beraber” (...). “Sebep?” dedim; ve ben bunu hakaten kızmak için söylemedim “ha hakaten niye” falan ki hiç alakası yoktur öyle bişeyle, elleri çok temiz bir insandır kendisi. Dedi ki, “e çok popüler”. İyi de, yani *woman and war* konusunun popüler olmasının o kadar bi manası var ki yani, bi Yugoslavya deneyimi yaşandı, Kongo deneyimi yaşandı. Kürdistan’da bir sürü tecavüz falan filan yaşandı cezaevlerinde. Bunla çok alakalı şeyler bunlar. (...) Ama sosyal bilimcilerin genelde elleri o kadar temiz ki! Ben de politik olarak bi feminist olarak düşündüğüm zaman bu kadın konusunda ellerinin çok temiz olması beni çok sinir ediyö. (Betül)
22. Hiç canının yanmamış olması aslında tam da bi mesafeye hiç dokunmamak, bi mesafeden söz ettiğimiz anlamına geliyo. Hiç dokunmadığın bişeyi akademide çalışmaman gerekir. Böyle bir ahlakımız ettiğimiz de olabilir. Ama bu rasyonaliteyi safdece akademiye hapsedmememiz lazım. Yani hayatın her alanında işlemiş bişey. Akademi de sadece parçası olduğu için etkileniyö. Yani çarpıcı ve tartışmalı bi örnek vermiş olduğumun farkındayım ama ben de kendimi feminist olarak kabul ediyorum, benim de [erkek olarak] feminist olmam politik camiada kabul görmüyo. Siyasal örgütlenme tipi de aslında çok rasyonel kategoriler üzerinden çalışıyo. Çok akli ayrımlar üzerinden çalışıyo. Bu çok sadece akademiye mal edeceğimiz bişey değil. (Aylin)
23. Akademisyen olmak entellektüel mi olmak? Bunlar biraz birbirinden ayrı şeyler. (...) Bu noktada şunu da diyoruz ama mesela örgütlü birisi de derse gelip bişeyler söylediğinde –hepimizin de muhtemelen böyle bi deneyimi olmuştur- “ya bu da slogan atıyo” –muhtemelen bazı konularda da denk düşünüyoruzdur- ama hani “o başka bi alan bu başka bi alan” diyoruz. “Yani slogan atma” diyoruz “ders yapıyoruz biz burda,” şeyi de biz sorgulamalıyız tamam politik alanda biz bunları bunları söylüyoruz ama hani böyle bi durum da var, o slogan attığımız şeyleri de dönüp içimize acaba böyle mi diyebilelim diyoruz, tam o noktada da işin içine format giriyo zaten sloganla yani ikisini de değersizleştirmiyorum hele sloganı hiç değersizleştirmiyorum ama, (...) idealize ederek konuşuyorum, eğer derdimiz var ve bunun üzerine de yazıyosak, sloganla bu tez arasındaki ince çizgide format var. bu noktada bu da belki koşulsuz uymamız gereken bişey oluyor ama, hani nasıl uymamız gerekiyo buna belki de soru da bu. (Zeliha)
24. Hapiste bile yazabilmek mesela. Umut meselesi biraz da. (Cemal)

25. Samet: Bütün o koparılmışla pratiğe vurgu yapmak, tam da faşizm aslında. Faşizm bunu yapıyo aslında, yani hayır diyo, ben diyo pratiğim diyo, okuma şu bu yalan bunların hepsi diyen şey aslında hani... şey bi zihniyet, faşistlerin yaptığı bi zihniyet.
Meryem: Ben senin yerine düşünürüm diycek çünkü.
Cemal: Evet!
Samet: Herşey pratiktir diyen zihniyet kötü bişey.
26. Ayşe: Senin böyle çok içinde doğduğun falan bişeyi sana dışardan birisi anlatıyo, tamam mı hani, bi sınıfın içindesin, hani birileri şaşırıyo böyle gerçekten, sen de böyle bi “helal olsun!” falan diyosun yani gerçekten, ona şaşırabilmelerine, falan. (...)
Zeliha: (gülerek) bi tane örnek verebilir miyim hemen sığağı sığağına?
Ayşe: Biliyorum hangisini vericeğini de senin vermeni bekledim ben de (gülerek).
Zeliha: [Bir Hoca] şey diyor yabancılar çok şaşırıyolarmış Türkiye’de, bu arada kendisi de böyle gayet dışardan bi veriymiş gibi anlatıyo, Türkiye’de eğitim seviyesindeki sıçramalar çok fazlaymış. Yani mesela İngiltere’de orta öğrenim mezunu bi ailenin oğlu veya kızının da yine hani ortaöğrenim mezunu olması beklenir (...) ama Türkiye’de neymiş, işte hani okuma yazma bilmeyen bi annenin işte oğlu veya kızı üniversite mezunu olabiliyormuş! “Yani böyle de olabiliyo gerçekten di mi arkadaşlar” falan derken ben böyle bi an neye uğradığımı şaşırardım, yanımda da işte bi arkadaş var (...) bana döndü, “e benim annem okuma yazma bilmiyo ama ben doktora yapıyorum” dedi, benim annem de okuma yazma bilmiyo ama ben de yüksek lisans yapıyorum, sonra baktık etrafımıza ve güldük yani sadece buna. Ve bi çok kişi de buna böyle hakaten dışardan diyo hani “ya evet öyleleri de var gerçekten”
27. Mualla: Yani, o kurallar bütünü, içeriği nası belirliyo, aslında senin sorduğun soruyu tartıştığım şeyi, bağlamını, ona yaklaşımını da etkiliyo. Bence ama bu iki taraflı bişey. O biçim ondan bağımsız diil. Bu zamanla zaten içerikten doğru gelişmiş bişey. Onun getirdiği bi, biçim işte, (...) hiç bi zaman içerikten bağımsız bişey diil.
Cemal: Ama içeriği gizleyebilir.
Mualla: (...) Yani hani o biçimin günlük dilden uzaklaştırması ve okuyucu kitlesini daraltması, aslında teması azalttığı için, o daha geniş potansiyel bir çevreni o içeriğe olan katkılarını engelliyo. (...)
Cemal: (...) Ya ikisi arasındaki diyalektik ilişki var ya... Mesela, şöyle anlatıym derdimi ben... Eğer biçimsel olarak çok iyi bişey varsa ortada, bunun içerik olarak çok iyi olduğu anlamına gelmeyebilir bu. Yani hatta o şey biçim o içeriği gizleyebilir de, saptrabilir de yani. Nası diyim sana mesela, para bişeyin biçimidir ama altında mesela emek sömürüsü vardır o onu gizler içerik olarak tamam mı. Böyle bişey anlatmak istediğim diyalektik ilişki. O da öyle olabilir.
28. İlyas: Burdaki yanılğı, akademiye gelen ya da bişeyler yapmak isteyen bu insanların hani hepsinin de belli bir düşünce kalıbıyla düşünüyor olduğunu varsaymak. (...) Yani sen hani bu tezi yazıcaksan şöyle bi düşünce dizisine sahip olmak zorundasın: giriş yazıcaksın, kendi düşüncelerini bi girişte genel olarak anlatıp ondan sonra chapterlara bölüp onları tek tek ayarlayıp falan filan, ya da bi

sonuç yazıp. Ya da işte bunları yazarken şu bilgileri kullanıcak nitelikte olmak falan filan. Ama bunların dışında bi insansan, mesela hani, atıyorum tezi yazmaya ortadan başlayan bi insansan (...) bu kalıpta olmayan bi insansan, senin için tez yazmak mümkün diil gibi bişeye geliyo. Yani bu, bi insanı standartlaştırmak gibi bişey diil mi?

(...)

Meryem: Ya evet doğru ama, diildir. Yazamaz.

Mualla: Bence de.

Meryem: Çünkü bu işin kuralı bu.

29. Cemal: Ya tamamen yok diyemem, bunların hiçbirine. Mesela metodolojinin varlığından rahatsızız ama, onu aşamıyoruz. Onu aşma noktasına nası, limitleri nası zorlayabiliriz mesela. Mesela burada şeyi kullanabilirsin işte postyapısalcı genel çerçeveyi. Hani belki merkezleri aşarak yıkarak vesaire. Ne biliyim ya da Derrida'nın dediği şeyi kullanabilirsin o söylemekle yazmak arasındaki şeyi.

Samet: İşte o da sonra metodoloji oluyo abicim işte!

Cemal: ama işte belki o şeyi limiti yaklaştırabilir yani.

Samet: O akademide başka bi şey açıyo o da başka bi school oluyo yani.

Meryem: Ya aslında postmodernizm bile eleştirdiğinin düzeneğinden çok aşırı bi farklı diil yani.

30. Aslında meşruiyet için yazılıyo tüm o teorik çerçeveler bilmemneler. İki tane öğrenci arkadaşım var, onların konuşmasına tanık olmuştum, bi tanesi dedi ki-konusunu bilmiyo daha yanında çalışan arkadaşının, “ya ne koyucuz nerden destekliyez hoca hiçbişey kabul etmiyo”, ona diyo ki “ya Giddens koy abi. Giddens çak” diyo. “Çak!” diyo bi de. Ne konu olsa gider diyo yani (gülüşmeler) koy Giddens'tan bağla diyo yani olay öyle bi duruma gelmiş durumda (Murat).

31. Yani bu amaç olduğunda elbette kötü ama araçsal bi tarafı da var. (Zeliha)

32. Ben mesela tezi bitiriceğime şöyle karar vermişim: referans sayısı 100 olunca tez biticek demişim. Ve mesela aynı şeyi iki farklı kişi de söylemiş ama referans sayısı kabarık olsun diye itiraf ediyorum birini ondan birini ondan aldım! (Betül)

33. Murat: Ama bak bi de tarihselliğimiz var.

Ayşe: Ay tam ağzımdan aldın biliyo musun.

Murat: İşte bağlı olduğumuz koşullar şu var bu var. Söz gelimi, işte o roman, Vedat Türkali'nin romanı şeyde mahkemede delil olarak kullanılmıyo burjuva mahkemede. Ama kıytırık bi tane tez, araştırma diye, tamam mı bilimsel araştırma diye delil olarak kullanılabiliyo, ya da bilirkişi olarak mahkemeye sosyolog çağrılıyo yazar çağrılmıyo atıyorum. (...) bi de hani bu tesir etme durumu var ya, kısa vadede geçerli olan, o meşruiyet dediğim şeyi ben o yüzden sokuşturup duruyorum. (...)

Aylin: Ama meşruiyet meselesini reddetmek değil de, bunun artık evrensel bi tanımını yapmaktan vazgeçmek. (...)

Ayşe: Benim için mesela tarihsellik derken, sen başka bi yerden aldın ama, ahistorik bi metin okumak istemiyorum ben. Benim için mesela ayaklarının yere bastığı yer hani metnin hani o tarihselliğinden kopmamış olması. Çünkü hem yazarın hem de okuduğum şeyin hani o tarihi var ve ben onu görmek istiyorum.

Betül: Bağlamına oturtmak istiyosun.

34. Kim okuyacak bu tezi? Ve onu sana söylüyorlar zaten. Hani ben yazdığım bi tezi sahadında çalışan kadınların anlayabileceği bi şekilde yazmıyorum, zaten yazmadığım da bana söyleniyo. (...) Onların okuyacağı seviyede olmadığı iddia ediliyo, ya da olmaması gerektiği. Ben akademi çevrelere yazıyorum. (...) Benim durumumda bi kaç tane exceptional durum da var hani, benim tez hocam biraz elitist bir adam, sosyal demokrat; kafasında farklı bi ajandası var... Hani zaten yazdığım konunun... (içini çekiyor) ne denir, koordinatları sakat. Yani hani kadın bilmemne, her an oryantalist olabilirim bilmemne olabilirim. Çok absürt bi yerde duruyo kendi içinde.

Betül: Her yola gelir!

Ayşe: Onun için de mesela yazarken tabii düşünüyorum, bunu mesela [] okusa ne der? O der ki mesela işte hani çok otantik olduğunu göstermek için, yaptığı interview'lerden kocaman kocaman paragraflar koymuş diye algılayabilir. Başka bi klikten, bi antropolog okusa, "a çok değerli bak bilmemne yapmış" der. Biraz daha pozitivist birisi filan okusa "e bunu böyle yapıyosak eğer sen gazeteci olmalıydın. Bunları gazeteciler yapıyo o kadar o insanı konuşturucaksak röportaj yapalım yayınlayalım, hani sen nerde duruyosun" gibi, zaten ben orda varım da o öyle bi kritik gelebilir. Ben onu düşünmüyorum tabii.

35. İmkansız bişeyden söz ediyoruz. Sadece vicdani olarak ne kadar genişletebiliriz, onu arıyoruz. (Aylin)
36. Yoksa yani benim yazdığım tezi beş kişi okursa ne alâ. (Ayşe)
37. Anam körler sağırlar biz böyle yıllaaar boyu birbirimizi ağarlayıp durucaz yani. (Betül)
38. Ya canım zaten orası çok kötü bişey yani. Tamamen onu kullanıyosun götürüyorsun falan. (Ayşe)
39. Başka bi akışın içine sokarsan anlamlı oluyo. (Zeliha)
40. Gülizar'ın bi mailinde şey vardı, o benim çok hoşuma gitmişti Betül'e yazdığı. "“Ya ben de işte tezi yazdım, rafa koydum, bu da ne işe yarıyo ki” derken sen bana mail attın “işte oturup konuşabilir miyiz ben de işte şöyle bi tez yazıyorum ilgisi var, işte hani paylaşabilir misin benle şu konuları falan.” Tam o noktada bu maili atman beni çok sevindirdi” falan gibi bi mail atmıştı. Böyle şeyler olabilir ama, bu yetmiyo yani. Bu bu kadar da öznel bişey diil yani, o zaman gidip resim yapalım. (Zeliha).
41. Bilim yapan tipler haddini bilmiyo gibi geliyo. Böyle bişey yazacaklar dünya sallanacakmış gibi geliyo onlara. Ama sallanmıyo. Çünkü bunun kendi hayatlarında bi karşılığı olmuyo genelde. Hiç biri gidip de Edward Said'in yaptığı gibi bi duvara taş bile atmıyo. Ve bunu yapmadıkları için, yapamadıkları için, bunu yazarak çizerek yapabileceklerini düşünüyorlar ama olmuyo.

42. Ayşe: Ama bi de benim kişisel bakış açımdan da şöyle bi yere oturuyo, hani yazma deneyiminin ta kendisi. Hani nası yazılır?
Murat: Bu da önemli yaa
Ayşe: ben bi fikri ya da bi sorunu hani kafamda bişey teşkil ediyo, hepsi böyle belli bi şeye bi yere hani kafamdaki o tablonun içinde bi yere oturuyolar, sonra ben bunu sorunsallaştırıyorum ve sonra ben bunu yazmaya kalkışıyorum yani hani. Deniyorum yani bunu hani nası yazabilirim? Benim için o kadar büyük bişey ki yani hani, o kadar zorlanıyorum ki gerçekten sürekli herhangi bişey okurken de nası yazabiliceğimi düşünüyorum bunu ben nası dile getirebilirim nası yazabilirim (...) bazen diyorum keşke böyle yazabilsem, bak bitiririm. Ama yazamıyorum. Samimi bi şekilde bunun bana nasıl yazabiliceğimi göstermesi en azından benim için bi deneyim bi pratik olmasını hani ümid ediyorum yani şu aşamada. Onun dışında pek bişey beklemiyorum hani kendime de güvenemiyorum yani o anlamda. (...) Ay çok kızabilirsiniz ama, öğrenmek ve bilmek beni mesela, müthiş tatmin edebiliyo yani hani. (...) Bilmek ya! Bana bazen çok şey yapıyo, çok heyecanlandırıyo.
43. Meryem: Biz bi yazı yazarken bi çıkarım yaparız “lan işte bu lan” falan diye yani, bazen abi orgazmdan falan daha zevkli oluyo yani!
44. Cemal: Mesela Marksizmin benim sosyolojiye kaymamda büyük etkisi oldu, çünkü daha böyle arka planı gösteriyo daha perspektifi geniş bişey falan. (...) Dolayısıyla onun için sosyolojinin bi yıkıcılığı olduğunu düşündüm diğer bölümlerden falan böyle ayırarak seçtim sosyolojiyi, çok memnunum seçtiğim için.
İlyas: Sosyoloji bence de acayip insana karakter ve bakış açısı kazandırıyo. Yani, sonuçta çok konuştuğumuz bişey mühendisler böyleyken biz niye böyleyiz falan hani. Muhteşem insanlarız demeye çalışmıyorum da, yani mesela adamın bu kesin böyledir dediği bişeye hayır öyle olmaya da bilir diye yaklaşmak aslında çok önemli bişey ve bence sosyoloji bize bunu veriyo.
45. Samet: Ya mesele, tıkanıdığı noktalardan bi tanesi şu: yani tamam bi, bahsettiği filozofları bilmemne olsun, bi metin ortaya koyuyo tamam mı. Ama o koyduktan sonra, ben şöyle düşünmüyorum o metin o kişiden o kadar da bağımsızlaşmamalı. Yani tamam elbette yani başka bişeyle artık metin kendini alıyo götürüyo falan filan ama, onu yazmış bi insan olarak o kişi hayatla ne kadar temas ediyo? Yani kendisi sonra fiziksel olarak ne yapıyo? (...) Mesele şu değil yani, “adam işte çok ağır bi metin yazdı, o yüzden sadece bu metin akademide kaldı.” Diil yani. O metni yazsın. Ama, dışarda başka türlü davransa, o metin hani sokak, bilmiyorum yani hani, başka bi şeye oturur, oturabilir, tamam mı, böylelikle hani o temas gerçekleşebilir. Kim ne kadar okuyo konusu biraz böyle. Türkiye özeline de gelirsek, ben hani mesela bilmiyorum bana öyle geliyo yani, 80den sonra akademinin almış olduğu bi şekil var, hani, ve hani biraz bi uzaklaşma filan gibi.
Meryem: Üniversitede belli kriterleri uygulayan, ama evine gittiğinde standart bi ev kadını ya da standart bir adam.
Samet: Aynen öyle.
Meryem: Sonuçta (sosyoloji/antropoloji yapmak) bize bi sürü şey öğretiyo. Adam da onu öyle yapmış koymuş, almış doçentliği profesörlüğü, hiç dışarda ne varmış...

46. Tamam ütopyayı koruyorum mümkün mertebe ama bu biraz da günlük hayatımdaki karşı çıkış temelini oluşturu. (...) umutsuz olabilirim, ama nefes alıp verirken bişeyler için mücadele etmek anlamlı geliyo. (Aylin)
47. Betül: Ama Ayşe bu dediğin şey bende çok tersi bi psikoloji de yaratabiliyo biliyo musun, yarattı. (...) Ya böyle öyle şeyler söylüyolar ki bana, hocam diye hitab ediyolar zaten yani seni daha üst bi konuma koyuyolar, “ya” diyolar, “lütfen gidip anlatın. Ne olur yazın bak bu söylediğimi ne olur yazın. Lütfen yazın,” tamam mı (...), ama anlattıkları şeyleri bi duysanız (...) o kadar acıklı şeyler ki. (...) kapıdan çıkarken diyolar ki işte “mutlaka anlatın bunlar akademide konuşulsun siz üniverste hocaları çok çok önemlisiniz mutlaka anlatın”, ama ben biliyorum ki ben bunları yazdıktan sonra hiçbişey olmayacak. Ama ben o insan için bir umut ışığı olmuşum, elimde olmadan. Ve bu beni çok etik olmayan bambaşka bi konuma hapsediyo.
Ayşe: Evet. O konuda haklısın.
Betül: Ben ne diyecem buna ben bunları yazdıktan sonra bişey olmayacak mı diyecem? Ama olmayacak. Yani haddimi bilmiyorum, olmayacak.
48. Ay belki çok iğrenç bişey söylicem ama, biraz pragmatist olmakta yarar yok mu sizce? Yani Seda Sayan değil Alto beni çağırrsa ben giderim yani, seve seve giderim yani hani. Ordaki 5 dakika 10 dakika çok önemli bence. (Betül)
49. Mesela psikolog, kişi. Akademik eğitimden geçmiş. Ünvan almış belki doktor falan bilmemne, hani bu kişi işte eşcinsellerle ilgili araştırma yapıyo. Hani diyo ki mesela işte geylelerin yüzde 90’ı işte babasız büyümüştür. Bunun anket sorunları istatistik bilmemne falan filan. Hani bunda mesela gerçeklik payı olmadığını biliyosun. (...) Ama o adam işte psikolog doctor falan bilmemne ve o adamın işte hürriyet gastesine manşet olarak verdiği bu yazıda insanlar “aaa tamam o zaman, boşanmayalım. Ailelerimiz birlikte kalsın ki çocuklarımız eşcinsel olmasın, falan. Yani burda bu olayın bi ideolojik boyutu var. insanlara neyin verilmek istendiği neyin düşündürülmek istendiği boyutu var. orda hani sen çıkıp “ben psikoloğum, böyle bi gerçeklik yok, ve bakın benim böyle bi araştırmam var ve mesela şu araştırmamın şu yönleri taraflıdır, deyip de, çıkıp bunu şey yapman lazım göstermen lazım. Ama yani iki tarafta da psikolog var, o psikolog da üniversteden mezun oldu o da üniversteden mezun oldu, bunun arasındaki farkı nasıl belirliyecez yani? (İlyas)
50. Meryem: Giderek çekiyolar kendilerini toplumdan çünkü anlayamıyorlar, olmuyo. Bizde şey vardır ya hani bi olaya bi çok perspektiften bakma hastalığı. Lafi uzatıyosun yani (...). Ama işte toplum bunu istemiyo. Toplum genellikle kendi perspektifinden bakarak tek bi yoldan tek bi cevap vermeni istiyio. Onu yapamıyosun, yapamıyınca artık ipler kopuyo yavaş yavaş. Sen de koparıyosun yani. Yani o da çekip gidiyo, o da lojmanda oturuyo hiç inmiyo falan filan şehre.
51. Mualla: Bu dilin böyle olması (...) böyle bi takım ancak başka metinlere bakarak ne olduğunu anlayabileceğin kavramlar, soyut soyut örnekler, uzuuun uzun cümleler falan, akademik dilden bahsediyorum işte, (...) bu böyle sanki bizi acaba koruyo mu biraz da? Ben çünkü hep şu varsayım vardı benim kafamda, eğer bişeyi çok iyi anladıysan, onu sen herkese anlatabilirsin (...), karşıdaki hemfikir olur olmaz ama onu tartışabilir hale gelebilirsin. Şimdi mesela bu fikir giderek sarsılmaya başladı yani, özellikle sosyolojiye başladıktan sonra. Şimdi mesela

soruyorum ben bunu anlatabilir miyim? Emin deęilim. Ve de Őöyle de bir kaygı da var içimde, ben bunu anlattığımda belki de aslında bunun çok anlamsız olduęu gerçeęiyle yüzleşmem gerekecek. Ben mesela bi sosyal hareket çalışıyorum (...) ve ben mesela bu tezimi onlara anlatmak istiyorum yazdıktan sonra. (...) Bana ne diycekler onlar? “ee-eee?” (gülüşmeler) “Eyvallah!” Bi çok noktasını zaten biliyolar, çünkü onlar yaşamış ben zaten hatta onlardan öğreniyorum. (...) Benim açımdan çok güzel eyvallah ama... (...) Ancak hareketin içinde olmayan başkalarına bunu aktarabilirim benim yapabileceğim şey bu olur. Ama benim yazdığım şey onlara anlamlı gelmiyosa, bu benim için kötü bişey yani. Ve bunu anlatmaya çalışırken sorun yaşamam zorluk yaşamam bu da kötü bişey. O dil bizi bundan uzaklaştırıyo, bu zor bişey, ben bunu yapıp yapmıycağımdan da emin deęilim.