

Chapter in an edited volume

Co-laborative anthropology
Crafting reflexivities experimentally

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Introduction: why co-laborate?

Anthropology is concerned with lived sociality, i.e. humans living in groups. It wants to understand how humans arrange themselves within particular environments as well as the dynamics of these arrangements. This is a straightforward research question – and a grand challenge. It is particularly grand for social and cultural anthropologists considering that they tend to work alone with little more support than a field notebook and a pencil. It is thus perhaps disciplinary prudence that most practitioners of anthropology today consider addressing this question futile if not dangerous. The history of anthropology shows how attempts at grand explanations or narratives remain contingent and ethically disputable. Practitioners feel more comfortable in their respective specialty niches. Yet shying away from the question does not mean that the question disappears.¹ I argue here that anthropology should not be offended by the fact that sciences and publics alike continue to find this question interesting. Instead anthropology should engage those who are trying to find answers and convince them with anthropological reasoning that grand challenges require humble responses.

Questions about human group life and human nature are today as prominent as ever. They are answered not by anthropology but by the life and earth sciences. These heterogeneous fields are driving our understanding of life itself in the anthropocene²: What makes us us and how does that shape the planet? In its core, the life and earth sciences are positivist natural sciences driven by large research platforms: genome sequencing and brain imaging in the life sciences, remote sensing and climate, ecological and integrated assessment modelling in the earth sciences. Both fields are becoming increasingly concerned with the fact that their once solely material research objects are embedded in and indeed infused with social and cultural contexts in significant ways. They extend invitations to the social sciences and particularly to anthropology to join their agenda: social and cultural neuroscience on the one hand, various social-ecological agendas on the other.³

In both fields, the response on part of the social sciences has been and continues to be in one of two modes of engagement. Critical engagement deconstructs the epistemic regimes to reveal illegitimate reductions of the richness of human group life to material quantities. Ebullient engagement joins the

natural sciences on their terms and provides empirical data about 'the social' to established positivist frameworks while redressing social theoretical concerns in the light of natural science findings.⁴

A third mode of engagement is necessary. It is necessary, because questions of the nature of human nature, sustainability and planetary boundaries are too fundamental and too anthropological to be left to natural scientific frameworks of inquiry. It is also necessary, because at least some of the findings emerging from the life and earth sciences offer rich food for anthropological inquiry and theorising. And it is necessary, because both fields of research are deeply entwined with normative agendas around health and sustainability that are shared by many in anthropology. If anthropologists care about lived sociality in all its facets, the life and earth sciences are less Other than their current treatment by anthropology suggests.

This third mode of engagement is currently taking shape. In this chapter I label it 'co-laborative anthropology'. With co-laborative, I mean temporary, non-teleological, joint epistemic work aimed at producing disciplinary reflexivities not interdisciplinary shared outcomes. The neo-logism co-laborative conjures up associations with laboratory and experiment as well as with labor. It is distinct from collaboration. I proceed in three steps to argue that

- co-laboration addresses formative anxieties in the discipline;
- co-laboration is nothing new (certainly not another 'turn') but anchored through rich genealogies within the history of anthropological research;
- practices of co-laborating help to diversify existing notions of reflexivity and critique, thereby broadening the analytical spectrum and adding interpretative degrees of freedom.

I conclude by arguing that the signature mode of knowledge production in anthropology – thick, reflexive inquiry – today needs to take co-laborative forms to remain productive and credible.

Addressing anxieties in anthropology

German European Ethnologists learn in their undergraduate classes about two field-related anxieties that have been formative for the discipline. The underlying topoi are discussed widely in anthropology internationally, but – as far as I know – only the debate in German European Ethnology refers to them in terms of anxiety.

The first anxiety is the anxiety that begins to befall the researcher right before the first contact with the field.⁵ "Will they like me?" is the question at the heart of this anxiety. Everyone who has

conducted fieldwork will recognise the specific feeling before entering a research field for the first time. The initial contact carries the risk of establishing a wrong-footed relationship with actors in the field that will stick and render fieldwork less than productive. Or – even worse – it carries the risk of being refused access altogether. Many qualitative social science textbooks on method hence suggest strategies to minimise these risks alluding either to the ideal of an invisible observer or to good manners in general. Anthropology has largely agreed upon a very different response. This anxiety needs to be taken seriously and the ethnographer can learn from it. Fieldwork is always and necessarily a social and interactive encounter that is structured by a whole range of supra-individual factors, e.g. social fields, discourses, power/knowledge. It cannot be conceived as a mechanical exercise of data extraction without running into ethically and epistemically questionable territory. Hence reflecting and perhaps understanding how one is positioned in the field is already the first piece of empirical material.

The second anxiety is the anxiety that occurs when “studying up”.⁶ ‘Will they tolerate me?’ is the crucial question. Already in the 1960s, Laura Nader suggested that studying elites was an epistemic, political and ethical necessity. Until then, fieldwork had been largely characterised by an asymmetry of power and knowledge in favour of the researcher. This has changed with fieldwork in elite settings. Suddenly informants have a higher degree of formal training than the researcher, they have little time, earn more money and are seldom interested in being disturbed by the presence of a fieldworker. Not gaining access or being treated harshly characterises this anxiety and the response is – more often than not as the history of the discipline shows – to look for a less powerful field. Those who choose to study up go the long road and study up on the field and its logics. This reduces the risk of looking like a naïve fool and, more importantly, it increases the chances of being able to understand how the field operates: the native language if you will. This enables the researcher to pursue research questions to do with the ‘content’ of the field and not only with its social form.

These two anxieties are well-known and their consequences have been discussed in depth as a matter of ethnographic method. Yet a third anxiety has been emerging somewhat more recently, the consequences of which are less obvious. ‘Will I have anything new to add to the native?’ is the crucial question.⁷ Dominic Boyer thinking through the anthropology of experts asks this question taking his cue from Ulf Hannerz’ work with foreign correspondents.⁸ The scenario they describe is one where the field under investigation operates with similar thought styles as the anthropologist her- or himself.⁹ In such cases, access to and position in the field may be unproblematic as both – observer and observed – share many concerns and perspectives. Yet the problem arises after the fieldwork phase when it turns out that the actors in the field knew all along what the anthropologists proudly present to them as their findings. Adding to the natives’ point of view becomes difficult. Pretending

to be able to may even seem presumptuous or impudent. Hannerz and Boyer both suggest that in such cases 'studying sideways' is the apt response, i.e. letting go of the pretence of some kind of (meta)-reflexivity and engaging in more cooperative formats.¹⁰ I want to argue that this analysis and this third anxiety does not only apply to an anthropology of expertise or an ethnography amongst intellectually like-minded people. Rather anthropology in general is today drawn into fields of research that are shaped by specialised knowledges and by different forms of reflexivity.

The rising importance of knowledge practices

Knowledge practices and have certainly been a key analytical category in anthropology since its beginnings in Boasian interest in language. And the recent surge of literature on the anthropology of knowledge has already attracted critics arguing that 'knowledge' is the new 'culture', i.e. an analytical category so all-encompassing that its use is compromised.¹¹ Nevertheless, that people in the Global North thrive and falter in knowledge societies and knowledge economies is a diagnosis not easily disputed.¹² And the fact that transnational and increasingly global entanglements through mobility and infrastructures have reached an unprecedented degree and speed, has made not only anthropology aware that information and knowledge have become global currencies.¹³ It is also clear that knowledge is a currency intricately tied up with power and the micro-politics of discourse and social order.¹⁴ And, lastly, the increasingly socially and geographically distributed production of knowledge also indicates that established anthropological frames of centre and periphery may no longer be able to adequately capture current – often highly disputed – flows of knowledge.¹⁵ Knowledge has thus become an established analytical category in cultural and social theorising.

From a different analytical angle, knowing as a way of being-in-the-world is an increasingly important (phenomenological) mode of existence within the ecologies of knowledge societies.¹⁶ Thus anthropology is paying attention to knowing as a relational practice, i.e. as a particular way of relating different kinds of actors to each other, which is qualitatively different from other relational practices such as informing, feeling, recognising or ruling.¹⁷

Reflexivity

Many research fields today are not only knowledgeable, they are also reflexive. My background is in science and technology studies as well as ecological and urban anthropology. So this is a partial perspective. Yet I do believe that this knowledgeable reflexivity is not restricted to highbrow

laboratories. Rather it is symptomatic for late modernity¹⁸ and the kind of questions that anthropology finds interesting these days. Similar diagnoses have been made albeit from different perspectives and with a different purpose. I have mentioned Laura Nader and Ulf Hannerz. Yet the writing culture debate was rooted in similar concerns and recent advances in post-colonial, critical whiteness or queer theorising also diagnose a more symmetrical distribution of knowledge and reflexivity between researcher and field.

On the one hand, reflexive actors within fields are great informants and collaborators. Much of what will be discussed further below as collaborative ethnography or public anthropology thrives on just this reflexivity. It transforms into partners with shared goals what has previously been discussed as informants. On the other hand, however, reflexive fields, e.g. hospitals, science labs, creative networks, artist studios, investment banks, NGOs, law and consultancy firms, regulatory bodies or high tech industries, are anxiety invoking for the reasons discussed above. They are full of clever people and they often nurture institutionalised forms of reflexivity as well as self-reflexivity. They leave the anthropologist with nothing to add. What could be scarier than that?

Learning from anxiety: co-laboration

I propose co-laborative anthropology as a necessary mode of anthropology that responds to this anxiety of having nothing to add. Some argue that anthropology still possesses a particular form of reflexivity that is in great demand in many fields of research; that post writing culture anthropology is producing researchers that carry a remarkable and productive ability to question established categories and boundaries. I have difficulties with this opinion. Either it is meant to suggest that anthropology carries a somewhat superior form of reflexivity compared to those reflexive subjects and institutions in its research fields. That seems absurd to me. Or it is meant to suggest that it possesses a different kind of reflexivity from others. Now that argument I take seriously but I would suggest that this reflexivity is not an intellectual or cognitive feature acquired by the anthropologist through learning understood as information intake. It is not a mentalistic reflexivity related to the anthropologist's training and experience of the Other as so often suggested from within anthropology. Rather it is a reflexivity grounded in mobility as German sociologist Stefan Hirschauer argues:¹⁹ mobility between theories, fields, colleagues, institutions, thought styles and ways of worlding, if you will. Reflexivity as mobility is part of what co-laborative anthropology is about. It is about creating space and infrastructure for 'reflexing' as a collective epistemic activity. Organising category and boundary work, representation, problematisation and intervention, analysis and interpretation as a collective epistemic practice is something that needs to happen in co-laboration

with research fields. This is not only a social and interactive process. Working together is always a material *and* semiotic practice: it involves technologies, infrastructures, material culture in a broad sense and it is an embodied practice. In the following two sections I will flesh out this argument by, first, providing genealogies for the notion of co-laboration suggesting that this is by no means a new idea or practice within anthropology. It is certainly not some kind of collaborative turn. Second, I will detail four important facets that I consider important about co-laborative practices.

Informants have always been collaborators

Ethnographic fieldwork is often portrayed as an individual accomplishment – not only in the history of the discipline. Even further: Only in the individual exposure to the Other can the anthropologist experience the fundamental alienation from her or his own culture that serves as the epistemic moment in the discipline. Or so the rather heroic story is often told. I do not want to belittle individual fieldwork. The practical, personal and epistemic accomplishments are often significant. Yet over the last few years, a different story has been emerging. This is a story that does not try to diminish the accomplishments of individual fieldwork, but analyses the processes and practices of their production. Specifically, the role of the local informant is under investigation. It appears that many of these informants were much more than mere translators or local brokers of contacts and information. In the language of science and technology studies: They were mediators rather than mere intermediaries.²⁰ Information did not simply pass through them unchanged from the ‘local culture’ to the interpreting anthropologist. Rather informants were active collaborators in joint knowledge production. The different strands of American, British and French cultural and social anthropology as well as European Ethnology would really need to be considered separately with respect to the role of informants. Space does not permit this individual treatment here. Suffice to point out three aspects:

Many of the early ethnologists in US American ethnology on its way to cultural anthropology, who worked within the institutional frame of the Bureau of American Ethnology (BAE), relied heavily on key informants.²¹ In the 1860s and 1870s L.H. Morgan and other BAE researchers worked closely with American Indian subjects. There were two important sides to this collaboration: On the one hand, it remains somewhat unclear to what extent Morgan and others profited from and relied upon interpretative input from local subjects without acknowledging it. It is hard to disentangle to what extent epistemic collaboration took place and how. On the other hand, the American Indians who got involved certainly did not do so without pursuing their own agenda. They were by no means naïve locals that could be studied for a fictionalised Indianness, but rather were actively involved in

Indian-American political conflict.²² Hence local informants did not simply channel information to the anthropologist and the anthropologist did not simply study a local group's way of life. Both acts were closely related acts with a more or less open political-normative agenda. They were collaborative acts pursuing shared goals.

Other constellations in early US American Ethnology emphasise the epistemic rather than the political aspect of collaboration. In the 1900-1920s, Franz Boas relied heavily on the expertise of James Teit, a Scotsman with Northwestern knowledge, who enabled Boas' work with Inuit groups. And Alice Cunningham Fletcher began to acknowledge Francis LaFlesche as a co-author in her extensive work on the Omaha tribe.²³ LaFlesche started as a field assistant and interpreter and in the end was adopted by Cunningham Fletcher as her legal son.²⁴ These types of collaboration disappeared from US American Ethnology with the professionalization of anthropology and the rise of the methodological standards developed in British and French social anthropology.

In addition to very close one-to-one relationships, most fieldwork relied on an extensive local network of other-than-anthropologist researchers and research-affine supporters. These included traders, missionaries, diplomats and many others. And in the case of large research and trade expeditions, these also included fellow scientists from a diverse range of disciplines with various kinds of expertise and (local) experience.²⁵ Few contributions to early anthropology were thus made as individual efforts. Rather the nature of the epistemic culture has always been one of manifold collaborations between researchers and various informants, including practical support, political agendas and joint knowledge production.

Two further strands to this genealogy need to be mentioned at least in passing. Firstly, British social anthropology as well as the early German *Volkskunde* and later European Ethnology have all been working historically and ethnographically in their home countries. The remoteness of the culture to be studied has never characterised these disciplines to the degree that it has shaped US cultural anthropology. Gluckman and colleagues at Manchester challenged structural functionalism with their extended case method, which informs much of the long-term community studies conducted by anthropologists in the UK.²⁶ The major 'Volkskunde' projects in Germany such as the 1930s 'Atlas der deutschen Volkskunde', during the GDR era the 1970s project 'Magdeburger Börde' or the 1970s and 80s Southwest German long-term community project in the village of Kiebingen²⁷: all of these research efforts relied heavily on the collaboration with local informants. Yet these informants were not in any way cultural Other. While they were not necessarily epistemic partners, they were more often than not collaborators in a shared social and cultural context. The question how analysis and interpretation were distributed amongst anthropologists and informants, i.e. the question whose analysis and interpretation is it anyway has largely remained a moot point.

Post-Writing Culture: dialogue, intervention, collaboration

Hence collaboration is not new, neither to early US cultural anthropology nor to various traditions of European anthropologies. Much that is of relevance to collaboration in the later history of the discipline has already been discussed in the context of the crisis of representation. The 'writing culture' debate and particularly the many strands of feminist and post-colonial critique deconstructed the relationships of power and identity in practices of fieldwork and, most importantly, forms of textual representation of the Other.²⁸ As George Marcus summarises: "(1) The researcher subject distinction is problematized and re-configured resulting in increasingly dialogic ethnographic approaches in various modes. (2) The textual representation is broadened away from an orientation on classic male ethnographies and towards a use of different textual genres. (3) The politics of representation thus become contested ground, so does the one-directional flow of information from field to text."²⁹ The rise of dialogic approaches in 1980s anthropology as an attempt to increase the diversity of experiences visible in anthropological representations was only a beginning. Over the last two decades a whole range of engaged, public, collaborative, critical or interventionist approaches has developed in anthropology or rather within a much more open epistemic culture that includes feminist critique, gender and queer thinking, post-colonial studies, human geography, cultural and urban studies, science and technology studies and social and cultural anthropology itself.

In most of these approaches, collaboration has been born out of primarily ethical and political concerns. While it is difficult to summarise such heterogeneous movements and research efforts, two elements appear to be central to this sense of collaboration: First, it denies the researcher the *a priori* primacy of interpretation over social phenomena. The negotiation of who may say what about whom becomes part of the ethnographic process. The researcher is primarily accountable to the field of research. Second, the boundaries between analysis, critique and intervention are deliberately performed as blurred. This blurring reaches from deprioritising knowledge production altogether in favour of political change³⁰ to a forceful situating of knowledge production not only in social, political and historical but also in biographical, personal and emotional terms. Moderate positions acknowledge this extensively situated nature of knowledge production, but emphasise accountability to a disciplinary body of knowledge and towards an institutionalised set of practices.³¹

In summary: anthropology today faces new conditions for the social (re)production of anthropological knowledge. More collaborative modes of knowledge production have been suggested primarily for ethical and political reasons, deconstructing the heroic stories of individual fieldwork and acknowledging more fully the many so far often hidden contributions from other

disciplines and local informants alike. The epistemic consequences of these developments, however, have not been fully thought through. As Boyer discusses in Paul Radin's work, this is about epistemically appreciating the social and intellectual constellations of knowledge production: "[...] all anthropology is in some ways already the anthropology of intellectuals. Any process of anthropological representation attempts to formalize knowledges of another social environment and involves the cultivation of intellectual exchange systems to mediate the acquisition and evaluation of local knowledge. In these exchange systems, anthropologists typically rely heavily upon the intellectual labors of local knowledge-specialists who provide not just testimony but rather cultural analyses that prefigure and orient the interests and language of the anthropologist [and vice versa]." ³² Appreciating these intellectual labours changes the epistemic culture and the formats within which ethnographic fieldwork is conducted. In the following section, I outline co-laboration as a way of epistemically acknowledging these changing conditions of anthropological knowledge production.

From collaboration to co-laboration

To begin with, I want to distinguish co-laboration from collaboration. This is a difficult and perhaps futile task, because the notion of collaboration is being used in very different contexts and with very different meanings and connotations as outlined in the genealogies above. My concern here is solely with a differentiation between co-laboration as a primarily epistemic mode and collaboration as a primarily ethical, political and public mode of anthropological research. Public anthropology is perhaps the better and most comprehensive label for a range of collaborative agendas. Public anthropology's essential argument is that anthropology ought to primarily strive to further our public rather than scientific understanding of human kind, activity and co-existence. Anthropology needs to strive to not only know but to change the world, to address the manifold injustices apparent to many doing research in the field and to help improve the living conditions of those traditionally positioned as informants. It argues in favour of a need "to serve humankind more directly and more immediately. [...] Collaborative ethnographic practice has the potential to pull academic and applied anthropology, feminist and postmodernist approaches, and Americanist and other anthropological traditions into the same stream, fashioning an engaged anthropology that [...] 'prob[es] the deep mysteries of the human species and the human soul' and encourages us to press outward, mobilizing our work and ourselves to make a difference beyond the discipline and the academy." ³³

It seems to me that trying to systematically define engaged, collaborative, reflexive and scientific practices into neat categories is an unproductive undertaking. They occur in various mixtures in

different contexts and projects and they all together make up anthropology. I am, therefore, not interested in demarcating co-laborative anthropology from applied, reflexive or engaged anthropologies as a matter of disciplinary identity. I conduct this boundary work with collaboration here because co-laboration and collaboration are similar words and most anthropologist will know collaboration in its public anthropological sense. My interests and intentions with co-laboration have some overlap with public anthropology but place a different emphasis.

I understand co-laboration to mean temporary joint epistemic work with two crucial emphases that distinguish it from collaboration: (1) Collaboration usually rests on a shared goal or at least intention – a similar sense of being-in-the-world perhaps. Hence collaborators are almost exclusively people or institutions that share political or social goals with the anthropologist and that now pool resources and capabilities to achieve those goals. Few collaborative projects are thus realised with the ideological Other, e.g. investment bankers, law firms or the military. This seems to me to be a bias that is hard to justify. Co-laboration on the other hand contains the concepts ‘labour’ and ‘laboratory’. It is not about a joint *opus* as in cooperation, but about a shared process of labour. And it is always experimental in character: without telos. It is about conducting joint epistemic work, experimenting with formats, without necessary aiming for a shared goal. A shared value system within co-laborative work is common but not a necessary prerequisite. (2) Co-laboration is a distinctly disciplinary project. It is not primarily aimed at changing the world or producing something permanent in between disciplines or actors. It is about experimenting with different ways of seeing- and being-in-the-world with the purpose of advancing anthropological knowledge production. It is a process designed to help anthropologists curate concepts that are good to think with.³⁴ In the following I want to outline four facets of this co-laborative mode of knowledge production.

From reflexive anthropology to anthropological reflexivities

Anthropology is centrally concerned with understanding the production of categories and their social consequences and efficacy. As such it is at its core a reflexive discipline that derives its epistemic momentum from using scientific and native concepts critically rather than applying them in a positive fashion only. Anthropological analysis applies concepts, but anthropological representations always afford the critique and development of these concepts. This is not unique to anthropology. Most disciplines harbour at least a small section, usually labelled history and theory of that discipline, that is primarily concerned with situating and critiquing the dominant thought styles within the disciplinary collectives. What is relatively unique to anthropology is the fact that this iridescence

between positive and reflexive conceptual work is standard practice within its main method: ethnography.

The difficulty arises when this reflexivity solidifies into a gesture, a “professional self-critique” of “the disciplinary pretension ever to have produced anything that could count as genuine knowledge given the prevailing ethics of anthropological connectivity”³⁵ – what Boyer describes as the downsides of second generation reflexivity. Boyer rightly points out that ‘reflexive anthropology’ (as opposed to an engaged anthropology) cannot be a subfield of anthropology.³⁶ Anthropological reflexivity, however, is increasingly central to the discipline “as part of the transformation and pluralization of social knowledge formation in Western intellectual cultures. Reflexivity [...] is not, or not only, a voluntary condition of intellectual practice (e.g., a particular critical or analytical disposition one chooses) but also an ideological reflection of the growing phenomenological and epistemological salience of contingency of intellectual culture.” Boyer argues that anthropologists need to be aware of the conditions and practices of the social reproduction of knowledge and how these interact with their fieldwork and their own knowledge production.

Reflexivity in this sense has nothing to do with the search for a normative scale. Instead, reflexivity is understood as an experiential and experimental problem, as curiosity how and why ‘they’ and ‘I’ are interested in what we are and how we are interested in it.³⁷ It is the primary purpose of co-laborative anthropology to produce spaces, formats and modes of knowledge production that enable this kind of reflexivity. This crucially depends on moving away from the model of the individual project as the standard mode of anthropological knowledge production. The individual project rests “on a myth of *sui generis* intellectual production. The individual project model assumes that interpretive and authorial virtuosity is the mainspring of good work.”³⁸ In co-laborative anthropology the emphasis is instead on mobility in the all-encompassing sense Hirschauer addresses: mobility within research fields, within empirical material, between material, literature and research question and between colleagues.³⁹ This mobility produces the iridescence between field, material and theoretical concepts that is anthropology’s signature mode of knowledge production. It enables conceptual work within and through empirical materials.

To go one step further means trying to work within and through empirical materials already as part of the *mise-en-scène* of fieldwork itself. This raises the idea of the para-site as experimental and epistemic encounter and way of “doing theory in continuous relation to the distinctly non-‘meta’ immersive quality of thinking during fieldwork”⁴⁰:

The para-site is thus a kind of deliberate experimental interruption or ‘disruption’ in the field research process with the intent of staging a reflexive (and potentially collaborative) encounter between research partners: ‘It embraces the opportunity to deal in unsettled working

concepts, analytic strategies, and ethnographic ways of thinking that the fieldworker may appropriate critically for her own eventual individual purposes.”⁴¹

Para-sites are designed in the hope of producing reflexive interventions. My notion of co-laboration is largely congruent but for one aspect: I do believe that co-laboration is about developing and experimenting with different reflexivities or rather ways of being or becoming reflexive. While there is a clear sense within the debate that a mentalistic notion of reflexivity misses the point, many of the alternative strategies nevertheless focus on thinking and on language. Boyer, for example, suggests the vernacularisation of anthropological language as an important step towards broadening anthropological connectivity and hence collaborative reflexivity. I could not agree more. Yet I think we need to go beyond language and consider a whole range of different ways of being-in-the-world that foster different ways of becoming reflexive – of ‘reflexing’ if you will. There is no reason why anthropologists should operate only within the framework of fieldwork instead of trying out experimentally different ways of seeing- and being-in-the-world. This includes first and foremost different modes of data gathering and analysis: seeing like a survey, seeing like a statistical model or seeing like remote sensor on a satellite all provide different experiences of being-in-the-world, suggest different ways of problematizing world and thus also broaden interpretative flexibility. And there is no reason why co-laborators should not go beyond talking to anthropologists but also trying different methods of field research to appreciate how anthropology problematizes phenomena.

From critical distance to experimental worlding

The notion of critique is intricately tied to the debate about reflexivity. Labelling an approach in anthropology ‘critical’ is common. ‘Critical research on X’ usually means that X is already being researched by other disciplines (or known more generally by powerful actors or discourses). Usually, these are disciplines that produce positive knowledge about X such as economics, law or psychology. They are disciplines operating within a strong theoretical and methodological framework that produce authoritative narratives about X. In their mainstream, they tend not to be curious about how they have come to be interested in X and why in this particular way. They appear to lack reflexivity.⁴² ‘Critical research on X’ attempts to introduce this reflexivity to the study of X. Co-laborative anthropology shares this intention, yet it suggests a different practice. Critical research commonly draws on a dialectic tradition. Its critical momentum resides within the deconstructive move that reveals the contingency of the hegemonic representations of X. Reflexivity here is the ability to stand back from X, to achieve critical distance. This entails taking the hegemonic

representations of X at face value and revealing their contingency vis-à-vis alternatives. Reflexivity means having access to an epistemological meta-level of analysis at which the insufficiency of the current way of organising and representing X becomes visible. Such practices of being critical are often radical in the sense that they operate with utopian alternatives and a strong normative scale. They are important in anthropology, because they open up spaces for thought. They are, however, ill-suited for co-laborative work, because co-laboration with fields requires operating closely to the relevances and logics of these fields rather than starting from an outright negation of these aspects.

Critique within a mode of co-laboration relies on reflexivity as curiosity about the genealogies and the performativities of theories, concepts and methods used to represent X. It is less concerned with thick description in a Geertzian sense, deconstruction or identity politics.⁴³ It resists the temptation to operate with an epistemological meta-level, but rather stays within the practices on the ground trying to develop a generative mode of critique.⁴⁴ It operates within the very ecologies of practice that it studies.⁴⁵

My conviction that this mode of critique is important for anthropology today stems from two concerns: The first one comes from French anthropologist Bruno Latour. He is making the simple but powerful point that political processes in knowledge societies tend to be understood as processes of representation.⁴⁶ Hence knowledge, i.e. facts, are produced within the sciences and are then debated within political processes. Instead of focusing on the political life of facts, Latour demands, the analysis should move forward to *matters of concern*, i.e. an investigation of the political nature of practices of fact-making. I find this to be a powerful argument and it is not only about fact production. It is more generally about the question how phenomena are problematized in knowledge economies. How do certain phenomena come to be known and come to be a problem? Rather than being critical about the dominant representations of X, co-laborative critique is curious about how X has come to be X and how and why it stays X.

The notion of representation needs adjusting in light of debates at the intersection of feminist science and technology studies (STS) and anthropology. Representation assumes that a relevant reality exists outside of its specific epistemic representation. Ethnographic, practice-theoretical and performative thinking in STS insists that scientific knowledge has ontological consequences.⁴⁷ Knowledge loops and in the process makes up people and worlds.⁴⁸ Karen Barad thus even speaks of 'phenomena' as the primary analytical and ontological unit in the world:

The primary ontological unit is not independent objects with inherent boundaries and properties but rather phenomena. In my agential realist elaboration, phenomena do not merely mark the epistemological inseparability of observer and observed, or the results of measurements; rather, phenomena are the ontological inseparability/entanglement of intra-

acting agencies. That is, phenomena are ontologically primitive relations - relations without pre-existing relata.⁴⁹

Knowledge production is thus much more than the representation of a separable world. It is material-semiotic practice and as such deeply situated within 'culture' as practice – like a string figure.⁵⁰ Anthropologist Anna Tsing takes her cue from this thinking to introduce the notion of worlding as the “always experimental and partial, and often quite wrong, attribution of world-like characteristics to scenes of social encounter.”⁵¹ Thought styles can thus be usefully understood not as trivial machines of representation but as patterns of practice constituting relations such that they gain world-like characteristics.

It seems sensible to me to broaden Latour's analysis of matters of concern with its emphasis on the politics of fact-making to the analysis of (the politics of) worlding. Much of that worlding happens not at the level of hegemonic representation or discourse but in what we might call infrastructural practices and debates, i.e. highly technical, scientific, legal and economic debates and processes that are rarely accessed by anthropology. Traditional ethnographic fieldwork tends to come in further downstream, when problems arise, because a dominant worlding does not fit lives as lived. Critique then often means pointing out this misfit. Co-laborative critique wants to move upstream into the infrastructural practices to ask about the legitimacy of choices that are made during infrastructuring.⁵² This is not radical critique from an epistemologically privileged standpoint. It is co-laborative critique within the practices, relevances and logics of the field.

My second argument in support of an alternative notion of critique takes me to Dutch empirical philosopher Annemarie Mol and while it is somewhat polemic and provocative, it illustrates well the concerns outlined above. Mol argues that what really happens when one views phenomena from a critical distance is that they become smaller. Less detail is visible. Things may even go out of focus.⁵³ This emphasises Hirschauer's point about reflexivity as mobility. The anthropologist is no longer modest witness with access to another plane of epistemological truth.⁵⁴ She rather relies on modest 'witness' as the *sine qua non* of reflexivity.⁵⁵ Critical distance is replaced with comparative involvement.⁵⁶ Putting different worldings next to each other, relating them to each other, becomes the basis for reflexive knowledge production and hence for critical engagement with and through X.

Co-laborative anthropology is about constructing spaces or processes that allow anthropologists to try out and experiment with different worldings. This is necessarily incomplete and it is not meant in any existential or deeply phenomenological sense. Neither is it simply about dialogue. It is about trying out methods, ways of seeing the world, experiencing new phenomena in Barad's sense, i.e.

changing the mediators that relate and entangle observer and observed. It is about experiencing and experimenting with worldings other than seeing-like-an-ethnographer.⁵⁷ The epistemic momentum lies in the comparative involvement in, with and through different worldings. Thus co-laborative anthropology is about bringing people together who in their normal lives are involved in different worldings, i.e. who are situated differently vis-à-vis social phenomena or who contextualise phenomena altogether differently, because of method, theory or walk of life. Rabinow makes the important point that co-laborative work need not be restricted to work amongst scientists:

I'm ethically convinced and committed to the fact that if you go across the street and talk to any group of people, you're going to find reflective people there. The guys who work in the labs or the patients that Rayna Rapp studies are not themselves doing science – they're not technicians of general ideas – but they're reflective about their lives, and their lives are cut by these scientific or legal or financial vectors of truth claims.⁵⁸

This makes clear that co-laborative anthropology addresses its former informants differently. They are not experts in the sense of keepers of specialised knowledges and they are not naïve experts of their own everyday lives. They are able to reflect about how their own lives are cut by different worldings. In this way, they are experts and good to think with about which worldings are significant worldings of the contemporary and how. Importantly, this does not privilege their analysis and interpretation as was often the case in the ethically motivated 'giving voice' type of collaboration. Rather their analysis and interpretation of the world is being juxtaposed or diffracted in co-laboration with the researchers' terms and positions. The result may be something co-laborators share. More likely, however, all participants will learn their own lessons from such processes and revise their interpretations of empirical material accordingly.

From interdisciplinarity to experimental entanglements

All kinds of collaborative research are being promoted recently under the label of trans- and interdisciplinarity, mode 2 knowledge production or co-production. One cannot help but agree with Des Fitzgerald and Felicity Callard who refer to this rhetoric of interdisciplinarity as "arid".⁵⁹ The 'regime of the inter-' holds little promise for exciting intellectual advances. It is largely driven by the need to acquire research funding and, at the level of science policy, it represents a strong trend that ostensibly corresponds with promoting a type of science that pretends to be closer to the real problems in the world; problems, which often upset the established intellectual division of labour.⁶⁰

Co-laborative anthropology has thus little to do with building research consortia, strategic alliances, synergistic research development or an additive understanding of knowledge.

While the rhetoric of interdisciplinarity has little to offer to a co-laborative agenda, a different picture emerges from a look at the role of ethnography in projects that cross the boundary between natural and social science. In an analysis of such efforts, British sociologist Andrew Barry and his colleagues distinguish three modes of interdisciplinary collaboration: an integrative-synthesis mode aimed at adding expertise up to a more comprehensive understanding; a subordination-service mode where one discipline provides external expertise to solve a disciplinary problem; and an agonistic-antagonistic mode. This latter mode operates in a logic of ontology and is characterised by one discipline trying to change the ontological status of the object of research in another discipline.⁶¹ For example, anthropology may try to infuse the life sciences with a more social understanding of the individual. This is an attractive proposition and one that moves away from the arid rhetoric of interdisciplinarity towards a collaborative ethnographic agenda. Yet it falls short of a co-laborative effort for two reasons: (1) It is one discipline deciding for itself that it wants to change the ontological status of an object of research in another. The decision to do so and how to do it does not derive from a co-laborative process between the two disciplines. Many projects in STS take this form. They are rooted within a deconstructive critique but by turning their revelation of contingency into propositions for the field, they hope to produce a productive intervention. This approach rests very much on the genius of the individual researcher, because the task of convincing another epistemic culture to change its thought style is immense. (2) A co-laborative effort is not a teleological process. It does not pursue a shared goal or follow a clearly defined strategy. It is an open process amongst technicians of general ideas willing to experiment with different ways of worlding. One thing that may come of such co-laborative efforts is a change in thought style, which a participant may take back into her or his own epistemic culture as a *Widerstandsavis*, i.e. as an element of resistance against the dominant thought style within a particular thought collective.⁶²

Hence thinking through co-laboration in current (inter-)disciplinary terms is difficult. I am much more taken by the suggestion of 'experimental entanglements'.⁶³ "Experimental entanglements are modest, often awkward, typically unequal encounters that work to mobilize specific and often serendipitous moments of potential novelty in and outside the laboratory." They "refuse preliminary decisions about the shape or outcome of such an interaction [...], denote an ad hoc process of shuffling histories, methods, and assumptions [and] are thus never not temporary, local assemblages of motivation, interest, people and machinery – in which we, and our collaborators, are able momentarily to think something exterior to both the conventions of experimental practice, and the taken-for-granted dynamics of epistemic power that underwrite its conduct." Fitzgerald and Callard

(F&G) work on the basis of their own entanglements in the neurosciences. With the notion of 'experiment' they literally mean experiments in the sense of laboratory experiments rather than in a looser sense of tinkering with a fixed and controlled set of ingredients. One might argue that experiments are anything but the open processes that F&G want them to be in order to produce these multifarious affordances.⁶⁴ STS has spent considerable time showing how experiments trim nature on the basis of prior expectations such that the experimental outcome must lie along a narrow dimension to which the experimental measurement apparatus is attuned. Yet F&G take their cue from Rheinberger and his reading of the experiment as a space that must contain epistemological and ontological excess to succeed, i.e. contain layers, remnants and traces of alternative worldings. It is these traces that F&G treat as affordances. These affordances are explored within a more open process that develops from the encounter of several participants each willing to treat an experiment not only as a positive fact-producing machine, but also as something akin to Deleuzian desire machines: continuously reintroducing contained excess to the wild in unexpected places and modes.⁶⁵ Andreas Roepstorff and his colleagues capture this productive moment of experimenting at the intersection of anthropology and neuroscience by distinguishing experiment as method, object of research and aesthetic of research practice.⁶⁶ This aesthetic emphasises doing research together, using texts and tools together, analysing and writing together, while always being alert to emerging trajectories and reflexive about one's own practice as well as the social reproduction of these practices. Roepstorff suggests that this approach to experimental entanglement would not need to be called neuroanthropology or follow in any other way the 'regime of the inter-'. It would simply be anthropology.

Conclusion: crafting a widerstandsaviso

The physician, microbiologist and philosopher of science Ludwik Fleck in his 1935 book "Genesis and Development of a Scientific Fact" coined the phrase Widerstandsaviso.⁶⁷ A Widerstandsaviso⁶⁸ is a point of resistance that arises from 'reality' within the practices of a thought collective. It is the world kicking back, in today's STS parlance.⁶⁹ Fleck envisages epistemic work to proceed from chaotic thinking through an increasing thought constraint towards a directly recognisable Gestalt: the scientific fact (Tatsache) always defined in relation to a particular thought collective and a particular thought style. The Widerstandsaviso is the initial kernel of resistance to thought caprice and thus the beginning of a journey towards a fact.

In my mind, co-laborative research is about crafting Widerstandsavisos that work the other way around. The co-laborative Widerstandsaviso is the kernel of resistance that increases thought caprice

against the thought constraint in the existing collective. This is not straightforward intervention. It is not teleological and it is not about providing answers for a discipline. To the contrary: it is about providing different problematisations and different questions. It is about re-establishing moments of thought caprice. It is about providing a space for co-laborative research within which seeds of resistance and seeds of reflexivity are produced, which may become generatively irritating when taken back to their respective thought collectives or epistemic cultures. It is then up to the collectives to adjust their thought style such that the irritation subsides – or not as the case may be. The *Widerstandsaviso* must not be confused with the tactical resistance of the subaltern against hegemonic ideology, discourse or regime. While it shares some political sympathies with these approaches, it is restricted to epistemic cultures and knowledge production rather than social action in general and it is certainly not grounded in epistemic certainties.

For the anthropologist, co-laboration is about building a *Widerstandsaviso* to take back into anthropology. This is what I mean when I argue that co-laborative work for me is a distinctly disciplinary project. In the case of anthropology co-labouring with the life sciences, this may be about another understanding of the role of materiality in shaping human co-existence. For example: Learning within a co-laborative process with molecular biology about the inheritability of materialised forms of social difference may lead an anthropologist to challenge Bourdieusian social theory and its a-material understanding of habitus and hysteresis.⁷⁰ Co-labouring with earth and geoscientists on social-ecological transformations may force anthropologists to consider the importance of spatial explicitness in the analysis of material-semiotic practice or think through how patterns of practice may be analysed when having to move across scales. Many other examples already exist.⁷¹ And, of course, there is always the hope that the participating fields may change for the better in the sense that the co-laborative effort will lead to something that can be taken back to their discipline and that their thought style will adjust in line with anthropological theory and reasoning. Neuroscientists may try to produce fMRI experiments that account for the embodied and social nature of communication.⁷² Geoscientists may begin to care about theories of globalisation, exchange and practice developed in anthropology.

These are just some suggestions from my own work and the work of my close colleagues. It is important not to understand them as teleological processes. Recounting them as a coherent narrative makes it sound as if these projects were strategic efforts set-up with a clearly identified research desideratum and research programme. This could not be further from what actually happens in co-laboration. All of these projects have emerged from continuous encounters between different technicians of general ideas: reading groups, joint empirical work, visiting conferences

together, writing together, designing and conducting experiments together and – all too rarely – sharing a meal and talking about something else but science.

Co-laborative anthropology is difficult research and it is demanding on anthropologists. It requires anthropologists to work themselves deeply into the logics and experiences of often highly specialised fields of expertise. It also requires effort and good fortune to find people in those fields who are interested in acting as technicians of general ideas. It demands that the anthropologists have their own framework of analysis and interpretation challenged by other ‘technicians of general ideas’ and seriously confront those comfort zones of theorising and critique. And it requires the material resources to create spaces and formats that afford the emergence of *Widerstandsavisos*. Most importantly, it requires time to build relationships and by time I mean many years of ongoing interaction. Only through time can relationships develop wherein researchers are prepared to push aside the everyday pressures of university-based science, suspend their scepticism towards the respective Other and engage in joint epistemic work. Scholars are only just beginning to chart the different practices with which people are experimenting and it will be some time before it becomes clear what this mode of anthropological knowledge production may have to offer to the discipline.⁷³ I hope it will strengthen anthropological reflexivities and infuse the discipline with some ideas that are good to think with. In any case: I am convinced that many of the important anthropological questions today require co-laborative efforts. As Paul Rabinow says: thickness today is localised differently.⁷⁴ It is not adequately captured by individual fieldwork and it is not addressed by collaboration. Perhaps co-laboration can help to produce the anthropological reflexivities necessary to retain thick inquiry as the signature mode of anthropological knowledge production.

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¹ Cf. Bloch 2005

² Rose 2001; Crutzen 2002

³ E.g. Nature 2012; US National Research Council 1998

⁴ See Fitzgerald and Callard 2014 for a more elaborate discussion of these two modes of engagement.

⁵ See Lindner 1981 for a more detailed discussion of this anxiety (in German).

⁶ Nader 1969

⁷ Boyer 2008

⁸ Hannerz 1996

⁹ The notion of thought style and collective refers to the work of Fleck 1935/1979

¹⁰ Boyer forthcoming I am discussing Boyer's lucid analysis of such encounters with respect to anthropological reflexivity further below.

¹¹ Cf. Harris 2007; Boyer 2005

¹² Stehr 1994

¹³ Cf. Jasanoff 2004; Nader 1996; Star 1995; Strathern 2006

¹⁴ Foucault 1970

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- ¹⁵ Hannerz 2001; Appadurai 1991
- ¹⁶ Star 1995; Ingold 2011
- ¹⁷ The German language offers besides 'wissen' as the act of knowing cognitively, the verb 'kennen', which is closer to being acquainted with, and 'können' as having the ability or skill to do something. It is perhaps worth noting that the English language connects all three practices with knowing: knowing, knowing someone, knowing how to do something. See also Ingold 2010
- ¹⁸ Beck 1992; Beck 1994
- ¹⁹ Hirschauer 2008, see further detail in the section on anthropological reflexivity below.
- ²⁰ Latour 2005
- ²¹ Cf. Lassiter 2005
- ²² Deloria (1998: 84) cited in Lassiter 2005
- ²³ Fletcher and La Flesche 1911; Fletcher, La Flesche, and Fillmore 1893
- ²⁴ See the literature cited in Lassiter 2005 for further detail on these cases.
- ²⁵ E.g. Hart 1998 and other texts from the 'Cambridge Anthropological Expedition to the Torres Straits' centenary conference.
- ²⁶ Burawoy 1998; Frankenberg 1965
- ²⁷ Jeggle 1977; Schmoll 2009; Rach and Weissel 1978
- ²⁸ E.g. Clifford, Marcus, and School of American Research 1986; Asad 1986; Rabinow 1986
- ²⁹ Marcus 1997
- ³⁰ Scheper-Hughes 1995
- ³¹ See e.g. Binder et al. 2013 for a detailed discussion of different strands of engaged European Ethnology/gender studies (in German)
- ³² Boyer forthcoming
- ³³ Lassiter 2005: 83; 2008; Lassiter and Campbell 2010. See also the intense debate in *Current Anthropology* and beyond: Scheper-Hughes 1995; D'Andrade 1995; D'Andrade 1995; Kuper and Marks 2011
- ³⁴ Curating concepts is an idea that Paul Rabinow develops to draw together intellectual and ethical work. Rabinow and Bennett 2012 "Good to think with" is a criterion for knowledge that Marilyn Strathern finds useful. Strathern 2002
- ³⁵ Faubion 2009: also cited in Boyer, forthcoming.
- ³⁶ Boyer forthcoming
- ³⁷ Rabinow 1996
- ³⁸ Collier 2004
- ³⁹ Hirschauer 2008
- ⁴⁰ Deeb and Marcus 2011: 68
- ⁴¹ Boyer forthcoming
- ⁴² These need not be scientific disciplines. They could also be multi-national companies, markets, regulators or any other part of the architectures of modernity with the ability to shape X through a hegemonic discourse.
- ⁴³ Cf. Rabinow et al. 2008
- ⁴⁴ Verran 2001
- ⁴⁵ Stengers 2005
- ⁴⁶ Latour 2001, 2004, 2005
- ⁴⁷ Law 2006
- ⁴⁸ Hacking 2006
- ⁴⁹ Barad 1999; Barad 2007: 139
- ⁵⁰ Martin 1997
- ⁵¹ Tsing 2010: 47-48; Tsing and Matsutake Worlds Research Group 2009; Tsing 2005
- ⁵² Star 1999; Bowker and Star 1999; [Anonymisoitu] 2014b, a, 2015
- ⁵³ Annemarie Mol. Personal communication during a tram ride at the 4S/EASST conference in Rotterdam 2008.
- ⁵⁴ Haraway 1997
- ⁵⁵ Sørensen 2009
- ⁵⁶ On the related notions of thick and multi-sited comparison see [Anonymisoitu] and [Anonymisoitu] 2010c, a, b; Sørensen 2008
- ⁵⁷ Scott 1998
- ⁵⁸ Rabinow et al. 2008: 78
- ⁵⁹ Fitzgerald and Callard 2014: 4
- ⁶⁰ Cf. Nowotny 2005
- ⁶¹ Barry, Born, and Weszkalnys 2008

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- ⁶² Fleck 1935/1979, see section 'crafting a Widerstandsavisos' below for further detail
- ⁶³ Fitzgerald and Callard 2014: 16
- ⁶⁴ Gibson 1977
- ⁶⁵ Deleuze and Guattari 1987
- ⁶⁶ Roepstorff and Frith 2012
- ⁶⁷ Fleck 1935/1979
- ⁶⁸ There is some debate as to the origin of the word 'avisos'. I like best the etymology from vulgar latin *mihi visum est = it occurs to me*.
- ⁶⁹ Barad 1998
- ⁷⁰ For further details see [Anonymisoitu] 2011
- ⁷¹ Marcus 2010; Meloni 2014; Rabinow and Bennett 2012; Rose and Abi-Rached 2013; Fitzgerald and Callard 2014; Roepstorff and Frith 2012; [Anonymisoitu], [Anonymisoitu], and [Anonymisoitu] 2010
- ⁷² E.g. Kuhlen, Allefeld, and Haynes 2012
- ⁷³ For first attempts at comparing experiences, see <http://somatosphere.net/2014/11/entangled.html> (last accessed: 10.01.2015)
- ⁷⁴ Rabinow et al. 2008