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M'charek, A.

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Genetics and its Others

On Three Versions of the Savage

Amade M'charek
University of Amsterdam

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This essay started out as a review of Karen-Sue Taussig's *Ordinary Genomes*. But during writing it kept changing shape. This is partly due to the theme of this special issue 'New Savages.' They come in many versions. As the introduction of this special issue shows we have obviously moved past the idea that 'savageness' inheres in the bodies of individual people or their culture. The savage is a myth. Thus rather than a singular ontological position, the savage is best seen as a multiple figure to think with. Based on different renderings of the savage I will attend to three versions of this figure.[1] The first is what I shall call a naïve version of the savage. This is a version that characterizes a practice in which groups of people are configured as *discrete entities*. This production of savageness, either in far-out places or in the centers, contributes to the naturalization of groups of people, as if these were a nature out there. The second is a methodological version of the savage. This figure attends to *culture*, and to qualities that had been ascribed to the savage's culture. One can think of the lack of universality and the locality of culture in space and time, or of rites and rituals that involve humans as well as objects. The third is a material semiotic version of the savage. This version attends to ways in which the *identity* of the 'savage' has been configured in ethnography. It draws on work that helped de-center the subject, e.g. by showing that social relations are not only articulated by humans but also by objects such as the Kula (Malinowski), or by teaching

us that lineage is not merely made by humans but also by cows (Evans-Pritchard). This version, then, helps us see that identities do not inhere in people but are made through relations between different entities in specific practices. Such takes on identity thus do not acknowledge alleged dichotomies between people and things, between words and the phenomenon, between subjects and objects. Rather it accounts for complex configurations and how these produce relational identities.

In addition to this the content of *Ordinary Genomes*, namely the entwinement of genetics and society and the role of genetics in identity making, evoked a multitude of shapes that this text could have taken. A highly creative process but how to tame it? In what follows I will elaborate on the three versions of the savage - the naïve, the methodological and the material semiotic version - and relate these to genetic research. In the final section I then turn to Karen-Sue Taussig's *Ordinary Genomes* and reflect on her findings in the light of the discussed versions of the savage.

The savage: a naïve version

This brief story is about a fairly traditional anthropological endeavor, though one with novel means. It is a story about the Human Genome Diversity Project. I will show how it helped to produce a *naïve* version of the *savage*. It all started twenty years ago. In 1991, a group of population geneticists embarked on an international project to collect samples from populations living in various places in the world to thus produce a map of human genetic diversity. These geneticists, organized in the Human Genome Diversity Project (the Diversity Project) wanted to profit from novel genetic technologies that enabled the large-scale international research they had in mind, and from the fertile political ground for genetic research to which the much bigger and prestigious multi-billion Human Genome Project had given rise.[2] The aim of the diversity researchers was to contribute to our knowledge of the history of human populations by producing a map of genetic similarities and differences. Based on the idea that humans emerged in Africa some 100,000 years ago, navigating that map would help us unravel the migration histories of populations (out of Africa), learn about genetic kinship and lineages, and determine which populations can be considered genetically older and which younger. A group of 500 populations were short-listed to be sampled. One of the initiators of the Diversity Project, the Stanford population geneticist Luca Cavalli-Sforza argued that 'if sampling is too long delayed, some human groups may disappear as discrete populations … At a time when we are increasingly concerned with preserving information about diversity of the many species with which we share the Earth, surely we cannot ignore the diversity of our own species.'[3]

However, the Diversity Project became controversial, almost overnight. In order to study human genetic diversity the initiators of the project had placed a special emphasis on 'isolated populations' and 'indigenous people.' The bodily fluids of 'isolates' and 'indigenous peoples' out-there were deemed crucial resources to produce knowledge about us, in-here. For whereas the West was seen as a melting pot, a result of complex historical encounters, these peoples were assumed to be homogeneous. Their blood was treated as a resource

conveniently located in the bodies of people without history (Wolf 1982). However, as many anthropologists have shown, the fact that some groups nowadays live relatively isolated lives does not mean that it has always been that way.

> The San peoples of South Africa, for example, at the top of the so called 'genetic isolate list,' and therefore a pristine example of an uncontaminated population by HGDP standards, embrace three different language groups, suggesting relatively recent formation as a single group. … [T]he San became isolated only in the 19th century, and their isolation is related directly to colonialism. (Lock 2001: 80)

Thus genetic isolation of populations might as well be an effect of the Diversity Project which is based on a rather naïve version of history. By skipping over the histories of populations and by sampling and studying them as if they were given natural phenomena, the project has helped to enact the isolation of these populations. One could say that it has thus contributed to a naïve version of the 'savage.' However, and as these things go, the project's object of research turned out to be a subject as well. The populations targeted for sampling by the Diversity Project objected to their objectification. Soon the project was accused of racism and neocolonialism by various organizations of indigenous peoples. Alerted to the Diversity Project by the Rural Advancement Foundation International (RAFI), on July 8 1993, the Third World Network placed a call on the list-server of Native-L (an aboriginal, First Peoples news net) aimed at stopping the Diversity Project:

> In the year of indigenous people and at the time of UN Conference on Human Rights we find such initiatives emerging from the west totally unethical and a moral outrage. We call on all groups and individuals concerned with indigenous peoples' rights to mobilise public opinion against the case of human communities as material for scientific experimentation and patenting. Indigenous communities are not just 'isolates of historical interest.' They have a right to be recognised as fully human communities with full human rights which include decision about how other countries will relate to them.[4]

The World Council of Indigenous People thus objected to a treatment of people, the so called 'isolates of a historical interest,' as 'savages' in the sense of being seen as remnants of a historical past, and therewith denied a place in global history.[5] The World Council therefore dubbed the project the 'Vampire Project.' This naming underlines the Project's interest in the blood of the virginal other, but also the fact that the blood-taking was for interests other than those of the populations at issue. Due to this criticism the Project had problems organizing political support and raising funding to get started. Officially the Project is now said to have ceased. Its two tangible achievements are an ethical protocol for the sampling of populations, and a biobank consisting of 1065 human cell lines from 51 different populations. This biobank has been housed in the Fondation Jean Dausset in Paris since 2002, and is nowadays intensively used in population genetics research.[6]

The discourse of the Diversity Project, I want to suggest, can be understood as a version of the naïve

savage. Although the diversity researchers did not engage in details in a discourse about the customs of the population they had targeted for research, the discourse of isolation, lack of history, ideas about kinship relations contributed to the idea of a natural kind out there. In addition this discourse was organized along neat divisions between self and other, nature and culture. It thus contributed to an understanding of the 'savage' as nature out-there, clearly distinct from the civilized and cultured in-here. In practice, as I will show below, the knowledge produced by geneticists working in this Project does not correspond to such divisions. Let us now turn to the second version of the savage, namely a methodological version.

The savage: a methodological version

Our notion of what scientific practice is has changed dramatically since the late 1970s. The idea that science is primarily a rational process, and that scientific facts are the result of a brilliant mind employing a sound scientific method was harshly disrupted. What came to the surface was a picture of science as an inherently material, messy and cultural process. What happened?

Inspired by the work of Thomas Kuhn scholars of scientific knowledge entered the laboratory to see with their own eyes what scientific knowledge is made of. Instead of assuming what facts are made of or what a scientific method is, they decided to make strange what is at the heart of contemporary western societies, the laboratories. To this end they conducted detailed ethnographies.[7] Scientists were *methodologically* viewed as a strange tribe with its own rites and rituals and an elaborate material culture. Not only did these ethnographers turn science into a 'strange' practice, they also made local the alleged universal claims of science. Rather than facts about a nature out there that are revealed thanks to a sophisticated method in here, they showed that the facts of science are intricately intertwined with the technologies and methods that are used to produce them. Here is for example what Bruno Latour and Steve Woolgar have said in this respect: 'Without a bioassay, for example, a substance could not [be] said to exist, the bioassay [technology] is not merely a means of obtaining some independently given entity; the bioassay constitutes the construction of the substance' (Woolgar and Latour 1986: 64). However, to make scientific facts travel outside the laboratory, for example into a scientific paper, scientists typically do some deleting work and detach the results from the material practice. This deleting work makes facts travel, but it also contributes to the illusion that these facts exist independently of the scientific practice in which they were produced. No one of us will be able to see his or her genes just by looking at his or her hand, while seated at the kitchen table. To do that you will need some DNA-typing technology. It is only with the help of such technologies that a gene becomes a meaningful object about which facts can be produced, an object that can be visualized, studied, compared and intervened in.[8]

In addition, the turn to practice in laboratory ethnographies was simultaneously a turn to the routines of science. A turn to scientific conduct, where nothing spectacular seemed to be going on. Following Malinowski one could say that it was a move towards the imponderabilia of actual (laboratory) life; i.e. 'series of phenomena of great importance which cannot

possibly be recorded by questioning or computing documents, but have to be observed in their full actuality' (Malinowski 1922: 18). And Malinowski adds:

> Indeed, if we remember that these imponderable yet all important facts of actual life are part of the real substance of the social fabric, that in them are spun the innumerable threads which keep together the family, the clan, the village community, the tribe – their significance becomes clear. (Malinowski 1922: 19)

By analogy, the turn to practice in science studies implied a focus, not on the scientist's *head*, but rather on her *hands*. Thus instead of unraveling what scientists say it was even more important to consider what they actually do. The suggestion was that the imponderabilia of mundane scientific conduct can teach us about the culture of scientific work and the content of the knowledge produced. To be sure, the point is not that because science is a cultural practice or because it is being studied by ethnographers, it is better viewed as a savage. Rather, by treating it *methodologically* as a savage practice, thus material, messy and local rather than rational, logical and universal, we produce different accounts of how scientific knowledge is produced and what kind of knowledge it actually produces.

The brief introduction to the Diversity Project above shows that it led to heated debates. What comes to the fore is its controversial character, its blunt 'science for the West and genes from the rest' kind of appearance. Yet instead of focusing on the controversy and debate, I became interested in what genetic diversity actually was. What was it made to be in laboratory science? If genetic diversity is not simply the end-product of theories and methods applied to a population, how does it come about? What does it take to produce it? What is it and thus what does it make of us and the not-us? To answer such questions I entered the laboratories and conducted ethnographic research. What I found there can best be illuminated by my third version of the savage: the savage as a material semiotic actor.

The savage: a material semiotic version

What is an object? What is a gene, a body, a people? Traditionally such objects have been seen as something passive and clearly distinguishable from the subject that acts. In accordance with this and contributing to a clear-cut dichotomy, an object seems to need the intervening technologies of the subject to be dis-covered, animated and re-presented. This dichotomy sits in a comfortable analogy to others, such as nature (given) vs. culture (changeable), savage (wild) vs. subject (tamed), body (natural) vs. mind (rational), and we can expand and compile ever longer lists. But how to account for the biological, the bodily, the material without reifying such dichotomies? And more importantly, how to do that without objectifying and essentializing nature and the biological? In her classical essay 'Situated Knowledges,' the historian of science Donna Haraway suggested viewing the body, nature and the biological as *material semiotic actors*. This notion 'is intended to highlight the object of knowledge as an active, meaning-generating axis of the apparatus of bodily production, without *ever* implying immediate

presence of such objects … [B]odies as objects are material-semiotic generative nodes. Their *boundaries* materialize in social interaction … objects do not pre-exist as such' (Haraway 1991: 200-201). So this notion allows us to attend to materiality without implying that it is fixed or stable. Materiality, it is suggested, is relational. It is made in specific practices, and it can be unmade if the relations that help enact it are not made durable. This is suggested by the semiotic part of the notion. Semiotics is the study of relations. Not just of the relation between words, but also that between things, including the relation between the knowing subject and the object. This suggests that boundaries between the subject and the object are not pre-given but shift and change depending on the specific practices in which they emerge. This is what is implied in the actor-ness of the notion. Such objects *affect* us. They are active and contribute to what we come to know about them, and about ourselves.

In addition, if objects are relational and enacted in specific practices, they necessarily come in many versions. This multiplicity is not a matter of multiple perspectives. It departs radically from the idea that an object is a singular thing and that the eyes of the different beholders of knowledge happen to highlight different aspects of it. In contrast to such perspectivalism, as Annemarie Mol (2002) has shown, different versions belong to different realities, they could be compatible with one another, but might as well conflict or exclude each other altogether.[9] Now why would we want to embrace the multiplicity of objects? I contend that it might safeguard us against the reification of difference (such as the dichotomies above) and the naturalisation of objects. Whereas social constructivist approaches have helped us understand that objects are made, they also seemed to have worked towards closure. That is, constructivist studies have shown that the histories of objects (and facts) are messy and contingent. Yet much of the analytical work has gone into showing how specific facts or objects and their spokespersons overruled other possibilities.[10] In this view there was plurality in the past, but it got lost in the present. The emphasis on a 'winning' object contributes to naturalisation of that object, and the trivialisation of other versions of it, that are relevant and real as well. If we would not only focus on how objects are made but also on what they do or on how they come to matter in different practices, we would make room for their multiplicity. Obviously, multiplicity is not a good in itself. Yet attending to multiplicity contributes to the denaturing of objects. And this is even more important in an era where the life sciences, such as genetics, are gaining pride of place in knowing who we are.

To go back to our example of the Diversity Project, at the start there was debate among the initiators as to how to define population: an important matter if you want to sample and study them. Two approaches were overtly debated. One was a rather open approach in which no prior knowledge about what a population is was to be assumed. It was suggested to produce a grid and sample any group of people every 100 miles or so. The second and more dominant approach wanted a definition of population based on linguistic separation (M'charek 2005). However, if we would focus not on issues of definitions, but on what population is made to be in scientific practice we immediately encounter its multiplicity (see M'charek 2000). There we find that different technologies and issues of concern produce

different versions of what population is. In the lab we might find a version of population based on family names: family names of individuals contribute to their clustering in one population or the other. A bit down the line in the same practice we encounter population as a matter of national boundaries, or of geographical distance, and then again as usual in the databank. Another way of doing population is based on genetic markers (DNA fragments that are known to vary between individuals). For example, depending on the number of genetic markers that are used individuals can be clustered in one population, or separated out as belonging to different ones. In addition, if we would analyse groups of individuals on the basis of so called mitochondrial DNA (DNA that is passed on to offspring by the mother only) they would fall in different populations than if analysed on the basis of the Y-chromosomal DNA (the so called male sex-chromosome). This makes clear that in diversity studies populations are no natural entities. Rather, they are part and parcel of the technologies that are applied to study them, so that in scientific practice the boundary between one population and the other shifts and changes. In addition since the DNA is often referred to as the ultimate locus where we can learn about genetic diversity, it is important to notice that in the laboratories DNA, in the form of a genetic marker, is really many different things. We might encounter a genetic marker as the *object of research*, the fragment of DNA to be visualised and studied. But we might also find that it is a *visualising technology* allowing us to learn about similarities and differences between individuals. And then again it is also a *methodological device* that, based on the variation it helps uncover, contributes to the analysis of genetic diversity, thus contributing to the analysis of what it reveals (M'charek 2005). So in short, genetic diversity is not something out there in 'isolated populations' and the like waiting for geneticists to uncover it. Rather, it is an effect of technologies and methods that are applied to investigate it. This does not make it less 'natural' or less 'biological,' but rather differently natural and biological. Not a singular entity out there, but rather a node elsewhere, requiring us to come closer and to engage with it.

I have suggested three versions of the savage and shown how they can be helpful in thinking about our current scientific practices as well as the scholarly work that has been going on in studies of science and technology. All three are relevant and real, but they do different work. The first version that we have encountered, the naïve version, can be found broadly in our contemporary society. This version, as we have seen above in the case of the isolated populations and genetic diversity research, helps us see how groups of people are naturalized and presented as discrete entities, and to attend to (re)productions of a serious parody of the savage, in the form of the other. This version obviously helps us unravel processes of othering. The second version, a methodological version, provides us with novel methodologies for making sense of the world. Rather than a well ordered and structured world, the methodological version of the savage sensitizes us to the messy, the trivial and to that which is othered. In the case above I have shown that the methodological turn to scientific practice has helped make visible that it is a cultural practice just like any other, a practice that is less about the discovery of facts about nature and more about the construction of these. This version is best seen as one that wants to bring the 'savage' home

as a figure to tinker with. The third version, the material semiotic actor, is helpful for showing that boundaries between objects and subjects are not given. They are made and they shift and change depending on the practice at hand. Also, both subjects and objects are relational and effects of complex configurations. In the case above it has become clear that nature and technology are mingled, and that identity (e.g. of an object such as population) is performed in practices and does not pre-exist them. This version of the savage sensitises us to the fact that neither subjects nor objects are singular things, even when they are performed as such. To put it the other way around, and to borrow from Thomas (1991), this version attends to the promiscuity of objects and subjects. That is both to their multiplicity and to their generative effects. Since the subject (either western or otherwise) can no longer be assumed the master of things, we cannot but grant the world its own sense of humor and its own effect on history.

Ordinary genomes, extra-ordinary Dutchness

Diversity Projects have moved from the proverbial South to the North. In recent years in many western countries genetic research into the identity and history of the original populations has been embraced. It has come to play different roles, varying from individuals' interests in their genetic genealogical ancestry, to research deemed relevant in forensic or medical practice, to genetic studies conducted in the context of archeological research about western local populations. Given this recently growing interest in western genes, *Ordinary Genomes* can be considered avant-garde.

In the early 1990s, the US-based anthropologist Karen-Sue Taussig hit upon interesting stories about the Dutch. They seemed to be proposing the introduction of something called a genetic passport. Instead of her planned research in the Czech Republic she decided to learn Dutch and study what was going on with genetics in the Netherlands. She spent one year conducting a multi-sited ethnography in a hospital, two schools and a community center in a small village, as well as various other social sites and gatherings. In addition, one of her chapters analyses the public debate and discourse about *Stier Herman* the famous genetically modified bull, whose female offspring produce a human form of the protein lactoferrine in their milk meant as an anti-inflammatory treatment in infants.

Eighteen years later Taussig has published her findings in *Ordinary Genomes: Science, Citizenship and Genetic Identities*. Taussig's book is extraordinary, not least for the claim that '[o]rdinary genomes are a distinctively Dutch phenomenon' (Taussig 2009: 198), suggesting an exceptional marker for what Dutchness is. This claim, especially given our current day multicultural, post-9/11 and post-Fortuyn society, obviously begs the question: what is Dutchness? These events have generated heated debates about the so called 'autochtone' and 'allochtone' populations, but also about what Dutchness is, and if there is such thing as a Dutch identity (WRR 2007). Despite the current loudness of some voices in Dutch society, in her book Taussig argues that 'contemporary genetic practices in the Netherlands are powerfully shaped by two highly valued Dutch social ideals: first, a desire for ordinariness and second, a commitment to tolerance' (Taussig

2009: 5). The notion of tolerance is framed as an effect of the Second World War as well as of the typically Dutch Pillarization system which was institutionalised in the mid eighteenth century. Instead of combating each other because of religious or political differences, the Dutch organized themselves in pillars that run through all layers of society. The elite in the various institutions of such pillars learned to come to a consensus and were capable of making their community comply with it. 'Pillarization plays a powerful role in rendering differences unremarkable by constructing them as part of a known group or category, thereby making them more manageable' (ibid.: 30). In the Netherlands, so Taussig argues, this tradition, even if it has faded away in current-day society provides a strategy to manage genetic difference through tolerance. In addition, ordinariness as a social dictum (as in *doe maar gewoon, dan doe je al gek genoeg;* literally*: Just be ordinary, that's crazy enough;* which means: don't try to be extraordinary, and comply with the norm) produces the need for conformity within groups (ibid.: 36-41). Ordinariness is therefore socially significant and contributes to a dynamics of normalization. Taussig argues that the ideal of tolerance is arrived at by demarcating and containing difference. She contends that this ideal is mirrored by the way the Dutch manage and construct the meaning of genetic difference.

The heart of *Ordinary Genomes* is a long chapter about a genetic centre at an academic hospital in the Netherlands. Taussig conducted observations during weekly meetings where patients were being discussed and cases assessed. This chapter offers a beautiful rendering of medical tinkering in the midst of uncertainties and complexities. And interestingly enough it gives us access to a practice that has nowadays become virtually archaic in hospitals because of its focus on the morphology of genetic disease, rather than simply the genetic evidence of mutations. Genetics has simply pushed away other repertoires of learning about diseases. In this chapter different scientific technologies meet, such as clinical pictures of the faces and bodies of patients, written records about the patients, and films and other representations of specific genes. During the weekly meetings, the cases were debated and diagnosis made on the basis of this material. In her analysis Taussig focuses on how genetic dysmorphy is dealt with in practice, and how it is talked about during interviews. While the material is beautiful and the *en passant* analyses to the point, Taussig's major analysis of processes of normalization is unfortunately less convincing. Her argument runs as follows: genetic 'abnormalities' (*abnormaal*) are seen as problematic and patients are rendered *ordinary* by classifying them in terms of a syndrome; the reason for this normalization is the Dutch management of difference which renders otherness ordinary, and thus organizes a tolerance for difference. This is the core argument of this book. But it is its weakest point. For does this procedure really differ from those that can be found in other countries? It is both surprising and intriguing that Taussig seeks to discover the Dutchness of the genetic practices that she studied. It is however disappointing to see how the various differences that she unravelled, and the complex practices that she has described, are subsumed in one simple structural scheme.[11] Especially since the values of tolerance and ordinariness, as they figure in her analyses, do not have sufficient analytical power to bring out the richness of the practices that the reader

encounters. That is, they do not reveal any specificity of her findings. Why these processes of normalization are inherently more Dutch than e.g. British remains an open question.[12]

To end, let me briefly revisit the three versions of the savage that we have encountered above. Let me start with the savage as *a methodological version*. Taussig's approach obviously is in line with the approach sketched above. In her ethnography, she renders strange a medico-scientific practice in order to learn about how it deals with similarity and differences. Her approach however also differs from that, because instead of understanding the practices she studied in their own terms, she has mobilized rather structural and rather general cultural schemes to explain the phenomena she encountered. The discussed chapter three has a lot to offer in terms of the savage as *a material semiotic version*. Again what is meant by this concept is not that material semiotics is a quality of the savage, who or whatever that may be. Rather insights from classical ethnographies in which qualities have been ascribed to the savage and his culture, especially pertaining to the identity of the savage, are used to analyse what identity is. Rather than presupposing a stated difference between humans and things, between the phenomenon and its representation, this version helps us articulate relationalities between people and things, and show that identities are the effect of complex configurations of these.

Taussig's third chapter reports on how diagnostic tools intervene in the object of interest. Tools such as photographic material help to produce a specific version of a genetic disorder, and different technologies may work together or against each other in that process. But when it comes to her object, namely genetics in the Netherlands, Taussig's analysis tends to close off what Dutchness is. Even when she pays attention to the multi-cultural society and emergent anxieties regarding difference, she trivializes these to preserve a dominant explanatory framework. She therewith risks naturalizing (or rather culturalizing) a highly contextual and diverse practice. This explanatory framework is presented as a more or less 'natural' phenomenon that is out there, in society. A framework that is less connected with genetic practice as such, and rather with the Dutch as a people. For example on page 176, we read several times about Dutch people: 'Dutch people continually enact values around …'; 'Dutch geneticists never cease in their effort …'; '… which Dutch people draw in Nazi example …'; 'the perception many Dutch people have about …'; '… distinction many Dutch people seek to make….' In this way Dutch people, Dutch culture, Dutch treatment of similarities and differences, and Dutch genetic practice seem to map on each other neatly, producing *a naive version* of Dutchness.

E-mail: A.M'charek@uva.nl

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Notes

1 I deliberately use the notion of *version* to stress that we are not dealing with different perspectives or interpretations of e.g. the savage, but with different realities of it. In a sense, different versions imply a different object.

2 See for a classical volume on the Human Genome Project, Kevles and Hood (1992).

3 Luca Cavalli-Sforza (1993) http://www.stanford.edu/group/morrinst/HGDP-FAQ.

4 Third World Network (July 1993), http://nativenet.uthscsa.edu/archive/nl/9307/0036.html

5 See for a counter narrative that stresses the shared histories by focusing on the role of objects, Thomas (1991).

6 The Diversity Project can however be seen as highly successful if we would not be looking for a well structured and organized version of it, but rather focus on the various networks it helped to establish in which genetic diversity research is being conducted on a daily basis for a variety of goals (see M'charek 2005, chapter 6).

7 The classics in this genre of Science and Technology Studies are Woolgar and Latour (1979); Knorr-Cetina (1981); Law and Williams (1982); Lynch (1985); Traweek (1988).

8 The turn to practice and to the making of facts in the Social Studies of Science bears resemblance and shares a theoretical background with the so-called linguistic turn in anthropology. Here is James Clifford (referring to Roy Wagner) on the latter; the emphases are crucial. A focus on writing and text-making 'undermines overly transparent modes of authority, and it draws attention to the historical predicament of ethnography, the fact that it is always caught up in the *invention*, not the *representation*, of culture' (Clifford 1986: 2, emphasis added, AM) .

9 In her ethnography of atherosclerosis Mol shows that what this disease is made to be under the microscope might be completely different from what it is in, for example, statistics and again differ from its rendering in the consultation room of the doctor. These different objects might suggest different interventions (for instance, operation, prevention, or physical exercise). The different versions of the disease might clash, for example when a patient suffers severely and has problems walking, whereas the laboratory tests would suggest a mild condition.

10 This is true also for many of the laboratory ethnographies in Science and Technology Studies.

11 Thomas (1991: 206) phrases this common anthropological problem as follows: 'Anthropology has tended to operate at two levels – the ethnographic and the theoretical. Although the former reveals a great deal about local nuances and the particular expressions of cultural meaning in exchange relations, theoretical discussion has almost always privileged the general type.'

12 And the fact that patients are nowadays more and more actively involved in the normalization of their diseases, and developing a so-called biological citizenship (Rose and Novas 2005) that transgress national boundaries and culture, should give us reason to think.

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