

**“Were it not for the post-it notes...”:
A LABORATORY LEADER’S DAY AT WORK**

An Undergraduate Thesis
(Bakalářská práce)

BY

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DEDICATION

To Them

DECLARATION

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Prague, June 26, 2007

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Veronika Řepíková

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Díky

ABSTRACT

This paper is based on over six months of participant observation at a national research institution in the Czech Republic. The work of the team I observed focuses on experimental chemistry. My work builds on the tradition of "laboratory ethnographies". In the methodological part I address some theoretical issues of ethnography as the taking and processing of fieldnotes and the generation of an ethnographic account, my position and role in the field as an observer, and ethical issues related to this type of research. In the ethnographic account I first present the physical and social setting, and then, drawing primarily on the work of John Law, I focus my attention on the laboratory leader. I show the lab leader in networks of relations which he is a part of and which allow him to exist and perform his position of manager. These networks are ever changing, they emerge and disappear on different occasions and for different purposes, they are not fixed and rigid, and one network is never exclusive. An important aspect of these networks is that they are materially heterogeneous – they are not composed of humans only, but also of non-humans (things, technologies, computers, post-it notes, telephone sets and cell phones, etc.). Thus, my question is not only "who" but also "what" is the lab leader. I show that a lab leader in a modern research institution works in several logics or modes of ordering between which he continuously shifts, contributes to their emergence, is performed by them. Also, these modes are always open and unsettled. I show that this specific lab leader must negotiate his position between these different logics – he is under clear pressure to be a creative scientist as well as an efficient manager of an enterprise - his team.

PŘEDSTAVENÍ BAKALÁŘSKÉ PRÁCE

Název: „Kdyby nebyly žluťáky...“: Pracovní den vedoucího výzkumného skupiny

(„Were it not for the post-it notes...“: A laboratory leader's day at work)

Vedoucí práce: Mgr. Yasar Abu Ghosh

Má bakalářská práce vychází z více než půlročního **zúčastněného pozorování v experimentální chemické laboratoři** v jednom výzkumném centru v České republice v době na přelomu let 2006/2007. Přístup na toto pracoviště a jedinečnou příležitost provádět zde výzkum jsem získala v rámci mezinárodního výzkumného projektu „Knowledge, Institutions and Gender: An East-West Comparative Study“, kterého se účastním jako výzkumnice (www.knowing.soc.cas.cz).

Má práce vychází z tradice sociálního studia vědy, konkrétněji **„laboratorních etnografií“** (Bruno Latour, Steve Woolgar, Karen Knorr-Cetina, Sharon Traweek, John Law a další), které od svých počátků na přelomu 70. a 80. let 20. stol. využívaly metody zúčastněného pozorování. Po krátkém představení výzkumného projektu KNOWING nabízím přehled nejdůležitějších momentů ve vývoji sociálního studia vědy, které ačkoliv se původně pojí s disciplínou sociologie, se dnes ustavuje i v antropologii jako antropologie vědy či kulturní studium vědy.

V **metodologické části** se vyrovnávám s obecnými východisky etnografie, praktickými záležitostmi jako bylo psaní poznámek v terénu a jejich zpracování a prezentace ve formě etnografické práce, svým postavením v terénu jako výzkumnice a vztahy s účastníky výzkumu a v neposlední řadě s etickými otázkami, které se s výzkumem a prezentací jeho výsledků pojí. Hlavní část práce tvoří etnografický narativ, kde nejprve popisují fyzické a sociální prostředí, kde se pozorování odehrávalo, tj. výzkumné centrum, důležité momenty jeho současného vývoje a zejména samotnou výzkumnou skupinu a její členy.

Svou pozornost zaměřuji a hlavním tématem mé práce je **vedoucí výzkumné skupiny**, ve které jsem pozorování prováděla. Ze zápisků z pole nelineárně, na základě dat z celého období pozorování rekonstruuji jeden den v práci vedoucího této laboratoře (Atkinsonův „fragmentovaný narativ“). Hledám tedy určité paradigmatické vztahy, tendence či lépe vzorce,

vycházející z analýzy doposud sebraných dat. Konceptualizaci mých analýz inspiroval sociální vědec z Lancasterské univerzity **John Law**. Jeho práce významným způsobem rozvíjejí teorii aktérských sítí, spojovanou zejména s Michele Calone a Bruno Latourem, a vychází ze zúčastněného pozorování, které Law prováděl v 90. letech 20. století.

Vedoucího výzkumné laboratoře ukazují v **sítích vztahů**, jichž je součástí a které jeho samotného v jistém smyslu vytvářejí, přičemž tyto sítě (množné číslo je zde důležité) se neustále proměňují, vznikají a rozpadávají se při různých příležitostech, jsou jiné podle účelu, ustavují se náhodně a nikdy zde není pouze jedna jediná či výhradní síť. Důležitým aspektem je také, že sítě nejsou tvořeny pouze jinými lidmi, sociálně, ale jejich součástí jsou také ne-humánní prvky - věci, technologie, počítače, papírky na poznámky, telefony... - tzv. **materiální heterogenita**. Proto se můžeme ptát nejen "kdo", ale také "co" je vědec/manažer. Podle Johna Law se vedoucí pracovník ve vědecké instituci pohybuje v různých rámcích, logikách jednání či **modech uspořádávání** (modes of ordering), mezi kterými neustále proplouvá, podílí se na jejich vytváření a ony vytvářejí jeho. Tyto logiky jednání nejsou nikdy uzavřené a pevně dané. Má práce ukazuje, že tento konkrétní vedoucí se musí vyrovnávat zejména se dvěma „logikami“ své práce - a to být na jedné straně kreativním vědcem a zároveň efektivním manažerem vlastního podniku - výzkumné skupiny.

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NOTATION

The research part of this thesis was carried out under the research project: KNOWLEDGE, INSTITUTIONS AND GENDER: AN EAST-WEST COMPARATIVE STUDY funded by the European Commission in the Structuring the ERA specific programme of Framework Programme 6.

1. **INTRODUCTION**

Who or what is the leader of an experimental laboratory? The goal of this paper is modest. On the basis of my participant observation in a Research Center in chemistry I want to show what a day at work of a laboratory leader is like. I am not claiming that this is what all days are like or that all leaders experience their work in this way. I acknowledge that my account is partial. However, the account testifies to some patterns prevalent at the Research Center which I wish to point out.

This paper is based on over six months of participant observation at a Research Center of a national research institution in the years 2006 and 2007. I have gained access to the Research Center thanks to participating as a researcher in the international project "Knowledge, Institutions and Gender: An East-West Comparative Study ("KNOWING") (more at www.knowing.soc.cas.cz)

My paper builds on the tradition of "laboratory ethnographies" of Bruno Latour, Steve Woolgar, Karen Knorr-Cetina, Sharon Traweek, John Law, and others which studies from the 1970s and 1980s made use of participant observation as a key source of their data and ensuing conceptualizations. After a short introduction of the KNOWING project I summarize the key developments in social study of science - originally dominated by sociology but now established also in anthropology as anthropology of science or cultural studies of science.

In the methodological part I address some general issues of ethnography as the taking and processing of fieldnotes and generation of an ethnographic account, my position and role in the field as an observer, and ethical issues related to this type of research. The core of my paper is an ethnographic account where I first present the physical and social settings of the observation, i.e., the Research Center, important moments of its recent history, and especially the research group and its members I have been observing.

Then, I focus my attention on the laboratory leader. On the basis of analyses of my fieldnotes for the entire period of observation I reconstruct a day at work of the laboratory leader. I look for paradigmatic relations, tendencies or patterns in the data. In conceptualizing my conclusions I draw primarily on the work of John Law. His work in important ways developed the

ideas of the actors-network theory of Michel Callon and Bruno Latour, and is based on Law's participant observation carried out in the 1990s.

I show the lab leader in networks of relations he is part of and which allow him to exist and perform his position. These networks are ever changing, they emerge and disappear on different occasions and for different purposes, they are not fixed and rigid, and one network is never exclusive. An important aspect of these networks is that they are materially heterogeneous – they are not composed of humans only, but also of non-humans (things, technologies, computers, post-it notes, telephone sets and cell phones, etc.). Thus, my question is not only “who” but also “what” is the lab leader. According to John Law a lab leader in a modern research institution works in several logics or modes of ordering between which he continuously shifts, contributes to their emergence, is performed by them. Also, these modes are always open and unsettled. I show that this specific lab leader must negotiate his position between these different logics – he is under clear pressure to be a creative scientist as well as an efficient manager of an enterprise -his team.

1.1 **The KNOWING Project**¹

The international project Knowledge, Institutions and Gender: an East-West Comparative Study is funded by the European Commission in the Structuring the ERA specific programme of Framework Programme 6 for the period 2006-2008. The coordinator of the project is the Institute of Sociology of the Czech Academy of Sciences of the CR.

The main objectives of the research are to:

- examine how scientists form links, collaborations and alliances in research environments under transformation at national and European level;
- analyse what and how power differentials influence the formation of the scientific community. Such power differentials might be gendered or conditioned by broader geopolitical contexts;
- analyse structural and institutionalised practices and procedures, including standards of excellence, that hinder and/or promote the participation of women in science;
- focus on practices and consequences of the divide of research activities

¹ This text was produced by the Czech KNOWING team and was used as presentation of the project to research participants.

between the Academy of Sciences and universities for knowledge production and transfer and for career building and on how the divide is managed by research institutions and researchers.

Scientific communities and knowledge production practices under study are chemistry and sociology, two scientific fields that have a significant impact on society. The research is simultaneously being conducted in leading institutions and labs in their field in each of the partner countries, eventually culminating in a comparative cross-national analysis.

Qualitative research methods, specifically inspired by STS research strategies, are employed. They build on an assumption that in order to understand knowledge production processes and the ways in which knowledge circulates between science and society (in the form of expertise or innovation) we need to approach scientific communities and their relations with society in a detailed ethnographic inquiry. We need to study "science in action". It is thus not enough to investigate textbooks and methodological handbooks and carry out interview with scientists. We need to do both and at the same time observe and analyse how these methodological rules apply in everyday lab practices. Several qualitative methods are used in the study including critical discourse analysis, participant observation and interviewing.

The Czech team consists of researchers from the National Contact Centre – Women and Science of the Institute of Sociology of the AC CR, namely Alice Červinková, Tereza Stöckelová a Marcela Linková, and from the Department of Gender Studies at the Faculty of Humanities, Charles University in Prague, namely Dagmar Lorenz-Meyer, Veronika Řepíková, Magdalena Górska and Jan Matonoha.

The international consortium of the project consists of:

1. Institute of Sociology, Academy of Sciences of the Czech Republic, Czech Republic
2. Faculty of Humanities, Charles University, Czech Republic
3. Comenius University, Bratislava, Slovakia
4. University of Leeds, United Kingdom
5. Department for Social Studies of Sciences, University of Vienna, Austria
6. Department of Sociology, University of Turku, Finland
7. Turku School of Economics and Business Administration, Finland
8. Department of Sociological Studies, University of Sheffield, United Kingdom

1.2 **Social Studies of Science**

I would like to start with a short historical and theoretical introduction to the field of science studies. As this paper is an ethnography of science, in this section I show that ethnographic methods are at the very heart of science studies, and I introduce the discipline of anthropology of science and cyborg² anthropology. Although little known or practiced in the Czech Republic, I think it is a field with great prospects and potential.

1.2.1 **Science and Technology Studies**

The terms social studies of science, science studies or science and technology studies (STS) can be used interchangeably referring to an area of multidisciplinary research with inputs from sociology, history, philosophy of science, anthropology, economics and psychology (Woolgar 1991).

To put this paper in context, let me now present a brief summary of sources and history of science studies³. The beginnings of STS can be traced back to **Ludwick Fleck** (e.g., 1979) who was one of first pioneers of a constructivist and relativist view of science, especially medical sciences. **Thomas Kuhn** (e.g., 1997) studied the stability and change in scientific cultures, and inspired a sociological turn in history and philosophy of science by challenging the traditional image of incremental accumulation of knowledge in rationally selected experimental frameworks. **Karl Mannheim** was among the founding fathers of sociology of knowledge, however, he excluded natural sciences as a subject of sociology of knowledge.

Mannheim inspired the classical sociology of knowledge of **Robert K. Merton** (e.g., Merton 1942 and 1968) in the 1930s through 1960s. In the tradition of structural and functionalist sociology, Merton studied first of all the institutional frameworks of science. He formulated optimum institutional conditions for the development of pure science and possible causes of failures thereof. It is much to Merton's efforts that sociology of science owes its establishment as an independent area of study. Together with **Ludwig**

² I explain the concept of cyborg below.

³ The historical exposition draws on a diagram accompanying an article by Konopásek (2001: 390-91). I use the diagram to reconstruct the developments and possible links in social studies of science, quoting works mentioned by Konopásek or known to me to have been written by relevant authors. The exposition should not be taken as in a fixed chronological order but rather in a complexity of mutual links and influences. Any mistakes are mine.

Wittgenstein's notion of language, as being comprehensible only in the context of its use, as a precursor to the sociological turn in epistemology, all these scholars created a niche for the new sociology of science in the 1970s.

A chief proponent of this discipline was **David Bloor** (e.g., 1976) and his "strong program". Sociology of knowledge does not address so much the issues of scientific institutions and communities, but he shifts more towards sociology of scientific knowledge studying not the contexts but the very content and nature of scientific knowledge.

Ethnographic methods found their way to social studies of science on the outset of 1980s with so-called **laboratory ethnographies** of Karin Knorr-Cetina (1981), and Latour and Woolgar (1986). These ethnographic accounts avoided questions of causality and focused on the construction of facts of laboratory sciences with an emphasis on contrasts between local and improvised actions in day-to-day laboratory work on the one hand, and rationally reconstructed reasoning and explanations presented in research reports and articles.

In the 1980s, **actor-network theory** (ANT) was developed by a group of scholars headed by Michel Callon and Bruno Latour, (e.g., Callon 1986, Latour 1987 and 1999) and the British sociologist John Law (e.g., Law 1994 and Law and Hassard 1999). In studying society and nature, it maps the relations that are simultaneously material (between things) and semiotic (between concepts), put simply, people, ideas and technologies, and the networks they form. It looks at how "durabilities" of this world come into existence, and tries to explain how networks come together to act as a whole.

This approach is closely related to French "**post-structuralism**" - the works of Michel Foucault, Gilles Deleuze, and others problematizing the dichotomous and essentialist thinking (binarities like subject and object, signifier and signified, etc.). And also to **feminist science studies** of Donna Haraway (e.g., 1989 and 1991), Annemarie Mol (2002), Vicky Singleton or Susan Leigh Star and their interest in studying the complex relations between science, body, industry, commerce, identity and power.

ANT can also be seen as drawing on some insights of **Garfinkel's** ethnomethodology (e.g., Garfinkel 1967) as a sociological study of "methods" people use to make things meaningful and ordered in the routines of everyday life with a focus on conversation analysis and local practices. A proponent of an ethnomethodological study of scientific work is **Michael Lynch** (e.g., 1985 and 1993) who is interested in studying "epistemic ordering" on the basis of

detailed descriptions of ordinary, routine and always situated practices of science (e.g., observing, explaining, evidencing, etc.).

In the 1940s, Merton presented his description of the “normative structure of science” by defining values and norms he believed actually guaranteed the functioning of science. These are known as four “institutional imperatives”:

1. *Universalism*: scientific claims and results are judged independently of the attributes of the individual who has advanced them, e.g., social class, race and religion. Scientists are rewarded exclusively on the basis of the results obtained.
2. *'Communism'* [later referred to as “communalism”]: results and discoveries are not the property of the individual researcher concerned, but belong to the scientific community and society at large. This imperative is based on the assumption that knowledge is the product of a collective and cumulative effort by the scientific community. The scientist does not obtain recognition for his/her activity if s/he does not publicize it and thus make it accessible to others.
3. *Disinterestedness*: every researcher pursues the primary objective of knowledge progress, indirectly achieving individual recognition.
4. *Organized skepticism*: every researcher must be willing to evaluate any hypothesis or result critically, including his/her own, suspending final judgment until all necessary confirmations have been obtained.
(Merton 1973)

While Merton’s institutional imperatives express a conviction how the doing of science should be organized and function, in the 1970s, David Bloor defined the “strong programme” as a set of methodological principles for the sociological analysis of scientific knowledge that should be:

1. *Causal*, i.e. concerned with the conditions which bring about beliefs or states of knowledge.
2. *Impartial* with respect to truth or falsity, rationality or irrationality, success or failure. Both sides of these dichotomies require explanation.
3. *Symmetrical* in its explanation. The same types of cause should explain true beliefs and false ones.

4. *Reflexive*. In principle its patterns of explanation should be applicable to sociology itself, which obviously cannot claim to be exempt from sociological analysis. (Bloor 1976)

In the 1980s, Bruno Latour concluded his book *Science in Action* with "Rules of Method" for the social study of science:

1. We study science *in action* and not ready made science or technology; to do so, we either arrive before the facts and machines are blackboxed or we follow the controversies that reopen them.
2. To determine the objectivity or subjectivity of a claim, the efficiency or perfection of a mechanism, we do not look for their *intrinsic* qualities but at all the transformations they undergo *later* in the hands of others.
3. Since the settlement of a controversy is the *cause* of Nature's representation, not its consequence, we can never use this consequence, Nature, to explain how and why a controversy has been settled.
4. Since the settlement of a controversy is the *cause* of Society's stability, we cannot use Society to explain how and why a controversy has been settled. We should consider symmetrically the efforts to enroll human and non-human resources.
5. We have to be as *undecided* as the various actors we follow as to what technoscience is made of; every time an inside/outside divide is built, we should study the two sides simultaneously and make the list, no matter how long and heterogeneous, of those who do the work.
6. Confronted with the accusation of irrationality, we look neither at what rule of logic has been broken, nor at what structure of society could explain the distortion, but to the angle and direction of the observer's *displacement*, and to the *length* of the network thus being built.
7. Before attributing any special quality to the mind or to the method of people, let us examine first the many ways through which inscriptions are gathered, combined, tied together and sent back. Only if there is something unexplained once the networks have been studied shall we start to speak of cognitive factors. (Latour 1987)

Very superficially, we are seeing shifts from an objectivist, normative (and rather wishful) image of science as a collective and cumulative effort and the disinterested and ever skeptic scientist with no particular subjectivity through explanation of science by primarily social factors to assigning humans and non-humans with the same potential agency.

1.2.2 Anthropology of Science

As I already mentioned above, **anthropological methods** were used from the very onset of modern science studies in the 1970s (e.g., Knorr-Cetina 1981, Latour and Woolgar 1986, Lynch 1985, Traweek 1988). The two key contributions of anthropology were participant observation - *in situ* observation and recording, and analytic skepticism, i.e., resisting the explanations and arguments of research participants, in STS, especially with reference to scientists' knowledge claims and other achievements (Woolgar 1991); provided that analytic skepticism should apply also to the representational practices of the observer.

The general impression might be that social studies of science are thought of as primarily a sociological endeavor⁴. However, it is not only that social and cultural anthropology would supply some partial methods to social studies of science. Even though some authors are clearly pessimistic about anthropologists being able to add anything new to the study of science (e.g., Latour 1990, Woolgar 1991), cultural anthropologists, especially from the United States, are carving out a distinctive approach of their own: anthropology of science or **cultural study of science**⁵.

In her article, Emily Martin explores how the distinctively **anthropological concept of culture** can provide new insights into the workings of science in its cultural context (Martin 1998). Natural sciences are infrequently seen as citadels, they claim to construct reality but not to be themselves constructed or posing as "cultures of no culture" (Traweek 1988). In response, science

⁴ For instance, it was not until 1994 when anthropologists were first invited to give keynote addresses at the Society for Social Studies of Science meeting (Martin 1998).

⁵ Examples of anthropological studies of Western science include Clarke and Fujimura (1992), Downey and Dumit (1997), Dubinskas (1988), Edwards et al. (1993), Escobar (1994), Gray (1995), Gusterson (1996), Hess (1995), Pfaffenberger (1992), and Rabinow (1996).

and technology studies have begun to depict how very rich and complex cultures⁶ natural sciences in fact are.

According to Martin, what anthropology of science can bring in are **historical and cultural contexts** of such cultures, which some of STS studies were leaving out, together with looking at what role **nonscientists** play in the agendas of scientific research as "science" and "society" as categories are produced inside the heterogeneous matrix of culture. Scientific knowledge does not flow in a one-way direction; scientific knowledge and its production are linked with nonscientific processes, developments in science participate in broader cultural developments, not simply reflecting them but not leading them either. No sharp borderline exists between science and society, natural and medical sciences are in constant and turbulent interaction with many parts of cultural landscape; science and culture are co-constituted in a space that is discontinuous, fractured, convoluted and in constant change. (Martin 1998)

Another area with affiliations to or roots in social and cultural anthropology - **cyborg⁷ anthropology** - adds new dimensions to social studies of science⁸. One of the areas of interest is to study contemporary science and technology as cultural activities and cultural phenomena. It comes with a claim that "**we are all scientists**" - we all reconstruct scientific knowledge in new contexts. These new contexts may also be across national and cultural boundaries as science and technology is the vehicle of much **cross-cultural interaction**, production and change (technologies may be empowering as much as constraining and disempowering). In the past, anthropology described alternative worlds and informed the imagination of radical difference, and cyborg anthropology continues in alternative world-

⁶ Traweek explains the anthropological understanding of culture as follows: „A community is a group of people with a shared past, with ways of recognizing and displaying their difference from other groups, and expectations for a shared future. Their culture is the ways, the strategies they recognize and use and invent for making sense, from common sense to disputes, from teaching to learning; it is also their ways of making things and making use of them and the ways they make over their world.“ (Traweek 1992: 437-38). It is also the concept of culture that Martin (1998) refers to.

⁷ Cyborg is shorthand for „cybernetic organism“: a symbiotic fusion of human and machine, a product of profound alterations of biological and physical landscapes of and by humans (e.g., Dumit 2007).

⁸ The section on cyborg anthropology draws on a position statement „Cyborg Anthropology“ presented at the 1992 Annual Meeting of the American Anthropological Association, and later published in *Cultural Anthropology* (Downey 1995).

making by critically examining the powers of imagination invested in the sciences and technologies of contemporary societies.

Cyborg anthropology is also a critique of adequacy of “**anthropos**” as the subject and object of anthropology. Anthropological discourse works with human-centered presuppositions, but human subjects and subjectivity are also a function of machines, not only machine producers and operators. Thus, human agency should be studied in connection with the agencies of technologies: “[anthropology] *must blur its own conceptual presuppositions that exclude machines from anthropos*” (Downey et al. 1995: 267). This brings me to a very interesting area of cyborg anthropology which focuses on cultural production of human distinctiveness by examining ethnographically the boundaries between humans and machines and our visions of the differences that constitute these boundaries and exploration of production of humanness through machines.

1.2.3 **John Law’s Modest Sociology**

In my paper, I want to draw on the work of John Law, especially the parts where he discusses the position of manager, what the manager is made of, what powers he or she has and in what frames or modes the scientist in a modern organization operates. I am presenting the details of Law’s concepts relevant for my paper in the last, ethnographic section. Here, I would like to introduce some starting points or aspirations of Law’s work.

Law has a concept of **modest sociology** which in wanting to order the social world, i.e., to give interpretations of the social world’s workings, “[does not] *want to do violence in our own ordering, [...] to pretend that our own ordering is complete, or to conceal the work, the pain and the blindness that went into [the ordering]*” (Law 1994: 9).

Law believes that any account of the social world we give is always incomplete. Sociologies should be “*aware of the context of their own production, and [that] the claims that they make tend to be relatively limited in scope*” (Law 1994: 9). I believe the awareness of the context of production and limitedness of scope is a well-established feature of ethnography⁹. The assumptions or starting points of modest sociologies are: symmetry, non-reduction, recursive process, and reflexivity.

⁹ As I show in the methodological chapter.

Symmetry was sought by David Bloor and Bruno Latour above. For Law, symmetry means that *everything* deserves explanation, and such explanation or description should be approached in the same way. Law writes about three¹⁰ aspects of symmetry, the one important for this work is that the assumption of symmetry challenges a priori distinctions between human actors and technical or natural objects. Are these distinction really given in the nature of things? To voice such doubts is to ask about the **character of agency**: what it is, or what it takes to be a human being? Agency is seen as a product or an effect - agents are not given by nature, and it should be investigated how they became what they are.

Non-reduction is the second attribute of modest sociology. It is an effort at avoiding reduction, i.e. the notion that there is a small class of phenomena, objects or events that drives everything else, that there are drivers and those driven. Non-reduction means finding relations with no privileged places. Regularities may emerge and be sustained but these should be treated symmetrically as effects. All phenomena, objects or events should always be presented as stories or thick descriptions.

The third part of modest sociology is that the social should be seen as a **recursive process** - a verb rather than a noun (or object). Order should not be taken for granted but as ordering accomplishments. And social processes are self-generating processes - there is no outside, no external drivers. "[T]he social world [...] in its processes [...] shapes its own flows" (Law 1994: 15). There is no underlying infrastructure, "*nothing beyond what goes on*" (Law 1994: 16).

The fourth principle of **reflexivity** says that we are not different from those whom we study, we are products - and our own ordering is a verb, too. We should question our own orderings of the social world. Ethnography is a product, no neutral observation or disembodied intellect exist. However, this is not an inadequacy failing of ethnography, as all empirical or theoretical projects are partial. And the way that modest sociology should treat this problem is to expose its own contingencies, uncertainties or vulnerabilities. It is not about deconstruction of ethnographic writing but about saying that "*given my concerns I think that the Laboratory was this rather than some other way*" (Law 1994: 17). Modest sociology accepts uncertainty.

¹⁰ These are the distinctions (or rather the avoidance of distinctions) between true and false knowledge, humans and non-humans, and micro- and macro-social.

In my interpretation of the fieldwork data collected during the observation in the last chapter of this paper I will be looking at the laboratory leader and what he is made of. I want to see what networks or orderings of humans and non-humans he is a part of. I explain and see whether I can identify Law's material heterogeneity and in what modes of ordering the laboratory leader operates.

2. **METHODOLOGY**

As explained above, my thesis is based on participant observation at the Research Center. My participant observation started in October 2006 and is still in progress¹¹. At the peak of data collection, I used to spend two days a week with individual members of the two teams I observed.

As I was new to the method of participant observation, at first, I studied theoretical literature on the methods of doing ethnography as well as specific laboratory ethnographies. As part of getting ready for participant observation, I have participated in an ethnographic workshop organized by the Czech KNOWING team where various issues of this method were addressed.

Emerson, Fretz and Shaw (1995). describe ethnographic research as a firsthand participation in some initially unfamiliar social world where drawing upon such participation written accounts are produced. He sees the researcher as entering a matrix of meanings, whereas these meanings are particular and selected and never all-encompassing. The authors also stress that the presence of the ethnographer is consequential and that it has reactive effects, i.e. research participants¹² respond in different ways to the researcher's presence. The consequentiality of the researcher's presence is not a defect, to the contrary, reactive effects are a source of learning and

¹¹ The official finish date of participant observation was April 2007. However, as enriching insight is being gained through participant observation. We have decided to continue with participant observation also during the phases of in-depth interviews and focus groups. Also, the option of further research beyond the scope of the KNOWING project is being considered.

¹² There are different ways of calling those the ethnographer encounters in the field during participant observation - informants, participants, research subjects, etc., depending on what aspect or characteristic she wants to emphasize. I find the term "research participant" to be quite suiting the situation from the researcher's point of view and for the purpose of academic writing. This way, the subjectivity of participants is acknowledged, they are seen as agents, not as rather passive objects, and as co-producers of the account. On the other hand, the adjective in "research participants" clearly shows that this is a research situation - a specific situation introduced "from the outside" where also the researcher is an agent and her presence recognized as affecting the situation (i.e. being consequential).

Occasionally, the issue of their position has been mentioned also by the research participants who referred to themselves mostly as "objects" or "guinea pigs" (probably as a species of laboratory animals but also maybe in link to a novel "Morčata" by Ludvík Vaculík where the main character experiments and observes guinea pigs at home). When this happened, I always added that they were not "objects" but "participants" of the research, emphasizing the fact that without them no research or participant observation would be possible.

observation, and the ethnographer needs to be sensitive and receptive of how she is seen and treated by others. According to Emerson et al., the ethnographer can and should engage in what goes on, even in highly participatory roles¹³. However, the ethnographer never becomes a member of the community where she does research, and many local events are seen as objects of possible research. (Emerson et al. 1995)

Ethnography is committed to uncovering and depicting indigenous meanings and concerns of research participants, however, fieldnotes necessarily convey the ethnographer's understanding and accounts of the research participants' experiences, meaning and concerns. (Emerson et al. 1995)

A key building block of developing an ethnographic account is the taking of fieldnotes. No consensus exists as to when and how fieldnotes should be written (Emerson et al. 1995: ix). Emerson et al. defines fieldnotes as accounts describing experiences and observations the researcher made while participating in an intense and involved manner, emphasizing that fieldnotes are actually inscriptions - they are created by the researcher during an active process of interpretation and reduction when witnessed events, persons and places are transformed into words on paper and presented or framed in particular ways (Emerson et al. 1995).

According to Emerson et al., such an approach implies that what is observed and ultimately treated as "data" is inseparable from the observational process. What the ethnographer finds out is inherently connected with how she finds it out, and therefore, the ethnographer needs to document her own activities, circumstances and emotional responses. What happened is one account, made by a particular person to a specific other at a particular time and place for particular purposes. (Emerson et al. 1995)

I have been writing fieldnotes contemporaneously with the observation¹⁴. According to what the opportunity allowed, I have been taking handwritten notes during the very process of observation as my position of a researcher

¹³ An interesting, though artistic, example of the unavoidable consequential presence of a scientific observer and the impossibility of being neutral, objective or invisible in the field, is the Swedish-Norwegian movie *Kitchen Stories* directed by Bent Hamer (released in 2003).

¹⁴ I was writing down the fieldnotes in English, Czech and occasionally in French, depending on the language spoken by the research participants I was with at the moment. For the purpose hereof, I have translated relevant excerpts of the fieldnotes into English.

was clearly declared and known to everyone¹⁵. Rarely, did I have a chance to be taking notes directly in my laptop¹⁶. The reason for taking handwritten notes was primarily the dispersion of laboratories where I observed throughout the Research Institute and the small size of the laboratories proper. Both conditions caused me to move constantly - be it inside the laboratories to make space for another person to get around me, or be it running different errands following the scientists around the Research Center.

My presence in the field has clearly been what is called "participation-in-order-to-write", in order to produce a detailed written record, with an open style of note-taking (Emerson et al. 1995). Thanks to the openness of the research participants, I have never been asked to stop taking notes or not to reveal an event. On certain occasions, when I felt private issues were being discussed with me, I stopped taking notes out of my concern about the quality of relationships with the people in the field. Overall, I remained flexible about the approach to note-taking.

I have tried to take notes in great detail, interactional and descriptive. This has not always been possible directly in the field. Fieldnotes taken on-site have often been jottings. At my desk, I have then elaborated on my hand- and computer-written notes producing fieldnote reports where I would note also my comments, ideas and feelings about the events observed. These fieldnote reports have been meant primarily for my own future use, and also have been shared within the Czech KNOWING team. Of course, the research participants have been informed about this procedure of note-sharing.

I agree with Emerson et al. that the ethnographer is inevitably an outsider and remains a stranger as long as she retains commitment to the exogenous project of studying or understanding the lives of others, and that participating in order to write means assuming the mindset of an observer, constantly stepping outside of scenes and events (Emerson et al. 1995).

This is, however, not a straightforward situation. Even though, I have remained in the position of a researcher and observer, with the passage of time, I have started interacting with some research participants on a more informal and friendly basis. It seems that the researcher cannot become invisible but she can somehow become a common, being-used-to part and parcel of the setting.

¹⁵ According to Emerson et al., an open taking of notes is what contributes to the marginalization of the researcher and reinforces her position as an outsider.

¹⁶ Unlike another colleague of mine who has been taking most of fieldnotes in her laptop.

2.1 Processing and Analysis of Fieldnote Reports

Following the procedure suggested by Emerson, Fretz and Shaw (1995), in order to write this ethnographic account, I have approached my fieldnote reports as a data set and performed line-by-line open coding, producing initial general memos as a way of commenting on the potential analytic input of open codes and possible links and themes. Through open coding and initial memoing, the ethnographer looks at all analytic possibilities, trying to capture as many ideas as possible, while staying close to the fieldnotes; at this time no primary analytical focus or organization is followed as the purpose of this stage is to explore all possible directions.

Based on this stage of open coding, my acquaintance with the field and my research interest, for the purpose of the thesis I have selected one core theme and subthemes. I have grouped the relevant parts of the fieldnotes into segments in order to do a focused coding, elaborating a finer analysis of the themes, looking for overlaps, similarities, differences or variations. I have captured the relationships between coded fieldnotes in integrative memos - these being quite detailed and comprehensive examinations of the theme.

For the purpose of coding and memoing I have used the Atlas.ti software. This is one kind of CAQDAS, i.e. computer-assisted qualitative data analysis software. The use of this tool is an object of disputes among qualitative researchers¹⁷. The primary purpose of CAQDAS is that it automates some analysis procedures. Some believe that CAQDAS can open up new directions of analyzing (Dohan and Sanchez-Jankowski 1998; Konopásek (in press); Richards 2002), while other criticize computer-aided programs for qualitative data analysis as reinforcing the positivistic model of the absent or neutral researcher as *"the use of technology confers an air of scientific objectivity onto what remain a fundamentally subjective, interpretative process"* (Mauthner and Doucet 1998).

However, Atlas.ti does not have any internal, preset logic of classification that would in any way be enforced on the data. In my view, it is a tool which greatly simplifies the process of coding and memoing, if an anthropologist decides to process the data in this way. It is a means of replacing the copy-and-glue or copy-and-paste handling. As such, Atlas.ti can be seen as ideal for applying the processing of fieldnotes suggested by

¹⁷ I am grateful to my tutor for bringing this issue to my attention.

Emerson, Fretz and Shaw (1995) who also conclude that "[w]hether or not an ethnographer decides to use a computer program to code and sort her data, she nonetheless is the one who conceptualizes, interprets, and reconfigures the fieldnotes. The quality and usefulness of the resulting coding categories always depends on the ethnographer's thinking." (Emerson et al. 1995: 230, note 3).

2.2 Writing an Ethnography

In this thesis, I will map the outcomes of my analyses onto the concepts of John Law, focusing on the laboratory leader. This should be seen as a first step, and in my future work, I will elaborate on the specificities of the field under study, and consider Law's concepts critically; at the moment, however, it goes beyond the scope of this thesis.

I am going to write an ethnography as a fragmented narrative constructed from the fieldnotes (Emerson et al. 1995). Atkinson distinguishes two basic forms of writing an ethnography: firstly, "fragmented narratives" as nonlinear, rearranged everyday events in atemporal, paradigmatic relationships, secondly, "chronological narratives" which provide a linear extended chronicle of events (Atkinson 1990).

In selecting an angle of view, Emerson and colleagues offer:

- 1) the first-person point of view where *"the researcher presents the details he saw, experienced, and now remembers from his own perspective and his own voice"* (1995: 53);
- 2) the third-person point of view used to describe what others are doing and saying;
- 3) the omniscient point of view where the writer uses an "objective" tone and style to report events as realist tales, assuming the position of a detached observer;
- 4) combining and varying points of view. (Emerson et al. 1995)

Although I structure the ethnographic narrative chronologically as a "day at work", the account is a fragmented narrative (Atkinson 1990) developed from fragments of fieldnotes pertaining to my observation of laboratory leaders, i.e., on the basis of my analysis I take events which are paradigmatic for the position of the laboratory leader. I am going to combine the third-person point of view with quotes by research participants.

2.3 **Ethics**

The question of protecting participants against potential harms arising from research is an ethical one. It is not easy to tackle, and research ethic is the object of much attention. It is difficult to follow any general ethical principles as the ethics of ethnography take very much place at the level of practice (cf. Murphy and Dingwall 2001). Generally, I see two major moments when ethical issues must be dealt with. Firstly, it is right in the field when being with the research participants and responding immediately to various situations as they arise. Leaving the field, however, does not mean having finished dealing with ethical issues. Secondly, ethical considerations must be made at the time of publication when decisions are being made what to publish and what not, whether and how to anonymize the setting and research participants and how to represent them (cf. Murphy and Dingwall 2001).

As a rudimentary measure of protection, for the purpose of this paper, I have decided to render the place where the research is taking place fully anonymous. This is due to the fact that the KNOWING project is still in progress and similar principles of anonymity may be applied therein. I have also used pseudonyms for and altered non-relevant details of the participants appearing herein. However, an absolute guarantee of anonymity is hardly possible (cf. Murphy and Dingwall 2001). This makes individuals virtually unidentifiable to outsiders. However, my fieldwork is overt and individual members of the teams know it is taking place and would most probably be able to recognize one another (cf. Ellis 1995).

2.4 **Me as an Observer**

Above I claimed that an observer's presence is always consequential. It is difficult, though, to judge what the actual consequences of my presence were. At the beginning, my idea was to observe and note down as much as possible about the goings-on in the team and at the facilities. I did not have any intention of bringing in or promoting my ideas and opinions; I wanted to keep a low profile. However, it was I think the very first observation session when at the time I was about to leave an interesting discussion developed between me and a foreign scientist I spent the afternoon with. We talked about the Czechs, European Union and history, and I stayed for another hour. In six months, when this man was leaving, he invited me and three other Czechs to

a good-bye dinner as he said that we were the only Czech friends he had here.

And I have had personal discussions with other members of the team. Gradually, we have shifted to the informal form of addressing with most junior members of the team. Once a PhD student asked me whether I knew an English teacher who would give private lessons to his friend, and I recommended one. Another time I offered to arrange for Hebrew lessons for a senior researcher who was interested in learning the language.

I feel I have a closer relationship with some of the people from the team, others are just tolerating my presence, and I think the reason that I was not explicitly rejected by those less welcoming was that the laboratory leader supported my work in his team. At one moment, about five months after the start of my observation, I realized how much I was confided in, how much trust I was granted being just let in to observe. Of course, the research participants could have always exerted a degree of control over their actions and words but I believe it would be difficult to maintain such control throughout. Also, me together with my colleagues on the KNOWING team were allowed to move freely around the facilities, and it always depended on the negotiations with and will of each lab leader whether he or she would let us into their teams.

As regards the issue of trust on the part of research participants, it is true that the laboratory leaders and anyone interested were provided with a one-page description of the project and its goals or were explained the mission of the project when they asked. However, at the beginning, we were let in the Research Center and the laboratories without them knowing what to expect of us as social researchers. As the time passed, the issue of "giving back" became more pressing for me, without any pressure on the part of research participants, and so I raised the question of "what do you expect to get out of our research" with the laboratory leader whose team I was observing. He told me what he would like to get a better idea of concerning the workings of his team, and in the days that followed I felt his goodwill toward our research renewed.

My idea about asking what specifically the lab leader was interested in was really to give something in return for their trust - I have also made sure that any paper I would write for the lab leader based on his questions would be made available to all team members as whenever I met someone for the first time I told that everything they told me was confidential and no special reports were prepared for the lab leader or managing director of the Research

Center. But after all, it is interesting for our research to learn what issues laboratory leaders are interested in.

Thus, this would be a clear example of how research participants became co-producers of the research. But why look for such specificities, my thesis would not be possible without them at all, without their open or restrained attitudes, without their biased explanations, without their friendliness or animosity - they have all contributed to the research and changed me as a researcher and a person.

And when the power of the researcher is being discussed, of course it does not consist in some rare intellectual abilities I would possess, but more in having access to multiple locations, in a kind of multi-presence - I have been spending time with different people at different times, and I have had a chance to put together the data gathered at these different locations. Also, I might have gained knowledge of some things exactly because I was an outsider or that I was not the manager. Some people might have perceived me as a channel to voice their doubts and criticism - to make themselves heard. And as regards the limitations of being an outsider, there are also advantages to it.

3. **ETHNOGRAPHY**

3.1 **The Setting**

The intention of this section is to introduce the physical and social settings where my participant observation was taking place.

3.1.1 **Introduction of the Research Center¹⁸**

This paper is based on an ongoing anthropological and sociological research at a major national research institution of the Czech Republic. The Research Center is funded from public and private sources and carries out research in natural sciences.

The Research Center was founded after World War II, from the very beginning focusing on an interdisciplinary research in the field of chemistry and co-development of theory and experiment. As regards the peopling of the Research Center, two major outflow waves of researchers - in 1968 and 1992-1993. The first one was motivated by the invasion of the armies of the Warsaw Pact in 1968, which resulted in the emigration of many prominent researchers. The second one was an effect of new opportunities abroad opened for Czech researchers after 1989, compounded by the fact that the Academy of Sciences was downsizing and there were funding cuts. This situation resulted in temporary depopulation of some labs of the Research Center. Some researchers left permanently (especially for the United States) and some returned - bringing along new research topics.

In its communication documents and in media coverage, the Research Center presents itself and is presented by the media as an exceptional and top-ranking research institution with a significantly increasing number of publications in impacted journals and unique research projects for the Czech Republic. It has an image of an institution with an outstanding scientific profile and achievements.

Recently, the Research Center has started a restructuring process, linked to the legislative transformation of national institutes from "State

¹⁸ The introduction of the Research Center is from a bigger part based on an unpublished progress report of the KNOWING project delivered to the European Commission in September 2006, produced under joint authorship.

Allowance Organizations" to "Public Research Institutions" which occurred as of January 1, 2007 and granted national institutes greater economic autonomy.

The restructuring started with a tender for team leaders and consists of introducing a new organizational system with redefined competences and hierarchical and evaluation systems. Existing departments were closed and replaced by independent teams with new team leaders subordinated directly to the director of the Research Center. The restructuring would result in changes in staff numbers which might be reduced by approximately 30%. According to available documents the transformation is expected to provide newly created teams with bigger institutional support, raise the quality of working space (which is a crucial issue for many departments and teams) and provide a higher level of autonomy and responsibility to research teams. After 5 years (in 2012) for senior teams and after 3 years (in 2010) for junior teams there will be a round of evaluation.

3.1.2 Physical and Social Settings

It is not only the organizational restructuring of the Research Center that is taking place. At the same time, the building is being reconstructed and new working facilities are being developed. The interior of the Research Center is being rebuilt, teams are moving to newly restored laboratories. Also, individual teams are "on the move" in terms of their membership - students and scientists are coming and going, former teams are included into the newly established ones. It means that that the social and physical phenomena are currently in a state of flux.

I will first describe the physical environment of my observation and the actual place of the participants' work. The team is seated at the main building of the Research Center, but within that building, the team members are scattered in a number of different laboratories on different floors. At the time of observation, most of the team worked in unreconstructed laboratories and only two post-doctoral fellows occupied a refurbished laboratory.

The difference between the "new" (reconstructed) and "old" laboratories is primarily in the furnishing and installations. The new laboratories have for example a better ventilation system, modern-design

furniture, modern cupboards¹⁹ - ensuring safer and probably better working conditions²⁰. On the other hand, there seems to be no difference between the old and new laboratories in their actual equipment and machines the researchers have access to. There is standard equipment present at every laboratory (an analytical weighing machine²¹, an evaporator²², computers, etc.). Other machines, obviously those more costly, are shared within the team, or with other researchers from the Research Center²³.

A significant feature of the old laboratories is the lack of space. This can occasionally cause some jamming at the cupboard. The first time I realized this was when a post-doc said that he would like to get some work done today as he is alone here and his colleague was soon coming back. I did not understand the significance of this remark until I saw them working literally side by side as the cupboard is about 1.20 meter wide. When I inquired about the lack of space, the researcher replied that it was not ideal but it was better than when there had been three of them using one and the same cupboard.

Nevertheless, space remains scarce also in the reconstructed laboratories; although it is true that in the new laboratory, three cupboards were available to two researchers. This seems to be quite a luxury because in both of the old experimental laboratories there was merely one cupboard.

In another old laboratory, having also only one cupboard, three researchers had been seated at the outset of my observation and lack of space had not seemed to be a problem. The reason probably had been that during the first two months of my observation, I had seen only one post-doc actually "working at the bench" (though I have not inquired about the situation), while the two senior researchers spent more time on administrative

¹⁹ A cupboard is a working area where experiments (or „*cooking*“ as the jargon has it) takes place. It is where most of the flasks, holders, lifters, boilers, etc., are. This is what researchers refer to when they say „*working at the bench*“.

²⁰ Even though I have been a witness to a discussion when a post-doc fellow from another team complained that the ventilation system of their "new" cupboards was out of operation and they were not even warned and continued working. This made the post-doc quite angry as such a situation is deemed dangerous because if not vented, the gases produced during the experiment could be breathed in by the experimenter.

²¹ It is basically an ordinary weighing machine, but very sensitive and weighing accurate to a ten thousandth gram.

²² An evaporator is used to remove solvents from a compound at the phase of purification. A PhD student commented that „*this [evaporation] is 80% of chemistry*“.

²³ This is the case of a microwave oven located in one of the laboratories I am observing.

work and consulting than working at the bench²⁴. Later on, two new members of the team started doing experiments in that laboratory, and three people were using one and the same cupboard.

At the turn of 2006-2007, only two post-docs worked at a newly reconstructed laboratory - at the beginning of my participant observation I had the opportunity to see how they were settling down in their laboratory, cooperating on procuring and installing new equipment. A PhD student and a post-doc shared another laboratory in the not-yet-reconstructed part of the building; and the head, a senior scientist, and a post-doc were seated in another "old" laboratory. Two other researchers occupied two more rooms (I have not had a chance to observe these yet).

The team I observe has a fluctuating number of members. The composition of the team was different in spring 2006 from what it is now. If I should concentrate on the formal delineation of the team, the research group's website gave the names of eight people as team members (and six others as former members, and yet six others as training placements).

Furthermore, during one observation I learned that there were actually two more (official) members of the team - two theoretical chemists. The head told me that they joined the team in summer 2006, and that they do "*some calculations*" for the team. To my remark that they were not included in the website presentation, the head replied that they must have been left out by accident, that the website was not updated. However, the post-docs who came in October and November 2006 were already named on the website.

During the time of my observation, a team member came back from an research stay in Germany, there were a few temporary placements of visiting students, another junior scientist who had left the team before came back and was employed as a research assistant with the outlook of doing her PhD studies at the team.

Together with the restructuring and reconstruction, also a new website of the Research Center is being developed. The website gives general information about each research group and its members. Some teams make the Research Center's website the primary place of their presentation, while other teams have separate sites, and the common website features only the names and positions of the team's members.

²⁴ I am not claiming that the senior scientists have not done any work at the bench, only that at first I did not see such "jamming" at the cupboard as in the other laboratory.

According to the information on this new internet presentation of the team I observed, the team's composition would be yet different, being made of one head of the team, five senior scientists, three postdoctoral fellows, a research assistant, five graduate students, a technician, and a secretary. It is just to show that due primarily to the restructuring even the formal membership is unsettled, not to mention the emergence of ad hoc "teams" or networks in relation to specific research projects, writing of articles, etc.

3.1.3 General Structure of Experimental Work

For a better understanding of what the "handwork" in experimental chemistry entails, I am now going to describe the general structure of experimental work. The structure was explained to me by a post-doc fellow²⁵ and confirmed by others. Personally, it was a great moment for me, and his explanation helped to make a big step further in understanding experimental chemistry done by the team.

An experiment is divided into five main stages:

1. preparation;
2. set up;
3. process/reaction;
4. purification
 - a. coarse
 - b. refined;
5. identification.

The first phase concerns mental and theoretical preparation - it means searching for relevant information in databases, on the Internet, looking up articles - all that in order for the scientist to avoid discovering how to do what has already been done (including repeating similar mistakes), to overcome possible pitfalls and difficulties, to get an idea whether some step of the experiment can be done differently - *"Maximum knowledge is needed in order to anticipate, for example the length of reaction, to prevent problems - preparation helps to make decisions."* The second phase is about setting up - getting ready the equipment, cupboard, glass.

²⁵ This person is in a very interesting position. In this team, he is in the position of a foreign post-doctoral fellow - this is his recognized position in this system, but back home he holds a post of a permanent researcher at a leading national research institution, and received habilitation in 2001.

The third phase is the process, the very reaction, the cooking, with varying duration. It is the moment when chemicals are mixed, and is critical for the quality of the final product. The scientist should have an idea about how the reaction would proceed on the basis of information she gathered during the first phase of preparation. The reaction is then "killed" either by cooling down the substance or by adding another chemical which reacts with a partner already present in the substance - here it is necessary to be very careful as the product should not interact with the substance (other than stopping the reaction). It is also at this phase that the scientist takes detailed notes of the course of the experiment into a laboratory book²⁶.

The fourth phase is when the product is purified. At the end of the reaction (phase three) and at the end of purification, there is a risk of losing individual components of the compound, e.g., by adding an unsuitable chemical, a risk of changing the product's characteristics - it is a serious problem as the scientist would not get what he was aiming at.

In the fifth phase the product is being identified. There are different ways of identification depending on the machine that is being used. The scientist wants to see and check what the actual product of the reaction is, whether she got what she wanted. If there are any unusual findings, i.e., the scientist did not get the product he expected, he goes back through the experiment, using the lab book records, and looks at what phase or moment might be at the root of the problem²⁷.

3.2 **The Laboratory Leader's Day at Work**

In the morning, at 8:29 a.m., Martin arrives to the meeting room next to the managing director's office. It is the day of management meeting which takes place once a month. He arrived at work earlier than usual, most days he comes around 10:00 a.m. When he arrived at the meeting room, as everybody else he was asked by one of two elderly female assistants whether he wanted a cup of coffee or tea, being promptly served the drink of his choice.

At 8:30 a.m., the meeting of laboratory leaders starts. The room is rather small with a plasma screen in front. Electric and network plugs are

²⁶ A postdoctoral fellow told me: „It's a lab book - I put down the timing, all I saw, how long it took.“

²⁷ This moment, the fact that the researcher called a variation between what is expected and the final product a problem, started an interesting discussion about what is deemed a failure and success in chemistry and how a failure can be fruitful (or publishable).

accessible from every seat. However, nobody uses any equipment beyond a pen and a notebook, taking notes very occasionally; except for the managing director who has a laptop with a PowerPoint presentation of the agenda.

Today, twenty people are present. They are mostly men, together with two women leaders, and an office manager sitting out of the inner circle, in the outmost corner. There is also a new public-relation manager - a woman - who has been hired only recently. All of them are Czech, but one. The language of the meeting is Czech²⁸. Most are seated around a rectangular table, some sitting on spare chairs with their backs to the wall. Everybody is dressed informally. The managing director is seated in the front with the plasma screen behind his back.

The managing director opens by introducing a well-groomed man in his fifties: *"Michael will visit your teams to discuss options of securing sources of funding for the Research Center to be able to keep working at the level we have gotten used to."*

Michael is a representative of a consulting company. He presents his mission at the Research Center: *"I am going to discuss with you your projects as to their commercial potential. I would like to have a short interview with everyone. Please, give it a thought in advance. It will be great if you have your ideas ready. I will start going around and bothering you beginning next week. I have signed a confidentiality agreement, all the information stays with the Research Center and nothing will be disclosed to a third party. So you don't have to be afraid that your ideas would be stolen. I will be around here for four months."*

The managing director adds: *"These issues must not be put aside. We want to have a fine working environment with sufficient financial resources. Should any of your ideas be put in life, it will be beneficial also for your family budget as you will receive a percentage of the profits. The outcomes will be presented to the Council, and if possible to be disclosed, they will be made available also to all of you."*

Michael leaves, and the managing director moves on to another item on the agenda: *"I have a rather unpleasant point to start with. There are laws which we must follow. In the managing director's decree number 04/2007 you will find the requirements of the Ministry of Health to submit inventory-taking lists*

²⁸ For the sake of clarity I repeat that for the purpose of my thesis I have translated all excerpts from fieldnotes into English.

of all chemical compounds in each laboratory. The deadline is seven days from today. No extension is possible. Martin is the person responsible that this will be done."

A lab leader: *"I would not be here next week. May I submit the list in two weeks? I wouldn't be able to check the list."*

Managing director answers in an uncompromising tone: *"No. It is not possible. You must comply with the deadline. You must learn to be a manager. You should assign the task to a subordinate. Learn to trust them."*

The managing director moves to the next point on the agenda: an allocation plan for the start-up financing package²⁹ was supposed to be submitted but not all lab leaders did so. One of the woman leaders and another male leader disagree claiming that no specific deadline was fixed. The managing director retorts: *"A deadline was fixed. You should at least come up with an estimate."*

It is only eleven minutes after the start of the meeting and a fourth point on the agenda is being discussed. It concerns the applicability of the labor code on international visitors. A separate contract must be signed with each visiting speaker and scientist for the lab leaders to be able to pay them any money. The leaders shake their heads in disenchantment. A discussion ensues. Comments as *"things always must be made difficult for us"* can be heard, and the managing director responding: *"Well, we could go on strike and picket in front of the government's office but I don't think it would be of any help"*.

And he goes on: *"There is quite a number of internal and external regulations and consequences for your work. For you, as managers, we are planning to organize special workshops where you would be informed about your rights and obligations - for you to know the grounds. It will be at the cost of your research work, but on the other hand you will know your way around."*

In the end, another topic is raised by one of the men. It had already been discussed at previous meetings. *"What about lockers? Students don't have a place where to leave their stuff. This issue should be solved. It's unbearable."* Few other lab leaders nod in agreement. After a short discussion with emotions building up, the managing director says: *"Well, everything has its time. There are more important issues than this. You certainly understand that not everything can be settled at once."*

²⁹ As part of the transformation and establishment of new research groups - each team was allocated a budget from the Research Center's funds. Once allocated, the lab leader has complete control over the budget.

The meeting is about to end. It is the Easter time. The managing director concludes with a question: *"Do the gentlemen have their Easter sticks ready for Easter thrashing."*

Several men nod in agreement, noting: *"The sticks must be fresh."*

And as a way of saying goodbye the managing director says: *"Although my wife has aged, she would be angry with me, if I didn't thrash her at Easter. Enjoy the Easter time."*

The management meeting is over in 20 minutes. These meetings are very matter-of-fact and businesslike. The managing director's office where the meeting was taking place is in a newly reconstructed building, across the yard from the historical building of the Research Center where many of the experimental laboratories are located. Not so long ago, the managing director's office was a tiny room 1.50 to 2.50 meters, divided from the corridor only by a thin paper wall. The managing director moved into a new office which was renovated as part of the rebuilding and reconstruction work going on throughout the Research Center's facilities.

Martin returns to his laboratory. He shares it with three other people - a senior researcher, a PhD student, and a postdoctoral fellow. The laboratory is full of equipment and rather crowded. He too will be moving soon to his own office. It will be the first time in years that he will actually have an office of his own. Otherwise, he has had a desk in one of the rooms his team occupied, in the area where experiments have been going on throughout the day.

However, now, Martin is sitting by his 80 cm wide table with a flat computer screen in front of him and two junior experimenters mixing their chemicals and doing reactions behind his back. On his left hand side he has a spare table with a pile of papers. And when I say a pile, I mean it. There is a computer box next to the pile of papers, and Martin explains jokingly: *"If I am able to push the button to turn the computer on, it is still alright. But if the pile of papers reaches above the button, I know paperwork must be done. The new office I am supposed to get will be my salvation. It is the only way how to put the papers and files in order."*

Just few minutes after Martin sat down, a colleague of his, a leader of another lab arrives to discuss the organization of a major European congress; the congress is part of a series, and each time it takes place in a different

European country. Together with a senior woman researcher from Martin's team, they are the organizers on behalf of the Research Center. For Martin it is *"a kind of organizational, administrative and scientific work in one"*. The two men are discussing who to invite as keynote speakers. The internal rule is that one person can give a keynote address only once in the entire series, and since this is a European congress the keynote speakers should be Europeans, or at least of European origin.

Martin explains to me that chemistry has a great information system, and 99% of all research is available in the form of articles. *"The database dates back to 1900. There is also the Web of Science where you can look up a particular person and see about his citation index, publication record and the like."*

At 10:42 a.m. Martin makes a phone call - the man he was calling is not in his office. He decides to do some paperwork. At 10:43 a.m. the phone rings - a team member, theoretic chemist, is calling. It concerns a synthesis which is already running and for which some calculations are needed. At 10:46 a.m. Martin answers an incoming phone call - a colleague from a university is seeking advice, Martin explains what additional devices are needed for a machine his team has recently bought and his colleague is planning to buy. At 10:49 a.m. Martin makes a phone call, followed by another one at 10:51 a.m. At 10:55 a.m. Martin calls a colleague telling him that he would send him some samples that he needs calculations for in order to publish an article. Having finished the phone call he explains to me that it is needed for a publication which has been dragging on for too long and should have been published a long time ago.

In between all the phone calls and interruptions from people coming to the office, Martin started processing an order for a colleague of his, Jan, who has his own team at the Research Center. However, the Research Center is not Jan's home institution. He works in the United States. Martin explains that Jan *"comes for two or three months a year and while he is not here, it is me who is looking after his team. It is very demanding in terms of time but I hope it will pay off. I hope Jan will teach us something."*

While he is explaining this to me, the team's secretary comes in. She is a woman in her thirties, and she is new. Their former secretary is leaving today; she is going to give birth soon. The team has had a secretary for little over a year. Martin hopes that a secretary would free him of some of his

administrative duties. Actually, the secretary is coming to solve an issue concerning Jan. Jan wants some of his travel costs reimbursed but it seems not to be possible for legal and accounting reasons.

Martin calls the managing director's office to talk to the managing director about this issue. A secretary answers the phone telling him that the managing director is not there at the moment. Martin says he would call later but takes the opportunity to ask about the payment of per diems for his conference trip to France.

Martin writes down a reminder to call the office manager: *"If it weren't for the post-it notes, I would not know what to do,"* he says and sticks a note on the edge of his computer screen.

After a few more incoming and outgoing phone calls, Martin says it is time to fill out an order for new equipment. He makes a sarcastic remark: *"This is my favorite activity."*

For this purpose, the Research Center has an internal ordering system³⁰ where some operations are automated. Martin, as the research group leader, must authorize all orders³¹ made by the members of his team. A request is made by a team member via the internal ordering system, and this request is delivered to Martin's computer screen. He can then confirm it.

Martin hardly starts filling out the order when an international postdoctoral fellow from his team comes in and says: *"Bad news - I've just broken the microwave."*

Martin: *"During the reaction?"*

Postdoc: *"It started making funny noises. Vašek³² must call the technicians to fix it."*

Martin: *"So, is he dealing with it?"*

Postdoc: *"No, he is preparing for his conference talk."*

Martin picks up the phone and calls Vašek: *"I've heard that the oven is down. Should I call a service person?"*

³⁰ At the time of my participant observation, the internal system was in Czech which must have complicated the work of the many international employees at the Research Center, and of course meant an extra burden of having to have someone explaining what the Czech words meant.

³¹ Depending on the value of the ordered item, it can be either purchased from a supplier selected by the team or it must be bought from a designated supplier. For items of high value, tender procedures are organized by the Research Center.

³² A PhD student and a colleague from the team.

Martin puts down the phone and tells the post-doc that the Research Center has another microwave available at another team's laboratory³³.

Meanwhile, a technician comes in and says that he is coming to put some equipment in use: *"You have already sent us a conveyance³⁴, so I can put the machine in operation now, provided that you have an issue slip³⁵ ready."*

While the technician explains this, Martin calls the managing director's office trying to get in touch with the managing director who is not there yet. Martin turns back to the technician: *"Where were we?"*

Technician: *"The issue slip."*

Martin: *"Oh yes, I will write up one. There was more than one thing, right?"*

Technician: *"The delivery note is with us. You can get it. And you have already got the shredder."*

Before the technician leaves, Martin has an incoming phone call. It is the managing director. Martin discusses with him the issue of reimbursement of travel costs, arranges for a meeting with a representative of a cupboard supplier, mentions that the heating in one of the laboratories has not been repaired yet.

The technician returns with a copy of the delivery note. Martin signs it and continues the conversation with the managing director: *"Do you have any idea how to distribute the laboratories among research groups? I have gone downstairs to look at the room and would like to ask whether it would be possible to exchange it for something more suitable for doing chemistry."* The phone call lasts nearly fifteen minutes, quite unusual, most of the other phone calls were finished within a minute.

Having finished the phone call, Martin is approached by the postdoctoral fellow seeking advice on an expert issue. Martin willingly explains him a *"possible trick to prevent polymerization"*.

While he is consulting with the postdoctoral fellow, a young man about 18 years of age comes in. Martin apologizes that he cannot talk to him at the moment, and asks him to kindly wait a minute. Martin is very calm in

³³ As many pieces of equipment are very costly, some machines are shared by all teams within the institute. After the restructuring when all teams are deemed financially independent and separated, ways of monitoring of and charging for the use of the common equipment are being developed.

³⁴ A conveyance is the document by which a property transfer is effected.

³⁵ An issue slip is the document by which a property is released from stock and put in use.

everything he does, never raising his voice, never acting as to suggest that the caller or a person coming to him is unwelcome or might be wasting his time.

The young man is a secondary school student who has been coming to the Research Center for nearly a year now learning to do minor tasks in experimental chemistry. He comes under the Open Science project aiming at making science accessible and introducing it to those interested. Martin discusses with him the possibility of getting him employed on a part-time basis.

This is not the only way Martin's team steps out of the laboratory walls. As part of popularizing natural sciences and reaching nonscientists his team has also been featured in a regular television program on natural sciences. Other scientists, usually team leaders, also communicate with the lay public - they write articles for non-expert magazines as *Vesmír*, for national dailies, and one lab leader is a host of a more philosophical or social-affair program on the national TV channel.

Martin calls another laboratory - it is Jan's laboratory, the one Martin looks after out of his duties toward his own team. He asks about the heating and that it should be fixed next morning. Martin puts down the phone, he turns to me: *"I have just finished filling out the order which took me, with some breaks, more or less two hours. And now I am entertaining a sinful idea of going for lunch."*

But he keeps on sitting by the computer, responding to emails and getting rid off things which can be done by a single click - usually internal orders. He makes a note in his palmtop: *"I use an electronic diary where I keep all my agenda. I synchronize it with the computer. Now, I have to take a paper order to our secretary. And after lunch I will be writing up a report on a center of excellence - another two hours wasted."*

We are leaving. Martin locks up the laboratory and tells me he would be back in half an hour. Whenever empty all laboratories should be locked. This is a measure of security as theft by outsiders is not uncommon. It is not difficult to pass by the reception unnoticed. There was a case when a young thief got in repeatedly even though his photograph was put up at the reception to prevent this.

When I come back from lunch there is unusual commotion in the corridors. Usually the corridors are rather quiet, a person here and there passing by. At first I thought all these people were going to or from lunch.

I enter the laboratory, Martin is there and he explains: *"The water is out. The only possibility now is to go home. One cannot even wash hands or flush the toilet. I am going to tell all my team to go home because water is not going to run until late at night."*

Martin makes a phone call and asks a colleague to tell everybody from Jan's group about the situation. Then he runs out to get a bunch of flowers for the secretary who is leaving today. All his and Jan's teams are going to bid a goodbye to and thank her as she has been the one who showed them how to get around when they came - how to open an account, get a transport pass, use the intranet system and the like.

It is getting late. Obviously, most people left because of the water problem. Martin is not receiving so many frequent calls. The atmosphere is much calmer. *"I am going to turn on the light for you to see us,"* Martin says to me. A member of Martin's team comes in, he does not say anything only looks for an instrument, takes it and walks out.

A member of Jan's team, a young woman postdoctoral fellow comes in quite angry asking Martin about an incident which occurred a few days back. Due to some new installations the cupboards taking away the fumes produced by reactions were turned off in their laboratory and they were not told. It was only by luck that they realized something was wrong and stopped working. They called Jan who was also very angry and said he would have to talk to the managing director about this. She understands that things like this can happen but since this was not an unexpected failure but something planned they should have been told in advance. When she leaves Martin explains that many chemical substances are harmless, but if you are exposed to them regularly over a longer period of time receiving low dosages of poison it can become serious. An associate professor is known to have died because he worked in a badly ventilated environment.

It is late afternoon. Everything is quiet. Martin started writing the report on the center of excellence, while typing he receives a phone call requiring about the report as it was already supposed to be handed in. Martin apologizes: *"I am working on it right now. It will take no more than 30 minutes. I am sorry,*

I had to look after other things. Now, water is out and people have left, so nobody is disturbing me."

Right after Martin sends the excellence report by email, Vašek, a postgraduate student of Martin comes in to consult a presentation of Vašek's experimental work that he is supposed to give at a students' conference. They do not discuss much of the expert content but rather the colors, pictures, diagrams, who to include in the acknowledgments, some issues of Czech grammar and the like. Martin advises Vašek to do the presentation first at home and time it to see how long it takes. It is supposed to be no longer than 15 minutes, and Martin warns Vašek that the worst things if you are told by the moderator to stop your presentation before getting to the end. Vašek leaves saying that he would incorporate the changes and send Martin the final version.

It is six o'clock in the evening. Martin decides to leave. On other days he stays until eight or ten in the evening, if the situation requires and also because it is more quiet in the evening and Martin can get more work done. But today he arranged for a rehearsal of a chamber orchestra he is a member of. He plays an oboe, and they are going to have a concert in two weeks.

3.3 **Laboratory Leader/Manager As a Network and Modes of Ordering**

In my interpretations, I draw on the work of John Law, especially the parts where he discusses the position of manager, what the manager is made of, what powers he or she has and in what frames or modes of ordering the scientist in a modern organization operates. I draw heavily on his book *Organizing Modernity* (1994) and the article *The Manager and His Powers* (2003b) which are concerned with organizing and ordering in formal organizations. The book is an organizational ethnography based on a year-long observation in a large scientific laboratory.

Law believes that no fixed order exists and that what we might see as order is rather a never complete process of ordering. Instead of organization as a noun we should talk about ordering as a verb, however, bearing in mind that there is not a single order but "*plural and incomplete processes of social ordering*" (1994: 2). Also, we are all implicated in these ordering processes, we are all producers of and at the same being produced by these orderings.

And finally, the social is materially heterogeneous - made of "*talk, bodies, texts, machines, architectures* [etc.]" (Law 1994: 2). Law sees all the social world as complex and messy, heterogeneous, where much effort is put into producing an appearance of ordered simplicity.

3.3.1 **Material Heterogeneity**

In his works, Law seeks and offers ways of how to avoid reductionism³⁶. His approaches are varied, and in the article entitled *Heterogeneities* (2003a) he starts explaining what he means by material heterogeneity by distinguishing between the structured order of architecture and ordering of minimalism (the style of music).

The idea of **architecture** as a structured order would be the desire of the Enlightenment project which, said simply, believed that one might know all and that one might control all. Law calls the Enlightenment project a dream (e.g., 2003a). He believes that knowing is limited, not foundational, its effects are ambivalent, good and bad intertwined... The structured order of architecture is an order as a noun, probably designed, it may be identified from a single location, it is a world filled with plans, of progress, of homogeneous spaces. (Law 2003a)

Minimalism is ordering, it is a verb - "*an effort at patterning that has some kind of shape, a shape that can, in one way or another, be discerned*" (Law 2003a: 3). It is an ordering which exists in tension, it is a process on the move, ever displacing itself, provided that the constant deferral and slippage³⁷, the dis-equilibrium is accepted and incompleteness embraced as part of being. What we see are impurities and heterogeneities.

Law identifies three types of heterogeneities³⁸, and for the purpose of my paper, I am going to focus my attention on **heterogeneity/materiality** or material heterogeneity. Law links the idea of material heterogeneity to post-structuralism, actor-network theory and feminist work on corporeality³⁹. The idea is that **relations are not simply social**, they are inserted into other materials. Or, to make it symmetrical, relations of other materials are

³⁶ I discussed the principle of non-reductionism in Chapter 1.

³⁷ Law refers to Derrida's concept of *différance*.

³⁸ Heterogeneity/Materiality, heterogeneity/Alterity, and Heterogeneity/Fractionality (Law 2003a).

³⁹ Works of Michel Callon, Bruno Latour, Donna Haraway, Emily Martin, Sharon Traweek, and others.

inserted into what we call "the social"⁴⁰. The distinctions between "social" and "material" are problematic - they are outcomes, for relations are performed, not given. Relations are held in a variety of different media: "words; bodies; texts; machines; buildings. All mixed up." (Law 2003a : 4).

What does this mean for Martin as the laboratory leader, the manager?

Law writes that at first, from distance, we see the powerful manager sitting in the cathedral of science. The cathedral's walls are clearly identifiable and seem to delineate science from the surroundings (remember Traweek's citadel). The cathedral of science employs hundreds of scientists; it is a huge organization, an administration, a bureaucracy. In what Law calls the distal, the place of power is equated with the manager, the source of power, a powerful man.

This was the way I saw Martin at first. I saw him as the location where all the threads come together, the man who pulls the strings, who has control over his team, who makes decisions that are then more or less straightforwardly put in practice.

However, the concept of material heterogeneity and the principle of symmetry make me to look at Martin differently. I see that Martin in his office, sitting in a chair, by his desk, with his cell phone, telephone set, in front of a computer screen, the Research Center's internal ordering system, the team's secretary, a technician coming in to put a piece of equipment in operation, filling out the papers for Martin, another technician being called to come and fix the microwave oven, and the microwave oven allowing (or at this moment not allowing) a postdoctoral fellow to work on the team's project... I see materials and people contributing to who and what Martin is.

At the management meeting in the morning, Martin was required to hand in a budget plan - he would not be able to do this without his computer, calculations, information on finances. In the afternoon he was working on the report on the center of excellence - it would not be possible without the word-processor and electronic mail to send it only shortly after the deadline. I saw Martin's files and binders with archives and documents, I saw his electronic diary, and let's not forget the post-it notes - he would not be able to

⁴⁰ Law writes that this was more a discovery of social theorists than social practitioners. In my opinion, a typical example of leaving out „other materials“ and reducing the world to „the social“ would be *The Social Construction of Reality* by Berger and Luckmann, published in English in 1967.

remember, to find or to have at his fingertips the information he needs at a time.

Law asks what we are left with if we take away "*the bits and pieces of power [...] of the powerful manager*" (Law 2003b: 2). If we take away the calculations, information on finances, computers, telephone sets and many other "bits and pieces" - there is no longer a manager-accountant, or a manager-writer without the word-processor; and without his archives, paper or electronic diary we have a manager without a past.

This frame of mind allows seeing the manager as a **network, a process**. Martin is nothing by himself, he is left only with his body, and according to Law, the powers of body (e.g., shouting, violence) are the least powerful (2003b). Martin's powers are extended and distributed through the arrangements of the organization. They lie in **people** - his secretary, the technicians, team members, the managing director, the office manager, but also in **technologies** - his computer and electronic mail, his telephones allowing him to communicate with other people within the organization, in another building, but as much easily with someone in the other end of the world.

Martin is a network of social and technical relations; he is made by the organizational relations⁴¹. Power is distributed, resides elsewhere, very little of it can be linked exclusively to Martin - Martin is the body (or the brain) and becomes a manager (or scientist) only as a part of a materially heterogeneous network. Martin's power is thus a product, an effect of this network. Looking at it from this point of view, it is not untrue to say that Martin is a telephone set or a post-it note.

3.3.2 **Ordering of Modern Organizations**

I mentioned above that Martin's power arises from the arrangements of the organization. Law also stresses that the manager is confined by the organization, she may act only within the logics of the organization, and as such is a creature or the expression of it - he performs the organization (2003b).

Law sees **organization as performance** (e.g., Law and Moser 2003) - performed by both people and materials. As mentioned above, any

⁴¹ I am going to address the issue of organizational relations in the section on modes of ordering below.

performance is heterogeneous. It involves a number of different objects and subjects - office, secretary, a desk, computer, telephone, internet database, reports, software, etc., they are all actively involved in performing Martin as a managing director. This is close to the idea of **cyborg** mentioned in the opening chapter - the manager is an effect of not only bodily, corporeal performance but also of other materials, it is no longer easy to determine where agency resides.

Law claims that agency is distributed between, and modern organizations ordered in, a fairly small number of ordering logics or **modes of ordering**. These are: bureaucratic or administrative, entrepreneurial, vocational, and visionary or charismatic.

Law emphasizes that organizational ordering is an **intersecting performance** of these logics, and the intersections are complex, never rigid, always open, and existing simultaneously. It means that the modes of ordering may be at times incompatible with one another, or combine together without difficulty, or include one another, or be interdependent.

Law describes organization as a *"continuing performance in which there is no dominant logic, no single plan, but instead a continual process of slippage and deferral"* (Lawand Moser 2003: 8). This "endlessly deferred ordering" is productive as decision-making and avoidance of blockage are possible thanks to not insisting on a single ordering mode, it allows the flexibility needed.

In the **administrative** or bureaucratic mode of ordering, an organization is performed as a hierarchical set of offices, each with own tasks, acting according to systematic rules, with proper conformity to due process. Authority is derived from office, delegated powers - from legality and due process. The proper administrative subject performs his tasks dutifully without regard to personal or emotional considerations. And no initiation of radically novel action is expected of the administrator/bureaucrat.

In the ethnographic account, the administrative mode of ordering clearly emerged in the second point on the agenda of the meeting with the managing director. A director's decree⁴² was mentioned requiring everyone to submit a list of chemicals at their laboratories in order to meet the stipulations of an act. I see several levels of ordering here. The Research

⁴² And this was one of a number of director's decrees issued every year, implying possibly to what a colleague of mine called a „high level of formalization“ of the Research Center.

Center, operating in a certain legal framework of a country is required to meet the law. And this requirement is further translated to the level of the Research Center's management, to individual laboratory leaders who are expected to deal with the situation, to somehow fulfill the requirement. To that purpose, Martin would mobilize the materially heterogeneous network of people and objects he is a part of.

The burden of administration is such that lab leaders, explicitly addressed as managers, would (have to) attend special workshops where all the internal and external regulations and consequences thereof for their work would be explained. And it is deemed to be of at least as much importance as scientific work because it was said that the workshops would be organized even though the attendance would be at the cost of research work.

An **entrepreneurial** organization is a set of risk-taking locations which are allocated resources, and then required to utilize those resources in order to secure an optimal return. The subjectivity that the entrepreneurial mode produces is an active, assertive, responsible, strategic, discretionary, resource using and calculated-risk taking person who achieves goals in an optimal manner; again without regard to personal or emotional considerations. The desire has to do with performance and success, and also authority derives from success.

The pressure on performing a manager in the sense of the entrepreneurial logic is becoming very strong in the Research Center. An example here would be once again the list of chemicals that Martin was required to submit. Above I wrote that to that purpose, Martin would mobilize the materially heterogeneous network of people and objects he is a part of - but he is also expected *to know how to* make use of the network. He must "learn to be a manager", and the managing director showed annoyance with what he saw as not knowing how to successfully manage when one of the leaders asked for postponement of the deadline. The request for postponement also shows that lab leaders are seeking ways of negotiating the administrative/bureaucratic demands. The same effort at negotiation of, or in this case more strongly dissatisfaction with the demands of legality and due process of the bureaucratic mode was when the issue of applicability of the labor code on international visitors was being discussed. The lab leaders were disenchanted and it was complained that "*things always must be made difficult for us*". In this complaint, I would interpret the "us" as "us visionaries, us creative scientists" of the vocational or even visionary modes of ordering.

This is a good example of the complexity of intersecting logics as they are enacted by the individual lab leaders.

The entrepreneurial logic of organization is becoming very important in another way. Research groups (laboratories) are expected to generate as much resources as possible. Remember Michael, the representative of a consulting company, who came to help the lab leaders (and the Research Center as a whole) to identify the products of their work that might be possible sources of such income. So there is another moment when Martin must perform a financially skilled manager - but at the same he must be an inspired scientist to come with things worth or capable of marketing.

And also be skilful in time management - to find the time to deal with the pile of papers by his computer or to make and receive short and polite phone calls, to respond to the requests made on him by technicians, his team members, secretaries, etc. And it should not be forgotten that Martin looks also after another team - the laboratory of Jan who resides in the United States and comes to the Research Center for two or three months in a year.

Having a successful team means also being able to attract students. And the lab leader as a manager carries with her such trivialities as the lack of lockers where students would be able to leave their stuff. At first sight maybe a triviality, but at the same an integral part of being able to create a welcoming and comfortable working environment for the team to be able to produce the desired results or to put visionary ideas in life.

Also the reconstruction of the building and moving to new facilities is something Martin must be managing to do nowadays. It includes negotiating the best of facilities with the managing director over the phone, but also planning the dates and coordinating the moving, installing new equipment or reinstalling old equipment which must be done by qualified technicians, etc.

Very soon, Martin will be moving to his new office. The office is in a separate room with no equipment for chemical experiments. Martin is very much looking forward to having a space of his own. I see this separation of experimental and administrative space as a way of attaching greater importance to the administrative or managerial duties of the lab leader. Of course, Martin can also consult expert matters and write articles here. Even though the writing of articles is not only about sharing your knowledge, letting others know about the outcomes of your work, but it has become an entrepreneurial tool, too - a way of enacting success of the team, and also a source of income for the team and possibly also authors. I shall return to the question of evaluation in the conclusion.

In the **vocational** logics, Law describes the performance as Kuhn's "skilled technical puzzle solver" (Kuhn 1997) - not radically innovatory (working within a paradigm), nevertheless creative. This person desires to know more about the world. Authority is lodged in the body of the scientific expert, and derives from qualified expertise, having little to do with organization.

I did not see Martin to perform this subjectivity very often. In the account above, it can be seen in Martin's consulting, advice and expert instructions to the postdoctoral fellow or the PhD student. However, from what I have seen, vocation is expected of postdoctoral fellows as junior scientists who were described as being "at the top of their technical and intellectual abilities". It is PhD students and postdoctoral fellows who are referred to as "hands", i.e., those who do the manual part of experimental work "at the bench", standing by the cupboards - they are the ones to whom the procedure of experimental work described in the "Setting" would apply.

And vocation is also expected of technicians who perform repetitive, rather uncreative, supportive work for the team. A technician might set up a reaction or do purification - provided it is a well known and established procedure with little surprise. Technician's duties include also going to pick supplies for the team, fetching away empty containers and the like. In the Research Center, a majority of technicians are women.

In the **visionary or charismatic** mode of ordering, authority derives from "*grace, insight and special access to the divine*" (Lawand Moser : 6), even if de-sacralized today, put differently, from access to a reality and a vision which transcends the mundane. Here, the scientist/manager is an inspirational leader, scientifically inspiring and personally attractive for men and women. It is a state of grace, given rather than achieved. For organization, it is a matter of leadership and discipleship. The logics of administration or enterprise are unimportant, even impeding, to the pursuit of vision.

If I understand the vocational logic as involving more the technique of research, I see the visionary mode of ordering as emphasizing "creativity" - as something "you have or you don't, and if you don't you cannot do anything about it". I find it difficult to judge whether Martin or a head of another team was "an inspirational leader personally attractive for men and women". From my observations, I am prone to see it more as a discursive mode, as an attitude, as a belief that there is something beyond the mundane one must

have to be a scientist, a real scientist doing pure science, free of the shackles of the administrative and entrepreneurial modes, and not having to waste time with the "handwork" of experimental science.

I would refer here to another laboratory leader who performed very well the entrepreneurial logic of the organization, e.g., he had a very good publishing record, and that is one of the two key criteria of the Research Center's assessment, and he seemed to know what it took to manage and motivate as he paid quite a high bonus for every article published as first author by a member of his research group. However, he would still strongly insist that it is only "creativity" that is the source of new knowledge and innovation.

The insistence on creativity, indeed as some divine quality, (while handling, and thus being aware of other influences on "doing science") might be a response to increasing pressures of the first two modes of ordering (administration and enterprise), that is to "external" influences of social, economic or political nature.

4. **CONCLUSIONS**

At this time, the work of Martin at the Research Center seems to be dominated by the entrepreneurial mode of ordering, particularly by managerial issues. In an earlier interview, Martin commented that 80% of his working time is taken up by administration and management, and my observation certainly confirms it. He is required to deal with clearly managerial issues throughout the day and for the bigger part of his working time. He must be able to make use of the organizational relations and of all objects and people in the network for his team to be recognized as successful.

My observations and institutional documents of the Research Center imply that successful means publishing in impacted journals, producing applicable (potentially profit-generating) results, or at the best, procuring patents for such results of scientific work.

During my observation, I heard Martin speaking about planning or having to write an article but I never saw him doing it. Of course, the writing of articles can be very easily done outside of the laboratory. What I would like to stress here is that I have seen Martin doing many other things which take up a lot of time, are important for the team's management but are in no way reflected in the team's evaluation. I am not saying that these "other things" do not contribute to the positive evaluation of his team's work. I would like to problematize the impression of presumed transparency and accountability of science through defining "clear" criteria of success (i.e., publishing and applications).

In experimental chemistry there is a lot of work at the bench, lot of manual work - and the outcomes of experiments are uncertain, some experiments may last months before putting together all the data needed and taking the experiment to a successful ("publishable" or "profitable") end. There are incomparably more direct "non-publishable" and "not-for-profit" works and actions that are part of "doing science" - organizing a major European congress adding to the prestige of the Research Center, going to conferences to meet with other scientists and establish new contacts, being at meetings, teaching at a university felt as a mission and also as a tool of recruiting young scientists, being part of an examination committee at a university, meeting with suppliers, filling out loads of conveyances, issue slips, signing off delivery notes, searching databases and other data resources, and much more of which I hope to have given an idea in the ethnographic account.

In such a system of evaluation, a greater share of science-related work is left out of official recognition. However, it is open to question and observation whether there are other channels and ways of recognizing this "extra" work. Martin's team does not have the greatest of publishing records (and no profit-generating patents), but he seems to hold a very good position within the Research Center. There was an occasion when Martin substituted for the managing director when he was ill and important foreign visitors were coming, or when Martin "bargained" over the phone for a better working space for his team at the to-be reconstructed facilities.

What I see as closely linked to the entrepreneurial attitude in the Research Center is the question of competition, secrecy and confidentiality. In a commercial enterprise there are business secrets which must be kept away from competitors. In being motivated to think about their work as about marketable products and potential sources of income for the Research Center, the laboratory, and their families (in the form of bonuses), laboratory leaders are led to thinking about their laboratories as about commercial businesses. Who are the competitors here?

If I stick to the level of the Research Center only, this type of entrepreneurial pressure might gradually change the relations between individual laboratories within the Research Center towards increased secrecy and separation of the teams and reduced willingness to share and exchange creative ideas. Or not?

It is true that, during my observation, I witnessed many occasions when scientists from different teams and different hierarchical levels helped each other more than willingly. Yet again, this seemed to me to be more in the technical (or vocational) sense than in the logic of vision or creativity. Even though, here I would make the reservation that due to my limited expert knowledge about chemistry I am not in the position to make a conclusive judgment as to what is an original idea and what is only a „trick“, a technical shortcut. And further, I have also witnessed when a laboratory leader explicitly pointed out to his team members that what they were doing and discussing as a team was confidential, and asked them as well as me not to discuss it with anyone outside the team.

The entrepreneurial mode of ordering at the Research Center can be seen in the recent restructuring. The purpose was apparently to improve the efficiency of management. It is interesting to note that the Research Center

has a very "thin" management structure. In terms of management (not institutional decision-making), there is the managing director with only laboratory leaders as his immediate subordinates. The team leaders are thus burdened by much purely managerial work which in my opinion is perceived negatively by Martin.

At the same time, a part of the restructuring were tender procedures for laboratory leaders - all former teams were cancelled, and new ones established to be headed by successful candidates. The goal of the tender procedures, I presume, was to choose the "best of scientists" (i.e., vocational and visionary modes of ordering).

On the one hand, under the new organizational structure, the laboratory leaders have a more immediate access to and control over allocated resources, on the other, much of their research time is taken up by administrative and entrepreneurial management of the team. In Martin's case, I see the ordering modes of administration and enterprise in clear tension with Martin's subjectivity as a creative puzzle solver and visionary. How these can be combined or whether they are incompatible is yet to be seen. At the moment, the laboratory leader is expected to be both a creative scientist with visions and a very efficient manager of an enterprise (his team).

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