

Journal article

Epigenetics: Localizing biology through co-laboration

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Abstract: This paper reports on a co-laborative laboratory ethnography in a molecular biology laboratory conducting research on environmental epigenetics. It focuses on a single study concerned with the material implications of social differentiation. The analysis briefly raises biopolitical concerns. Its main concern lies with an understanding of the human body as local in its working infrastructure or “inner laboratory”, an understanding that emerges from the co-laborative inquiry between biologists and anthropologist. This co-laborative mode of inquiry raises productive tensions within biology as to the universal or local nature of human nature and within anthropology as to the status of human biology within social theory. The paper cannot resolve this tension. Rather it explores it as an epistemic object in the context of interdisciplinarity, ontography and co-laboration. In concluding, it specifies co-laboration as temporary, non-teleological joint epistemic work aimed at producing new kinds of reflexivity.

Keywords: epigenetics, local biology, new materialism, ontography, collaboration

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Introduction

The entanglement of nature and culture is being increasingly problematized in the social and natural sciences alike – albeit in very different ways. In the social sciences and anthropology, a long-standing interest in the body has recently been complemented by various new materialisms produced at the interface of anthropology, science and technology studies, human geography and diverse feminist critiques. Assemblage thinking and relational and processual approaches are being developed in order to build material dimensions into social inquiry (Mol 2002; DeLanda 2006; Haraway 2008). This is particularly true for research fields in and around medical practice and life scientific research. This material “turn” is rooted *inter alia* in a dissatisfaction with deconstruction of scientific facts and hegemonic natural science discourse as the sole analytical purpose. While that remains an important part of social scientific inquiry, researchers are beginning to explore the ways of engaging their fields of research more generatively (Verran 2001). Many inquiries in health and environmental fields recognize that important questions of human health, social inequality or environmental justice are at stake and that the social sciences may have more to contribute to these debates than revealing the contingent nature of hegemonic discourses.

This paper contributes to an exploration of new ways of engaging research fields in the natural sciences. It does so in the field of molecular biology. I report on a collaborative laboratory ethnography in an environmental epigenetics laboratory in Canada, a lab that is interested in the effects of changes in an organism’s material and social environment on gene regulation. My argument proceeds in three steps: first, I briefly introduce the scientific field of epigenetics, particularly environmental epigenetics. I focus on one particular study that correlates indicators of social difference with molecular markers. I argue that this study – thought through co-laboratively with biology and anthropology – points to an important research question: how to “localize human biology”? Second, I discuss how to engage this research problem social scientifically. I argue that the concept of “local biology” and debates surrounding new materialism are important starting points. In the third and concluding step, I explore “co-laboration” as a mode of temporary joint

epistemic work well suited to engage “localizing biology” as a research agenda. I situate co-laboration within recent debates about interdisciplinarity, the ontological turn and collaboration in order to specify co-laboration as a non-teleological, experimental practice aimed at producing a different kind of critical reflexivity.

Environmental epigenetics

Epigenetics

Much of the recent life scientific attention to “the social” and the entanglement of nature and culture stems from research in environmental or social epigenetics. I will use the term “environmental” epigenetics in this article, but include within it social as well as material aspects. Epigenetics is commonly defined as the “the structural adaptation of chromosomal regions so as to register, signal or perpetuate altered activity states” (Bird 2007). Put more simply, epigenetics refers to those mechanisms of gene regulation that do not involve changes in DNA¹ sequence. The two mechanisms that have received most attention in recent research are methylation and histone modification. Methylation describes the process whereby a small molecule (CH_3 , methyl group) becomes attached to particular places within the DNA sequence. A section of DNA is said to become methylated. It cannot be read off in this state. Hence genes encoded in methylated strands are down-regulated, that is, not expressed or expressed to a lesser degree. Histone modification refers to the structural change in the histone molecules around which the DNA strand is wrapped. Depending on the way these histone molecules are folded, sections of the DNA strand can be read off or not. Hence histone modification also correlates with gene regulation and expression.

Epigenetics is a rapidly growing subfield of molecular biology. Multiple genealogies can be written: one is conceptual, extending deep into the twenty-first century, evolutionary biology as well as research on germ cells and developmental biology in the 1930s and 1940s, imprinting in the 1950s and molecular findings about (de-)methylation in the 1980s. The other is technological as epigenetics largely mimics the technologies and experimental systems installed in the successive steps of the Human Genome Project (Landecker and Panofsky 2013).

Epigenomics

It is important to note that most research into epigenetic mechanisms follows the mapping logic developed within the Human Genome Project. The basic idea is to identify and map functionally relevant sections within the molecular architecture of DNA and its regulatory superstructure. What started with the actual DNA sequence, genome-wide and whole genome mapping, now proceeds to epigenetic patterns, that is, the epigenome. The transcriptome and the metabolome in increasing detail are the obvious next steps.

¹Deoxyribonucleic acid.

The fact that epigenetics has been integrated so rapidly into (epi)genomics demonstrates the power of the existing research infrastructure and experimental systems. Early epigenetic findings suggested that methylation patterns are cell and tissue-specific as well as highly dynamic. Dynamism is not something that lends itself to being analyzed with the static technology of the map. Yet the potential therapeutic and financial benefits of finding marker-like stable epigenetic patterns are such that epigenetics has been integrated into genomics research as just another layer of data, much to the dismay of many in the epigenetics research community, who saw in the early findings the possibility and need to develop different approaches (e.g. Madhani et al. 2008).

Epigenetics as a field of research is today driven predominantly by advances in three kinds of technology: chip technology to increase throughput, that is, the number of sites to be investigated per sample; computing and statistical-analytical technology to handle the increasingly large amounts of data; database and network technology to form international consortia with access to even larger amounts of data. The main funding for this research comes from public bodies and the pharmaceutical industry. Where it is not labeled as basic research, the main objective is to develop biomarkers, that is, identify functionally relevant epigenetic markers that might be therapeutically altered. The most important and lucrative developments have been made in cancer research. The promise is improved diagnosis, targeting and prognosis. The number of clinical applications derived from epigenetics is surprisingly large compared to the investment, which is small relative to the vast amounts spent on genetics (Nebbioso et al. 2012; Pickersgill et al. 2013). Epigenomics of this kind becomes part of the narrative of personalized medicine.

By far the largest proportion of research funding is directed towards epigenomics research. Environmental epigenetics is a niche compared to this internationally networked, consortium-driven research field. Hence I am arguing in this paper that environmental epigenetics is producing results that are good to think with for anthropology and social science as well as for interested scientists within biology. They are findings that challenge established notions of the body and of biology as a science of nature clearly separated and separable from culture. I am not arguing that molecular biology as a whole will move into this direction. My point is epistemic not prognostic. It would be naive to expect the vast field of molecular life science to change tack on the basis of some early proof of principle findings and against the dominant political economy of the field. It will be interesting to observe how environmental epigenetic findings consolidate and how they become integrated into or challenge the mainstream of mapping-based research.

Environmental epigenetics

Environmental epigenetics as discussed here is a rather small and still exploratory field. Yet two arguments speak in favor of considering this field as important within wider life scientific developments. First, a number of paradigmatic experiments have been published in high-profile journals such as *Nature Neuroscience* and *PLoS One* as matters of proof of principle. They are being cited widely in as well as outside of molecular biology (cf. Waterland 2003; Weaver et al. 2004; McGowan et al. 2009, see also Landecker and Panofsky 2013 for a detailed discussion of the field with regard to its sociological and

Niewöhner 2011 for its anthropological implications). Second, environmental epigenetics is part of a wider movement in the life sciences to incorporate “matters social” into experimental, functional and mechanistic inquiry. Sociogenomics and social and cultural neuroscience are further examples (e. g. Meloni 2014). Taken together, then, there is good reason to take note of environmental epigenetic developments and analyze how this field constructs linkages and exchanges between nature and culture, society and the body.

Environmental epigenetics is a field of empirical molecular biology that wants to better understand how the material and social environment of an organism influences that organism’s gene regulation. Current research is trying to understand the (molecular) mechanisms that link changes in environment to changes in gene regulation and it is trying to find out how stable such changes are through mitotic and meiotic cell divisions, that is, through individual development and across generations. Environmental epigenetics is a dynamic field that is predominantly driven by research on rodents with fewer studies involving human subjects or human data. Its basic experimental system rests on the notion of stress (cf. Selye 1956; McEwen 2000). Very much in the tradition of behavioral psychology, stress is understood as an adverse stimulus to a homeostatic organism, that is, to an organism in a dynamic equilibrium, which responds in measurable ways. Stress may include toxins or other material intake such as food, but extends far into behavioral (or social) realms to include maltreatment or displacement. In epidemiological studies on humans, countless studies have investigated the response of the stress system particularly to all kinds of chronic stressors from deprivation to unhappy marriages (e. g. McEwen 1998). Stress, or “adversity” as it is often referred to in epigenetics, is introduced into animal studies through experimental treatment, for example, alterations in the cage environment or electroshocks, or in case-control designs through epidemiological data, for example, low socio-economic status as a stressor. The experimental system is geared up to measure the effect of the stressor on the regulation of gene expression with essentially genomic technologies adapted to epigenetic needs.

In early 2015, the state of the art in this rapidly moving field understands epigenetic modifications to be metastable epialleles (e. g. Rakyan et al. 2002). Epigenetic alterations form allele-like patterns, yet “above” the DNA sequence: hence epialleles. And they have been shown to remain intact through mitotic divisions: hence metastable. Much current research is directed towards understanding the stability of epigenetic patterns across generations. Such transmission of epigenetic effects is well known in plants as well as in animals. Yet studies that have tried to demonstrate the material continuity of acquired effects through the germ line, that is, trans-generational inheritance as conceived in genetics, have failed to produce positive findings. In controlled environments, the first generation, which has not been exposed to the experimental stressor, reverts back to the original epigenetic patterning (Waterland, Travisano, and Tahiliani 2007). Other paths and modes of trans-generational transmission of epigenetic patterns, also involving gametes, have been discussed and shown experimentally (cf. Daxinger and Whitelaw 2010, 2012). More recently, paternal routes have also received attention after research had focused almost exclusively on maternal lines (Soubry et al. 2014). Yet the mechanisms that may transport epigenetic patterns through the resetting machinery of

meiotic division remain elusive to date. Recent research is beginning to focus on RNA biology and on trans-effects, that is, effects that are mediated via extra-nuclear routes and agents. While not new in principle, this adds another layer of complexity to the field and it is entirely unclear, whether the puzzle of trans-generational stability will be solved at the level of (epigenetic) mechanism any time soon. Most likely, perhaps, the question will shift from a dichotomous “yes/no” to a more differentiated “how important” is trans-generational epigenetic inheritance in what contexts? (e. g. Grossniklaus et al. 2013)

Biopolitical implications: molecularizing the environment and embedding bodies

Those concerned with the interactions between life scientific knowledge, social and cultural order and patterns of everyday life are watching environmental epigenetics carefully. As yet, the field is too young to state with any degree of confidence or thick empirical support what the implications might be for life itself (Rose 2001) or life as such (Fassin 2009; Raman and Tutton 2009). Yet two significant trends have been identified. First, research particularly in nutritional epigenomics has set out to classify the environment in terms of its relevance for human epigenetic response and performance (Landecker 2011). The result is an increasing molecularization of the environment as metabolically active substances are identified and mapped. Second, individual biographies and social milieu are equally transformed into sites of exposure with epigenetic consequences (Niewöhner 2011). These developments challenge various established notions of the social and material environment, for example, milieu, class, landscape or nature. The molecular biological gaze helps to dissect the social and material environment into functionally relevant molecular correlates. What emerges is an understanding of society and ecology akin to structural-functionalist notions with the difference that the organizing principle behind human group life is now rooted within human molecular biology rather than comparative longitudinal understandings of human nature and sociality. Hand in hand with this changing notion of the environment emerges a new notion of the body. The individual, skin-bound, autonomously and rationally captained body is replaced by a body that is heavily impregnated with its social and material environment (cf. Bentley 1941; Niewöhner 2011). It is a body deeply embedded in manifold temporal and socio-spatial scales reaching from evolutionary to real time and from the molecular to “culture”.

These are significant epistemological shifts within molecular biology. They may well develop ontological relevance through new biomedical interventions or changes in health and social policy or public discourse. Already public health and the ever-popular “self-management” literature have taken up this new relevance of the organismic environment. Interestingly, environmental epigenetic knowledge is equally readily adopted by those in favor of increasing social welfare spending and public health measures to reduce social inequality as it is by those in favor of increasing individualistic attention to early life development. The main biopolitical concern must therefore lie with the crude naturalization and subsequent reification of complex material-semiotic configurations

inherent in both biopolitical positions. Already, deterministic readings of gender, of social class and of group behavior emerge in metaphors and discourses with direct reference to environmental epigenetics (Pickersgill et al. 2013). Interventions and infrastructure development may follow. The translations of life scientific knowledge into policy, discourse and everyday practices, the emerging means and modes of interpellation, the reactions of vernacular sets of practices and narratives will hopefully be at the center of forthcoming social inquiry.

Zooming in: researching the material effects of social differentiation

Circulating references: from social differentiation to methylation patterns

I have sketched the potential for biopolitical implications only briefly, because the main emphasis of this paper is neither primarily about deconstruction nor social critique. Rather the paper is concerned with the anthropological endeavor to better understand the entanglement of nature and culture in shaping aspects of human group life. It tries to elicit whether environmental epigenetics offers an opportunity to reframe biology such that it ceases to be the enemy of critical thought (Tsing 2000) and instead becomes an epistemic partner in the analysis of material-semiotic practices (Haraway 1991).

In order to begin this endeavor, it is not sufficient to consider environmental epigenetics from a distance as a field of research. A better understanding of environmental epigenetic practices is necessary. The following sections hence focus on a single study conducted by a research group at McGill University, Montreal, Canada, with which I was fortunate enough to be able to conduct a laboratory ethnography over four months. Moshe Szyf and his team and collaborators in Montreal and in the UK have for some time been concerned with epigenetic patterning. Szyf comes from a background in cancer research. Questions of cell proliferation and its control are central in cancer research and the role of epigenetic mechanisms in these processes has been discussed for some time. The group has attracted significant attention for showing in rats that (1) nursing behavior impacts methylation making rat pups more vulnerable to stressors and (2) these effects are metastable (Weaver et al. 2001, 2004; Weaver, Meaney, and Szyf 2006). They have also shown in human biomaterial that stress may affect methylation in functionally relevant brain regions (McGowan et al. 2008, 2009; Sabunciyan et al. 2012).²

The study of concern here set out to analyze how an individual's social and economic position within society impacts on methylation (Borghol et al. 2012). To do so, global methylation was analyzed in peripheral blood samples from a British long-term social epidemiological cohort (Power, Atherton, and Manor 2008). This cohort originally

²The studies compared suicide-completer brain material with controls and suicide-completers who suffered from child abuse to control to find significant differences in the methylation of glucocorticoid receptors implicated in mediating the timing of human stress responses. The authors of the study are the first to admit that such studies are necessarily “dirty”, that is, fraught with assumptions and hence highly contingent upon empirical, analytical and conceptual choices. Nevertheless they have been published high profile as a proof of principle despite the relatively small number of samples.

included all children born in Britain in one week in 1958. It has collected information on physical and educational development, economic circumstances, employment, family life, health behavior, well-being, social participation and attitudes.³ At 45 years of age a blood sample has been drawn. The epigenetic study selected 40 people of high and low social position at birth and age 45, respectively, that is, people who went from low to high position and *vice versa*, as well as people who stayed within their original classification. The statistical analysis shows that social position during childhood is a better predictor of adult methylation patterns than social position at the time of methylation measurement in adulthood. Social position was operationalized as parental occupation and access to household amenities (childhood) and as current occupation and housing tenure (adulthood) (Borghol et al. 2012). For the research group this study is important, because it demonstrates in human subjects that social position leaves molecular traces, that is, that social position writes itself into the molecular architecture of human subjects and that these changes to the architecture exert a significant influence over decades even if the environmental circumstances change for the subject concerned. As Landecker has pointed out ‘This is not just a matter of the social and material environment affecting the body in a lasting manner.’ It is the social and material environment affecting the “inner laboratory”, that is, the genetic regulation and thus the working infrastructure of the body (Landecker 2011). This type of effect differs substantially from, for example, the intake of environmental toxins that are incorporated as a substance and may do harm if and where they are metabolized. The methylation process signifies a change in the body’s infrastructure changing the organizational patterns and potential breadth of effects as well as their temporal stability across generations. Of course, social scientists such as Pierre Bourdieu have described this type of hysteresis effect as an elementary part of an individual’s field-bound habitus more than 20 years ago (1990). Yet while Bourdieu was theoretically concerned with the role of material contributions to hysteresis – bodily and environmental – it did not cross his mind to inquire into the possible materially embodied mechanisms that may contribute to such a time lag.

This study is in many ways typical of current environmental epigenetics research in its attempt to relate significant biographical and social events or states to epigenetic responses. Yet relating social differentiation within a given social group to the methylation profile of an individual member of that group is not a trivial affair. It takes effort to translate from the uncertainty and variability of a complex phenomenon such as social difference and its embodiment to the clearly defined epidemiological concept of “social position” that can be correlated with a validated indicator such as methylation patterns in peripheral blood. Latour discusses these translations (Latour 1999). He describes and analyses the scientific practices that translate an actual soil in the Amazon forest into a series of scientific publications and a collection of specimen. In the tradition of an ethnographic laboratory study (Latour and Woolgar 1986), Latour pays great attention to the infrastructures and technologies, the documenting and writing practices, the distribution of knowledge across people and epistemic culture and the different forms

³See <http://www.cls.ioe.ac.uk/default.aspx> for detail. Last accessed 3 November 2014. The cohort is still active.

of representing world at different stages of the research process towards publication. His main focus is on demonstrating that the steps from world to data to factual knowledge are laborious practices that produce their own historical and social contingency. In fact, data in his dictum should be treated as *sublata*, that is, as achievements rather than representations. Key to the analysis of their production is to follow the traces between different stages of representation as a matter of referencing the residues of world in increasingly abstract *sublata* and their representations (Latour 1999, 43).

This circulation of references can also be described for the research group at work in Montreal (cf. Niewöhner 2011). The data on social position imported from the British cohort study is the achievement of a complex set of practices from recruitment of a cohort via archiving and documentation of empirical material to the large effort that is needed to maintain and take care of a cohort over 45 years. This work can be traced to and is contained in the small blood sample, which the cohort management agrees to provide for the methylation study. Several conceptual and analytical steps then follow before social difference is translated into a matrix of low/high social position at two different time points. The same holds for the methylation analysis. Ideally, biologists would like to measure methylation directly in relevant tissues such as stress-related receptors in brain tissue. For obvious reasons that route is not feasible in living human subjects. The development of peripheral blood as a valid proxy is a contested process as peripheral blood contains a heterogeneity of cell types that is analytically hard to differentiate (Lam et al. 2012, 2013; Suderman et al. 2013).

This brief sketch demonstrates how much work goes into producing a relation between the scientific paper on the embodiment of social position and actual people born in the UK in 1958 and their changing social and economic situation. Further analysis would reveal the contingency contained in individual choices, in technological infrastructure and in standards of statistical analysis and publication. This is the necessary basis for the biopolitical critique voiced above and elsewhere (Niewöhner 2011; Pickersgill et al. 2013; Meloni 2014). While I do not believe that this critique has run out of steam (also Latour 2004), it became clear to me during the ethnographic work that this group was really interested in questions of the universality and particularity of the human body. These are tropes deeply rooted in the history of anthropology and it occurred to me that the biopolitical critique would not relieve me as an anthropologist of a certain disciplinary responsibility for developing ways of dealing with them in ways that others could continue to invent around (Strathern 2002). The lab group seemed to think so too: anthropologists ought to know about social differentiation, should they not? And further: the lab group very much shared my concern about their experimental designs. So in continued conversation with the members of the lab, part of me turned from observant ethnographer to epistemic partner around the question of material dimensions of social differentiation.

Molecular biology of social position

This oscillation between critique and co-laboration has been received with skepticism in the social sciences. Hence I want to explicate how this is not about silencing critique but

rather a co-laborative attempt to tackle a long-standing anthropological problem in a new way.

Of course, the everyday laboratory practice is dominated by purification work (Latour and Woolgar 1986) and a pragmatic reductionism common in the sciences (Beck and Niewöhner 2006). It is easy to see in the daily routines the opportunistic search for suitable data about social and environmental change. This change is not informed by (social) theoretical considerations but rather by a molecular gaze and its in-built criteria for statistical significance and ecological validity of proxies. Discussions in the lab's morning journal and data sessions illustrate this opportunistic search. Events of "social change" suitable for research are suspected in post-socialist and post-war transformations or the well-known Bucharest orphan study (cf. Zeanah, Fox, and Nelson 2012). "Suitable" in this case means well-defined, clearly demarcated and offering access to pre-/post-event data. In these discussions, complex socio-political events are reduced to events of exposure. A careful discussion of these cases in terms of natural experiments would be productive (Dunning 2012).

Yet the group regularly discusses what they are really interested in: the material consequences of social dynamics and inequality, what Szyf terms the "molecular biology of social position". Three aspects are of particular relevance: (1) the group shares an understanding of society as differentiated and assumes social position to be an inevitable yet relative rather than absolute phenomenon. This understanding is social scientifically naive, that is, it is not informed by the literature on social differentiation or any particular social theory. It is a worldview and a political concern about social inequality that informs their choice of research agenda. (2) Through their lab work, they are well aware that methylation processes are extremely sensitive to contexts at different scales (molecular, organismic and environmental). Already the handling of rodents in experimental work, for example, when switching cages, exerts an influence on methylation that can be comparable in effect size to drug-induced changes. Hence the crudeness of social epidemiological indicators such as social position is of concern to them. (3) The group is also aware of the fact that environmental epigenetics raises questions that are hard to study. Study designs do not easily produce the experimental control required by peer-review in molecular biology tied to the gold standard of statistical significance, large sample sizes and high-throughput standardized methods. Szyf comments in a lab session that this is now a time to think in biology and polemically contrasts this with a caricature of the era of the knock-out mouse model where you got a nature publication by knocking out a gene and seeing what that did. The group necessarily accepts statistical significance. Yet they recognize that statistical significance is hard to achieve when experimental control is as low as is necessarily the case in natural experiments. Internally, therefore, they operate with a notion of significance based on the layering legitimacy from different sources: biological, for example, does a statistically insignificant phenomenon make biological sense; ecological, for example, does a study design have ecological validity; disciplinary, for example, can the group recruit findings from other disciplines to support their working hypotheses. In this way, the group produces a thick significance (Niewöhner 2011) for findings that are statistically insignificant. This is an important working mechanism in

the group particularly for proof of principle studies.

I am not pointing out these three aspects of research practice to naively suggest that this research group is of particular cleverness and somehow held back by the mainstream of molecular biology. Neither am I fishing for sympathy for the subjects in my research. Yet I take these researchers to be similar to many others in the field of environmental epigenetics to whom I had the chance to speak, in that they are pragmatic but not ontological reductionists. From a biopolitical perspective, this does not make a major difference. Foucault rightly pointed out that what people “don’t know is what what they do does” (Dreyfus and Rabinow 1982, 187 citing Foucault). Pursuing science with good intentions may still contribute to a problematic dispositive.

Yet from the anthropological angle pursued in this paper, it does make a difference. It makes a difference, because the discussions with the biologists during the ethnographic work have allowed me to raise the question of the material dimension of social difference in a new way, in a way that does not treat biology as the enemy of critical thought (Tsing 2000). This lab group is prepared to accept the anthropological idea that human biology is a local phenomenon to a relevant degree. This is a major shift significant for the field of environmental epigenetics and its emerging biological thought style (Fleck 1935/1979). The field of molecular biology is predominantly interested in identifying the law-like rules according to which life at the molecular level unfolds (cf. Daston 2002). It expects to find these rules at the molecular level itself and, therefore, it shows little interest in levels of analysis above the molecular (Anderson 1998). Much of the social scientific critique directed at the human genome project and related public discourses has focused on exactly this eagerness of biology to explain biological life at molecular level – and rightly so (e. g. Rabinow 1992; Pálsson and Rabinow 1999; Rose 2001; Franklin 2003).

Now here is a research group that cannot escape that dominant logic of the field in their publications. Yet in their discussions they can. They understand and try to show it experimentally that “matters social” are inscribed at the molecular level. It makes little sense for them to attempt to explain aspects of biological life bottom-up or bottom-up only. While molecular biology tends to operate with the omnipresent “onion” metaphor that conceptualizes *bios* to be built up in layers from genes to culture, this group shows an active interest in what geography is called scale jumping (Smith 1984, 1996), that is, effects across and structural coupling between scales that are not adjacent to each other and where the mechanism may not be operating neatly along the layered levels of analysis. I am not arguing that this view will change molecular biology. I am not even suggesting that the early findings will stand. I hold this to be significant, because it is a suggestion from within the field of biology itself that the research object – biological life – may be a more or differently *local* phenomenon than the dominant molecular thought style portrays it to be. This opens up to negotiation the dominant understanding of the body as a universal inner laboratory operating in local social and material environments: just how and to what extent is the inner laboratory itself localized or situated?

This is a research program with which particularly anthropology, but also the wider social sciences, ought to engage. It raises questions about the social and historical impregnation of biological life at the molecular level. It raises questions about the

material dimensions of social practice, which are of concern as much to biology as they are to anthropology and it is often rather a material theoretical work. And it asks how the dominant mode of social and deconstructive critique in science and technology studies (STS) may be complemented by a more generative mode of critique and interaction (see also Verran 2001). In the following, I will first discuss the notion of local biology and the new materialisms in the social sciences as useful but insufficient theoretical anchoring points for interaction with biology. In a last step I develop co-laboration as a means of localizing biology and as a means of engaging anthropology and biology without falling into simplistic natural or cultural reductionisms.

Anchoring points: local biology and new materialisms

The calls within the social sciences and anthropology to take the material body empirically and analytically more seriously have been growing louder for some time now (Lock 1993a; Beck and Niewöhner 2006; Timmermans and Haas 2008). While the return of attention to the body in the social sciences dates back much further (Scheper-Hughes and Lock 1987), most scholars have been interested in the body in its metaphorical, symbolic and political dimensions, as a carrier of meaning and a means to perform subjectivity and social relations. The notion of “local biology”, already dating back to the late 1980s, has emerged from a somewhat different context. Margaret Lock and her colleagues’ work on menopausal symptoms in North America and Japan remains one of the few studies in the social sciences that brings together thick long-term ethnographic research on symptom experience within a cultural context with physiological measures of the relationship between nutritional intake and menopause (Kaufert et al. 1986; Lock 1993a, 1993b; Lock and Kaufert 2001; Melby, Lock, and Kaufert 2005). This approach has neither looked for the simple integration of a multi-method approach, nor has it simply perpetuated the dominant distinction between disease and illness. The study – published in a series of books and papers over many years – unfolds a dense narrative that relates local food cultures and quantified nutritional intake with changes in style of media coverage and gendered patterns of illness experience. It tries to bring material and social etiologies and pathways into dialogue. In so doing, it does not shy away from natural science methods (Melby 2005a, 2005b, 2006; Melby and Lampl 2011). Yet it remains skeptical of simple causalities. This approach to “local biology” stands in the tradition of anthropology as a discipline that has always demanded that ecological, social and moral dimensions of human group life be considered in relation to each other. For all but few exceptions, this tradition has been lost with the end of structuralist anthropology and the rise of constructivist agendas in the 1970s. The notion of local biology is rooted firmly within this critical constructivist stance inspired by feminist critique as well as the emerging science and technology studies. Yet at the same time it produces continuities with the old anthropological desire of a relational anthropology aimed at understanding the manifold linkages between nature and culture (see also Beck 2008 on the concept of relational anthropology). “Local biology” suggests that human bodies may be more deeply situated in local material and cultural environments than previously thought. This is not to say

that humans do not share large parts of their physiology. Yet the degree of impregnation of shared physiology with manifold local and situational factors, its quality and relevance might be more considerable than expected. It is important to note that local biology does not only mean local *bios*, that is, the “embeddedness” of the material body in a broader social and material context. It also means local *logos*. The way bodies are represented, known and treated is socially and historically contingent (Niewöhner 2011). Biological knowledge is situated. Even further biological categories are part of the social fabric within any given society and people live with them. Biological categories become part of the way bodies are known and treated by their owners, by medical practitioners and through infrastructures. These manifold looping effects between biological categories and those categorized contribute to the production of particular kinds of people and turn them into moving targets rather than a stable universal agential platform (Hacking 2006). These looping effects – and the dynamic nominalist stance within which they are rooted – add a layer of complexity to the study of material-semiotic practices. And they are one of the main reasons why the social sciences are so reluctant to engage with biological thinking and methods (cf. Niewöhner and Lipphardt 2006).

A recent editorial in the journal *Nature* commenting on the role of the social sciences in the biological science of environmental stressors remarked:

Many sociologists, however, are still immured in their fortress, struggling to catch up with a debate that has shifted from nature-or-nurture to nature-and-nurture, or are unable to shake off their distrust of scientists, worrying that scientists will force them to play second fiddle in their own territory: the environment. (Nature 2012, 143)

This polemic is certainly ill conceived. Yet it hits a sore spot in the social sciences even if by accident. To say that the social sciences are not taking care of the material world is simply ignoring the entire turn towards a whole set of new materialisms. I have discussed the renewed attention towards the body. Yet social theory and social science more generally could currently hardly be more concerned with the role of the material world and the environment in social practice: the entanglement of nature and culture in material-semiotic practice (e. g. Haraway 1991); nature modelled on culture understood as practice in the discussion of biosociality (e. g. Rabinow 1992); the emergence of nature cultures from genetic engineering technologies (Franklin 2003); the development of a relational materialism in all its variants (e. g. Law and Mol 1995; Barad 1999) including a reconceptualization of “the social” as in continuous assembly through heterogeneous agencies (e. g. Latour 2005); the rise of a new materialism in post-human or non-linear thinking of social and cultural theory (DeLanda 2006; Dolphijn and Tuin 2012). To speak of the social sciences as being immured in their fortresses is thus missing the point.

A theoretical fortress does not exist. Methodologically, however, the picture looks a little different. The social sciences continue to *narrate* the material body and its environment. They operate largely within what one might call praxioscopic regimes, that is, methods that value unmediated interaction between observer and world based on vision and talk (Niewöhner and Beck, forthcoming). Visual and linguistic–discursive methods aimed at

understanding material-semiotic practices are valued above the technologically mediated access that the biological sciences provide to the material body and its environment. These preferences exist for good, long-standing and important reasons: they reflect the theoretical and empirical interests within the social sciences. They have been developed in conjunction with theoretical concerns and they provide stability to epistemic cultures and disciplines. Yet being confronted with the role of the material dimension of social differentiation raises questions that cannot simply be ignored. Neither the material effects of social differentiation nor the material contributions to practices of social differentiation can be adequately understood within praxioscopic or biological methodologies alone. Local biologies demand research designs that oscillate between different modes of knowledge production.

Co-laboration: localizing biology

The preceding paragraphs have demonstrated, first, that a new thought style is emerging within the field of environmental epigenetics. This thought style produces a notion of the human body as embedded in new ways in different temporal and spatial contexts. The human body does not only incorporate matter from its local environment, for example, food or toxins. Neither is it only situated within local discourses and symbolic systems. The very “inner laboratory” of this human body adapts to or “is alive to” the social and material environments within which it dwells (cf. Ingold 2011; Landecker 2011). The body is thus localized in a more fundamental way than either anthropology or biology has discussed so far. This raises questions as to the kind of research that would be able to adequately deal with social and molecular differentiation at the same time. Second, I have discussed the notion of local biology and some aspects of a new materialism as starting points in the social sciences to engage with the material aspects of social differentiation as well as the shortcomings of this approach. Some may argue that much of the work on local biologies has been done in population health and social epidemiology. Both fields bring together social and material variables with health outcomes. While these studies certainly take research conceptually a long way further than genome-wide association studies or narrow epidemiological studies of inequality and health, they tend to remain rooted in indicator-based and ultimately structuralist understandings of society. They neither reflect nor engage current state-of-the-art conceptualizations of social practice or social order – with some noteworthy exceptions, such as the work of Nancy Krieger, who pursues an interesting route of arguing from an eco-social framework for emergent embodied phenotypes rather than innate biology (Krieger 1999, 2012). Yet none of these approaches really engage with the question how biology and social science might work together. In this final section, I, therefore, want to situate my own work as an anthropologist in a molecular biology lab in recent debates in the social sciences about interdisciplinarity, co-laboration and ontography to better understand how localizing biology as a co-laborative research agenda may be conceptualized.

Interdisciplinarity

One might argue that localizing biology is a paradigmatic example of interdisciplinary cooperation. Social science knowledge about social differentiation and biological knowledge about molecular differentiation are brought together to yield a more comprehensive understanding of processes of human differentiation. While the *nature* editorial seems to suggest that this is a possibility, this suggestion can be safely discarded for much of qualitative social science. Biological and social science knowledge stem from incompatible epistemological frameworks, namely positivism and constructivism or variants thereof. Their respective findings do not simply add. Various attempts at bringing social and biological phenomena under one roof have been suggested throughout the nineteenth and twentieth century, most strongly perhaps particularly within German vitalist and holist thought styles (Harrington 1996). The fate of four field anthropology in the USA, of sociobiology or of biopsychosocial approaches all attest to various nontrivial barriers to bridging the great divide of natural and social science disciplines.

Barry and others have suggested a more differentiated perspective. They distinguish three modes of interdisciplinarity on the basis of an empirical investigation of three different kinds of cross-disciplinary collaboration between the natural or technical and social realms (Barry, Born, and Weszkalnys 2008): the integrativesynthesis mode with the rise of biochemistry from biology and chemistry as its prime example does not apply here for reasons discussed above. The subordination-service mode with one discipline delivering expertise or data into another's research framework marks the solution to localizing biology that the *nature* editor would favor. Social science delivers data about "matters social" to biology so that it may be added into the existing experimental system.

The third mode Barry and his colleagues call is the agonistic-antagonistic mode. "[Fi]elds are intended to affect qualitative transformations, experimenting with and establishing new forms of practice that exist in an agonistic or antagonistic relation to, and that may destabilize, existing disciplines and practices" (Barry, Born, and Weszkalnys 2008, 12). The authors suggest that a "privileged relation between the agonistic-antagonistic mode and the logic of ontology" may exist (Barry, Born, and Weszkalnys 2008, 13). The epistemic moment in this mode of interdisciplinarity lies in changing the ontological status of one discipline's research object through engagement with another. Localizing biology is a research agenda that attempts exactly that. It tries to situate the universal human body of biology and it tries to materialize the constructed body of the social sciences. This raises an obvious problem vis-à-vis interdisciplinarity: localizing biology is an agenda that emerges in between disciplines. Yet both biology and anthropology *as disciplines* are ill-prepared to engage with this agenda. Both are keen to see the respective Other integrate one's own ontological commitment, but both also have high stakes in defending their own territory: anthropology defending a particular kind of reflexivity and biology defending objectifying representation.

I believe Barry and his colleagues point toward an important aspect of disciplinary engagement: changing the research object's ontological status. Localizing biology is about changing human biology's ontological status and monitoring the reverberations this change sends through biology and anthropology. Still, I worry about framing this

agenda in terms of (inter-)disciplinarity, because it all too quickly settles into an “arid” and largely rhetorical debate between the synthetic or antagonistic interactions between two disciplined groups of people⁴ (Fitzgerald and Callard 2014, 4). It also suggests that both sides know how the respective other’s object of research should change. I would like to emphasize the experimental nature of the process of localizing biology. I am concerned with the careful increase in thought caprice (Fleck 1935) within established thought styles, how that can never simply be forced upon one discipline by another and how this needs to go through a joint process of producing reflexivities that each discipline then needs to appropriate on its own terms. To do so, I develop here the notion of co-laboration and embed it in recent debates on ontography and “para-sitic” collaboration (Marcus 2000; Lynch 2013).

Ontography

The ontological turn has been proclaimed recently in STS to indicate that knowledge production always loops to have ontological consequences (Escobar 2007; Woolgar and Lezaun 2013). In close conjunction with this turn, many have started empirical research investigating the ontological multiplicity of phenomena (e.g. Asdal and Moser 2012 and the related special issue). While in some circumstances this may be extremely productive in breaking up established epistem–ontological configurations (Mol 2002; Barad 2007), others have rightly cautioned against an a-priori commitment toward the multiplicity of reality (Lynch 2013). They advocate ontography as the empirical investigation of the ontological status of phenomena as the more prudent research strategy. In this sense, localizing biology must not be understood as a normative commitment and demand from anthropology toward biology. It is not in this sense an interdisciplinary agenda to change biology. Rather it is an empirical question that has the ontological status of human biology as its research object. Only in this mode could I approach my lab group during the ethnography. They were not particularly interested in biopolitical critique. While as “private individuals” they were sympathetic to the points I raised, as “professionals” their science did not benefit from and could not constructively handle this critique. Nor were they keen on a normative position suggesting to them from the anthropological high ground that they ought to localize their biological research objects. Yet suggesting the degree of “local-ness” or the process of localizing the human body as an empirical question made immediately sense to them and led to fruitful discussions. In fact, it was a behavioral psychologist in the group who raised the point that in the end the degree of “local-ness” of the human body would as much depend on the type of intervention in the researcher’s mind than it would on “reality”. Hence engaging in joint ontography as part of the lab ethnography moved us both forward in our thinking.

⁴I very much appreciate that this is not what Barry and his colleagues have done in their own actual work. My concern lies with the framing of the debate that takes over whenever the “regime of the inter” is called upon (Fitzgerald and Callard 2014).

Co-laboration

This type of joint epistemic work on how “localizing human biology” might make sense within two disciplinary thought styles is at the heart of what I want to call collaboration. Co-laboration is temporary joint epistemic work. It is non-teleological in character. It does not require a shared outcome. It is an undertaking with a disciplinary vanishing point that requires participants from different epistemic cultures. It may well be experimental in the sense of conducting experiments of various types together. In the case of my ethnographic work with epigeneticists, the agenda of localizing biology has emerged from co-laboration. It has emerged from talking through biological findings from their (and others’) experiments, talking through findings from the history of anthropology and the notion of local biology, and talking through alternative research designs. The outcome was not known to any individual participant in advance but emerged in the co-laboration. It was not about one convincing the other. And it is not a straightforwardly shared agenda. Biology will continue to conduct experiments that anthropologists will find problematic and reductionist with regard to the notion of “the social”. Anthropologists will continue to produce thick narratives about the human body that biologists will find idiosyncratic, anecdotal and a-material. Yet the notion of “localizing biology” will be out there providing resistance to particular forms of reduction.

Of course, the epigeneticists have already been working in a service mode of interdisciplinarity importing social science expertise as part of an epidemiological data set. There have also been moments of antagonistic interdisciplinarity during my stay, when I criticized members of the lab group for jumping to conclusions about social configurations all too quickly with the aim of making them change the ontological status of the human body. There has also been joint talk about the ethical, legal and social implications of the research agenda “molecular biology of social position” after the experimental findings were in. Yet I insist on co-laboration as something that goes beyond interdisciplinarity and an ELSI⁵ agenda in that it tries to set in motion an experimental process aimed at inducing reflexivity in all participants (cf. Boyer, forthcoming) – whatever participants and their respective disciplines will do with that in the end. In this sense, co-laboration is a disciplinary undertaking, not an interdisciplinary one. It is aimed at producing reflexivity that challenges the dominant thought styles within a discipline to move that discipline along.⁶

I need to justify the necessity for the neologism “co-laboration”. First of all, I want to distinguish co-laboration from collaboration. The latter has been established in the context of public sociology and anthropology and marks a mode of research in close collaboration with one’s field of research (e. g. Lassiter 2005). I find this problematic as it conjures up the problematic notion of the *collaborateur*, that is, the one who cooperates with the enemy – not necessarily the prototype of a participant in a shared epistemic process. Collaboration is also based on shared intentions and goals between researcher and the one formerly known as informant. Co-laboration on the other hand evokes

⁵Ethical, legal and social implications of a particular development.

⁶In Barry’s terminology, this would be a kind of epistem–ontological service, but I do not believe that this is what Barry had in mind, when he wrote service.

associations with laboratory and labor, that is, the process of transient, non-teleological joint epistemic work without the commitment to a shared outcome. The notion of co-laboration thus relates more to what Marcus and others have discussed under the notion of para-site and epistemic partnership (Marcus 2000, Rabinow et al. 2008). Paul Rabinow provides an example of this mode of working through his co-laboration⁷ with the newly founded Synthetic Biology Engineering Research Center Synberc. (Rabinow and Bennett 2012, compare also with a similar initiative by Nikolas Rose in London, UK). Rabinow starts in ontographic mode revealing how the institute is involved in producing biological objects of a new ontological status and that these developments have potentially far-reaching consequences. Yet his concern is not with exploring and intervening into the ontological status of these new objects on the basis of anthropological knowledge but rather analyze the ethical implications of their coming about. He suffers through the frustrations of biologists not sharing his ethical concerns.

His mode of working, however, oscillating between involvement and reflection, engaged with the field not only as experts but also as technicians of general ideas (cf. Rabinow et al. 2008) exemplifies how I think localizing biology needs to be undertaken. Rather than focusing on biology as a set of human practices, my focus has been on using co-laboration to create what Ludwik Fleck called a *Widerstandsavis* (Fleck 1935/1979), that is, a resistance of reality to the thought constraints of a particular thought collective. “The world kicks back” as the current jargon might say (Barad 1998). Co-laborating with environmental epigenetics has only just begun with the project reported here. The project funding did not allow a proper para-site to be set up. Yet a process began through which the participants from anthropology and biology tried each other’s perspectives. Ideally, one would create co-laborative experimental spaces where the participants could try each other’s ways of being in the world or each other’s worldings (Tsing 2010): exchange concepts, theoretical lenses and methods. “Seeing like a biologist” (Scott 1998) becomes an experimental moment from which an anthropological participant can derive reflexive potential to take back into her own discipline.

It is important to note that co-laboration is a two-way process. It has an anchoring point in anthropology through the work of Margaret Lock and her colleagues and challenges anthropologists to consider the material aspects of the human body and social phenomena without naturalizing them (cf. Landecker and Panofsky 2013; Meloni 2014). Judging by the reception that this line of argument got in the last few years in anthropology, the *Widerstandsavis* is working: discussions abound about the role of materiality in social theory. As for the effect of the *Widerstandsavis* in molecular biology: here I share some of Paul Rabinow’s frustration in that “my” group saw the point of it. Yet being sympathetic to the approach and finding money to set up a co-laboratory are two different things. I do not believe that we are about to see major funding for investigating the local nature of human nature in the biological sciences.

Co-laboration as an epistemic style is only in its infancy. It is currently a highly individualized affair usually funded asymmetrically through social science or – in case of

⁷Paul Rabinow himself speaks of *collaboratory*, but for the above reasons I prefer the less military verb to *co-laborate*.

natural science funding – as a matter of earmarked “ethical, social and legal implications of x” funds. The debate on the methodological value of co-laboration – or experimental entanglement as others have called it (Fitzgerald and Callard 2014) – has only just started. It needs to be explored systematically in its empirical facets and with regard to the type of results it produces.

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