

# FOLK

---

Journal of the Danish Ethnographic Society, vol. 44, 2002

SPECIAL ISSUE

**Cultures of Institutions, Institutions of Culture**

Editors: Nils Bubandt and Cecilie Rubow

FOLK

Journal of the Danish Ethnographic Society

Vol. 44 · 2002

# TRANSFORMING SUBJECTS INTO OBJECTIVITY – AN “ETHNOGRAPHY OF KNOWLEDGE” IN A BRAIN IMAGING LABORATORY

---

Andreas Roepstorff

The article describes processes of knowledge formation in a brain imaging laboratory and it uses concepts from Ludwik Fleck and Bronislaw Malinowski to outline basic principles of an ethnography of knowledge. The argument is structured by an analysis of the organisation of and flow through the physical building. This provides two models of the laboratory: that it transforms subjects into objectivity, and that it is a connecting node in a knowledge tradition. These findings are used to discuss whether ethnographic “facts” can be brought back to the field of inquiry.

## **Introduction: Two “Poles” in the Ethnography of Science**

Since the late 1970s, scientific institutions have emerged as important loci of ethnographic research. From the seminal contributions by Bruno Latour and Steve Woolgar (1979) and Karin Knorr-Cetina (1981), ethnographers, philosophers, and sociologists have applied versions of the fieldwork method, usually attributed to Bronislaw Malinowski (1922), to examine the workings of science. As indicated by Gonzales et al. (1995), traces of two strands of thinking can indeed be found in the contemporary anthropology of science. The less known line of thought was developed by Ludwik Fleck (1896-1961), a medical doctor and microbiologist. Before WWII he was based in the then Polish town of Lvov, where, along with his basic research in immunology, he wrote on the construction of scientific know-

ledge. Malinowski's role as a founding father of a scientific tradition is well known. Fleck's work was, however, forgotten in the aftermath of WWII, and it only indirectly entered history, philosophy and anthropology of science through Thomas Kuhn (1970).

Malinowski and Fleck, located only a few hundred kilometres and a few decades apart, may to some extent be considered products of their respective intellectual traditions (see the discussions in Cohen & Schnelle 1986; Ellen et al. 1988). Their writings are alike in that both used detailed empirical field studies to challenge well-established conceptions of science, knowledge and identity; and they both radically questioned the assumption of an absolute divide between Western science on one hand and the forms of thinking and knowing used in "primitive society" on the other (Fleck 1979; Malinowski 1954). However, they disputed it in almost opposite ways, in terms of both field locality and conceptual apparatus. Malinowski pitched his tent on the beach among the natives, while Fleck "studied up" (long before the term was coined) both in traditional scientific laboratories and, during WWII, as a prisoner in Buchenwald (Fleck 1946). Malinowski argued that natives, too, have a science, thereby granting them rationality, while Fleck problematised the reality of scientific facts by demonstrating how they came to be constructed. These two writers thereby epitomise two very different poles in the anthropological discussion of knowledge in general and of science in particular.

In their seminal comparison of Fleck and Malinowski, Gonzales et al. (1995) argued that although they share a common history, a space has been left open between these two approaches. I will in the following examine this space by combining a Malinowskian type of fieldwork in a scientific institution with a Fleck-inspired constructionist theory of knowledge.

Taking as my example the workings of a brain imaging laboratory, I shall argue that pursuing an analysis of scientific institutions by drawing on Malinowski as well as Fleck provides a set of tools that appear useful both for an ethnographic study of science and for the more general project of an anthropology of knowledge. Furthermore, combining Fleck and Malinowski in the laboratory allows one to look into some of the more puzzling aspects of the two writers: on the one hand, the role played by science in Malinowski's thought; on the other, the strange almost autopoietic closure (Maturana & Varela 1980) of thought styles in Fleck's writings. Finally, the analysis establishes novel facts about the practice of doing functional brain imaging, which may potentially be brought back into the field, thereby "closing the ethnographic circle" (Otto 1997).

## The Institute

A typical high-profile scientific institute is a condensing node in a complex, international network wherein circulate people, technologies, instruments, ideas, competences, organisms, and materials. As these elements get together in the institution, they merge and interact in producing knowledge, which is set back in circulation in the network that, ideally, is both international and trans-cultural in its scope. An institute is, however, also a concrete and specific physical space that allows for these elements to come together in particular ways.

I first entered the site of my fieldwork in London by invitation from the director of the institute to be interviewed before it was decided whether I would be granted access. The institute is located in a newly built house that blends well with the Georgian architecture on a quiet square in central London. I was admitted in through the locked front door, presented myself to the secretary in the large reception room, and signed the visitor's list. She asked me to wait a few minutes on a leather sofa with a view to a small garden before showing me to the lift that led to the director's office on the third floor. As I went upstairs, a recorded female voice announced: "level one, level two, level three..."

On the third floor resided the principal researchers, known in the lingo of the house as *principals*. During the interview I was mainly asked about my methods, my research questions and my publication strategy. I was then taken one level down to see the "fellows' floor", which was occupied by graduate students, post-graduates and visiting scientists. It had two main rooms, one facing the square, the other the back of the house. In each room a large continuous table ran along the entire wall and extended into the room where space permitted, to form a small peninsula. Along this table were small workspaces, each equipped with a powerful computer. At most of them, a researcher was sitting, and the whole room was buzzing with computers and concentration. I was introduced to some of the fellows, among them an English MD with several very famous papers on amygdala activation in processing emotional stimuli to his credit, and a young Spanish Ph.D. student investigating modal cognition and learning. Later that day I joined her in the basement laboratory where she was to perform an activation study using the MR-scanner. Having deposited keys, wallet and other potentially magnetisable items in a small locker, we sat in the control room, discussing, while the "experimental subject" in the scanner had her brain activation registered as she was exposed to various sounds and visual stimuli. That evening I left the building together with the subject. On the way out she received in cash her "travel compensation" and a t-shirt with a picture of a scanned brain, while leaving behind megabytes of data waiting to be processed.

I returned to the building some months later to conduct my fieldwork. Due to lack of space on the second floor, I was assigned a desk in the visitors' room on the fourth, topmost floor. Over the subsequent weeks, I became assimilated into the daily workings of the institute and began to participate in many of its regular activities. Each day at 4 PM those fellows who could find time for it would gather in the board room at the second floor for tea and biscuits, which were sponsored by the department and prepared by two of the fellows in monthly shifts. On Mondays, there was the *Methods Meeting* in the large seminar room on the fourth floor. This was the most important venue for discussing data analysis. The principal researcher in charge of this meeting, generally considered one of the strongest theoreticians in the brain mapping field, would usually begin by asking everyone present, one by one, whether they had any problem regarding data analysis. As the problems were identified, they were summarised on the black board and then solved and explained, one by one, during the session. Alternatively, new analytical methods or options would be presented and discussed.

One very important product of the institute is a software package, distributed free of charge via the Internet. This program allows for very efficient, flexible, standardised and relatively easy analysis of brain mapping data. It is used worldwide, and it sets one of the most important standards for how to identify significant results in the massively complex raw imaging data. By deciding which options should be available and by introducing certain topics and methods onto the widely read e-mail helpline to the program, the methods group at the institute has a strong impact on the methodology of the whole brainmapping field. They are well aware of their impact on "the brainmapping culture" and novel issues are, obviously, discussed at the *Methods Meeting* before they are released to the public.

On alternating Thursdays, meetings of either the *Memory Club* or the *Consciousness Club* were held in the more informal boardroom on the second floor. These meetings, open to interested participants both from within the house and from the neighbouring institutions, usually had a topical presentation either from a local researcher or from an invited speaker, followed by a discussion. Finally, on Fridays were held – again in the seminar room – first the *Project Presentations* at 15.15, then, an hour later, the weekly *Brain Meeting*.

All researchers in the house were expected to be present at these Friday events, and the principals usually all sat together in the front left row of chairs. At the *Project Presentations*, novel research proposals were scrutinised and criticised before they were allowed to proceed to the scanner. At the *Brain Meeting*, invited guest speakers or researchers from the house would

present completed findings. Although there were often only 15 minutes between the two meetings, they would very rarely merge since practically everybody would leave for their desk in the short interlude.

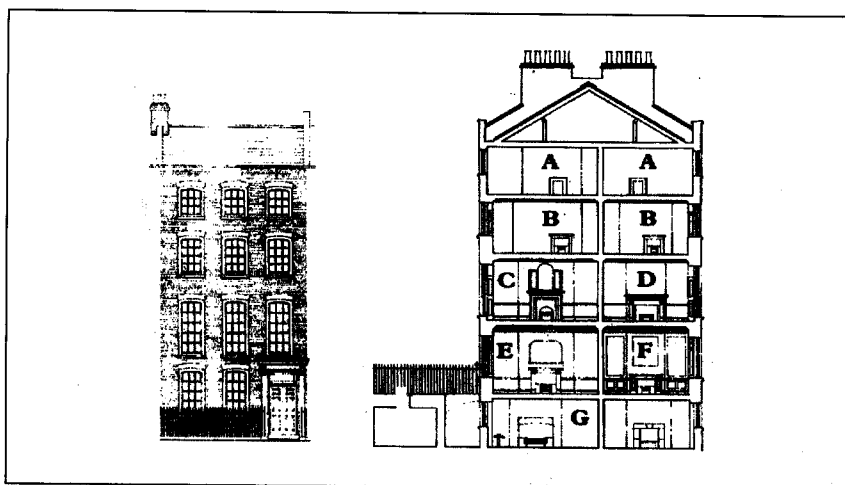
After a while I became adopted into one of the research groups, and I was asked to participate in their informal, regular Tuesday morning breakfast meetings at the principal's office. One fellow prepared the breakfast and for an hour or so, discussions would go on about everything from experimental design and new conceptual ideas to gossip, London housing problems and interesting art events.

Between these regular weekly events, I would "work with my subjects" (e.g. conduct interviews and participant observation), participate in experiments, discuss ideas and results with the other fellows and spend much of my time at my computer to analyse and write up data and prepare for talks. In other words, I was doing much the same as every other fellow in the house.

Although the product of this institute – scientific articles – was similar to that of the institutions I had visited in Denmark, many small details in the "culture" of the place, visible in the day-to-day interactions, were quite different from what I had seen before. In order to articulate these differences, I began to study in more detail the physical layout of the place. This approach was based on the standard anthropological assumption that studying the physical layout of a space, which is considered by its users and inhabitants to be straight forward and *doxic* (Bourdieu 1990), may serve to elucidate important elements structuring the daily praxis and culture.

## The Georgian House

The institute is located in a region of London known for centuries for its eminent medical practitioners and hospitals in a neighbourhood notable for its combination of old Georgian residential houses built in the eighteenth century, and various larger buildings built mainly as hospitals in diverse styles from the nineteenth century and through to the second half of the twentieth century. The house itself was specifically built as a brainimaging laboratory in the middle of the 1990s, replacing an early twentieth century edifice, *St. John's House*, that had served as a convent. To conform to urban planning policy, the new house was built with similar materials and proportions as the neighbouring Georgian houses. The imaging laboratory thus got its name, *St. John's House* and a prominent part of the façade (with a statue of St. John over the entrance symbolically baptising every visitor) from the convent, while more or less retaining the outer proportions of a large Georgian house (see Roepstorff 2000b).



*Figure 1: The spatial outlay of a typical Georgian house. Top floor: servant's bedroom or nursery (A). Second floor: family bedrooms (B). First floor: drawing room (C), dining room (D). Ground floor: front parlour (E), breakfast room (F). Basement: kitchen (G).*

Apparently, it was not only in terms of the physical design that there was a continuity between the new St. John's House and its predecessors at the same site. Rather, the functional and spatial organisation of the house appeared to be a contemporary transformation of a classical Georgian house (Figure 1, 2). The ground floor, with its large reception, paralleled the front parlour of the Georgian house. In the basement, the scanning rooms, sites of transformation as I shall shortly argue, supplanted the kitchen, another site of transformation. As one walked up the stairs, the first floor provided a combined library and boardroom and facilities for psychological and psychophysical investigations, while the second floor, where the fellows sat in large open office-spaces, was basically one large study. Thus, the first and second floor almost exactly paralleled the functions located on the second floor in the drawing room of a typical Georgian house. The third floor, where the principals had their personal, well-decorated offices, and where we had our weekly breakfast meeting, would, then, map onto the equally private bedroom level in the Georgian house. Indeed, the top floor was the only level that did not fit into this scheme. The visitors' room, the lockers for the cleaners and an AV room all mapped well onto the servant's bedrooms and nursery, but the large seminar room appeared impossible to fit into the Georgian structure. We shall return to this room later.

The parallel with a Georgian house suggests the presence of an "upstairs-downstairs" like hierarchical organisation. This there certainly was, but in a reflexive way. It was, for instance, quite usual for fellows to begin talks at

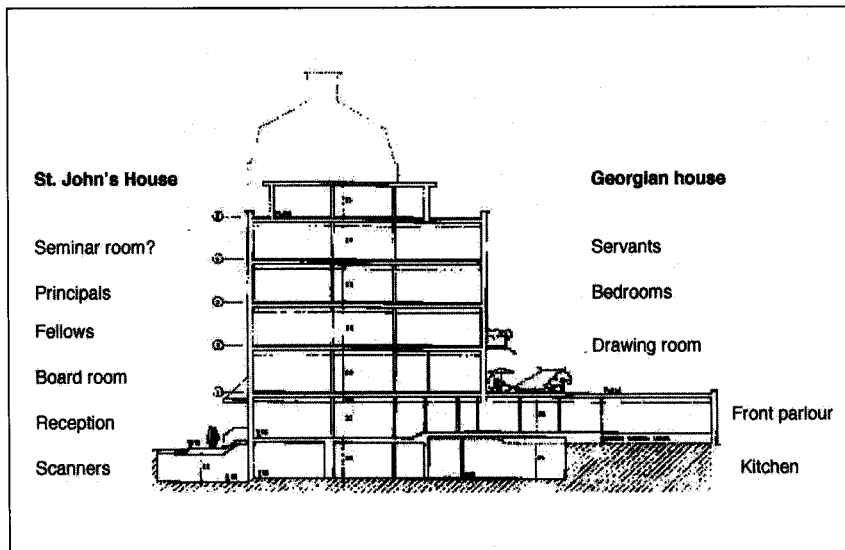


Figure 2: A Comparison of St. John's house and a typical Georgian house.

the *Brain Meeting* by making rather rude jokes about the age, intellectual capacity or temper of the principals, and these jokes were highly appreciated by the principals. During the various regular scientific meetings, the hierarchy usually gave way to a disciplined discussion where, in the main, the better argument counted and was valued, no matter who put it forward.<sup>1</sup>

In this attempt to study the practice of a scientific institution as one would any other cultural institution, i.e., through participant-observational fieldwork, I am, like my famous predecessors, inspired by the methodological principles laid out by Bronislaw Malinowski. But Malinowski's importance lies not only in the fact that he developed a method for anthropology. His work has also had paramount importance in outlining systematic comparisons between concepts and institutions wellknown to his European readers, such as science and religion, and apparently exotic and incomprehensible practices, such as magic, among the natives. This project of comparing and translating cultures is one of the most important legacies of Malinowski (Otto 1997). However, it remains to be asked what the consequences are when the object of ethnographic scrutiny becomes one of those central institutions which Malinowski – and indeed many before and after him – have taken as a somewhat undisputed starting point for comparison? This is the question to be considered in the following by way of a discussion of Malinowski's position.

## Malinowski: Science, Magic and Religion

In an important essay from 1925, *Magic, Science and Religion*, Malinowski vehemently argued against the opinion held by many of his contemporary anthropologists and philosophers, most notably Lévy-Bruhl, that

...primitive man has no sober moods at all, that he is hopelessly and completely immersed in a mystical frame of mind...incapable of dispassionate and consistent observation, devoid of the power of abstraction, hampered by a decided aversion towards reasoning...unable to benefit from experience, to construct or comprehend even the most elementary laws of nature..

Instead, Malinowski identified in “primitive society” a division of the world into two different and sharply delineated domains: that of the sacred and that of the profane. In the domain of the profane, the world of practical activities, *knowledge* reigns, and there is no fundamental qualitative difference between science and “primitive knowledge”. Rather, the latter forms the matrix from which the higher developments must have sprung (p. 34-36). This means that there is a parallel between the domain of the profane among the primitives and the domain of science in modern society, since a body of rational knowledge, which is incorporated in a tradition, a social setting and in certain activities, simply constitutes the domain of the profane (p. 87). In primitive society, however, the domain of the sacred exists in parallel with, and sharply delineated from, the rational domain of the profane. The former arises and functions in situations of emotional stress, and it is split in two: the magical and the religious. The two differ in that magic is a practical art consisting of acts that are intended to be means to a definite end, while religion is a body of self-contained acts that are in themselves the fulfilment of their purpose (p. 88).

This examination establishes a threecornered constellation of science, magic and religion (p. 89), each serving a particular function. Primitive knowledge, the forerunner of science, acquaints man with his surroundings and allows him to employ the forces of nature. It thereby “bestows on man an immense biological advantage, setting him far above all the rest of creation”. Religious faith, then, “establishes, fixes, and enhances all valuable mental attitudes, such as reverence for tradition, harmony with environment, courage and confidence in the struggle with difficulties and at the prospect of death.” Magic, finally, “supplies primitive man with a number of ready-made ritual acts and beliefs, with a definite mental and practical technique which serves to bridge over the dangerous gaps in every important pursuit and critical situation” (p. 89-90).

One of the main points in Malinowski’s article is to grant “the natives” knowledge that is similar to “science” in that it is “empirical and rational”

(p. 59). The acknowledgement that “primitive man can observe and think, and that he possesses, embodied in his language, systems of methodical though rudimentary knowledge” (p. 33) addresses a “problem of primitive knowledge [that] has been singularly neglected by anthropology” (p. 25).

This insistence makes Malinowski an important forerunner of an “ethnoscience” approach (Gonzales *et al.* 1995: 867) which attempts to take seriously the knowledge of the ethnographic subjects by taking the *a priori* stance that the knowledge of the natives is not altogether radically different from what we know as science in the West West (Roepstorff 2000a; Turnbull 2000). We do not, however, learn much about Malinowski’s own society in the essay. It is rather as if science is a self-evident given, understood in an unproblematic and straightforward way by Malinowski as well as his readers. It is this implicit understanding of science that allows us to find something analogous among “the natives”.

I have no way of confirming whether Malinowski gives a correct account of his natives, but his overall project of granting the natives an ability to think for themselves is obviously laudable. For that purpose, the Magic-Science-Religion triangle has served well to delineate an abstract conceptual field (see also Tambiah 1990). But what happens when the focus of ethnographic analysis is shifted to an institution embedding one of the apparently stable corners in this mental topology? Can the distinction be upheld when the object of the inquiry is a scientific institute? I shall in the following try to argue that to account for the actual activities taking place within St. John’s House, an eminently modern and Western institution, all three corners of Malinowski’s magic-science-religion constellation appear indeed to be active.

### The House as a Site of Transformation

When I was asked to give a *Brain Meeting* in order to present the results of my fieldwork at the institute, I decided to structure it around the Georgian house parallel in an attempt to attain that combination of ironic lightness and serious work which was the ideal of these events. To examine the limits of the model, I discussed it beforehand with the director of the institute, who had been responsible for designing and planning the department from its inception.

*AR: If you look at the physical outlay of the building, you can see it very much as a modern paraphrase of a Georgian town house...*

D: That’s pretty much how I conceived the structure of the building, of course: that the subjects would come in, they (1) would go down, but I thought of it much more in terms of the data. They (2) will go down for their primary [examination], and the data will flow up for the second [floor]. They (3) will go up to the first [floor] for

their auxiliary investigations. Then there will be an iteration between the second and the third floor, and then they (4) will come out into the open through the seminar room.

AR: *Because the seminar room is something that doesn't really fit...*

D: *Piuh, it goes up!*

AR: So that was the logic behind it?

D: That was the logic behind it!

(transcript from taped interview, my additions in squared brackets)

My initial analysis of the building, which suggested that the structure emulated a Georgian house, was not entirely refuted by “the natives”.<sup>2</sup> However, once the narrative is closely followed, a novel aspect is forced into the analysis. Two main narrative actants occur as grammatical subjects: the [experimental] subjects and the data. In four sentences, “they” is used to refer to one of these actants, but it is not always entirely clear which. The first “they” (“they” (1)) clearly refers to the experimental subjects entering the house. Grammatically speaking, “they” (2) refers to the data. However, the presence of “the data” in the sub-clause suggests that, semantically, “they” (2) refers to the experimental subjects. This interpretation allows “they” (3) to refer grammatically to the experimental subjects as well, and that is clearly the semantic reference. This entails that “they” (4) grammatically refer to the experimental subjects, but this interpretation makes no sense at all. It has to be “the data” that “come out into the open through the seminar room”.

This “logic” of the place appears, on closer examination, quite magical. Something (experimental subjects) enters the building, “they” go up, and “they” (the data) exit through the top. This suggests that the house is described – and can indeed be conceived of – as a site of transformation where subjects are turned into data.

We don't hear anything about the fate of the physical subjects, but what happens to the data as they exit the house? On one of the walls in the gangway on the third floor, two display cases were hanging side by side, containing reprints of articles from the institute. In one of them were articles published in *Nature*, in the other articles from *Science*. A vase with dried flowers stood underneath.

As every natural scientist knows, getting an article into either *Nature* or *Science*, understood as concrete journals, is probably the best that can happen to one's data. It is a sign that one's results have been found to be objective, novel, reliable and worthy of distribution in the general scientific community. The ideal destiny of a subject entering the house under the baptising hand of St. John is therefore to be transformed into an objectivity, worthy of being inscribed into *Science* and *Nature* – this time understood not only as concrete journals, but also as those metaphysical entities that

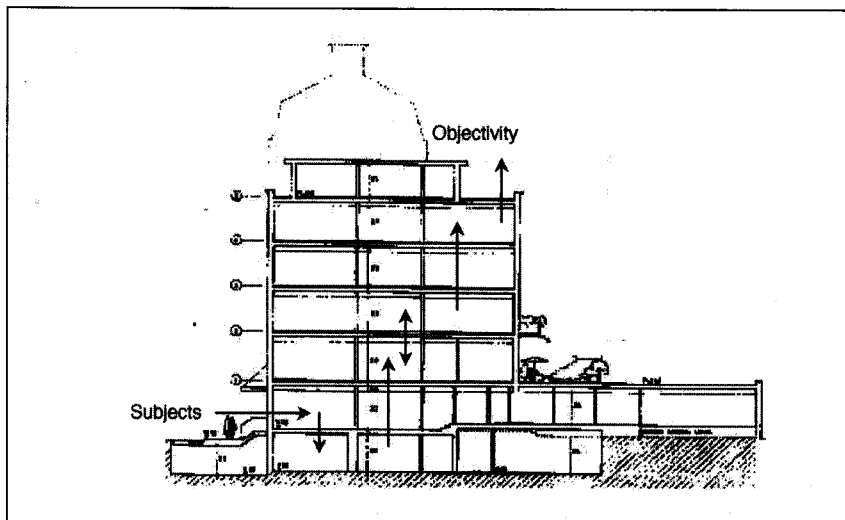


Figure 3: St. John's house as a site of transformation.

their names designate. In same manner as the former St. John's house that stood on the same site, the present building appears thereby to be connecting a very concrete and mundane world with another world "up there": a transcendental world of Science and Nature. Using a metaphor from Bruno Latour (1987), the house may therefore be described as a *black box*, where subjects enter at the bottom and objectivity exits at the top (Figure 3).

This examination of the native logic of the function of the house is hardly compatible with Malinowski's understanding of Science as being merely located in a profane world of practicalities. It would, rather, place the function of the house in the domain of the sacred, on a par with religion.

An interpretation of Science along these lines has recently been suggested by the sociologist of science Steve Fuller (1997). Although his analysis makes sense at one descriptive level, it can be argued that it fails to account for the actual process of knowledge production, both within the scientific institution, and in the relationship between the institution and the outside world (Roepstorff 2001c). Replacing a conception of science as being opposed to religion with a conception of science *as* religion also appears to disregard the third corner in Malinowski's useful constellation: magic.

To examine this issue further it seems important to follow in more detail the actual transformational processes within the institution, which allows to establish something as knowledge. As "science" is more of a stable anchor point than an analytical issue for Malinowski, I will turn to our second Pole, Ludwik Fleck, for a conceptual toolbox.

## Ludwik Fleck and the Construction of Facts

In the short treatise from 1935, *Genesis and Development of a Scientific Fact*, Ludwik Fleck presented an analysis of the history of knowledge about syphilis from medieval times to the Wasserman serological test for syphilis, one of most advanced biotechnologies at the time. Although the narrative is in itself interesting, it is used mainly as an illustrative example of the workings of science. And the story is very different from Malinowski's pragmatic and secular "matter of fact" description.

Like Malinowski, Fleck vehemently opposed the anthropologists and sociologists of knowledge of his time, most notably Durkheim, Lévy-Bruhl, and Gumpłowicz, but the focus of his contention was almost orthogonal. Malinowski claimed that these researchers took away rationality from "the primitives" thereby granting them a life emerged in magic and rituals. Fleck's argument was that these writers "commit a characteristic error, [t]hey exhibit an excessive respect, bordering on pious reverence, for scientific facts" (Fleck 1979: 47). They do so by claiming that "we", that is the Western scientific culture,

"are in possession of 'correct thinking' and correct observation, and therefore what we declare to be true *is ipso facto true*. What others, such as the primitives, the old people, the mentally ill or the children declare to be true *seems to be true only to them*" (op. cit. p. 50).

Thereby, Fleck suggested, epistemologists trained in the natural sciences, like those of the Vienna Circle, and epistemologists trained in the humanities, like Durkheim and Lévy-Bruhl, commit similar but structurally opposite mistakes. The first group claims that human thinking, as an ideal, is an absolute, fixed point while empirical facts are relative, while the latter group claims that scientific facts are an absolute, fixed point while human thought is relative. Consequently, "both parties relegate that which is fixed to the region with which they are unfamiliar" (op. cit.).

As an alternative, Fleck argued, there is a need for an epistemology without a fundamentally fixed point, since both thinking and facts are changeable. Changes in thinking manifest themselves in changed facts, while fundamentally new facts can only be discovered through new thinking (op. cit.). An outline of this alternative is the main theme of Fleck's work. Briefly, he proposed that by shifting the focus from facts "in themselves" and thinking "in itself" to the processes in which thinking is embedded, that is, the processes whereby facts become facts, one may formulate an alternative and, ultimately, more realistic model.

Fleck's analytical stance and theoretical position was arguably the first attempt at a thoroughly constructionist account of knowledge formation

*avant la lettre* (Hacking 1999: 60). Even in the aftermath of the recent science wars, it appears so well thought out that it merits further attention.

The act of cognition is, Fleck claimed, not merely the occurrence of a link between a knowing subject and an object to be known. Mediating between these two entities is that which is already known, since knowledge always becomes inscribed in a network of already established facts that extends into the social, beyond the cognizing individual. This intermediary between subject and object and between thinking and facts can, Fleck proposed, be analysed and understood through two terms: the “thought collective” and the “thought style”. The former is taken to imply any community of people mutually exchanging ideas or maintaining intellectual interaction (Fleck 1979: 54-55). This community is always a carrier of a particular “thought style”: “characterized by common features in the problems of interest to a thought collective, by the judgement which the thought collective considers evident, and by the methods which it applies as a means of cognition” (Fleck 1979: 98).

The existence of thought collectives equipped with particular thought styles may carry associations similar to Durkheim’s mysterious superorganic group mind. However, this is probably not a correct parallel (Douglas 1986: 16). For Fleck, himself a practicing scientist, the existence of relatively well-defined groups of people exchanging ideas, and the identification of particular, “stylistic” properties of their thinking was simply an empirical observation; as was the claim that both the styles and the facts change over time. The paramount problem was rather, given this observation, to understand what a “fact” is.

Fleck’s solution is quite original. Facts never exist in isolation, they are part of an interlaced pattern that forms an entire corpus of knowledge. A fact is a node in this network, that, at the level of cognition, is “a sign of resistance to the thought collective”. A fact is marking a resistance, which opposes the potential arbitrariness of thinking. It restrains and constrains thought while, simultaneously, as a node in the web of knowledge, it becomes a relatively fixed point that not only reconfigures the original network, but also becomes a starting point for new lines (op. cit. p. 78).<sup>3</sup> When a branch of knowledge becomes increasingly developed, the differences in opinion diminish; it is as if when the number of nodes in the network increases, free space is reduced, as if more and more resistance arises, and the free unfolding of thought is restricted (op. cit. p. 83). This claim implies that in a well-developed system of knowledge, every new fact ever so slightly changes all earlier facts, and every new discovery becomes a recreation of the world of a thought collective. This interlaced pattern bestows perseverance on the world of facts and awakes the feeling of a fixed reality, of an independent existence of the universe (op. cit. p. 102).

It is important that in Fleck's dynamic epistemology, facts and styles are intrinsically linked, and therefore every fact is connected to a particular style. The deeper one enters a particular field and the denser the texture of facts, the stronger also becomes the connection to a particular "thought style". This does not mean that "truth" becomes a convention. Rather, truth is, in historical perspective, an event in the history of thought, and in its contemporary context it is a stylized "thought constraint" (p. 100-102). With a change in "thought style", so-called passive connections, which in the previous context appeared self-evident and "real", may suddenly be made active, open for examination and in need of explanation.

Although he mainly described scientific processes, Fleck intended that his approach could be extended as a general comparative epistemology that could examine any "thought collective", since they all have more or less the same general structure. Around every thought formation, no matter whether it is a dogma of faith, a scientific idea or an artistic thought, two circles of thought collective members appear: a small esoteric and a larger exoteric one. A thought collective consists of several intersecting circles of this kind, and each individual belongs to several exoteric and fewer, if any, esoteric circles. There are hierarchies of initiation and connections running between the levels and also across the circles. The exoteric circle has thereby no direct link with the thought formation, the relation is mediated through the esoteric circle. This means that for most of the members of the thought collective, acceptance of the formations of the thought style is based on confidence in the initiated.

### **An Esoteric Centre**

Following Fleck's analysis, we may suggest that St. John's house is the pivotal centre for an esoteric circle of knowledge within the brain mapping thought collective. The house is the origin of what is probably the most important analytical framework in brain mapping community. Talented Ph.D. students and post-doctoral fellows from all over the developed world come here to be initiated into the art of functional brain mapping. It is an official policy of the department that fellows should stay for a minimum of 2 years and leave the place after 3-4 years. After having finished their training at St. John's House, they are in high demand in the imaging community because their experience enables them to set up and implement the analytical framework, which makes possible the interpretation of data. Hence, several former fellows have set up small satellite institutions that use similar tools and concepts. Many return to St. John's house at least once a year to teach at an introductory course to the software package, which over the years has been attended by hundreds of people from the brain imaging community.

Not everybody in the brainmapping field agrees with the particular framework of St. John's House and there are competing analytical tools,<sup>4</sup> particularly in the US and Canada, each surrounded by an esoteric circle of followers. Several researchers located in the exoteric circle relative to St. John's house complain that the framework is being overtly marketed, and informal, gossipy stories circulate about how articles made with alternative analytical frameworks have had problems getting through a peer-review process.<sup>5</sup>

Fleck's considerations on "thought collectives" describe very well both the brainmapping field in general and the location of St. John's house within it in particular. But is it possible to identify also in the field a parallel to the "thought style"? To address this question, I will follow in closer detail the practice of the institute, guided by the "native exposition" of the logic of the place.

### **Transforming Subjects into Objectivity**

They will go down for their primary...

When a "subject", who is going to take part in an experiment, enters St. John's house, he or she is met by the researcher in the reception room for a short, prior briefing. Typically, this conversation touches on a short introduction to the experimental paradigm, the kind of task that the subject needs to perform, and some information about the potential risks of the examination due to the exposure to radioactive radiation in the case of a PET-scanning, and strong magnetic fields in the case of MR-scanning. Finally, the subject is invited to sign a form of informed consent for participation in the experiment. The subject and the experimenter then go to the basement and prepare for the scanning. For PET-scans, the subject has a needle inserted into a vein in the arm to allow for intravenous injections of radioactive water. Prior to the MR-scan, the subject must remove all metal and magnetisable items such as watches and credit cards. After the subject has been transferred to the scanner, a short test stimulus is usually presented before the actual scanning begins. During the scanning session, the subject is exposed to specific stimuli requiring a response in accordance with the task. In the mean time, the scanner registers physical parameters, such as density of radioactivity or the contrast between oxygenated and de-oxygenated haemoglobin. These recordings provide the basis for calculating physiological parameters, which are considered to be related to the neural activity during the actual task. The data are stored as large time series carrying information about the approximate time and place of the activity in the brain.

...the data will flow up for the second...

After the scanning session is completed, many megabytes of data are transferred to a temporary buffer on one of the large computer servers. Here they are accessible to the fellow from his computer on the second floor.

When all subjects in the experiment have been scanned, the real analysis begins. The actual calculations are very demanding, mainly because of the enormous amounts of data in the files. However, due to the user-interface of the software program, these processes are almost entirely black-boxed, that is, most of the work of the analysis (including all the methodological assumptions, the elegant analytical details, and the actual computation) is done automatically. In a standard analysis, the researcher's main task is to indicate where the data are located, choose between various parameters and options and specify which model should be used to fit the data. The computer, then, grinds the data for a while to produce a figure and a table. They indicate those volumes of the brain, where the synchrony between a model of the brain activity assumed to be evoked by the task and the actual measurements exceeds a chosen statistical threshold. The results are shown as a table of brain co-ordinates with concurrent statistical significance, which may be graphically displayed as so-called "blobs" (patterns of dots and areas superimposed on a standardised representation of a brain).

...they will go up to the first for their auxiliary investigations...

After the scan, the subject may undertake further psychological and psychophysical examinations. Whether this is needed depends on the actual experiment, and many paradigms do not specify any investigation outside the scanner.

...there will be an iteration between the second and the third floor...

Once the scans have been transformed into significant brain activations, schematically represented on a stylised brain, the interpretation of data and the writing of articles may begin. The manuscript must contain a discussion about the relationship between the experimental task and the significant activations identified by the experiment. Very importantly, these findings have to be placed in the context of previous literature, that is, the findings and theories that already exist in the field. This writing up of the results is usually done as a collaboration between the fellows involved and the principals.

...they will come out into the open...

When the story has been written up, the data are ready to leave the house. They enter the wider world through talks at scientific meetings, posters at conferences and ultimately, if they are to have any lasting impact, as articles in specialised peer-reviewed journals.

## Subjects and Objects, Subjectivity and Objectivity

Based on a “native expert” analysis, I have suggested that the flow through the house could be described as a transformation of subjects into objectivity. It appears that this process can be broken down into two different stages. The first is a transformation of the experimental subject into a mathematical object, a data matrix. This takes place in the scanner. The data matrix contains a time series of datapoints that, if minds, brains and machines have been co-ordinated properly, correspond to the stimulus imposed on that person whose brain was in the scanner.<sup>6</sup>

The second transformation involves using the computer software to transform the mathematical object arising from the scanning of each subject into an objectively valid “fact” about the brain activation resulting from the experiment. This is the one that is discussed every Monday at *Methods Meeting*, and is embedded in the software package.

The analysis runs through a number of separate steps, each of considerable mathematical complexity, but conceptually quite understandable. Although one is instructed to lie still while in the scanner, a subject’s head always moves a bit, and the first step in the data processing is to correct for these motions. This is done mathematically by identifying automatically how the outline of the brain recorded during each scan should be twisted and turned so as to match up with the first scan. Once this mathematical transformation has been determined, the program simply *realigns* all the data recorded to one position of the head.

The next problem is that subjects’ brains do not look the same, but in order to look for activities across subjects, one must place them in a comparable framework of standardised co-ordinates. This task is accomplished by mathematically turning, twisting and stretching the presentation of each brain so that the result of this *normalisation* fits, in certain fixed points, with the representation of a standard, canonical brain.

These two processes have transformed the representations of each individual scan in each individual brain into one common co-ordinate system. This makes it possible, mathematically, to treat the data as if they were all recorded from the same generalised brain. After a few extra manipulations, the data are tested against a particular model; in the simplest case, this could be two alternating activation conditions. This statistical test, which uses well-established and relatively simple models on a massive data set, is performed for all volumes of the normalised brain data. The outcome is an identification of the brain volumes, where the time-dependent pattern of activity significantly fits the model. They can then be reported as those brain regions that are activated by the experimental paradigm with a certain level of statistical significance. This is the

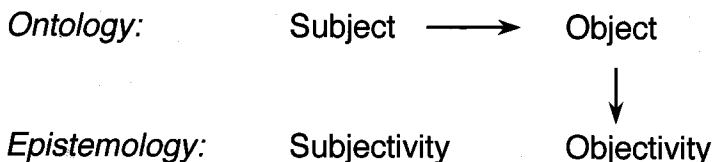


Figure 4: The function of the house as one possible transformation within two binary oppositions, an ontological and an epistemological.

objective output of the software programme, which may be written into an article and set into circulation.

In a somewhat simplified manner, I have split the transformative process into two stages (Figure 4). The first takes part in the scanner, where subjects enter and data objects flow out. It appears to involve an ontological distinction: subjects and objects are both entities in the world, and the two concepts form a binary opposition pair. The second transformation, objects → objectivity, moves from an ontological to an epistemological level: objectivity concerns knowledge about this world. At this level, “objectivity” forms a structurally similar pair with “subjectivity”. These concepts may metaphorically be described as knowledge from a particular point of view, that of a subject, and knowledge that only relates to the object without a knowing subject (Popper 1972).

From a structural semantic point of view, the “subject → object → objectivity” scheme appears to presuppose “subjectivity”. However, that concept seems completely absent from the transformation. This does not mean that “subjectivity” is not talked about among the people in St. John’s House, but the “subjectivity-objectivity” opposition is embedded in a value system where “objectivity” is positive and “subjectivity” is negative. The term is therefore used mainly in a derogatory sense, for instance to describe the works of other researchers, such as the visiting anthropologist, who do not make certain that their findings are general and universal. So “subjectivity” looms around, but as a problem rather than as a useful resource or potential.

We should now be equipped to touch on the question of “style”. Can we say, following Fleck (1979: 98), that the facts generated are characterised by

common features in the problems of interest, by the judgement which the thought collective considers evident, and by the methods which it applies as a means of cognition... accompanied by a technical and literary style characteristic of the given system of knowledge?

Obviously, there is agreement as to the means of cognition: it is considered self-evident in the field that brain scanners are perfectly valid ways to obtain knowledge not only about brain activity, but also about cognitive processes. This issue has been a matter of dispute within cognitive psychology, and brain mappers tell, full of contempt, about those traditional psychologists who still think that old fashioned psychophysics such as “reaction time” is indeed a more direct access to cognitive processes than are brain scanners. As described above, the actual judgements and the whole analytical process involved in establishing a claim as valid involves a highly complicated set of mathematical manipulations. The normalisation across subjects means that any individual differences are evened out. This is the only way to generate a dataset sufficiently homogenous between the different scans and subjects to search for significant differences within the canonical standardised brain. These manipulations lend to the facts a classically scientific and technical style, and the data are, furthermore, ideally presented in a literary style characteristic of a scientific paper – a short, precise presentation focusing on the generality, factuality and reproducibility of the finding.

At first, it may be more difficult to identify common features in the problems of interest. The field has made possible a whole range of investigations, from the neural substrate of love and musical apprehension to language acquisition, logic and morality. There appears to be hardly any human experience that can not be made subject to a brain scanning analysis. However, as discussed in more details elsewhere elsewhere (Jack & Roepstorff 2002), there is one general feature that seems to characterise the present “style of brain imaging”. Although every experience can be turned into an experiment, there are very few, if any, ways within the established thought style to take seriously the actual experiences of the subjects during the experiment. This problem is, of course, closely related to the valorisation of the epistemic distinction in terms of which “objectivity is good”, “subjectivity is bad”.

This outline of some of the main traits characterising cognitive brain mapping has indicated how the production of facts is both made possible – and constrained – by a highly particular set of stylistic properties. In other words, the methods and the results are intricately linked. Put this way, such a statement would meet little resistance within the brain-mapping field. On the contrary, the discussions in the *Methods Meeting* – for example, on the value of a certain analytical framework or a certain implementation in a computer program – are important *precisely* because the methods and the results are so closely interlinked. This connection can, to some extent, be obscured by the fact that so much of the actual analytical process is invisible because it is black-boxed (Latour 1987) in the computer software. The

naïve observer may consequently think that the well-known images of brains with brightly coloured spots are like a photography of the brain.<sup>7</sup> It is generally acknowledged in the field that many – if not most – researchers are unable to reconstruct fully what goes on inside that black box. Rather, just like in Malinowski's analysis of magic stated above, the software "supplies [the researcher] with a number of ready-made ritual acts and beliefs, with a definite mental and practical technique which serves to bridge over the dangerous gaps in [an] important pursuit and critical situation" (Malinowski 1954: 90, "primitive" man replaced by "researcher" and "every" replaced by "an"). Just like magic, the experimental procedures allow for a radical substantive transformation: subjects into objects into objectivity, and the crucial step from ontology to epistemology – from objects to objectivity – is undertaken and authorised by the software package.

It does not necessarily discredit this operation if a researcher does not understand it in every aspect. Indeed, one of the advantages of being inside an esoteric circle is that although one may not be able to understand all that goes on, there is usually access to someone who does. This personifies the relation of trust inherent in the relationship to the thought formation.

### The House as a Connecting Node for a Knowledge Tradition

I used the "native model" of the house to structure the exposition of that chain of transformations (Latour 1999), which carried knowledge as it ran vertically through the house, but there were apparently some lacunae in that model. Firstly, it did not see the subjects safely out of the house again. Secondly, the apparently seamless transformation from subjects to objectivity appeared, on close examination, to involve transformative steps with almost magical properties. From an ethnographic perspective, the actual use of the physical structure suggests a slightly modified understanding of the workings of the house. The cue to this analysis may be found on Friday afternoons at the fourth floor in the seminar room that apparently did not fit any of the models. The seminar room was not only the site for presenting results at the *Brain Meeting*. Before any experiment could take place, it was here that the project was made public at the *Project Presentation*. On these two occasions – usually separated by several months and 15 minutes – ideas and results went public in the very same room where the new analytical method had been introduced and validated for a more limited forum at the *Methods Meetings*. Rather than having one chain of transformations going up through the house, *piuh*, the ethnographer seems to find in the house a meeting between two different chains. One is horizontal such that a person enters, accepts to take part in the experiment and follow the instructions,

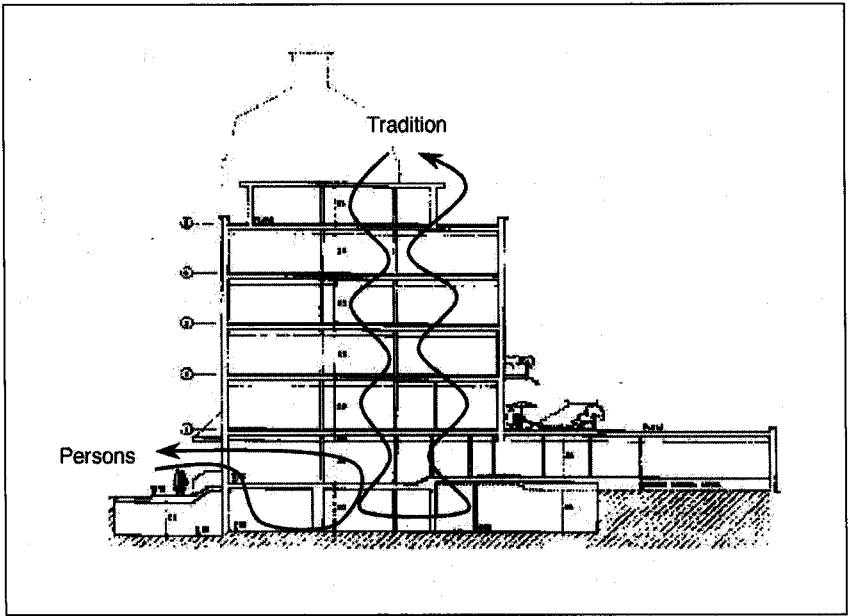


Figure 5: The house as a connecting node.

goes to the scanner, leaves an inscription of brain activity, and departs again, marginally richer. The other is vertical, wherein something descends through the house from the fourth floor, meets up with the data from the scanner in the basement, and ascends again to exit the house, ideally through the top.

What is it, then, that moves horizontally through the house? It is not the thought collective since that is an association of concrete individuals. It is not the thought style, since that exists only as a modality, carried and created by the thought collective. At the *Brain Meeting* as well as at the *Project Presentations* and the *Methods Meeting*, what is being evoked is a combination of continuity and innovation, of already established knowledge brought over from those with whom one identifies, and new knowledge to be brought over to those from whom one craves recognition. The Latinate word for “bringing over” is *tradition*, and as has been shown in a range of ethnographic studies, tradition is precisely located at the nexus between continuity and invention (Otto & Pedersen 2000), and it serves as a conduit of knowledge and simultaneously of identity identity (Roepstorff 2001b; Roepstorff & Simoniukstyte forthcoming). This becomes, then, the anthropologist’s cosmological interpretation of the St. John’s house (Figure 5). It is a node connecting two different flows: a tradition moving vertically

down and up the house, and persons playing the part of an experimental subject in a highly stylised and tightly scripted play, moving horizontally in and out of the house.

### Closing the Ethnographic Circle

This cultural analysis of a brain imaging institution expands on the internal representation in two ways. The general and somewhat trivial point is to stress how much “culture” it takes to produce facts worthy of being inscribed in Nature. The specific (and perhaps more interesting) finding is that the central transformation of the brain imaging laboratory – from subjects to objectivity – presupposes an exchange between very concrete persons: the experimenter and the experimental subject. As discussed in more details elsewhere (Jack & Roepstorff 2002; Roepstorff 2001a), this “fact”, which to the visiting ethnographer may be obvious, touches on an issue hardly discussed in the cognitive brain imaging literature. Two differing analytical observations could be made from this. One possibility is to see this as a clear case of how thought styles and thought collectives close around themselves and enforce a thought constraint on their members. One could clearly find support for this autopoietic closure (Maturana & Varela 1980) in a cursory reading of Fleck, and it would be on a par with much contemporary social theory (Luhmann 1995). This analysis seems, however, at odds with Malinowski’s basic position. The very idea of anthropology that he promoted was that it is possible to communicate across differences and, through comparative investigations, establish meta-stable categories that inform both “the primitives” and one’s own society. In an updated version of this understanding (Otto 1997; Roepstorff 2001b), the goal of ethnography is not only to describe these cultural differences and translate them to an audience “back home” – be that a geographical or an intellectual one. Arguably, the most interesting potential, both as an analytical strategy and as a method of engagement, lies in “closing the ethnographic circle” (op. cit.) by bringing the ethnographic “facts” back into the field again.

Drawing on Fleck’s understanding of facts, this means that one should attempt to establish in the field novel signs of resistance, which must be taken into consideration. In this case, framing the cognitive experiment by an ethnographic analysis appears to allow for and perhaps even enforce a shift in perspective. The story of subjects and objects – equipped with negatively valued subjectivity and positively valued objectivity – becomes challenged by a narrative of interacting persons. This creates novel resistance to the interpretation of classic psychophysical experiments (Roepstorff

2001a), and it allows simultaneously to foreground the question of how people may share representations, “scripts”, and experiences (Frith 2002; Jack & Roepstorff 2002). For interesting reasons, this issue, which has hardly been examined in cognitive science, is currently receiving much more attention.

As argued above, “science” in contemporary society appears to be straddling Malinowski’s sacred-profane distinction. In a society where “epistemic cultures” (Knorr-Cetina 1999), i.e. knowledge traditions grouped under the loose label of science, provides a framework that is both practical and conceptual – both instrumental here-and-now and projecting into an unknown future – the work of science is not just about finding practical means and ends. Science is also about constructing a world that is both conceptual and very real. This is Fleck’s strong insight: facts are not simple reflections of reality. Through their interconnections they set up a particular world, not an objective independent “from the outside” world, but a world as it seems from the inside, accessible as reverberations through that mesh of active and passive connections that a body of knowledge consists of. This places scientific knowledge right in the middle of that triangle, which Malinowski delineated magic-science-religion.

Some analyses suggest (Dumit 1997; Roepstorff 1999a) that discussions on brains are currently a major place for defining categories like morality, personhood and love. It is therefore not a trivial issue how the brain is conceptualised, and how facts are made inside the esoteric circles of neuroscience. I have in this paper tried to establish the case that an ethnography focusing on knowledge (Barth 2002; Roepstorff 2001b) may be a tool for a description of scientific institutions and processes that has the potential to “close the ethnographic circle” and engage in the process of knowledge formation in the field. For that project, Fleck’s tools descriptions of knowledge processes, and Malinowski’s insistence that commensurability is possible, may not be the worst companions.

#### NOTES

1. The Georgian house analogue also serves well to point out differences to the brain imaging laboratories that I visited in Denmark as part of my fieldwork. Their spatial outlay were better matched with a one-level Danish “parcelhus” from the 1970s, with activities circulating around the combined kitchen-family room. Ideologically, they were dominated by ideas of a non-hierarchical structure, although not every foreign scientist experienced it in that way. See (Roepstorff 2001b) for a discussion.
2. In the first draft of this article, this exposition was seen as “emic” since it represents the point of view of the “natives” and is expressed in their tongue. Consequently, the reconstruction, from the anthropologists’ perspective, would be an etic model, which, in parallel with the original import of the -mic, -tic opposition from linguistics (Hjelmslev 1970), would highlight the structural aspect. This usage would, however, as pointed out by one of the editors of this volume and by Tim Ingold at the author’s Ph.D. defence, run against another usage in anthropology which usually is congruent with the first one, where “emic” is experience near and “etic” is experience far and abstract. There are perhaps two lessons to draw from this. The boring one is that the emic/etic distinction in anthropology is a highly problematic one (D’Andrade 1995). The perhaps more interesting one is that doing field-

work in scientific laboratories has a tendency to render well-established anthropological dichotomies highly problematic (Roepstorff 1999b).

3. There are interesting similarities between Fleck's idea and the network-like descriptions of knowledge-practice-power configurations found in contemporary anthropological theory (Latour 1996; Strathern 1996).
4. The differences between the various software packages are partly a matter of divergent opinions about what statistical methods should be used in validating and identifying significant activations of the brain. They also reflect different choices between user-friendliness on one hand, which implies that the entire analytical process is more or less black-boxed, and individual control over the data analysis, which means that the researcher consciously has to consider and understand more analytical options.
5. Conversely, European researchers claim that some American journals will not accept the European analytical models.
6. In St. John's house this transformation takes place in the cellar which in a Georgian house would contain the kitchen. In classical structural analysis, the kitchen is a paradigmatic site of transformation where "the raw", that is Nature, is turned into "the cooked", that is Culture (Lévi-Strauss 1970). If anything, the transformation in the scanner is the opposite one: individual persons, carriers of culture, enter and out come data, hopefully ready to be inscribed in Nature. Those initial data coming from the scanner are, furthermore, in the lingo of the field called "raw data". One may later metaphorically "cook" them, but this is an expression usually employed to describe data that have been processed a little bit too much to be really trustworthy.
7. The seductive power of the beautiful pictures is generally accepted in the field. Part of the initiation is to be told – again and again – not to perceive the pictures as realistic photographs but rather regard them as highly mediated representations, maybe even cartoons – to use a metaphor of Per Roland, one of the pioneering brain mappers in Scandinavia. In semiotic terms, one may say that the picture looks like an iconic representation of brain activity. It is, however, a complicated symbol that, hopefully, in a complex but indexical way is related to a set of processes in some general brain during highly particular constraints.

#### LITERATURE

- Allen, Stephen, Hollinshead, Liz & Wilkinson, Sue  
1998 *Using Houses and Homes*. London: English Heritage.
- Barth, Fredrik  
2002 An Anthropology of Knowledge. *Current Anthropology* 43: 1-18.
- Bourdieu, Pierre  
1992 *The Logic of Practice*. Abingdon, UK: Polity Press.
- Cohen, Robert S. & Schnelle, Thomas (eds)  
1986 *Cognition and Fact. Materials on Ludwik Fleck*. Dordrecht: D. Reidel Publishing Company.
- D'Andrade, Roy  
1995 *The Development of Cognitive Anthropology*. Cambridge: Cambridge University Press.
- Douglas, Mary  
1986 *How Institutions Think*. Syracuse, N.Y.: Syracuse University Press.
- Dumit, Joseph  
1997 A Digital Image of the Category of the Person. Pet Scanning and Objective Self-Fashioning. In: G.L. Downey & J. Dumit (eds): *Cyborgs & Citadels: Anthropological Interventions in Emerging Sciences and Technologies*. Santa Fe, New Mexico: School of American Research Press. Pp: 83-102.
- Ellen, Roy, Gellner, Ernest, Kubica, Grazyna & Mucha, Jamisz (eds)  
1988 *Malinowski between Two Worlds. The Polish Roots of an Anthropological Tradition*. Cambridge: Cambridge University Press.
- Fleck, Ludwik  
1946 Problems of the Science of Science. In: R.S. Cohen & T. Schnelle (eds): *Cognition and Fact. Materials on Ludwik Fleck*. Dordrecht: D. Reidel Publishing Company. Pp: 113-128  
1979 *Genesis and Development of a Scientific Fact*. Chicago: Chicago University Press.
- Frith, Chris D  
2002 How Can We Share Experiences? *Trends in Cognitive Sciences* 6 (9): 374.
- Fuller, Steve  
1997 *Science*. Minneapolis: University of Minnesota Press.

- Gonzales, Roberto J., Nader, Laura & Ou, C. Jay  
 1995 Between Two Poles: Bronislaw Malinowski, Ludwik Fleck, and the Anthropology of Science. *Current Anthropology* 36 (5): 866-869.
- Hjelmslev, Louis  
 1970 *Language, an Introduction*. Madison: University of Michigan Press.
- Jack, Anthony I. & Roepstorff, Andreas  
 2002 Introspection and Cognitive Brain Mapping: From Stimulus-Response to Script-Report. *Trends in Cognitive Sciences* 6 (8): 333-339.
- Knorr-Cetina, Karin  
 1981 *The Manufacture of Knowledge: An Essay on the Constructivist and Contextual Nature of Science*. Oxford: Pergamon Press.  
 1999 *Epistemic Cultures: How the Sciences Make Knowledge*. Cambridge, Mass.: Harvard University Press.
- Kuhn, T.  
 1970 *The Structure of Scientific Revolutions*. Chicago: Chicago University Press.
- Latour, Bruno  
 1987 *Science in Action. How to Follow Scientists and Engineers through Society*. Milton Keynes: Open University Press.  
 1996 On Actor-Network Theory. A Few Clarifications. *Soziale Welt* 47 (4): 369-381.  
 1999 *Pandora's Hope : Essays on the Reality of Science Studies*. Cambridge, Mass.: Harvard University Press.
- Latour, Bruno & Steve Woolgar  
 1979 *Laboratory Life: The Social Construction of Scientific Facts*. Beverly Hills: Sage.
- Lévi-Strauss, Claude  
 1970 *The Raw and the Cooked*. New York: Harper & Row.
- Luhmann, Niklas  
 1995 *Social Systems*. Stanford: Stanford University Press.
- Malinowski, Bronislaw  
 1922 *Argonauts of the Western Pacific*. London: Routledge and Kegan Paul.  
 1954 *Magic, Science and Religion*. New York: Double Day.
- Maturana, Humberto R. & Francisco Varela  
 1980 *Autopoiesis and Cognition: The Realization of the Living*. Dordrecht: D. Reidel Publishing Company.
- Otto, Ton  
 1997 Social Practice and the Ethnographic Circle: Rethinking the "Ethnographer's Magic". In: E. Roesdahl, H. Thrane & T. Otto (eds): *Tre Tilrædelsesforelæsninger på Moesgaard*. Aarhus: Det Humanistiske Fakultet, Aarhus Universitet. Pp: 53-96
- Otto, Ton & Poul Pedersen  
 2000 Tradition between Continuity and Invention: An Introduction. *FOLK, Journal of the Danish Ethnographic Society* 42: 3-17.
- Popper, Karl R.  
 1972 *Objective Knowledge: An Evolutionary Approach*. Oxford: Oxford University Press.
- Roepstorff, Andreas  
 1999a Det Neurale Menneske – et Antropologisk Perspektiv. In: O. Høiris, H.J. Madsen, T. Madsen & J. Vellev (eds): *Menneskelivets Mangfoldighed*. Moesgaard: Aarhus Universitetsforlag & Moesgaard Museum.  
 1999b Fra Det Tællelige Til det Fortællelige. Noter fra et Feltarbejde blandt Hjerneskannere og Videnskabsfolk. In: M. Gullestad, C. Hasse, T. Otto, A. Roepstorff & K.E. Spannow (eds): *Antropologiens Muligheder og Metoder i Eget Samfund*. Århus: Dansk Etnografisk Forening. Pp: 58-75.  
 2000a The Double Inter-Face of Environmental Knowledge: Fishing for Greenland Halibut. In: B. Neis & L. Felt (eds): *Finding Our Sea Legs: Linking Fishery People and Their Knowledge with Science and Management*. St. Johns: ISER Books. Pp: 165-188.  
 2000b *Laboratoriekosmologier: En Fortælling om Hjerner, Natur og Videnskab*. Jordens Folk (3/2000): 6-14.  
 2001a Brains in Scanners: An Umwelt of Cognitive Neuroscience. *Semiotica* 134 (1/4): 747-765.

- 2001b *Facts, Styles and Traditions. Studies in the Ethnography of Knowledge*. Ph.d. dissertation, Aarhus University: Department of Ethnography and Social Anthropology.
- 2001c *Insiders and Outsiders Unite! Science and Science Studies in/of the 21st Century*. In: K. Siune (ed.): *Science under Pressure*. Aarhus: The Danish Institute for Studies in Research and Research Policy. Pp: 107-122.

Roepstorff, Andreas & Ausra Simoniukstyte

Forthcoming *Cherishing Nation's Time and Space. The Tradition Maintaining Lithuanian Identity*. In: T. Otto & P. Pedersen (eds): *Anthropology and the Revival of Tradition*. Aarhus: Aarhus University Press.

Strathern, Marilyn

1996 *Cutting the Network*. *Journal of the Royal Anthropological Institute* 2: 527-535.

Tambiah, Stanley J.

1990 *Magic, Science, Religion and the Scope of Rationality*. Cambridge: Cambridge University Press.

Turnbull, David

2000 *Masons, Tricksters and Cartographers. Comparative Studies in the Sociology of Scientific and Indigenous Knowledge*. Amsterdam: Harwood Academic Press.